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THE NATURAL HISTORY OF EXPERIENCE.

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The standpoints of science and metaphysics—The scientific method of interpretation—The relation of mental process to physiological process—An interpretative ideal construction—Some implications of the doctrine of continuity—A hybrid universe of discourse—Instinctive and intelligent behaviour—The primary tissue of experience—Metaphysical criticisms of a scientific conception in genetic terms—Sketch of the stages of development of experience—Plea for keeping distinct the scientific and the metaphysical universe of discourse.

I PROPOSE to approach the problem of the genesis of experience in the individual mind along lines that are purely naturalistic, through the avenue of biological and psychological considerations. I propose also to consider the relation of this problem to that with which the metaphysician is concerned. I shall endeavour to render clear what I understand by science: what by metaphysics. The standpoint and the initial postulates of science are profoundly different from those of metaphysics. So radical is the distinction that the student of the one branch of human enquiry has some difficulty in understanding what the votary of the other is driving at. For the student of science, seeking to give some systematic statement of the natural history of experience self-consciousness, as a mode of that experience, is the *terminus ad quem* to which, or towards which, the developmental process leads up. For the votary of metaphysics, seeking to elucidate the ultimate ground of experience, self-consciousness is the *terminus a quo* from which he starts forth on his quest. Thus T. H. Green says[1] that self-consciousness is "at its beginning formally or potentially or implicity all that it becomes actually or explicitly in developed knowledge." Hence it follows that "a natural history of self-consciousness is impossible since such a history must be of events and self-consciousness is not reducible to a series of events." For the student of science, again, ideational knowledge is developed from and out of the perceptual experience which is its precursor; but for the votary of metaphysics the very beginnings of experience imply the existence of an ideational subject. For him "sensation has no meaning apart from thought." "In order to the impress of any impression being conscious, is not," he asks, "the existence of a self—that is to say of a subject capable of being impressed,—necessary?" and he replies "we must first assume the existence of a conscious self[2]." Thus, for the student of genetic psychology as a branch of science, the "concept" is the outcome of development from fore-running perceptual experience; but for the votary of rational psychology, as an application of metaphysical principles, the "percept" is the particular and concrete example through which the pre-existing "concept" is rendered explicit or actually realised. "The universal (concept) is the archetype of which the individuals (percepts) are an illustration."

It appears to me that for one who would presume to deal with the genesis of experience from what he regards as the scientific point of view, it is before all things necessary, that he should distinguish as clearly as possible this point of view from that of metaphysics. If I say, to begin with, that science, as such, does not seek to explain anything, knows nothing of the cause or causes of phenomena, and makes no reference to any power or agency, I must hasten to qualify these assertions by adding—in the sense in which the metaphysician uses these terms. It would not a little conduce to clearness of thought, and would prevent much confusion, if those men of science who accept the views I seek to state, could be induced to abandon these terms altogether when they are dealing with the philosophical aspect of their subject. This, however, is too much to hope for. The next best thing is to define exactly what is meant by these terms as they are used in a scientific universe of discourse. Let us take as a concrete example the formation of a crystal in an appropriate solution. The metaphysician

[1] *Introduction to Hume*, Vol. I. of Treatise, p. 166.

[2] Wm Knight, *Hume* (Blackwood's Philosophical Classics), pp. 137, 140.

explains this by reference to an underlying cause, such as a principle of crystallisation, through the agency of which it is produced; or his explanation may take a theological turn as he bids us regard the crystal as one of the innumerable examples of the exercise of Divine Power. I am not, be it noted, scoffing at a metaphysical interpretation. I merely seek to distinguish it from that of science, according to which the growth of the crystal is explained when it is referred to the general rules or laws which, as a matter of observation and inference, hold good in the particular case and in other like cases. For science the cause of the production of the crystal is nothing more and nothing less than the antecedent and accompanying conditions which may be observed or inferred from the fullest and most minute study of all the phenomena and of nothing but the phenomena. Modern science has given up all reference to Crystalline Force or any such agency by or through which the crystal is produced; and if in other cases the term "agency" is employed—when we say, for example, that an engine is driven through the agency of steam—it is obvious that the sense in which the word is used is a different one.

Now when one is dealing, not with a crystal which is differentiated within a solution but with a percept which is differentiated within experience, I conceive that the same limitations should be imposed on scientific treatment. The metaphysician, no doubt, may explain it by reference to an underlying cause, the conscious ego, through the agency or self-activity of which it is produced; but the man of science can only explain it by reference to the antecedent and accompanying conditions in relation to the generalisations which have been found to hold good in such cases. It cannot be too roundly asserted that for psychology as a science (in the sense in which I, for one, accept its limitations) the mind is *not* an active agent or producing cause. Professor Knight tells us[1] that "the notion of mind as a passive product of external influence—and not at the same time an active agent or producing cause—is a radical flaw in the psychology of Hume." I am not sure that I fully understand the exact implications of the phrase "a passive product of external influence"; but I am quite sure that the description of mental processes as a series of happenings concerning which generalisations may be formulated is too often vitiated by the radical flaw of interpolating reference to metaphysical conceptions. Let me once more repeat that not for one moment do I presume to deny the validity of

[1] *Hume*, p. 148.

metaphysical explanations. My sole contention is that they are wholly out of place in psychology, if psychology is to be correlated with other branches of science.

The method of science as it is applied in the study of inorganic nature is to reach by induction from an adequate number of carefully conducted observations a generalisation, to frame an ideal construction within which it is assumed that this generalisation is universally true; and to test its universal truth by applying it to further cases of like order and by submitting it to the test of deductive verification. We say, in effect:—If the generalisation be true, as assumed, then this or that will follow as a logical conclusion and may be put to the test of further observation. For example, if the revolution of the planets round the sun be a true generalisation and if the earth be travelling on its orbit, there should be an apparent shifting of the position of the stars in accordance with another generalisation concerning the aberration of light. This can be verified by observation. Always and in all cases the (postulated) universal validity of any sound induction is tested by its consequences for further scientific procedure and its application in further observation. So far with regard to the several inductions of science. But there is one all-embracing induction, the universal validity of which is postulated by science, as common to the whole range of scientific procedure. This is the uniformity of nature. The only justification for its validity is—the whole system of scientific knowledge as (*a*) a rational system, and (*b*) a system which can on these terms be applied to the elucidation of the observed facts.

It should be noted that in saying that any scientific generalisation is universally true we may mean one of two things: (1) that it is absolutely and unconditionally true *within the ideal construction as such*; or (2) that it is universally true *in* the world of perceptual experience or *of* the objective reference in that experience. The latter assertion, in any absolute or unconditional sense, is beyond the scope of science. All that we have any right to say, within the universe of discourse of science, is (1) that any given generalisation is true within the limits of exact observation and measurement; (2) that it has, so far, not been proven false in any case; and (3) that, since it works and aids us in the interpretation of further observations, we shall continue to accept it as true and postulate its universality, until it *is* proven false. In other words the regulative ideal constructions of science are only working approximations to the constitutive truth of the world of objective reference for experience.

I conceive that mental happenings, no less than any other happenings in this wide and varied universe, afford material for scientific study and should be studied under the recognized canons of scientific procedure. Any given mental process has constituent factors which are definitely connected in accordance with the rules or laws of such occurrences. The process, as part of a continuous series, is connected with certain foregoing events and leaves its impress on events which follow. It is part of the business of science to observe, describe and interpret these psychological occurrences, to formulate generalisations concerning them within an ideal construction, and to bring them into relation with physiological events, and with happenings outside the body in the surrounding world.

There can be little question that physiological events form part of the same continuous train of happenings as comprises also those events which we describe as physical. An organism subjected to stimulation exhibits responsive behaviour. No matter how much or how little of the metaphysics of vitalism be falsely (from the standpoint of science) introduced, here is a series of events of the same physical order—a series which we believe to be capable of interpretation, though they may not as yet be adequately interpreted, in terms of antecedence, co-existence and sequence. It is wholly beside the question to say that physiological events have a specific character of their own which serves to distinguish them from other physical and chemical events. No doubt they have. It is the aim of physiology to determine these differences as matters of fact. It is not the aim of physiology, as a science, to enquire why the facts are what they are. There is really no difficulty here, for science, if metaphysical questions be excluded—excluded, be it noted, not from the field of human enquiry but from a specific universe of discourse.

The relation of mental process to physiological process does however present serious difficulties to the investigator who seeks to keep within the limits of scientific interpretation. I can but indicate here what appears to me to be the scientific position. In the first place it should, I conceive, be frankly admitted that of direct evidence of connexion between mind-process and brain-process there is little enough. On the other hand there is a very considerable body of indirect evidence which, when it is critically examined, justifies the current belief that such a connexion of a peculiarly intimate kind exists. In the second place I am not aware that there are any scientific grounds for inferring or assuming that brain-process is the *antecedent* condition (and therefore in the terminology of science the cause) of mind-process or *vice versâ*. The hypothesis of co-existence or concomitance appears to be more acceptable, so long as it is regarded frankly as a working hypothesis. In the third place the doctrine of interaction and that of parallelism must both be set aside; partly because they are from the standpoint of science unnecessary, partly because they are charged with metaphysical implications.

Revert then to the organism which responds under the stimulation of the environment in such a way as to lead us to believe that the response is of the intelligent order. That implies that in accordance with the ideal construction in terms of which such behaviour is interpreted by science, a specific mode of stimulation is the antecedent condition of a psycho-physical disposition which is in turn the antecedent condition of the resulting behaviour. Broadly speaking, and regarding the sequence of events as a whole, the interpretation is in accordance with the canons of scientific procedure. At the same time it is necessary that we should distinctly realize and not in any way attempt to slur over the fact that, in so far as certain links in the chain of antecedence and sequence are psycho-physical, they differ from those other links which are, so far as we know, physical only. This from the standpoint of science we must be content to accept as a fact, or, if it be preferred, as something which we postulate in the ideal construction in terms of which an interpretation of the facts is formulated.

Presumably the latter mode of statement will be regarded as the more satisfactory. Let us grant then that, in the interpretative scheme in accordance with which we endeavour to describe and interpret intelligent behaviour, the occurrence of psycho-physical links in the chain of antecedence and sequence is postulated. We may study these psycho-physical dispositions either from the physical and physiological aspect or from the mental or psychical aspect. But what exactly do we mean by aspect? I take it that we mean or should mean nothing more than that the enquiry is conducted in each case from a specific point of view. Just as we may consider the merits of a rose in reference first to its form and then to its colour so in dealing with psycho-physical events we may concentrate our attention on either their psychical or their physiological aspect in accordance with the exigencies of the enquiry. There is no metaphysical implication in this use of the word "aspect." It simply stands for the point of view from which the same occurrences may be studied.

Of course it may be said that we cannot directly observe in our neighbours the psychical aspect of psycho-physical occurrences, in the same way that we can see both the form and the colour of a rose. That is true enough. But if science is to be restricted to matters of direct observation not only will modern psychology but modern physics also have to be carried on under a different name. Science deals with ideal constructions in terms of which observable facts may be interpreted. The molecule and atom of the physicist, the psycho-physical disposition of the psychologist, are ideal constructions, of value only in so far as they contribute to a scheme of interpretation. We may follow Sir J. J. Thomson in regarding their acceptance as "a policy rather than a creed"; but a successful policy is one which we trust and in the value of which we believe.

Regarded from the physiological aspect psycho-physical processes are, in accordance with the ideal construction under consideration, whether we regard it as a policy or a creed or both, continuous with other physiological processes as these in turn are continuous with those occurrences in the environment which precede and accompany stimulation and those occurrences which accompany and follow response. There is one continuous sequence susceptible of interpretation in terms of changes of configuration in one material system. No doubt the physiological changes which are the concomitants of mental processes are highly specialised and run their course in accordance with the specific biological rules or laws which characterise them and which serve to distinguish them from sundry other changes in the material world. But this does not imply any breach of continuity. So far as we know there is within the continuous process no leakage of energy and no influx of energy. So far as we know the law of the conservation of energy and that of the conservation of moment of momentum hold throughout. That in any case is postulated within the determinate ideal construction which deals with the physical aspect of psycho-physical interpretation.

When, however, we turn to the other aspect we enter a new universe of discourse whose subject-matter is of a wholly different order. It includes sense-impressions, percepts, concepts, and judgments, memories and anticipations, pleasures and pains, and so forth. These are organised and developed within the field of conscious experience; but the laws of their sequence and the generalisations concerning their mode of development are different from those of physics or physiology. Of course there is a sense in which it may be said that this universe of discourse, including as it does include, the whole range of experience and human knowledge, is all-embracing. It is, no doubt, true enough that all our acquaintance with, and all our knowledge of, the physical universe is nothing less and nothing more than an elaboration of conscious experience. We may go further and say that, for scientific interpretation, that is to say apart from metaphysics, there is no valid distinction between the external world as it is, and the external world as it exists for experience and knowledge. With what the world is or may be independently of experience science has no more concern than the plainest and most unsophisticated common sense. But for science, as for common sense, the interpretation of experience is organised and elaborated in terms of a duality of reference—on the one hand a reference to the external world and all the objects of experience therein contained, and on the other hand a reference to the conscious experience to which the world and its objects are, in current phraseology, presented. Ideal constructions in terms of the former reference fall within the physical and physiological universe of discourse; ideal constructions in terms of the latter reference fall within the universe of discourse which deals with conscious or mental processes. The group-name for the product of the one ideal construction is the world; that for the product of the other ideal construction is the mind.

But neither product is adequate so long as it is regarded as the outcome of merely individual experience, no matter how completely this experience is systematised. It will, at any rate be admitted that my personal experience and knowledge in its world-reference is by no means co-extensive with the world as a product of ideal construction. And it will also be admitted that my personal experience and knowledge in its self-reference is very far from exhausting the universe of discourse which deals with mental processes. It is wholly unnecessary to adduce evidence in favour of the obvious fact that our ideal construction of the world on the one hand and of mind on the other hand are social products. Their existence depends on inter-subjective intercourse and the co-operation of many people. But how is this social co-operation effected? In many ways, from which we may select spoken language and the written or printed record as conspicuous examples. It is effected by means which fall within the physical universe of discourse. The point to which I desire to lead up is this: the mental universe of discourse can only be unified and rendered consistent for scientific interpretation by introducing connecting links which lie outside that universe and belong to that of world-reference.

If now we revert to the conception of psycho-physical processes as links in a continuous chain of causes and effects, or of antecedent and consequent occurrences; and if we agree that they may be studied either in their physical or in their mental aspect; we find that in the former aspect they are continuous with other physical processes, but in the latter aspect they form isolated systems. To relieve them of this isolation we must either (1) pass over to the physical aspect of which they are the concomitants, or (2) accept the hypothesis that *all* processes are psycho-physical.

Whatever may be said in support of the latter hypothesis from the point of view of philosophical theory, there is little to be urged in its favour from the standpoint of scientific interpretation. Even if it be granted that not only do certain nerve-centre occurrences have mental or psychical concomitants, but that all organic processes have a conscious or quasi-conscious sentient aspect, it is not obvious in what way, if any, scientific interpretation is advantaged. It would seem that of this quasi-conscious aspect, concomitant let us suppose with the segmentation of the fertilised ovum, we know and can know nothing. *A fortiori* therefore the assumption that all mechanical, physical, or chemical processes are of like nature—that they too are psycho-physical—does not appear to be of the smallest service for scientific interpretation. The man of science, with his strong pragmatic tendencies, will ask what is the use of any such assumption; and it is difficult to give him a satisfactory reply. Regarding the matter solely from the standpoint of scientific interpretation the truth or error of such a view does not seem to be a living question. Admittedly useless as a policy it may however be accepted as a creed by those who are unable to conceive the development of consciousness save out of that which bears with it at least the elements or germs of the conscious order of existence.

Apart from such a speculative creed which we may leave on one side, though it is one to which I myself provisionally incline, we must either frankly acknowledge that mental processes, as they occur in individual organisms, form isolated systems, or we must link them up by passing over into the world of physical reference. That, in fact, is what even those who accept the speculative creed actually do as a matter of methodological procedure. And the science of psychology, as that which deals or attempts to deal with mental processes, definitely accepts this latter alternative. Modern psychology as a science fully, frankly and wholly adopts in its methods of interpretation what we may term a hybrid universe of discourse in part physical and in part psychical, not only in dealing with inter-subjective intercourse between different persons, but also in treating of the psychology of the individual. By its doctrine of concomitance it weds the two universes. Fully alive to the fact that the rejection for practical purposes of the assumption that all processes are psycho-physical involves the abandonment of the conception of continuity in mental development and places mental phenomena outside the pale of evolutionary treatment, genetic psychology faces the situation boldly. It accepts continuity as part of and as essential to the evolutionary thesis; but it regards some of the links of the chain as not only physiological but as also psycho-physical. Modern science broadens its conception of biology so as to include the concomitant mental processes; and in like manner it broadens its conception of psychology so as to include the concomitant physiological processes. Refusing to be bound by limitations which restrict it to the purely psychical realm, genetic psychology claims that mental happenings, as aspects of natural phenomena, are only comprehensible, only continuous, only susceptible of scientific treatment in so far as they are concomitant with happenings in the brain and are thus related to other biological processes and through them to events in the physical environment.

Revert now to the assumption that, for purposes of scientific treatment, and apart from the unverifiable and speculative creed of panpsychism, there is no direct psychical continuity between the mind of parent and the mind of offspring. This implies that the mental organisation of the individual, *quâ* mental, starts *de novo*. But the physiological organisation does not start *de novo*. There is direct continuity between the psycho-physical happenings of the adult brain and the physiological happenings in the fertilised ovum. It is true that in accordance with the teaching of modern biology the direct physiological continuity is that of the germ plasm and may be represented diagrammatically as under, where g g' g'' are the fertilised ova derived from the germ plasm while b b' b'' are brain structures differentiated from the fertilised ovum.

$$\begin{array}{cccccccc} b & & b' & & b'' & & b''' & \\ | & & | & & | & & | & \\ g & — & g' & — & g'' & — & g''' & — \end{array}$$

It may be noted that there is here no direct continuity between brain and brain though there is direct continuity of cell products. If now we convert this structural schema into a functional schema we have

$$\begin{array}{ccccccc} ps & . & ps' & . & ps'' & . & ps''' \\ \vert & & \vert & & \vert & & \vert \\ p\,. & — & p'\,. & — & p''\,. & — & p''' \end{array}$$

Here $p\,.\,p'\,.\,p''\,.\,p'''$ are physiological processes and $ps\,.\,ps'\,.\,ps''\,.\,ps'''$ are psycho-physical processes. Since the latter are also physiological processes there is continuity from the biological standpoint. There is no continuity from the psychical standpoint. In the view here taken, however, psychology combines (and in order to be a science must combine) both standpoints in one hybrid universe of discourse and is thus, and thus only, in a position to discuss problems of development and evolution.

The functioning of the organism as a whole in relation to the environment is what we may broadly term behaviour. Only through behaviour is experience unified. I shall assume that in the higher organisms such behaviour involves the functional activity of the central nervous system. I shall apply the term "instinctive" to those factors in behaviour which are prior to individual experience, and the term "intelligent" to that behaviour which involves what we may term "factors of reinstatement," such reinstatement being dependent upon previous experience, the net results of which are revived. The instinctive factors depend entirely on how the nervous mechanism has been built up through heredity under that mode of racial preparation which we call evolution; intelligent behaviour depends also on how the nervous system has been modified and moulded in the course of that individual preparation which we call the acquisition of experience. Let us however descend to particulars.

Some years ago I had under observation two young moorhens or waterhens which I had hatched in an incubator and watched from day to day with some care. One of these about two months old was swimming in a pool at the bend of a little stream in Yorkshire. A vigorous rough-haired puppy, highly charged with canine vitality, ran down from the farm, barking and gambolling. In a moment the waterhen dived, disappeared from view, and reappeared beneath the overhanging bank. Now this was the first time the bird had dived. I had repeatedly endeavoured to elicit this characteristic instinctive response but had failed to secure the appropriate presentation which should supply the necessary conditions of stimulation. It is of course difficult to say how much in this dramatic situation was new to the experience of the waterhen. Unquestionably there were many "factors of reinstatement" gained as he swam about in the stream. There was an already-established body of experience which could assimilate the newly introduced instinctive factors. But one may feel justified in saying that there was something about the total puppy-presentation which was so far new as to elicit an instinctive response which supplied to experience the group of kinæsthetic factors that accompanied the new mode of behaviour in diving. I do not think that the young bird had ever really been scared before; and we may probably infer that there was a specific quality of emotional tone which had not been hitherto felt. When I took it out of the water the bird was panting, its heart-beat was strong and quick, and I dare say it had queer sensations in its little gizzard.

If then I interpret the situation aright there was, concomitant with the brain processes of the waterhen as he swam in the pool, a certain amount of experience actually present, and a certain amount of individual preparation of the brain such as to afford the neural conditions of revived experience. So much to begin with. Here we have the waterhen as actual or potential experiencer. Then comes a new situation which the experiencer can assimilate. In this case, in so far as a new instinctive response is called forth, the conditions are largely supplied by the racial preparation of the nerve-centres as the outcome of evolutionary process. The new factors comprise (1) a specific presentation differing from previous presentations, (2) a specific response affording new data to behaviour-experience, and (3) a hitherto unfelt quality of emotional tone. But though we may analyse the newly experienced situation in some such way as this the bird presumably gets the whole as the coalescent net-result with a bearing on behaviour. It is not so sophisticated as to place its felt presentation, its felt instinctive response, and its felt emotion into separate chapters and only come to realise by effort of thought that experience is one and indivisible. He just lives through one palpitating situation, assimilates its teachings, and emerges from the ordeal a new bird. As experiencer he is never again what he was before.

We started with our birdling as experiencer swimming about in the stream. How did he reach this level of conscious organisation? There was a time when he had no experience of water or swimming. I remember the day when I first placed him gently in a tepid bath. Even then he was an experiencer, though his store of factors of revival was exceedingly limited. Of swimming experience he had none. Racial preparation had however fitted the tissues contained within his black fluffy skin to respond in a quite definite manner. And in the first act of swimming there were afforded to his experience, analogous factors to those I have given above in considering his later dive—a specific presentation, a specific response, a specific emotional tone, all coalescent into one felt situation. And if we go yet one stage further back when the moorhen was struggling out of the cramping egg-shell, there came what we may fairly regard as the initial presentations, followed by the initial responsive behaviour in the earliest instinctive acts, accompanied, we may presume, by the initial emotional tone, coalescent to form what I have ventured to term the primary tissue of experience. Thus I conceive that, for scientific interpretation, experience has its genesis. A number of instinctive responses occur in virtue of the organisation established by centuries of racial preparation as the outcome of natural selection or of other factors in organic evolution. These run together, overlap, coalesce and unite synthetically to form a primary body of experience. Just as there is one waterhen with inter-related parts and organs, one central nervous system correlating the incoming data of presentation and co-ordinating the outgoing nerve-impulses in responsive behaviour, so too there grows up in concomitance with the brain-processes one experience for which the presentative data acquire meaning and become percepts for the guidance of further behaviour. Thus is it, I conceive, in the case of the moorhen: thus is it in the case of the human infant. Such in all cases is the starting-point of the natural history of experience, the unification of which finds expression in behaviour.

I am well aware that the metaphysician, if he should chance to cast a passing glance over these benighted lucubrations, will groan in spirit and, if he be tender-hearted, pity for his lack of insight or ineptitude an erring fellow mortal. For a century he has criticised Hume's doctrine of the origin of knowledge from the more vivid impressions and their fainter echoes in ideas. He has repeated, until he is weary with well-doing, that "a sensation never exists, and cannot possibly exist, without a conscious subject," that "sensation has no meaning apart from thought," that to have any being as a constituent part of experience it must be known, and obviously to be known presupposes a knower. All this, he insists, is familiar to the veriest tyro in philosophy. And here in the twentieth century comes along a third-rate biologist who has meddled a little with psychology and repeats in a hashed-up form, garnished with evolutionary terms, the exploded fallacies of the so-called philosopher of Ninewells. And the pity of it is that after reading so many metaphysical sermons, and really enjoying them, perhaps even profiting by them on my philosophical Sundays, I remain in the week-day and work-a-day world of science, unconverted and unregenerate.

Let me use the phrase "group of impressions" for the net result in experience of the felt development of an instinctive situation—such as the swimming of the waterhen when first he is placed in water. Is it the same thing to say that such a group of impressions—or let us for simplicity say "an impression"—exists and to say that an impression is known? Is Huxley right in saying[1] "There is only a verbal difference between having a sensation and knowing one has it; they are simply two phrases for the same mental state"? Note that even here Huxley speaks of "having a sensation": now to have a sensation implies a "haver" and therefore a consciousness that has it. So we may extend our question and ask:—Are the following three statements simply differing forms of one statement: (*a*) an impression (or group of impressions) exists; (*b*) I have an impression; (*c*) I know that I have an impression? I conceive that they are all three different. I am unable to agree with Huxley that there is only a verbal difference between having an impression and knowing that one has it. I believe that Dr Stout[2] is truer to fact when he says that for one to be angry and to know that one is angry are *not* psychologically equivalent. To know that one is angry and to know that one has an impression are products of reflective thought and involve ideational process. The distinction between having an impression (*b*) and knowing that one has it (*c*) is this: to have an impression implies previous experience to which it can be assimilated—implies therefore an experiencer; to know that one has an impression implies reflective thought by which it can be apperceived—implies therefore a thinker or knower.

So far so good—or bad! What about the statement I placed first, (*a*) the bare statement:—An impression or group of impressions exists? Well, we start with the organism as part of the ideal construction of the world of things in terms of objective reference. The organism is the recipient of stimuli which affect the sense-organs, and, through the nerves, set up molecular changes in the brain. In terms of the scientific ideal construction a given impression is the concomitant of a given group of neuronic changes set up by a given group of (say) visual stimuli. Consider then the very first group of stimuli, giving rise to the very first group of neuronic changes, with its very first concomitant

[1] *Collected Essays*, Vol. III. p. 86.

[2] *Manual of Psychology*, Introduction, Chapter I. § 3, p. 8.

impression. Can we from the standpoint of scientific interpretation say more in this case than that the impression exists?

No doubt within what I have termed the hybrid universe of discourse of psychology it may be said that the organism has the impression. But it would be better perhaps to say that the organism has the psycho-physical disposition. Regarded from the psychical aspect and that alone, the impression is not yet possessed—there is no previously gotten experience to possess it: it simply comes into being: it simply exists.

Consider an ideally simple case—that of a newly-hatched chick pecking at a small object within striking distance and either swallowing or rejecting it; and suppose (what is of course not quite true to fact) that this is the first presentation to sense. The visual stimuli call into being an impression (A) and also initiate the pecking behaviour which itself calls into being a group of kinæsthetic impressions (B); the object is seized and gives rise in the bill to a taste impression (C); in accordance with the nature of which there occurs the responsive behaviour, say of vigorous rejection, with its kinæsthetic impression (D). Of course this is much simpler than the actual occurrences; but we have the four impressions A B C D following in close sequence and forming part of a continuous piece of behaviour. These four form a coalescent group or "disposition"—a psycho-physical disposition with physiological and psychological associative connexions—a bit of the primary tissue of experience, unified through the behaviour it promotes.

Now consider a second occasion on which the chick receives a visual stimulus similar to the first. There is a concomitant impression which owing to the established associative connexions revives the whole psycho-physical disposition. The impression has meaning and is raised to the level of a percept. It calls up or reinstates the past experience to which it is assimilated. The old experience thus revived functions as "assimilator" to the new impression as "assimilated." Otherwise stated, the old experience as "experiencer" *has* the new experience as "experienced." We thus reach the conception of the perceptual subject as the revived experience which assimilates or possesses the new impression. Similarly at a later stage of mental development the conceptual subject, as knower, is the revived knowledge which apperceptively assimilates a new fact.

It should be observed that from this point of view (1) the ego, as knower, is the result of a process of development; (2) any item of knowledge is apperceptively assimilated to that part of the system of knowledge which has already been organised and which is representatively revived; (3) when we say that a percept or a concept is *mine* all that is implied is such apperceptive assimilation with a reflective realisation of its occurrence. From the metaphysical point of view, on the other hand, the ego exists as a producing agency *ab initio.* It is not the result of but the cause of (or agency in) the developmental process. For science, as I said at the outset, conceptual thought and the ego are the *terminus ad quem*; for metaphysics they are the *terminus a quo.* To quote Green again: self-consciousness is "at its beginning formally or potentially or implicitly all that it becomes actually or explicitly in developed knowledge."

It should also be observed that, in the interpretation suggested above, the impressions A B C D do, as a matter of fact, or in accordance with the ideal construction, enter into relationship. If the question be asked: Through what agency are they thus related—by the activity of what relating principle or producing cause? Science replies: "We don't know. We just accept the fact within our ideal construction." But it is obvious that metaphysics (seeking the *raison d'être* of what does happen) cannot and should not rest content with this. It is absurd however of metaphysics to say that science either denies or ignores the fact of the existence of differentiated centres of synthetic organisation. It does nothing of the sort. If it did not accept the fact of such synthetic organisation in body and mind how could it interpret, under its own canons of interpretation, either the development of the nervous system or the development of experience. When it is wise it does not deny the existence of the principle of synthesis or the synthesising agency postulated by metaphysics. All that it urges is that this in no way contributes to its own interpretation *quâ* scientific. It forms no part of either the policy or the creed of science.

Consider the perception of space as genetically interpreted in any modern text-book of psychology. Certain data,—visual or tactual sensations, local signs, kinæsthetic impressions, and so forth,—when combined in certain ways give rise to the perception of space. But why do they take on that particular "form of experience" which we call space? Science again replies:—I don't know: that is simply the form they do take. That is what we find to be the nature of experience, and there's nothing more to be said of it within my universe of discourse. Metaphysics adds:—It is the form which mind as agency impresses on the data. You only *find* it there because in the very act of perceiving you *put* it there. You can't have spacial perception without it, because space is a "constitutive form" of the percipient mind. In all perception "we employ those *à priori* categories which make experience what it is. Their source is within; and, when elicited in self-consciousness they partly constitute and partly regulate, our perception of the objects of sense[1]." Professor Knight, in criticising Hume, says in effect:—You can't explain the physical processes in nature, or the mental processes in man, unless you are ready to postulate mind as an agency; they are produced as you find them because it is part of the inalienable nature of conscious agency so to produce them. To which Hume, and his scientific followers reply:—Thank you. But we can get along quite well without your postulate. We don't want to "explain" as you explain. It does not get us a bit "forrarder." *Our* "explanations" *do* help us on. Hence science has been forging ahead, while metaphysics has been marking time for a century. Go on mumbling your categorical creed by all means. But don't ask us to spend valuable time in repeating its barren formularies. We have business on hand and a definite policy to pursue.

Those who attempt to study the natural history of experience in the humbler forms of animal life,—chicks and ducklings and waterhens and the like,—carry on guerilla methods of conquest apart from the philosophical battle which rages round the mind of man. But I suppose that, for the metaphysician, the categories here also hold sway. We endeavour to interpret the genesis of experience in a young chick pecking at *things* we call maggots, which are situate within reach of its bill in *space*; the process of swallowing succeeding that of pecking in *time*, and *causing*, we may suppose, some sort of satisfaction. If the comparative psychologist is asked how he accounts for the fact that the chick's experience assumes this form with its things, its space and time, its connexions named causal, and so forth, he can but reply that this is the way in which experience is constituted. The metaphysician gives the same answer, but he gives also the reason why its nature is such as we find it to be. This is, he says, because the experiencing mind has, as part of its *à priori* constitution the power of impressing certain forms—the categories of thinghood, space, time, causality and other such,—upon its contents, the products of its inherent activity. Thus the judgment of causality is not reached by any process of development,—at most it is only rendered explicit by such a process,—"it flashes forth from the

[1] Knight's *Hume*, p. 166.

mind *à priori*." Both man of science, then, and metaphysician agree that such is the way in which experience is constituted—such is its nature. The metaphysician calls in an agency as *causa causans* of this nature or constitution while the man of science says that whether we postulate such an agency or not makes no practical difference. The constitution is what it is whether we call in a supplementary creed to account for it or not.

Some men of science will however say that we are in error in admitting that thinghood, space, time, causal sequence and so forth are the forms which experience has—are parts of the constitution of *experience*. They will urge that these are parts of the constitution, not of experience, but of the objective world with which experience deals. The student of physical science, as well as the representative of common sense, too often fails to grasp the fact that for science the external world of things and processes is the objective reference of our experience. For science "nature" and "experience of nature" are one and the same. It is part of the constitution of experience to have objective reference; why this should be so science does not attempt to determine; what the physicist and the man of science mean by things and natural processes is just this objective reference of experience—nothing less and nothing more. Space, time, causal connexion (in the scientific sense) are therefore parts of the constitution of experience, and of the systematic knowledge founded thereon, in this objective reference. But this objective reference of experience *is* what we call nature; hence things and the rest are parts of the constitution of nature, which is just what the physicist and the man of common sense contend for. Where the man of science and the metaphysician part company is at the postulate (for the former unnecessary, for the latter essential) that constituent forms of experience are *à priori* categories under which *the acts of an agency called Mind* are conditioned.

I must once more repeat my own conviction that psychology as a science has no concern with Mind as an agency, with Consciousness as a principle of activity, with Will as a producing cause. Of course I am well aware that I advocate a psychology without a Soul. What then? Psychology is not co-extensive with the whole field of human enquiry. It is, or should be, a branch of science which is aware of its limitations. It deals with natural *processes* instinctive, perceptual and ideational or volitional, not with *acts* of an agency. *They* are dealt with in mental philosophy. Only on condition that psychology, as a branch of science, purges itself of all metaphysical implications can it pursue its policy effectually. Only thus can it afford to metaphysics the data for its further enquiry. It surely seems reasonable to say:—Let us first study the problems from a scientific point of view, and then see what further problems are left over for metaphysical treatment. This, however, the metaphysician urges is impossible. For since the method of science is to employ the ideal constructions of thought, it is clear that science itself presupposes the very thought itself which, for that thought, is a product of development. Well and good. What does this imply? First that an evolutionary scheme is an ideal construction for thought; and secondly that in this evolutionary scheme there is, *for* thought, the production *of* thought. That, I suppose, no one denies; but that assuredly does not imply that in the evolutionary scheme thought must be present *ab initio.* For science there was a time in the world's history when thought had not been evolved from lower forms of naïvely perceptual experience; and a yet earlier stage of world-development when not even perceptual experience had been evolved. But here again it may be urged that, if all that we know of the physical world is that it is the objective reference of experience, how can it be said that it existed at all before (on the evolutionary supposition) experience itself had come into existence? The reply is that, within our ideal construction, we are dealing with what took place prior to the existence of experience *in terms of that experience*: in other words we are dealing with what man would, we conceive, have been able to observe, if he had been there to observe; just as we fill in gaps of observation in daily life with what we might have seen had we been present.

Only on such presuppositions as these can we formulate, in terms of an ideal construction, a natural history of experience. Of course there is also the further presupposition, common to all branches of science, that the processes with which we deal and which we endeavour to interpret are rigidly determinate. For science determinism is a *conditio sine quâ non.* For metaphysics the central question is:—What is the nature of the Agency that freely determines?

This is a question, I conceive, which must inevitably present itself to a rational being. It is a question, however, which I do not propose to discuss. Only this will I say: It appears to me to be a perfectly legitimate assumption or postulate that not only is every organism a centre of determinate processes physical or psycho-physical, bodily or mental, but also a centre of determining agency; that there is not only the empirical ego—the group-name for actual or possible mental processes, but also the metaphysical ego—the principal of activity which is its *raison d'être.* The former is determinate: the latter determining. It does not appear to me to be at all unreasonable to assume that just as any individual is in related connexion with (1) other individuals, and (2) the whole universe (since as a whole it is throughout inter-connected and is *one* universe); so too is any individual, as agency, in related connexion with (1) other individuals as agencies, and (2) one Universal Agency. What I do think eminently unreasonable is to commingle these, or other such, metaphysical assumptions or postulates in any scientific interpretation of the natural history of experience.

CONFLUXION AND CONTRAST EFFECTS IN THE MÜLLER-LYER ILLUSION.

By E. O. LEWIS.

(*From the Psychological Laboratory, Cambridge.*)

A. Introductory.

1. *Previous work.* Psychologists maintain very different opinions as to the manner in which the Müller-Lyer illusion varies in amount, when the arms and angles vary in size. Brentano[1] and Auerbach[2] maintain that when the arms are of constant length whilst the angles

[1] *Ztschr. f. Psychol. u. Physiol. d. Sinnesorg.* Bd. 3, 5 and 6.

[2] *Ibid.* Bd. 7, S. 152.

increase from 0 to 90 degrees, the illusion increases at first, but reaches a maximal value when the angles are a certain size; and that on further increasing the angles the illusion becomes less. Auerbach states that the maximal illusion occurs when the angles are 30 degrees, whilst Brentano judges it to be at 60 degrees. Heymans[1], Wundt[2], and several other writers maintain that there is a maximal illusion when the arms are lengthened, the angles being constant; but if the angles vary from 0 to 90 degrees, the arms being of constant length, the illusion decreases continuously.

Whether the Müller-Lyer illusion has this feature of maximal value, and if so, under what conditions it occurs, are matters of importance when we are investigating the precise nature of the illusion. Indeed they may be regarded as the touchstone of the validity of the various theories put forward to explain it. Thus if the illusion is due to the overestimation and underestimation of angles, as Brentano maintains, it would be expected to have a maximal value when the angles reach a certain size, in order to agree with other illusions similarly explicable. Those who favour a confluxion and contrast theory of this illusion might expect a maximal illusion when the arms reach a certain length, as is the case in other figures which are generally explained by this theory.

These differences of opinion would never have appeared, had the writers made quantitative determinations instead of contenting themselves with *à priori* conclusions. Hitherto, the most systematic investigation of this problem has been made by Heymans[3]. As already stated, his results indicated that a maximal value of the illusion was attainable when the length of the arms varied, but not when the angles varied. It will be seen that the present investigation confirms the principal features of Heymans' results. Heymans' investigation, however, was of a very incomplete character, and had several defects. In the first place, he obtained readings by the method of adjustment. Readings obtained by this method are considerably influenced by such factors as expectation on the part of the observer (when adjusting the one line equal to the other), the rate of movement of the variable part, and the initial length of the variable. Another defect of his investigation was that he apparently took no precaution to prevent

[1] *Ztschr. f. Psychol. u. Physiol. d. Sinnesorg.* Bd. 9, S. 221.
[2] *Abh. d. kön. sächs. Gesell. d. Wiss.* Bd. 24, S. 55.
[3] *Ztschr. f. Psychol. u. Physiol. d. Sinnesorg.* Bd. 9, S. 221.

the effects of practice influencing the results. Moreover, the majority of the readings were made with the compound figure (Fig. 1). Con-

Fig. 1.

clusions based on such figures as this are liable to be invalid, because the complicated nature of the figure is overlooked.

2. *Method and Apparatus.* The writer has undertaken to investigate experimentally the nature of the Müller-Lyer illusion. The results of one portion of this investigation, dealing with the effects of practice upon this illusion, have been previously published in this Journal[1]. The apparatus used in those experiments for obtaining prolonged and momentary exposures of the figures, was also used in the present series of experiments[2]. Also, the same method of obtaining readings was adopted, namely, the method of right and wrong cases.

In the present investigation, observations were made with Figs. 2 and 3, separately. When observations were made with the 'feather-head' figure (Fig. 2), the middle line *ab* was of constant length (50 mm.), whilst *cd* was variable. Similarly in the 'arrow-head' figure, $a'b'$ was constant (50 mm.), and $c'd'$ variable. A plate was devised by means of which it was possible to obtain similar figures with arms and

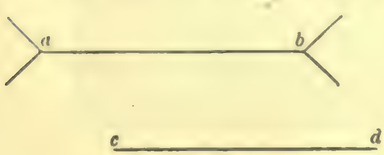


Fig. 2.

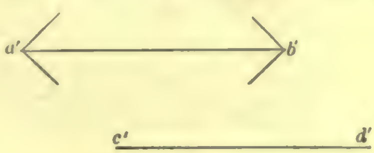


Fig. 3.

angles of various sizes. It was important that in these figures there should be no interruption or flaw at the points where the arms meet the middle line. Any interruption at these points caused a considerable decrease in the amount of the illusion. As it was necessary to have considerable accuracy, it was found impossible to vary the angles of the figures by placing pivots at these points. The contrivance by means of which this difficulty was overcome consisted in moving the outer ends of the arms round the circumference of circles whose centres were at the ends of the middle line.

[1] Vol. II. p. 294. [2] *Ibid.* p. 243.

The plate used for obtaining feather-head figures with arms and angles of various sizes is illustrated in Fig. 4. The frame portion of it is shown in Fig. A. The middle line of the illusion is indicated by *ab*. Symmetrically situated at the ends of this middle line are four quadrant-shaped gaps. The parts marked *y* are four narrow pieces of metal, placed on the circumferences of circles whose centres are at the extremities of *ab*. Fig. B shows a brass quadrant with one of the arms of the feather-head figure. Near the edge of this quadrant is a groove into which *y* fits. When in this position, the

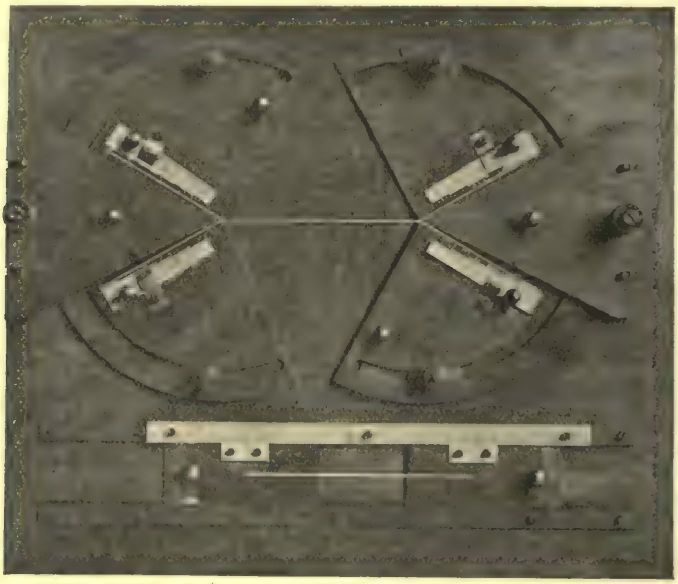
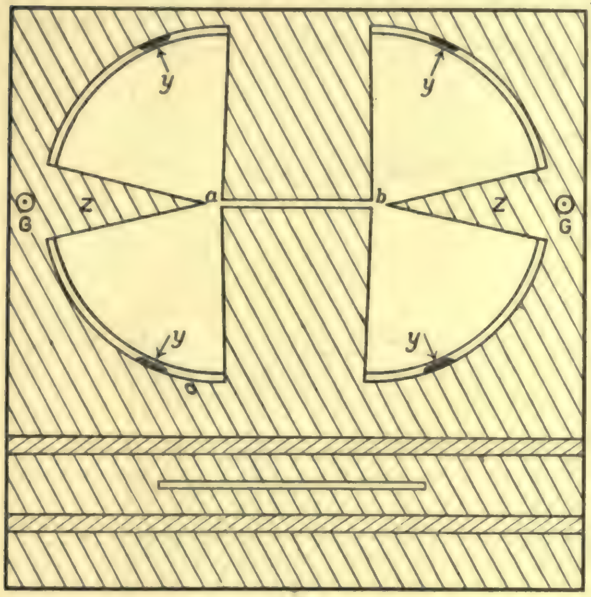


Fig. 4.

extremity *c* of the arm is exactly at one of the extremities of *ab*; and when the quadrant is moved whilst *y* is inside the groove, the arm *cd* moves as a radius of the circle whose centre is at the extremity of the middle line. The quadrant is kept firm in any one position by means of a binding screw attached to *y*. The greatest length of the arms of the figures obtained by this plate was 35 mm., whilst the middle line was 50 mm. long. In order to vary the length of the arms of the figures, a portion of the slit was covered with a piece of aluminium, which was kept in its position by means of a small steel spring attached to the quadrant. The other arms were similarly fitted. With the plate used in these experiments it was not possible to get figures in which the arms made angles less than 10 degrees with the direction of the middle line, because of the presence of the piece *Z* in Fig. A. It was found advisable to have this part in the frame of the plate as it ensured more accurate joining of the lines of the feather-head figures.



A

Fig. A.

B

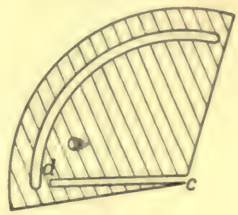
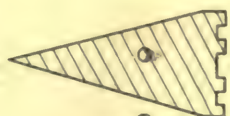


Fig. B.



C

Fig. C.

In order to understand more fully the manipulation of the plate, let us assume that all the quadrants are fitted so that the arms make angles of 10 degrees with the direction of the middle line. In this position it is evident that the quadrants cover the gaps in the frame of the plate. If we now move the quadrants so that the arms become nearer the vertical position, there are two practical difficulties. In the first place, as the arms are 35 mm. long and the middle line 50 mm., the quadrants will interfere with each other's movement. This difficulty is overcome by placing a thickness of plate round the circumference of two of the gaps, so as to raise the quadrants moving over these gaps. In this manner one of the plates moves over the other. The other difficulty is that gaps appear on the sides of the plate when the quadrants are thus moved. This is overcome by procuring a number of plates of different sizes, similar to that shown in Fig. C, which cover these gaps. These plates were so made that it was possible to obtain figures in which the arms made angles of 10, 20, 30, ... 90 degrees with the direction of the middle line. These plates were kept firm in their position by means of binding screws *G* (Fig. A).

A plate of similar design could be made to give arrow-head figures. But with such a plate it would be impossible to obtain figures in which

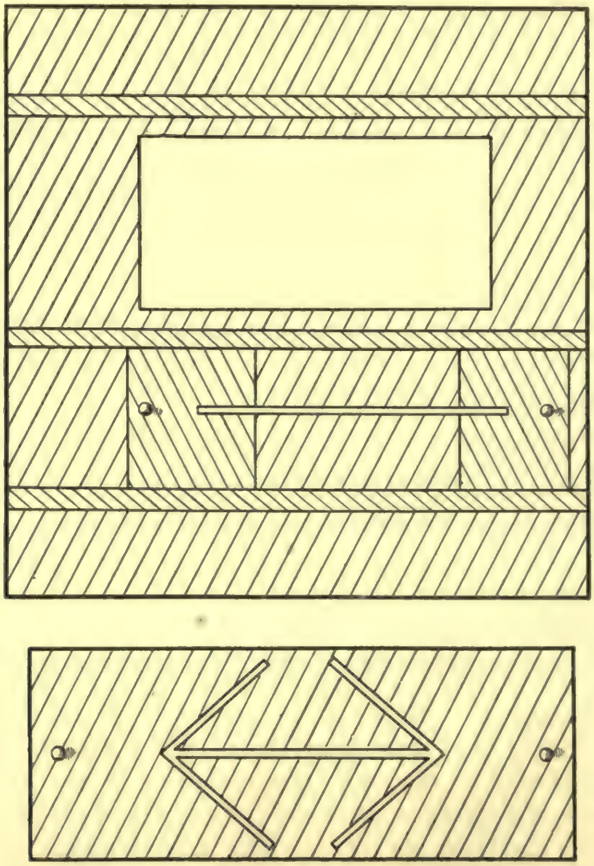


Fig. 5.

the arms met or crossed each other. In the present investigation the arrow-head figures were obtained by means of the plate shown in Fig. 5. The arrow-head figure, which is shown on the slide in the lower part of Fig. 5, was placed over the large oblong gap in the frame of the plate. Four such slides were used in these experiments, with figures in which the arms made angles of 18, 36, 54 and 72 degrees with the middle line. In the figures with angles of 36 and 54 degrees, the arms could be varied up to 25 mm. in length, but in the figure with angles of 72 degrees the maximum length of the arms was 20 mm. In the figure with angles of 18 degrees the arms crossed each other as in Fig. 6, and the maximum length of the arms was 50 mm. It is obvious

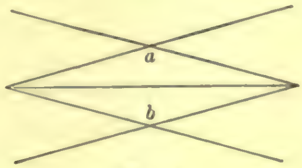


Fig. 6.

that in the slide with this figure, the parts *a* and *b* were detachable from the rest of it. In order to keep these detachable parts in their proper places, the whole figure was covered with a plate of glass to which the various parts of the slide adhered.

B. EXPERIMENTAL DATA.

3. *Experiments in which the length of the arms varied whilst the angles were constant.* Three subjects made observations in this series. Two of them, S. H. H. and J. A. V. R., made the whole of their observations with the figures momentarily exposed. Previous experiments had shown that practice causes but little change in the amount of the Müller-Lyer illusion when momentarily observed[1]. Besides, it was found that after a little practice, the readings made with the momentary exposures were more definite and consistent than those with the prolonged exposures. But so as to make certain that no feature peculiar to the momentary method of observation should determine the nature of the results, the other subject, L. C., made readings with the figures exposed for prolonged periods. In this case the effects of practice were eliminated to a great extent by allowing three or more days to elapse between successive sittings.

The average sitting lasted an hour. In order to prevent the eyes of the subject becoming fatigued, a pause of a few minutes was allowed once or twice during the course of the sitting. Twenty readings usually sufficed to determine the amount of the illusion in any one figure, the maximum range of the variable being 5 mm. and the

[1] Vol. II. p. 300.

smallest variation 1 mm. During any one sitting the angles were kept constant, whilst the length of the arms was varied. The length of the arms was gradually increased or decreased 5 mm. at a time. Thus in the case of the feather-head figures, the arms varied between 5 and 35 mm. in length, and the amount of the illusion with seven different figures was determined at one sitting. If at one sitting the arms had been gradually increased in length, at the next sitting they were gradually decreased. The subjects S. H. H. and L. C. observed figures with angles of 18 degrees at the first sitting, the former with the feather-head figure, and the latter with the arrow-head figure; and at the following sittings they observed figures with angles of 36, 54, and 72 degrees. Subject J. A. V. R. observed the arrow-head figure with angles of 72 degrees at the first sitting, and at the following sittings observed the figures in the reverse order to that just described. It was found that these different orders of observing the figures did not materially modify the main features of the results. The observations with Fig. 6 required two sittings to complete the series, as it was necessary to determine the amount of the illusion with ten different figures. If at any sitting the effects of fatigue were obvious, the series was not proceeded with but was repeated at the next sitting.

4. *The Limiting Series.* Figs. 7 and 8 are, *geometrically*, limiting cases of the Müller-Lyer figures. Fig. 7, which may be called the

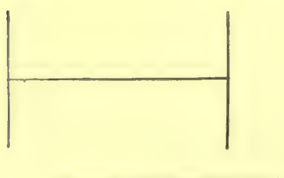


Fig. 7.

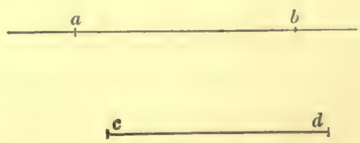


Fig. 8.

'illusion of the bounded line,' is the limiting case of either the feather-head or the arrow-head figure where the arms make angles of 90 degrees with the middle line. Fig. 8, usually known as the 'illusion of contrast,' is the limiting case of the feather-head figure in which the arms make angles of 0 degrees with the direction of the middle line. When the three observers had completed the series previously described, they made observations with these limiting figures, a similar method of procedure being adopted.

With Fig. 7 the vertical arms were gradually increased or decreased in length. Their shortest length was 5 mm. and their greatest length

35 mm., being gradually increased or decreased by 5 mm. in successive figures. In Fig. 8, the marks *a* and *b* at the ends of the middle line were just clear enough to enable the observers to distinguish it from the arms on each side. The variable line *cd* was bounded by similar marks. A series of observations was made when there were no arms on either side of *ab*, in order to make certain that it was seen equal to *cd* when both of them were 50 mm. long. The amount of the illusion was then determined in figures with the arms on both sides of *ab* of the various lengths 5, 10, 15 ... 35 mm. In both of these limiting figures, the middle lines were of constant length, 50 mm. Fig. 7 was obtained with the plate shown in Fig. 4. In order to obtain Fig. 8 a different plate was necessary, which requires no detailed description.

5. *Experiments in which the length of the arms was constant, whilst the angles varied.* Two subjects made observations in this series. R. F. H. made a complete series of observations with momentary exposure of the figures, whilst F. W. made observations with prolonged exposure of the figures. In all the figures observed at any one sitting, the arms were of constant length whilst the angles varied. The same plates were used as in the previous experiments, namely those shown in Figs. 4 and 5. In the series with the feather-head figures the arms made angles of 10, 20, 30...90 degrees with the direction of the middle line. In the series with the arrow-head figures the angles were 18, 36, 54 and 72 degrees. At the first sitting, R. F. H. made observations with the feather-head figure with arms 5 mm. long, commencing with the figure with angles of 10 degrees. At the following sittings the arms of the figures were 10, 15 ... 35 mm. long. F. W. made observations with the arrow-head figure at the first sitting, the arms being 5 mm. long whilst the angles varied from 18 to 72 degrees. The same precautions were taken to prevent fatigue of the subject's eyes as in the former series.

6. *Results diagrammatically represented.*

EXPLANATION OF CURVES.

Curves A represent the results with the feather-head figure >——<. Curves B represents the results with the arrow-head figure <——>.

The middle lines of the feather-head and arrow-head figures were of constant length, 50 mm.

Figs. 9, 10, 11. Each curve of these figures represents the results of observations with figures in which the length of the arms varied whilst the angles were constant. The numbers opposite the curves indicate the size of the angles. The abscissae represent the lengths of the arms in mm. The ordinates represent the amounts of the illusion in mm. Fig. 9 gives the results for S. H. H. with momentary exposures of the figures. Fig. 10 gives the results for J. A. V. R. with momentary exposures of the figures. Fig. 11 gives the results for L. C. with prolonged exposures of the figures.

Figs. 12, 13. Each curve represents the results of observations with figures in which the lengths of the arms were constant, whilst the angles varied. The numbers opposite the curves indicate the length of the arms in mm. The abscissae represent the sizes of the angles. The ordinates represent the amounts of the illusion.

Fig. 12 gives the results for R. F. H. with momentary exposures of the figures. Fig. 13 gives the results for F. W. with prolonged exposures of the figures.

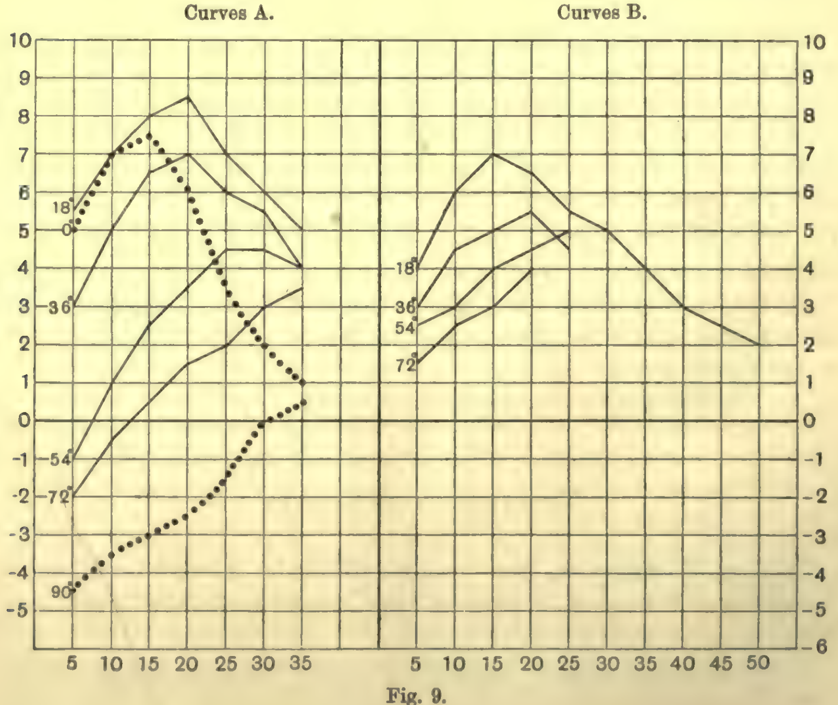


Fig. 9.

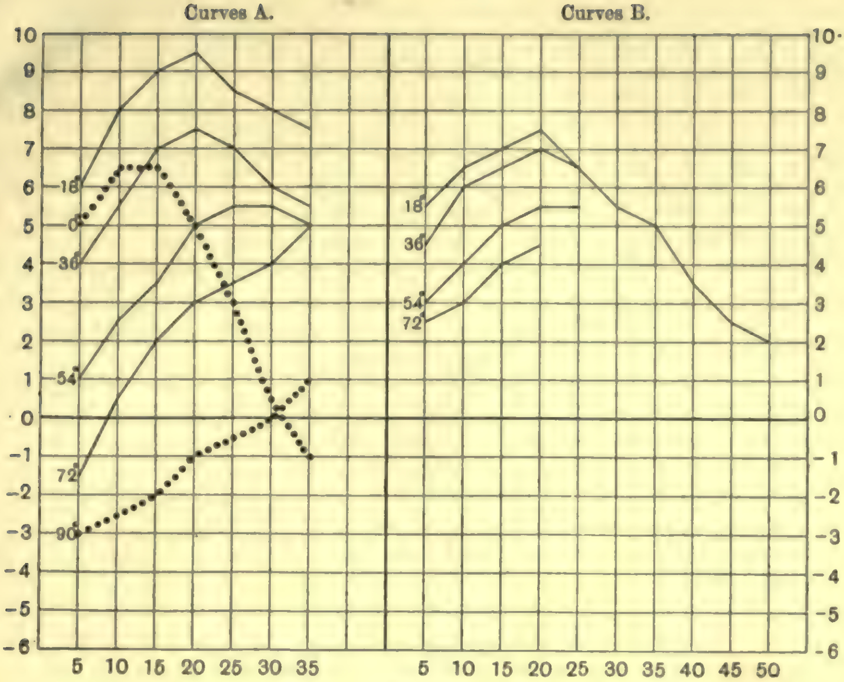


Fig. 10.

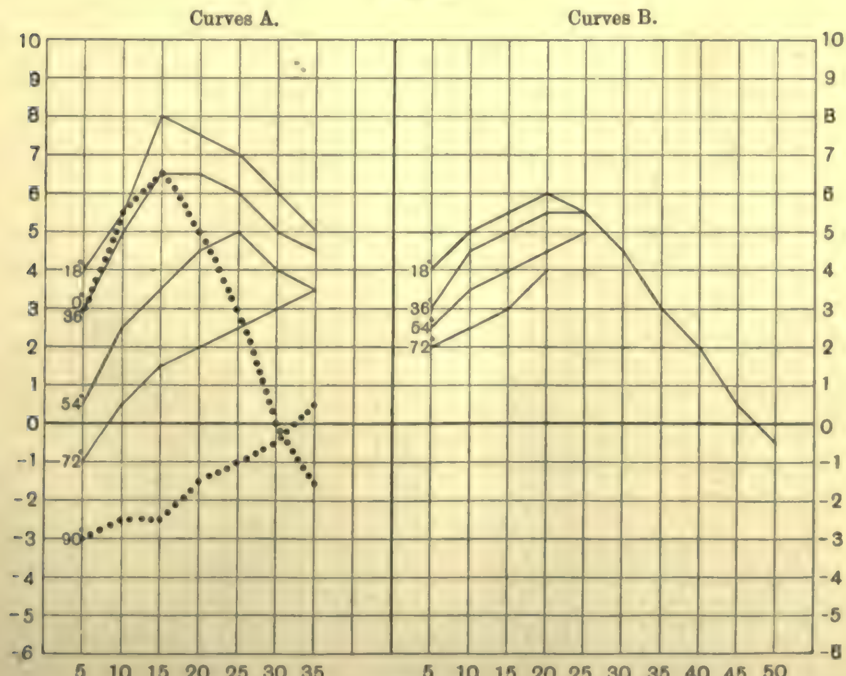


Fig. 11.

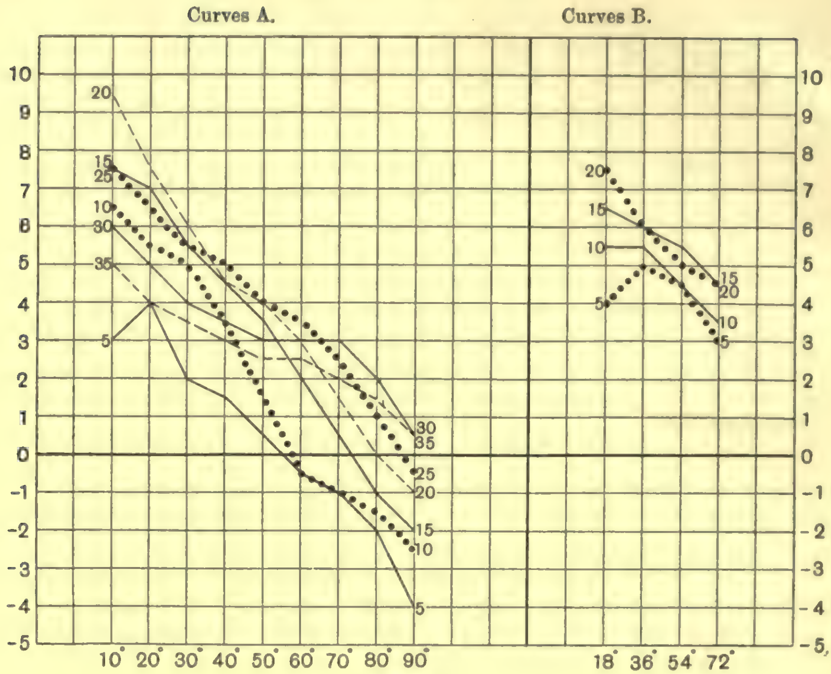

Curves A.
Curves B.
10 9 8 7 6 5 4 3 2 1 0 -1 -2 -3 -4 -5
10° 20° 30° 40° 50° 60° 70° 80° 90°
18° 36° 54° 72°
20
15
25
10
30
35
5

Fig. 12.

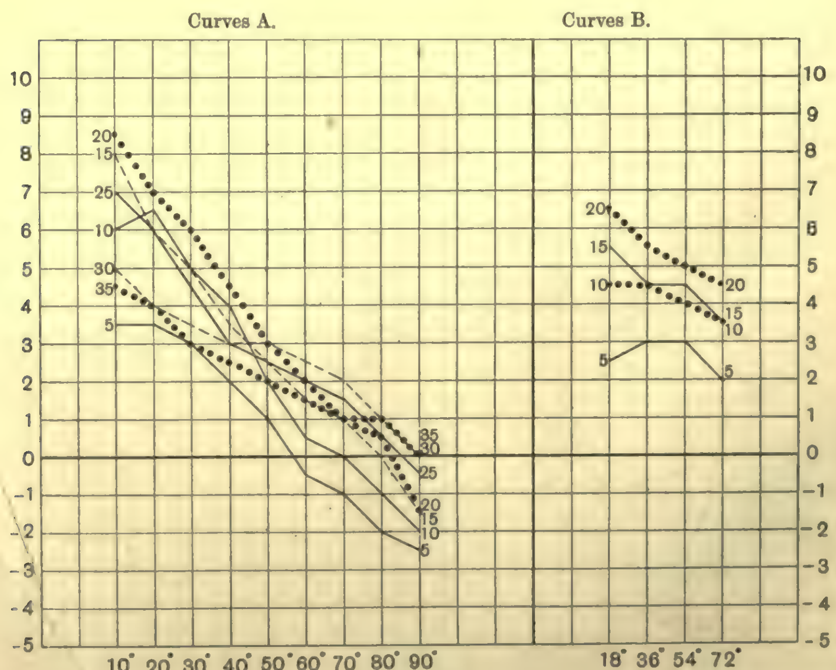

Curves A.
Curves B.
10 9 8 7 6 5 4 3 2 1 0 -1 -2 -3 -4 -5
10° 20° 30° 40° 50° 60° 70° 80° 90°
18° 36° 54° 72°
20
15
25
10
30
35
5

Fig. 13.

C. CONCLUSIONS.

7. *Results of the above data.* Let us first note the manner in which the amount of the illusion varies when the angles vary, the arms being of constant length. Figs. 12 and 13 indicate that as the angles vary from 10 to 90 degrees, the illusion decreases almost continuously. It is true that when the arms are short and the angles vary from 10 to 30 degrees, the amount of the illusion varies in a somewhat uncertain manner. It does not seem, however, that these exceptions have any important connection with the real nature of the illusion. Evidently, when the arms are long enough to produce the ordinary illusory effects of the figures, this uncertain variation of the illusion is no longer present. Hence it may be safely concluded that the phenomenon of maximal illusion when the angles are varied, is not a characteristic of the Müller-Lyer illusion. This is one consideration which suggests that this illusion cannot be closely related to such illusions as the Zöllner illusion which have been found to have a maximal value when the angles are varied.

Figs. 9, 10 and 11 agree in demonstrating an important fact regarding the Müller-Lyer illusion. The curves of these figures indicate that when the arms of the figures are lengthened, the angles being constant, the illusion increases at first; but sooner or later it reaches a maximal value, beyond which further lengthening of the arms causes the illusion to decrease. This fact has been stated previously by Heymans; but by the above results we can understand the nature of this feature more fully than can be done by those he obtained. The above figures show that this is a feature of both the feather-head and arrow-head figures, although the character of the variation in these figures is somewhat different.

The curves of the above figures, especially those which represent the results of observations with the feather-head figures, indicate two interesting features of this maximal illusion. The one feature is that the smaller the angles, the shorter is the length of the arms at which this maximal illusion occurs. Thus, if we look successively at the curves corresponding to angles of 18, 36 and 54 degrees in Figs. 9, 10 and 11, the positions of the points of maximal illusion move to the right. The other feature is, that the amount of the maximal illusion decreases as the angles increase. Thus the curves just mentioned are seldom found to intersect each other. The cases in which they do intersect are undoubtedly due to the daily fluctuation of the amount of the illusion. This second feature is fully substantiated by the results indicated by the curves A of Figs. 12 and 13. In these curves, the maximal ordinate for each successive abscissa decreases continuously as the angles increase. Thus, taking into consideration these two features of the maximal illusion, we may indicate by means of 'ideal' curves, such as those in Fig. 14, the variation of the amount of the illusion when the

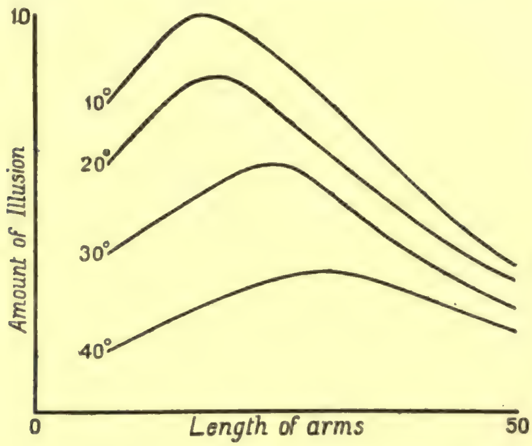


Fig. 14.

arms are lengthened. The abscissa represents the length of the arms, and the ordinate the amount of the illusion. The curves of greatest curvature are those corresponding to figures with small angles.

Heymans, Schumann[1], and other writers maintain that there is no point of maximal illusion in the arrow-head figure as in the feather-head figure. According to these writers, the amount of the illusion of the arrow-head figure varies but little as the arms are lengthened until they meet and thus complete the rhombus; but on further lengthening them the illusion decreases. My own results are by no means in complete agreement with this description. Curves B of Figs. 9, 10 and 11, especially the curves corresponding to figures with angles of 18 degrees, show clearly that there is a maximal illusion with the arrow-head figure. In the arrow-head figure with angles of 18 degrees, the arms meet when

[1] *Ztschr. f. Psychol. u. Physiol. d. Sinnesorg.* Bd. 30, S. 286.

they are about 26 mm. long; but according to my results the point of maximal illusion is reached before the arms are of this length. My results, however, seem to show that the illusion of the arrow-head figure does not vary as much as that of the feather-head figure until the rhombus is completed; but that on further lengthening the arms, the illusion decreases rapidly, in one case there being ultimately an illusion of overestimation.

One feature of the above results indicates a fact which seems to have been overlooked by previous writers. Curves *A* of Figs. 9—13, which represent the results of observations with the feather-head figures, show that when the arms are short and the angles exceed 60 degrees, the middle line is underestimated. On lengthening the arms, the usual illusion of overestimation is obtained. This is but one feature out of many which prove that the perception of this illusion is not determined solely by a single principle. The portion of the arms adjacent to the middle line of the figure has a limiting influence, which is more marked the nearer the arms approach the vertical.

Some writers have attempted to establish mathematical formulae expressing the amount of the Müller-Lyer illusion and the variations it undergoes. Thus Brentano states that there is a maximal illusion when the ratio between the lengths of the middle line and the arms is 1 : 0·35. Heymans also has formulated what he calls 'the cosine law.' This law states that the amount of the Müller-Lyer illusion, when the length of the arms is constant, is proportional to the cosine of the angles. It is evident that these writers consider the illusion to be of a very homogeneous character. It is especially unfortunate that Heymans should have based his law upon results obtained by investigating the compound figure (Fig. 1). Experimental investigation of the Müller-Lyer figures soon shows that their perception is complicated by several factors of a psychological nature, which make it impossible to express the amount of the illusion by simple mathematical formulae. It is true that the illusion within certain limits increases as the length of the arms increases, and decreases as the angles increase, and thus resembles a cosine variation. But it is obvious that the illusion may vary in this manner and yet not be proportional to the cosine of the angle. When dealing with such figures as these, the perception of which is so much determined by fluctuating psychological factors, it is better to be content with stating the general features of the variation, rather than to attempt any exact mathematical formulation of them.

We now come to the results of the observations made with the limiting figures (Figs. 7 and 8). Several persons, other than those whose readings are recorded, made observations with these two limiting figures, and the results thus obtained agreed with the principal features of those recorded here. The results of the observations with the limiting figure in which the arms are at right angles with the middle line (Fig. 7), are indicated by the '90° curves' in Figs. 9, 10 and 11 (Curves A). It is seen that the illusion of underestimation decreases continuously as the vertical arms vary in length from 5 to 35 mm. on each side of the middle line, and ultimately, there is an illusion of overestimation. This result is contrary to that which Kiesow[1] states. He states that the illusion of underestimation in this figure increases continuously as the verticals are lengthened. There is no doubt that the illusion of underestimation becomes more marked as the verticals are lengthened within certain limits. However, in the case of the majority of the subjects who made observations in the course of the present investigation, this limit was reached before the verticals were 5 mm. long on each side of the middle line; and on further lengthening them, the limiting influence was counteracted by a tendency to overestimate the middle line. This limiting figure, as compared with the other, is of little importance to the study of the Müller-Lyer illusion.

The other limiting figure is that in which the arms are in the same straight line as the middle line (Fig. 8). The results of the observations with this figure are clearly indicated by the '0° curve' in Figs. 9, 10 and 11 (Curves A). The illusion of this figure is well known to psychologists, and is usually attributed to contrast effects between the terminal and central portions of the line. It is evident that this figure is 'geometrically' the limiting case of the feather-head figure of the Müller-Lyer illusion. Nevertheless, not until recently has it been urged that these illusions are due to similar psychological factors. The results of the present investigation give considerable evidence in favour of correlating these two illusions. The curve indicating the variation of the illusion in this limiting figure resembles those corresponding to feather-head figures. The curve, indicating the results of the observations with the limiting figure, shows that there is a point of maximal illusion as the arms are lengthened, just as in the case of the ordinary Müller-Lyer figures. The position of this point of maximal illusion agrees with what would

[1] *Arch. f. d. ges. Psychol.* Bd. 6, S. 289.

be expected; it lies to the left of the point of maximal illusion in the curve of 18 degrees.

However, there are certain differentiating features of the '0° curve' which require some explanation in order to justify this correlation. One of these differentiating features is that the maximal illusion is less in the limiting figure than in the feather-head figure with angles of 18 degrees. It has been observed that as the arms of the feather-head figures make smaller angles with the direction of the middle line, the greater becomes the maximal illusion; and hence the greatest maximal illusion would be expected in this limiting figure. That such is not the case is explained by the fact that it is necessary to mark the extremities of the middle line in this limiting figure with short lines. These marks aid the observer considerably to restrict his attention to the central part of the line, and thus the perception of it is less influenced by the terminal parts of the figure. If these marks become very prominent it is possible to overcome the illusion altogether.

The second differentiating feature of the curve corresponding to this limiting figure is, that after reaching the point of maximal illusion it descends very rapidly, signifying the decrease of the illusion of overestimation. This is again explained partly by the fact that the marks at the ends of the central part of the line aid the observer to apprehend separately the various parts of the whole line; which, as will be shown later, counteracts the tendency to overestimate the central part of the line. Apart from this, we should expect, in accordance with what has been already observed, that the curve corresponding to this limiting figure would descend more rapidly than the other curves in the same figures; because, as Fig. 14 indicates, the smaller the angle which the arms make with the direction of the middle line, the more rapidly does the corresponding curve descend after the point of maximal illusion is reached.

8. *Explanations of previous writers.* Wundt, Heymans, and Einthoven, as well as many others, have offered explanations of the Müller-Lyer illusion and have endeavoured to account for the feature of a maximal illusion. Wundt explains the illusion as due to the free or impeded movements of the eyes when observing the feather-head or arrow-head figure respectively. He explains the phenomenon of the maximal illusion as follows. When the arms of the feather-head figure are short, the eye moves beyond the extremities of the middle line to the ends of the arms, but when the arms are long, they cause a backward movement of the eye towards the middle line; and it is this backward movement that

accounts for the decrease in the amount of the illusion. Evidently, Wundt does not take into consideration the fact that there is also a maximal illusion in the arrow-head figure, as the arms are lengthened.

Heymans has formulated what has been termed 'a contrast-movement' theory of this illusion, in which he also attributes the illusion to the character of the movements of the eyes. His explanation of the maximal illusion is as follows. When the eyes of the observer begin to move from one end of the middle line, the arms at that end are seen directly, whilst the arms at the other end are seen but indirectly. The arms that are seen directly produce the illusory effects of the figures, whilst the arms seen indirectly counteract the illusory effects. When the arms are short, those seen directly have the greater influence upon the perception of the figure; whereas when the arms are long, those seen indirectly have the greater influence; and it is for this reason the illusion has a maximal value. It would be very difficult to justify these various assumptions which Heymans makes to explain this feature of the illusion.

Einthoven's 'dispersion theory'[1] attributes this illusion to the fact that the end parts of the figures are indistinctly seen, because their images fall on the peripheral parts of the retina. He explains the fact that the illusion does not increase continuously as the arms are lengthened, as partly due to the arms becoming more independently seen, and partly due to their increasing limiting influence. Einthoven refers to this feature of the Müller-Lyer illusion as the 'limiting value' (Grenzwerth); in so doing, he neglects the fact that the illusion decreases in amount after the maximal value is reached.

9. *The author's explanation.* A result of theoretical interest in the present investigation is that which justifies the correlation of the Müller-Lyer illusion with that hitherto known as the 'illusion of contrast' (Fig. 8). These two illusions, in spite of the obvious geometrical relation between them, have previously been considered to be due to quite different factors. Thus Wundt explains the Müller-Lyer illusion as due primarily to the character of the movements of the eyes, whilst he attributes the 'illusion of contrast' to psychological factors of a very different nature. It is true that Müller-Lyer considered both these forms of illusion to be of a psychological nature; but he attributed the one illusion to confluxion effects, and the other to contrast effects.

[1] *Arch. f. d. ges. Physiol.* Bd. 71, S. 1.

Müller-Lyer emphasised the difference rather than the similarity between these illusions.

Of late years there is a growing tendency to regard the Müller-Lyer illusion as due to psychological factors. After considerable experimental investigation of the Müller-Lyer figures, the writer has been led to regard the illusory appearance of these figures as due to the unrecognised influence of the perception of the whole figures upon the perception of their parts. Thus in the feather-head figure, the outward arms increase the length of the figure as a whole; and when judging the length of the middle line the observer fails to restrict his attention completely to that part, with the result that he unconsciously attributes to it, more or less, the greater length of the whole figure.

The so-called 'illusion of contrast' has generally been considered to be of a psychological nature. It has been supposed that the illusory appearance of this figure is due to the contrast effects between terminal and central parts of the line. The results of the present investigation do not support this view. The illusion of overestimation of the central part in Fig. 8 increases within certain limits as the terminal parts are lengthened. Moreover, the central part (50 mm. long) is underestimated when the terminal parts are 35 mm. long. Both these facts seem inexplicable if we attribute the illusion to contrast effects between the terminal and central parts. An explanation in accordance with the above results is the following. The middle line is overestimated because the observer fails to restrict his attention to the central part of the line, with the result that he tends to attribute the length of the whole line to the central part alone. Hence the illusion of this figure is due to confluxion and not contrast, and it is a limiting case of the Müller-Lyer figure not only in a geometrical, but also in a psychological sense.

It is natural that the illusion of these figures should increase as the arms are lengthened. But as the difference between the length of the middle line and that of the whole figure increases, the tendency to contrast these two magnitudes manifests itself. Confluxion and contrast effects are opposing factors which influence our perception of these figures, and we may thus account for the feature of maximal illusion. As the arms are lengthened the confluxion effect predominates at first; but sooner or later, the counteracting contrast effect becomes more marked, and ultimately predominates. It is important to emphasise that the confluxion and contrast effects are not due to comparing or contrasting the various parts of the figures. The confluxion effect consists in the magnitude of the part partaking of the magnitude of the whole; and the contrast effect consists in the exaggeration of the difference between the magnitude of the part and the magnitude of the whole.

The conditions which determine whether confluxion or contrast is manifested are problems awaiting future investigation. Müller-Lyer formulated those conditions as follows. 'Extensionen treten in Konfluxion, wenn sie parallel laufen, und sie kontrastieren wenn sie in entgegengesetzter Richtung liegen oder senkrecht zu einander stehen[1].' It is obvious, however, that confluxion and contrast are not determined to any great extent, if at all, by the directions in which the lines lie. The results of the present investigation suggest one important condition of confluxion and contrast. (It is a well-known fact, which needs no demonstration here, that the Müller-Lyer illusion diminishes, the more independently the observer is able to perceive the middle line.) When the middle line forms the most important part of the whole figure, the arms being short, the confluxion effect predominates, because the magnitude of the middle line is confused with the magnitude of the whole figure. If the arms are considerably lengthened whilst the middle line remains constant, the increased difference between the length of the middle line and that of the whole figure prevents the observer confusing them. This independent apprehension of the middle line is evidently accompanied by the tendency to exaggerate the difference between its magnitude and that of the whole figure, which is the contrast effect observed. Thus confluxion is due to the confusion of two magnitudes which are nearly equal; whereas contrast is the exaggeration of the difference between two magnitudes which are independently apprehended.

10. *Summary.* The following are the chief results of the present investigation.

i. There exists a maximal illusion in the Müller-Lyer figures, when the arms either of the feather-head or of the arrow-head figure are lengthened.

ii. Similarly, in the so-called 'illusion of contrast' (Fig. 8)—which is geometrically a limiting case of the feather-head figure—there is a maximal illusion when the terminal parts are lengthened.

iii. The fact that the Müller-Lyer illusion and the 'illusion of contrast' possess this feature in common, supports the view that they

[1] *Ztschr. f. Psychol. u. Physiol. d. Sinnesorg.* Bd. 9, S. 1.

have a similar psychological explanation. The explanation which is most in accordance with experimental investigation is that the illusions are due to confluxion.

iv. Confluxion and contrast effects are counteracting factors in these illusions; this explains the feature of the maximal illusion. As the arms of the figures are lengthened, the confluxion effect decreases whilst the contrast effect increases.

v. The confluxion and contrast effects in these illusions are determined by the relative magnitudes of the whole figure and the middle line, and not by the relative magnitudes of the middle line and the remaining parts of the figure.

vi. Confluxion is due to the confusion of two nearly equal magnitudes; whereas contrast is due to the exaggeration of the difference between independently apprehended magnitudes.

COLOUR PREFERENCES OF SCHOOL CHILDREN.

By W. H. WINCH.

I. The Problem stated. II. The Method Employed. III. Results from Girls' Schools. IV. Results from Adult Women. V. Results from Boys' Schools. VI. Results from Adolescent Males. VII. Results from Adult Males. VIII. Results from Standard I children. IX. Colour Preferences in relation to Age. X. Colour Preferences and the teaching of Colour-Work in School. XI. Colour Preference and Colour Sensibility. XII. Pedagogical value of these experiments. XIII. Summarized conclusions.

I. The Problem Stated.

It is, I think, beginning to be thought by some of those who are working at the aesthetics of colour-combinations that the principle of harmony by contrast and the principle of harmony by analogy, which, at first sight, seem hopelessly at variance, may be themselves harmonized if, as appears likely, the range of each principle varies with a series of positions in an evolutionary scale of preference. But I do not think that it is so usually believed that a similar regularity of change and development may attach to the preferences felt for the various colours themselves, not as combinations, but separately.

Some work on colour perception with very young children led me to think that, so far as these colour preferences were concerned, there might be some sort of regularity of choice and some sort of regularity of development in choice, as children increased in age and mental proficiency.

Accordingly, I set out to endeavour to determine:

(1) Which, if any, of the simple colours were preferred by school children.

(2) Whether the preferred colours changed as the children increased in age and intellectual proficiency.

(3) Whether there were any sex differences of a constant nature so far as colour preference was concerned.

(4) Whether these preferences varied according to the social status and home environment of the child.

(5) Whether the colour work done in schools influenced the development of colour preferences.

(6) Whether any differences of a constant nature existed between the simple colour preferences of children and those of adults.

II. THE METHOD EMPLOYED.

A first series of experiments was made in the Senior departments of municipal schools. The method adopted was identical throughout.

The following words were written by the teacher or myself in the order:—White, Black, Red, Green, Blue, Yellow.

The children were then asked to write on a small slip of paper the name of that one of those colours which they liked the best and thought the prettiest.

When this had been done, I asked them which they would choose supposing they could not have the one they had picked out first. When they had made up their minds they were to write down the second name under the first.

When this was done, I said to them, 'Suppose you could not have either the prettiest one or the next best one, but *had* to choose from those that are left, which would you have? When you are quite sure, write the name down under the other two.'

So we proceeded until only one colour-name was left and that was written at the bottom of the list.

The operations appeared to arouse great interest, especially among the girls; and that the method was well understood is shown by the fact that, though there were some 2000 papers from children ranging from 7 to 15 years of age, there was not a single voting paper spoiled: in no case was any colour-name omitted: in no case was any one name written twice. This is to be attributed largely to the excellent tone and discipline of the schools in which the work was done, and somewhat to the method, which requires one preference to be made before any other is asked for, and so proceeds by easy steps. No child, class or school which did the work communicated in any way with any other child, class or school until all the papers had been written. The choices made represent the individual judgments of the children and, as I was anxious to guard against any form of unconscious suggestion by the teacher, I took all the observations myself.

As is obvious, any sort of purposive or ulterior considerations would have been fatal to the regularity of aesthetic choice. Nor were they operative, I think, except in one class. One teacher noted that the 'grubbiest' girl in her class had put 'Black' at the top and 'White' at the bottom. This she thought was very natural, but, with careful non-suggestive questioning, I found that the girl had not written 'Black' at the top because, as the teacher thought, she liked dirt, but because it 'did not show the dirt.' This was the only class in which questioning about any unusual answer revealed that the choice was not a fair one. By a fair choice I mean one made on the grounds of aesthetic preference for the colour. A fiercely contested election, in which Red and Blue figured largely, Red being triumphantly victorious, had raged around two of the schools a day or two previous to these observations; but the results show no variation from those of other schools which were in parts of London quite untouched by any political disturbances.

Each worked paper looked liked this, though the order of the colours was, of course, not the same on all the papers—

John Smith
Standard II
red
blue
yellow
green
white
black.

If we allow 6 votes for the 1st choice, 5 votes for the 2nd choice, 4 for the 3rd, 3 for the 4th, 2 for the 5th, and 1 for the last one, we have a very easy method of clearly exhibiting the summarized preferences for each colour.

It must, of course, be conceded that this is not a perfectly adequate measure of the differences in preference. One child may, for example, write 'Blue' first, after long hesitation between that and 'Red'; another may unhesitatingly prefer Blue; but in our scale of marks each ranks the same, namely, as having given 6 marks to Blue. And, of course, our scheme supposes that the difference in felt preference between, say, the first and second colour written down is equivalent to the difference in preference between, say, those written fourth and fifth.

And we give one mark to the colour-name which is not chosen at all, but merely placed at the bottom of the list.

But, whilst fully admitting all these imperfections in the method, the results themselves show that it is capable of giving us, with surprising uniformity, a very accurate measure of the colour preferences of normal groups, by which I mean groups representative of whole school populations and selected on principles among which knowledge of, and interest in, colours were not ostensibly included.

There is little doubt in the main that the children voted for full-toned and saturated colours, and that less range of choice in tint and shade actually occurred than the method makes possible. I had purposely refrained from presenting coloured objects and deliberately used the names only. I expected many questions as to what tint or shade I meant; did I, for example, mean light blue or dark blue, light red or dark red; but did not receive a dozen altogether in all the schools in which the observations were taken.

I thought, if I used names only, it would be much easier to compare the preferences of these English school-children with those of children in other countries, say, those of America, France and Germany, and also, perhaps, those of the Far East, e.g., Japan. If the method is used for this purpose, it will be well to remember that our English Standard VII approximates to Grade VIII in the United States, and to the Eighth School Year in Germany, that is, to Class 1 in the *Achtstufigen* Schools; Standard VI corresponds to Grade VII and Class 2; and so on.

It would be interesting to discover whether the 'red, white and blue' of the flags of England and the United States and France is an historical accident, so to speak, or whether it is a consequence of some fundamental colour preferences, national or racial? Is the green, yellow and black (white, we may remember, is the colour of mourning in some parts of the East) as typical of Oriental choice as red and blue seem to be of the Occident?

III. Results from Girls' Schools.

I have now some two thousand worked papers from eight or nine different schools in various parts of London; and, since I am trying to find whether sex makes any difference to a child's colour preferences, I must tabulate the results of the boys' and girls' schools separately: and, since the schools were chosen in order to be unlike in environment and to include schools attended by children of different social classes, I must tabulate the results of the different schools separately. Let no one suppose that the children attending these schools are all of one social class. There is in London, just as in the great cities of the United States and just as there is in Berlin and Leipsic, a very considerable difference in social character between schools confounded together in our country under the general term Public Elementary Schools, in America as Primary and Grammar Grades, and in Berlin as *Volkschulen*. And, since I am trying to discover whether there is any relation between colour preference and general mental proficiency, I must tabulate the results of each Standard separately. Our Standards do not, and rightly do not, represent stages of mental proficiency common to all our schools. But there is some approximation to equality between a given Standard in one school and the same Standard in another, though, perhaps, not quite so much as in Germany is produced by the *Lehrpläne*, and in the United States by the official Courses of Study, neither of which are in use in this country.

The first school in which the observations were made is pleasantly situated in a good neighbourhood. The results follow:

TABLE I.

Showing the average vote given for each colour by the pupils of each standard or grade. School F. 285 girls.

Standard	No. of children	Average age yrs.	mths.	White Av. vote	White M.V.*	Black Av. vote	Black M.V.	Red Av. vote	Red M.V.	Green Av. vote	Green M.V.	Blue Av. vote	Blue M.V.	Yellow Av. vote	Yellow M.V.
II	54	9	3	3·3	1·2	1·4	·6	4·5	1·1	3·1	·9	4·9	1·0	3·7	1·2
III	70	9	11	3·9	1·2	1·5	·7	4·0	1·0	3·6	1·2	5·1	·9	2·9	1·0
IV	66	11	2	3·8	1·1	1·2	·4	4·2	1·1	3·6	1·2	5·0	·9	3·1	1·2
V	48	11	11	3·9	·9	1·3	·5	4·3	1·1	3·6	1·0	5·6	·6	2·2	·7
VI	21	12	5	3·6	1·1	1·3	·4	4·3	1·1	4·0	1·1	5·1	·8	2·5	1·1
VII	26	13	10	3·7	·8	1·5	·6	4·1	1·0	4·0	1·0	5·3	·8	2·3	1·1

* The mean variation (M.V.) is the average deviation of the vote of the individuals comprising the group from the mean of the total votes given. Those who wish to subject these results to further statistical treatment may perhaps be reminded that it is not unusual to multiply this number by the constant 1·2533 to obtain an approximation to "σ," the standard deviation. Working out "σ" by this method for Standard VII, and comparing it with the standard deviation obtained directly, we find

	White	Black	Red	Green	Blue	Yellow
"σ" (calculated)	1·00	·81	1·30	1·30	1·02	1·42
"σ" (found directly)	1·02	·79	1·25	1·31	·99	1·51

A rough glance at this Table will show that, in every class, the highest average vote is given to Blue, and the variation in the voting for that colour is small. Black is unhesitatingly placed at the bottom with an average vote that never rises above 1·5, and with extremely little variability in the voting. Red is placed second with an average vote which varies between 4·5 and 4·0. The variability of the vote for Red, though surprisingly regular from class to class, is still higher than that for Blue and Black. Now, however, we come to colours whose attractiveness appears, on a close scrutiny, to vary with the age and mental proficiency of the children. As we glance down the column showing the average vote for White it seems, at first sight, to vary but little from standard to standard, and we should be disposed to say that White was 3rd in order of attractiveness; but closer attention reveals that, with the youngest group (Standard II) Yellow has obtained more votes than White and, in the two oldest groups (Standards VI and VII), White, though still maintaining a high average vote, has been beaten by Green, the attractiveness of which appears to increase as the children develop mentally. Standard II gives an average vote of 3·1 to Green, placing it 5th in order of choice; in Standards III, IV and V it rises to the 4th place with an average vote of 3·6, and in the first class, consisting of Standards VI and VII, it climbs still higher and takes the 3rd place with an average vote of 4·0. The attractiveness of Yellow which begins high, with an average vote of 3·7, and takes the 3rd place, falls rapidly to the 5th place and stops there; and there is no doubt at all, even if we depended wholly on the results of this one school, that the preference for Yellow diminishes as the children develop

It will, I am sure, be found of assistance to the reader, if, for a moment, neglecting to consider the exact numerical differences in the voting for the different colours, I arrange the results to show the order of preference in which the various colours were placed by the various standards or classes. Thus a figure 1 means that the colour to which it is opposite obtained most votes, a figure 2 that the colour obtained the highest vote but one, and 6 means that the colour received the smallest number of votes.

TABLE II.

Showing the order in which the colours were chosen by the various standards of School F.

	II	III	IV	V	VI	VII
Blue.........	1	1	1	1	1	1
Red	2	2	2	2	2	2
White	4	3	3	3	4	4
Green	5	4	4	4	3	3
Yellow	3	5	5	5	5	5
Black	6	6	6	6	6	6

The second school in which the observations were made is situated in an extremely poor neighbourhood; it is indeed classed as one of 'special difficulty.' I propose to set out the results in the two forms already used.

TABLE III.

Showing the average vote given for each colour by the pupils of each standard or grade. School L. 277 *girls.*

Standard	No. of children	Average age		White		Black		Red		Green		Blue		Yellow	
		yrs.	mths.	Av. vote	M.V.	Av. vote	M.V.	Av. vote	M.V.	Av. vote	M.V.	Av. vote	M.V.	Av. vote	M.V.
II *a*	59	9	5	3·4	1·2	1·5	·7	4·1	1·2	3·3	1·1	4·8	1·0	4·0	1·3
III	99	10	6	3·4	1·2	1·4	·6	4·2	1·0	3·5	1·2	5·0	·9	3·6	1·4
IV *a*	48	11	2	4·0	1·0	1·3	·5	4·2	·9	3·6	1·2	5·2	1·0	2·7	1·1
V *a*	39	12	7	4·0	1·0	1·7	·9	4·3	·8	3·3	1·4	5·3	·8	2·2	·9
VI	21	13	0	4·0	·8	1·8	·9	4·7	·8	2·9	1·0	5·5	·6	2·1	·9
VII	11	13	4	4·2	·9	1·7	·8	4·3	1·3	3·3	·8	5·5	·5	2·0	·9

TABLE IV.

Showing the order in which the colours were chosen by the various standards of School L.

	II *a*	III	IV *a*	V *a*	VI	VII
Blue.........	1	1	1	1	1	1
Red	2	2	2	2	2	2
White	4	5	3	3	3	3
Green	5	4	4	4	4	4
Yellow	3	3	5	5	5	5
Black	6	6	6	6	6	6

In this school, as in the last, Blue heads the list in every standard, and rises steadily in attractiveness from class to class; and Black again occupies the lowest place. The votes given for Blue are very similar to those given in the former school, but Black receives a higher vote, though the variability of the voting for it is greater than before. Red again clearly occupies the 2nd place in order of preference. White, after occupying the 4th position in Standard II, as in the case of the former school, drops slightly in *order* of preference in Standard III (the average vote for White is 3·4 in Standard II and 3·4 in Standard III, but the vote for Green has risen from 3·3 to 3·5), rises again in Standard IV *a*, and occupies the 3rd place, which it maintains to the end. It is not in this school, as in the last, pushed into the 4th position again by a rise in the attractiveness of Green. Green begins in the 5th place, rises to the 4th position and stops there. Yellow stands 3rd in Standard II and does not, in this school, sink to the 5th place until Standard IV, whereas, in the former school, it sank to that position in Standard III.

TABLE V.

Showing the average vote given for each colour by the pupils of each standard or grade. School S. 263 girls.

Standard	No. of children	Average age		White		Black		Red		Green		Blue		Yellow	
		yrs.	mths.	Av. vote	M.V.	Av. vote	M.V.	Av. vote	M.V.	Av. vote	M.V.	Av. vote	M.V.	Av. vote	M.V.
II	57	8	11	3·2	1·0	1·5	·8	4·2	1·3	3·1	1·1	5·0	·9	3·9	1·2
III *b*	27	11	1	3·6	1·3	1·4	·6	4·3	1·1	3·5	1·0	4·9	1·2	3·5	1·5
III *a*	47	10	8	3·5	1·2	1·4	·6	4·4	1·0	3·1	1·0	5·0	1·0	3·4	1·3
IV	50	11	2	3·4	1·2	1·5	·9	4·5	1·1	3·3	1·2	5·0	1·0	3·0	1·2
V	38	12	1	4·1	1·1	*2·2	1·3	4·2	1·0	3·3	1·0	5·0	·9	2·2	1·0
VI	37	13	1	3·1	·9	1·0	·0	4·8	·6	3·6	1·0	5·6	·6	2·9	·7
VII	7	13	3	3·3	1·2	1·0	·0	4·3	·8	4·0	1·3	5·6	·7	2·8	·7

Note. In Standard III *b* the total number of votes for Yellow = 96.
,, ,, ,, ,, Green = 94.
,, V ,, ,, Yellow = 84.
,, ,, ,, ,, Black = 82.

* This high vote for Black was given in a class containing the girl alluded to on p. 44. I had apparently not made it altogether clear in that class that I wanted the *prettiest* colours picked out. The unusual variability seems of itself to suggest that a new consideration is influencing the choice of some of the children.

Summing up: Whilst the most marked characteristics of the two tables for schools F and L are strikingly alike, yet I suggest that it is important to note that, in the latter, the preference for Yellow is higher at the commencement of school life and does not drop so soon; and that the preference for Green does not show the same rise in the higher classes.

The third school in which observations were taken was situated in a neighbourhood intermediate in type to the two already mentioned.

The results are shown in Tables V and VI:

Table VI.

Showing the order in which the colours were chosen by the various standards of School S.

	II	III *b*	III *a*	IV	V	VI	VII
Blue	1	1	1	1	1	1	1
Red	2	2	2	2	2	2	2
White	4	3	3	3	3	4	4
Green	5	5	5	4	4	3	3
Yellow	3	4	4	5	5	5	5
Black	6	6	6	6	6	6	6

Again Blue heads the list throughout, Red stands second, and Black takes the bottom place. Our interest then attaches itself to the colours which are chosen with varying preferences by different classes. White, as in both the previous schools, occupies the 4th place in Standard II but, unlike the second school, though like the first one, it rises to the 3rd position in Standard III, and maintains it, until it is pushed down one place by the increasing attractiveness of Green. Again, as in the first school, but unlike the second, the preference for Green rises markedly in the highest classes. Yellow, as before, begins 3rd in Standard II and sinks in preference as in the former schools; less sharply, however, than in School F, where it occupies the 5th place as early as the Third Standard.

The Girls' School in which the next observations were taken is situated in a very good suburban neighbourhood with surroundings almost rural in character. Both the children and their surroundings are probably of the highest type to be found in connection with urban elementary schools.

TABLE VII.

Showing the average vote given for each colour by the pupils of each standard or grade. School D. 271 girls.

Standard	No. of children	Average age yrs.	mths.	White Av. vote	White M.V.	Black Av. vote	Black M.V.	Red Av. vote	Red M.V.	Green Av. vote	Green M.V.	Blue Av. vote	Blue M.V.	Yellow Av. vote	Yellow M.V.
II	41	9	2	3·4	1·1	1·3	1·1	3·8	1·3	4·0	1·1	4·7	1·1	3·8	1·0
III	55	10	8	3·4	1·2	1·5	·7	3·7	1·0	4·2	1·2	4·8	1·0	3·4	1·4
IV	61	11	3	3·4	1·3	1·2	·3	4·1	1·1	4·4	1·1	5·0	·8	3·2	1·2
V	48	12	4	3·5	1·2	1·2	·4	4·0	1·0	4·0	·9	5·4	·7	3·0	·9
VI	36	12	9	3·2	1·0	1·1	·1	3·7	1·0	4·7	·6	5·2	·8	3·1	·9
VII	30	13	10	3·5	1·0	1·1	·1	4·2	·8	4·2	·9	5·6	·6	2·5	·8

In Standard II the total vote for Yellow = 156.
" " " Red = 155.
" III " " White = 187.
" " " Yellow = 185.
" VII " " Green = 126.
" " " Red = 125.

TABLE VIII.

Showing the order in which the colours were chosen by the various standards of School D.

	II	III	IV	V	VI	VII
Blue	1	1	1	1	1	1
Red	4	3	3	3	3	3
White	5	4	4	4	4	4
Green	2	2	2	2	2	2
Yellow	3	5	5	5	5	5
Black	6	6	6	6	6	6

As in the three schools previously considered, Blue occupies the 1st place in each class, and Black occupies a more than usually decisive position at the bottom of the list. Again, in similarity with the other cases, Yellow is chosen 3rd in the lowest standard but sinks rapidly to the 5th place, without passing through the fourth position as it does in School S. The average vote for Yellow shows a steady decline and it is really only one vote above Red in Standard II. Red takes the 3rd place throughout and White rises from the 5th to the 4th place. But the feature of greatest interest in this Table is the extraordinary position occupied by Green, which ranks 2nd in every class. We have seen in previous cases that Green becomes a favourite only in the higher standards and then never rises higher than the 3rd place. Can we account for our present result by the superiority of the type of children or the verdant character of the surroundings of their school? I should incline to say, having regard to the rise of Green in Schools S and F, and to the fact that it does not increase in favour in School L, that both factors operate. Children of superior development in other schools appear to favour Green when they are from 12 to 14 years old; and the vote for Green is more decisive in the upper standards of this school, as we see by the smaller variability of the voting. But it may very fairly be urged that this preference for Green is not a preference which is shown just a year or two earlier in this school than in others—which is what we should expect if the result were due to superiority in type—but is well established from the start. This *fact* seems to indicate that much of the difference may be due to environment.

IV. Results from Adult Women.

A second series of observations was made on adult women, and I propose at this point, for purposes of comparison, to give the results of some observations on themselves kindly made for me by the women teachers of five municipal schools. In this case the question at once arose as to the use and purpose of the colour. My answer was that each person should, as far as she could, consider simply a 'splash' of colour without thinking of use or purpose in any way. The observers were 41 in number, ranging in age from 18 to 46 years, with an average age of 30 years 7 months.

Table IX.

Showing the colour preferences of adult women.

	Average vote	M.V.	Order of preference
Blue	5·1	·8	1
Red	2·9	·9	4
White	3·8	1·2	3
Green	4·7	1·1	2
Yellow	2·8	1·7	5
Black	1·7	·8	6

This Table shows some of the characteristic features common to girls. Blue clearly takes the 1st place, Yellow the 5th and Black the last. Green occupies the second place and we may remember that in two of the schools the preference for Green rose steadily as the pupils advanced in mental development, and that, in the school situated in the best neighbourhood, it was a pronounced feature of choice in all standards, but rose very little indeed, if at all, in the school situated in the very poor neighbourhood. I suggest then that a preference for Green may probably be a characteristic of fairly high mentality. Red sinks, restoring White to the 3rd place. I am not sure that there is any clear indication in the results from Girls' Schools that this decrease in the liking for Red was likely to happen, though, in so far as any change in position occurred at all, it seemed to fall. I regret that I have no observations on a group of adolescent females intermediate between the school children and these well-educated adult women.

V. RESULTS FROM BOYS' SCHOOLS.

A third series of observations was made in Boys' Schools and it will probably be convenient, before making any further suggestions, to turn our attention to the results obtained from these. The first Boys' School in which these observations were made is situated in a rather poor neighbourhood and contains a very homogeneous class of children.

TABLE X.

Showing the average vote given for each colour by the pupils of each standard or grade. School OK. 297 *boys.*

Standard	No. of children	Average age yrs.	Average age mths.	White Av. vote	White M.V.	Black Av. vote	Black M.V.	Red Av. vote	Red M.V.	Green Av. vote	Green M.V.	Blue Av. vote	Blue M.V.	Yellow Av. vote	Yellow M.V.
II	61	9	0	2·8	1·0	1·6	·8	4·8	1·0	3·5	1·1	4·5	1·2	3·8	1·2
III	64	10	0	3·0	1·1	1·3	·5	5·0	·8	3·4	1·2	4·5	1·1	4·0	1·0
IV	53	10	11	3·0	1·1	1·5	·8	4·7	1·0	3·5	1·6	4·6	1·1	3·6	1·0
V	63	12	0	2·6	·9	1·6	·8	4·6	1·1	3·8	1·1	4·9	·9	3·3	1·0
VI	34	12	8	2·8	1·1	1·0	·2	4·7	1·1	4·0	·9	5·0	1·2	3·3	·9
VII	22	13	4	2·5	·9	1·4	·6	4·7	1·0	3·7	1·1	5·3	·7	3·4	1·1

TABLE XI.

Showing the order in which the colours were chosen by the various standards of School OK.

	II	III	IV	V	VI	VII
Blue.........	2	2	2	1	1	1
Red	1	1	1	2	2	2
White	5	5	5	5	5	5
Green	4	4	4	3	3	3
Yellow	3	3	3	4	4	4
Black	6	6	6	6	6	6

I have written the names of the colours in Table XI in what, perhaps, I may now be permitted to call the 'Girls' order' of preference. By so doing I shall bring to light at once certain outstanding differences between boys and girls, and I do not think it will be much more difficult to make out the order of choice for boys of different classes than it would be if the names were arranged in 'Boys' order.' In fact we may note at once that, in the three higher standards, the boys' and girls' order of preference would be the same were it not for the very different position of White, which is 5th throughout in the boys' classes, and does not appear to rise in preference as it does in the girls', but even to lose votes (see Table X) as the children develop. As with girls, Yellow begins well but loses place afterwards, though more slowly

TABLE XII.

Showing the average vote given for each colour by the pupils of each standard or grade. School W. 258 boys.

Standard	No. of children	Average age		White		Black		Red		Green		Blue		Yellow	
		yrs.	mths.	Av. vote	M.V.	Av. vote	M.V.	Av. vote	M.V.	Av. vote	M.V.	Av. vote	M.V.	Av. vote	M.V.
II	58	8	5	2·7	1·1	1·7	1·0	4·4	1·2	3·3	1·0	4·6	1·2	4·4	1·2
III	60	9	9	2·8	1·1	1·2	·3	4·3	1·1	3·7	1·1	5·2	·9	3·9	·9
IV	38	10	8	2·8	1·1	1·3	·5	4·5	1·1	3·4	1·1	4·8	1·1	4·2	1·1
V	41	11	10	2·6	1·0	1·3	·4	4·5	1·1	3·9	·9	5·0	·9	3·9	1·0
VI	35	12	8	3·0	1·1	1·7	·9	4·7	1·0	3·6	1·2	5·0	·9	3·0	1·1
VII	26	12	10	2·9	1·0	1·4	·6	4·4	1·3	3·9	1·1	5·3	·9	3·1	·9

In Standard II the total vote for Yellow was 257.
,, VI ,, ,, Red ,, 256.

than in the case of Girls' Schools. Green rises as with girls; it takes a higher place than in all the Girls' Schools except School D; and, in this school, Red in the lower standards takes precedence of Blue. I am not quite sure that this is normal for boys. Black as before is hopelessly at the bottom.

The next school in which the observations were made was one attended by children of a superior class to the boys of School OK, though not by any means equal to those attending the Girls' School D.

TABLE XIII.

Showing the order in which the colours were chosen by the various standards of School W.

	II	III	IV	V	VI	VII
Blue	1	1	1	1	1	1
Red	3	2	2	2	2	2
White	5	5	5	5	5	5
Green	4	4	4	3·5	3	3
Yellow	2	3	3	3·5	4	4
Black	6	6	6	6	6	6

Blue, as in the case of all the Girls' Schools, takes the first place throughout, differing from its positions in the three lower standards of the Boys' School previously given. Yellow has an unusually high place at first; but the Standard II of this school is much younger than those of all the preceding ones. White, as in the case of the preceding Boys' School, and unlike its position in the Girls' Schools, maintains a steady place as 5th, and, as also in the case of the previous Boys' School, with a remarkably steady mean variation in the voting. The curves of preference for Green and Yellow cross each other at Standard V. The children of this school are younger, standard for standard, which appears to me possibly to explain the apparently later superiority of preference for Green over Yellow. But Green rises in preference and Yellow falls, just as in all the schools, both boys' and girls', hitherto dealt with. There is, I think, a suspicion aroused that still younger children might place Yellow first of all, a question reserved for further inquiry. Black has never varied from the bottom place in any class of either boys or girls, and does not here.

The third Boys' School in which these observations were taken was situated in a rather better neighbourhood than School W. The results follow:

TABLE XIV.

Showing the average vote given for each colour by the pupils of each standard or class. School FA. 270 boys.

Standard	No. of children	Average age yrs.	Average age mths.	White Av. vote	White M.V.	Black Av. vote	Black M.V.	Red Av. vote	Red M.V.	Green Av. vote	Green M.V.	Blue Av. vote	Blue M.V.	Yellow Av. vote	Yellow M.V.
II	57	8	6	2·8	1·1	1·7	·9	4·3	1·0	3·3	1·1	4·7	1·2	4·2	1·3
III	54	9	9	2·4	·8	1·3	·5	4·6	1·1	4·1	1·2	4·3	1·0	4·2	1·1
IV	54	10	11	3·0	1·2	1·2	·4	4·4	1·0	4·0	1·2	5·1	·8	3·3	1·1
V	50	11	10	2·5	·8	1·2	·3	4·4	·9	4·3	1·2	5·2	·8	3·5	·9
VI & VII	55	12	9	2·4	·8	1·2	·4	4·3	·9	4·1	1·2	5·0	·8	3·9	1·1

Standards VI and VII are taught together as one class; the Standard VII is very small owing to the yearly transfer of suitable pupils to a neighbouring Higher Grade School, not to natural inferiority of the children or bad teaching.

TABLE XV.

Showing the order in which the colours were chosen by the various standards of School FA.

	II	III	IV	V	VI & VII
Blue	1	2	1	1	1
Red	2	1	2	2	2
White	5	5	5	5	5
Green	4	4	3	3	3
Yellow	3	3	4	4	4
Black	6	6	6	6	6

It will readily be seen that the order of choice is quite characteristically what I may now, perhaps, call the 'Boys' order' of preference. It would be tedious again specifically to state what its main characteristics are, but I cannot refrain from calling attention to the crossing of the curves for Green and Yellow, this time somewhat earlier, namely, between Standards III and IV. Red stands above Blue in Standard III. Taking this result in conjunction with that of School OK, we may, perhaps, be justified in asserting that, with boys, the preference for Blue over Red is not, as it apparently is with girls, secure at all ages.

The Boys' School in which the next observations were taken was situated in a neighbourhood which was similar to that of School OK, though, perhaps, not quite so homogeneous. The results follow:

TABLE XVI.

Showing the average vote given for each colour by the pupils of each standard or grade. School SA. 240 boys.

Standard	No. of children	Average age		White		Black		Red		Green		Blue		Yellow	
		yrs.	mths.	Av. vote	M.V.	Av. vote	M.V.	Av. vote	M.V.	Av. vote	M.V.	Av. vote	M.V.	Av. vote	M.V.
II	68	9	4	3·1	1·1	1·2	·4	4·3	1·0	3·2	1·0	5·1	·9	4·2	1·1
III	51	10	5	2·9	1·1	1·3	·5	4·5	1·1	3·4	1·0	5·1	·7	3·8	1·2
IV	40	10	10	2·5	·8	1·2	·3	4·7	·9	3·8	·8	5·2	·9	3·6	1·1
V	45	11	9	2·7	·9	1·2	·4	5·0	·9	3·6	1·0	4·8	1·0	3·7	1·2
VI & VII	36	12	10	2·2	·4	1·1	·1	4·5	1·1	4·7	·8	4·8	·9	3·7	1·0

Standards VI and VII are taught as one class; there were only two or three children of Standard VII attainments.

TABLE XVII.

Showing the order in which the colours were chosen by the various standards of School SA.

	II	III	IV	V	VI & VII
Blue	1	1	1	2	1
Red	2	2	2	1	3
White	5	5	5	5	5
Green	4	4	3	4	2
Yellow	3	3	4	3	4
Black	6	6	6	6	6

These results follow the same general lines as those of the former school for boys. White and Black remain 5th and 6th in every class. Green commences in the 4th position and Yellow in the 3rd and their curves of preference cross in the middle of the school; Green, indeed, eventually reaches the 2nd place, pushing Red down to the 3rd position. But my reader has, doubtless, already noticed the unusual order in Standard V. The observations were made again in this class a week later, but no alteration occurred in the order of choice; and I have given, of course, the actual figures of the first exercise so that they should be properly comparable with those of the other standards. I can only suggest the possibility that this class contained a number of boys of unusually primitive taste for their age: they not only rank Red above Blue but Yellow (slightly, see Table XVI) above Green, both of which features appear to belong to a less developed type than that of the usual Standard V boy. The class is not, however, regarded by the Head Master as generally weak, though he admits the presence of a bad "tail end." I regret that I am unable to present a separate table for the "tail end."

VI. Results from Adolescent Males.

The next observations were made in an Evening School. The pupils, 21 in number, ranged from 15 to 20 years of age with an average of 16 years and 11 months. They were occupied during the day in various forms of mechanical engineering.

Table XVIII.

Showing the average vote given for each colour and the order in which they were chosen. Evening School L. 21 *Males.*

	Average vote	M.V.	Order of preference
Blue	4·9	·9	1
Red	4·4	1·0	2
White	3·1	·9	4
Green	4·3	1·3	3
Yellow	2·7	1·3	5
Black	1·6	·6	6

There is very little difference between the choice of these pupils and the upper standards of the boys in day schools, but what differences there are seem to me significant. Green runs Red very closely for the second place and has a higher vote than in any day-school standard except that of Standard VI and VII in School SA. Yellow continues its downward movement; it has a lower vote than in any day-school class whatever, whilst the attractiveness of White definitely rises above that of Yellow, for the first time as far as males are concerned.

VII. RESULTS FROM ADULT MALES.

The next observations, for which I am gratefully indebted to Mr Cyril Austin Bostock, were made in the Solicitors' department of the London offices of the National Telephone Company. The 24 observers were all males, ranging from 20 to 39 years of age with an average of 28 years 0 months.

TABLE XIX.

Showing the colour preferences of Adult Males.

	Average vote	M.V.	Order of preference
Blue	4·8	1·0	2
Red	4·4	1·2	3
White	2·9	1·0	4
Green	5·0	·6	1
Yellow	2·4	·9	5
Black	1·5	·7	6

Green, after rising throughout with increase of age and mental proficiency, has at last attained the first place. This is the only difference in the order of choice between these results and those of the adolescent males attending evening school. The smallness of the mean variation of the voting for Green shows how decisive is its victory over Blue, which now sinks to the 2nd place. White, which with school boys (not girls) is always placed 5th, maintains the advantage over Yellow which it obtained amongst the evening school students, and that by an increased lead.

VIII. RESULTS FROM STANDARD I CHILDREN.

A sixth series of experiments was begun with younger children. Doubtless those of my readers who are acquainted with the organisation of schools have wondered why I have omitted to include Standard I in my tables. In some of the schools where the observations were made, this standard was either absent or very small indeed; and in others, in fact, in almost all senior departments, it does not form a 'class' of children in any true sense of the word 'class.' Moreover I had some doubts as to how far such a method would be satisfactory with children at this stage of development. However, I took some observations with such a Standard I in School SA (Boys) and in the same way. The results follow:

TABLE XX.

Showing the votes given for each colour by Standard I of School SA. 19 boys of an average age of 8 yrs. 10 mths.

	Total votes cast	Av. vote	M.V.	Order of choice
Blue.........	67	3·5	1·3	3
Red	82	4·5	1·0	1
White	59	3·1	1·8	5
Green	66	3·5	1·3	4
Yellow	81	4·3	1·1	2
Black	44	2·3	1·5	6

Apparently we may say, with some confidence, that there is a marked preference for Red and Yellow, a less marked preference for Blue and Green, whilst White and Black, as usual, rank 5th and 6th; but the variability of the vote is high in every case: this feature also, by its diminution in the cases of Red and Yellow, does seem to emphasize the decided preference for these two colours. Again, there seems a suggestion that, at an age earlier than this, Yellow may take the 1st place; and the predominance of Red over Blue is just what we should have expected from a consideration of the results in Standards II, III and IV in the Boys' Schools generally.

Similar observations were made in Standard I of Girls' School D. The results follow:

TABLE XXI.

Showing the votes given for each colour by Standard I of School D. 34 girls of an average age of 8 yrs. 0 mths.

	Total votes	Av. vote	M.V.	Order of choice
Blue.........	138	4·1	1·1	2
Red	149	4·4	1·3	1
White	115	3·4	1·3	4
Green	128	3·7	1·5	3
Yellow	109	3·2	1·5	5
Black	75	2·2	1·4	6

In this standard we find, for the first time in the case of any class of girls, that Red obtains the first place and Blue the second. The high position of Green is possibly explained by the school environment, as I suggested when dealing with School D as a whole. I am at a loss to explain the position of Yellow; this result is *not* in accordance with the previous figures. In every Standard II dealt with in both boys' and girls' schools except one, Yellow stood 3rd; and, in one Standard II of a boys' school and in the boys' Standard I quoted above, it stood 2nd. But, as I said above, I do not feel much confidence in the application of this method to Standard I children, and I included these observations, with their comparatively high variations, rather as a check to hasty conclusions than to serve any other purpose.

IX. COLOUR PREFERENCES IN RELATION TO AGE.

A critical reader will, doubtless, have noticed that I have made no endeavour to separate the influence of increasing mental proficiency as measured by the standard or class in which the children are placed and that of such mental changes as are more directly correlated with age.

Possibly the separation is not of serious importance, since the positive correlation between age and standard in London schools lies between ·8 and ·9*.

I have, however, thought it advisable, at least in one case, to show the preferences in relation to age only. The result follows:

TABLE XXII.

Showing the order in which the colours were chosen by children of different ages. School S. 263 *girls.*

	Ages (years)						
	8—9	9—10	10—11	11—12	12—13	13—14	14—15
Blue	1	1	1	1	1	1	1
Red	2	2	2	2	2	2	2
White	4	4	4	3	4	3	4
Green	5	5	3	4	3	4	3
Yellow	3	3	5	5	5	5	5
Black	6	6	6	6	6	6	6

* *A First Study of the Inheritance of Vision and of the Relative Influence of Heredity and Environment on Sight*, by Professor Karl Pearson and Miss Barrington, p. 37.

Turning to the previous classification of the results of School S, it will be observed that, whilst the differences between the two classifications are not great, yet the former is decidedly the more regular of the two, particularly in the regularity of the upward and downward movements of those curves of preference which cross during this period of school life.

Some evidence is thus afforded that colour preference is more closely related to general mental development than to age irrespective of that development. It did not seem to me, therefore, very profitable to make any further tabulations of this kind. Indirectly, too, some strength is added to the suggestion that young children of superior type would exhibit the colour preferences of older children of an inferior type; a conclusion, too, which seems to harmonize certain differences in the results for schools attended by pupils of different social and intellectual classes.

X. Colour Preferences and the teaching of Colour-Work in School.

One of the fundamentally interesting questions in all researches of this kind is the apportionment of the influence due to natural growth on the one hand, and direct educational environment on the other. This issue is, indeed, vital for Educational Theorists—the neglect of it has rendered many of the educational concepts of to-day of merely historical value—and I am sorry that, despite very great care, I can contribute nothing on this question with which I am completely satisfied.

I was careful to note the kind and extent of the colour-work, if any, done in each school. Let us consider the Girls' Schools first. In School F, no systematic colour-work had been done. In School L, about 30 minutes colour-work per week was done in every class, and that of Standards VI and VII is of a high order. In School S, a few minutes per week had been given during the current year only. In School D from 20 to 30 minutes per week had been given in all classes. This is the school with rural surroundings in which 'Green' occupies an unusually high position in every class. I enquired whether Green was a favourite paint in this school and was informed that the green paint supplied is a comparatively poor paint and comes off badly; it is therefore used less than the others, but that, of course, may be due to the mechanical difficulties involved, and not to lack of liking. And a glance back at the tables will show no corresponding development in the colour preferences of School L as compared with those of Schools F and S; on the contrary; but School L is in a very poor neighbourhood.

Turning now to the Boys' Schools, I found that School OK gave twenty minutes a fortnight to colour-work in Standards II and III; none was done in Standard IV and, in Standards V, VI, and VII, one hour a month was given; there are slight indications of a more primitive preference than usual in the lower standards, but the children are younger, standard for standard, than those of School SA situated in a similarly poor neighbourhood. In School W, about half-an-hour a week is given to colour-work throughout. In School FA, no colour-work of any kind is done, but the colour preferences are well developed; Yellow sinks and Green rises as early as Standard IV: the school is, however, well situated as compared with School OK. School SA gives one-and-a-quarter hours per fortnight; there are no noteworthy differences from the results of School FA.

I can only suggest that, if the colour-work has any influence in developing the colour preference of the children, it must be slight. Perhaps we have too many factors of influence, varying in each case, to be in any way sure of this negative conclusion: an experiment must, I think, be arranged *ad hoc*.

XI. COLOUR PREFERENCE AND COLOUR SENSIBILITY.

Some years ago, in attempting to draw some inferences from the order in which colour-names become known to young children to the order of the development of their sensibility to colours, the opinion was expressed in criticism that the order of naming might depend on aesthetic preference rather than on sensibility. Children, it was said, would learn the names of the colours they liked. They might *perceive* other colours just as well, but, if not attracted aesthetically, would not learn their names. It is difficult not to allow some force to this reasoning. It should be easy, however, now that we have ascertained a clear order of preference for boys and girls, to test this hypothesis by finding the correlation, if any, between this order and the sensibility to the selected colours. This might easily be done by the Tintometer test described by Dr Rivers in the Report of the Cambridge Anthropological Expedition, or by means of the colour wheel. But the position of White alone, which invariably takes the 4th or 3rd place with girls and the 5th with boys, renders it somewhat improbable, in my view, that a positive correlation would be found to exist, for I think it is usually accepted that White is perceptible equally early and equally clearly by both sexes.

XII. Pedagogical Value of these experiments.

The outstanding impression left on my own mind, and shared by the teachers who were present with me during these observations, was the great decision (with a few exceptions) with which the choices were made and the intense interest shown in making them. We are now-a-days very careful in the best teaching which is addressed to the intellect of the child, to require him to come to conclusions for himself on the basis of the facts with which he is himself acquainted. It is no extravagance to say that this sort of teaching is the most valuable that we have in the intellectual field and it will probably be found to be of equal value in the aesthetic field. I commend the method particularly to those teachers who wish to find out what their children really like; too often, teachers only find in children a miniature edition of themselves; indeed, to say they *find* it is a misuse of terms, they just guess without any clear notion as to what is fact and what is inference.

One practical head-master, on seeing the preferences shown by his boys, made a note as to the proportions of the different colours which he would henceforward order on his next requisition for school supplies. And perhaps I may repeat here that even the older children appear to have in mind, not pale tints or almost colourless shades of the colours they choose, but full-toned highly saturated samples.

It might, of course, be fairly argued that 'art shades,' as they are called, should be used in preference to these crude colours; that we should no more bow to the child's aesthetic preferences than we do to his moral judgments; but both those who would have us follow the child and those who would have us teach always in advance of the child can join on the common ground of finding out what his preferences are.

And perhaps, in any case, makers of children's toys would be justified in accepting sheer attractiveness as a guide to their products; nor should I suppose that school architects ought to be wholly uninfluenced by a knowledge of children's likings for colour. And above all, seeing that our school reading books are frequently bound with coloured covers, and that our school maps are printed in colours, there can be little objection to those colours having some relation to the child's varying preferences with varying age and sex.

XIII. SUMMARIZED CONCLUSIONS.

1. That colour preferences show regular changes in definite directions as children rise in age and mental proficiency; directions which are continued into adult life.

2. That there are some differences of a constant nature between male and female preferences.

3. That there is some evidence in favour of the view that the development of the preference depends on the social status of the children.

4. That there is no satisfactory evidence that its development depends on the colour-work done in schools.

5. That there is some evidence that colour preference is a function of general mental proficiency rather than of age.

ON MONOCULAR VISUAL SPACE.

By W. HEINRICH.

Professor of Psychology and of Methodology of the Natural Sciences in the University of Cracow.

1. OUR attitude towards the psychology of space-perception is closely related to the development of philosophical views since Descartes. For if sensory qualities are subjective modifications, space-perception, too, must be subjective—the product of psychical activity.

This point of view is responsible for the lines along which the methods of investigating space-perception have hitherto been pursued. The existence of geometrical space was presupposed, and an attempt was made to find out the sensations (or sensory characters) corresponding to every position in geometrical space. Moreover, as the quality of such sensations was regarded as dependent mostly on the quality of the stimulus or on the specific nature of the nerve stimulated, it was necessary to invoke the aid of the sense-organ in question, e.g. the eye, and the sensations (or sensory characters) arising in that organ and its muscles. Thus arose that line of investigation which inquired into the characters of sensations corresponding to the geometrical determination of the place of origin of the stimulus in order to discover, by this means, the connexion between subjective space-perception and geometrical space. Helmholtz and Hering differ, *inter alia*, in the different geometrical constructions which they employ as a means of ascertaining this connexion. And it is to this general attitude that we must attribute the assertion that monocular visual space is merely bidimensional.

2. This is not the place to trace in detail the origin of the formulation of the problem. I have already dealt with it elsewhere[1], and I hope soon to return to it in a different connexion in the Transactions

[1] *Teorje i wyniki Badań psychologicznych.* Varsaw, 1902, Vol. I. pp. 25–86.

of the Academy of Sciences in Cracow. I have there come to the conclusion that the problem of space-perception may be experimentally approached in a way different from any hitherto attempted, and that the conceptions of the *relation between geometrical space and space-perception*, which have hitherto been employed, are by no means the only, and still less the only correct, ones available. I have also shewn there that all attempts to create space-perception synthetically from spaceless qualities must end fruitlessly. It is not the province of psychology to discover a mechanism that allows of the construction of the totality of psychical life from simple elements. Its aim is on the one hand to analyse the given facts of experience (subjective analysis), and on the other to determine the connexion between these facts of experience and the changes both in the environment of the experient and in his organism (objective analysis).

If the field of psychology be thus limited, then in our present subject, viz. space-perception, the following problem arises. We have, as given, the spatial order of the surrounding world,—a tridimensional order alike in binocular and in monocular vision. We construct the geometrical space and we ask, what is the relation between the spatial order that confronts us and the geometrical space which we construct? It by no means follows that this relation must be discoverable on purely geometrical lines.

If further we take into consideration the reports provided by the subject of the experiments, we may say that our entire problem consists (a) in determining the connexion between the function of the sense-organ of the experient and the position of objects in the outer world, and (b) in formulating the connexion between these data and the experient's reports concerning the spatial order confronting him[1]. It has been the aim of our experiments to discover for this connexion other relations than those which have hitherto been sought for.

3. In the course of Dr Loria's experiments[2], which were designed to discover the planes which by means of the dioptric apparatus of the human eye are in optical correspondence with the retina, the subject of the experiment spontaneously declared that as soon as the objects exhibited were placed on a line the points of which, with a given accommodation of the lens, formed a sharp retinal image, these objects

[1] I have developed this standpoint in *Psychologia uczuc*, Cracow, 1907; see also *Bulletin de l'Academie des Sciences de Cracovie; Psychologie des sentiments*, Janvier, 1908.

[2] *Bulletin de l'Academie des Sciences de Cracovie*, Octobre, 1907, and *Zeitschrift für Psych. und Phys. der Sinnesorgane*, Bd. 40, 1905.

were seen as lying equidistant from the eye and every change from this position was interpreted as increasing or decreasing the distance of the object moved in relation to a fixed standard object.

It was evident that these observations could be utilised as a starting-point for the investigation of visual space; and accordingly I have made it my special object to examine the matter more closely.

4. The problem was as follows.—The form of the line had first to be determined (first of all in the horizontal meridian) which is such that the points composing it appear equidistant from the subject. The determination must then be made of the positions of points in the same plane, which appear to the subject as lying nearer or further than that line. (I speak here of points, but I actually used very minute lines in these experiments.)

The experiments were arranged as follows. The experimenter looked with one eye into a tube 25 cm. long and about 5 cm. in diameter. This tube was provided with a screen, measuring about 40 cm. from side to side, so that all objects were concealed from the subject, and his vision was confined to what he could see by looking through the tube.

The further end of the tube was closed by a plate in which there was a horizontal slit measuring 7 mm. in breadth. The subject looked through this slit on to a uniformly lighted white surface of smooth cardboard which formed the background. Between the tube and this background two black threads of uniform thickness were introduced as objects of observation. These were kept strictly vertical by the aid of conically pointed weights, the points of which indicated on the table positions which were the perpendicular projections of the positions of the points in the horizontal plane of the eye. This position was noted on the cardboard upon the table.

By constructing radii and arcs from that point which marks the perpendicular projection of the eye on to the plane of the table, and drawing the corresponding arcs, we can completely determine the position of the points in the visual plane projected on the plane of the table in terms of the corresponding radius and arc. This projection is identically similar with the position of the various points in the horizontal plane of the eye, with the eye as centre, and the direction of fixation as the principal radius.

In the accompanying figures the radii are $0^\circ{\cdot}96$ distant from each other. The arcs are 1 cm. distant from one another.

5. When the observer looks through the tube, he sees a uniform background and nothing more. In the middle of this one black thread

is introduced first of all. When this thread is fixated by one eye, it occupies in space an indeterminate position as regards distance, a feature which Hering clearly explained many years ago. If now a second line be introduced on one side of the field of vision, while the eye is still fixating the central thread, the spatial position of this second line can be determined relatively to the other. The one appears further or nearer than, or equidistant with the other. These judgments are immediate; it is very easy to show that *they are independent of the absolute and relative thickness of the threads and of their relative distinctness*[1].

In the following investigations Mr Jan Kurtz acted as the observer. His eye had 2·5 D of myopia, and the experiments for determining the accommodation curves, carried out according to Dr Loria's methods, gave curves having a very steep descent. Mr Kurtz had been well practised in indirect vision, as he had previously taken part in other experiments.

The following was the procedure. Mr Kurtz fixated the central line and was asked whether the lateral line appeared as nearer or further than or as equidistant from the central line. Meanwhile of course his eye was kept absolutely at rest. The lateral line was then pushed either along the radii or along the arcs; and in this way[2] were determined the curves expressing the equidistance of the lateral lines from the central ones. These curves are here produced in natural size; they represent the data obtained.

6. Figure 1 shows the results obtained when the fixation mark was 35 cm. distant (the thread being 0·3 mm. thick). As Mr Kurtz had 2·5 D of myopia, this point lay within the range of his accommodation. Mr Kurtz fixated the mid-point of the central thread; the lateral thread was then moved to and fro until he reported that the two threads were equidistant. The curve will be seen to descend very abruptly when the lateral thread is at a distance of 30 cm., the point falls on the radius 5; that is to say, the fall amounts to fully 5 cm. when the lateral displacement is $4^\circ{\cdot}8$. Leftwards the descent is somewhat less abrupt than rightwards. At a distance of 35·8 cm. the point falls on the radius 1; that is to say a radial displacement of 0·2 cm. corresponds to a lateral displacement of $0^\circ{\cdot}96$. Strictly speaking, the curve is not

[1] To show it, we made some series of experiments with threads of different thickness, and got always the same curves, without any difference.

[2] We marked on the cardboard table the points, reported as appearing at the same distance as the central point.

a line but a very narrow area which continuously widens towards the periphery. The width of this area indicates the range within which the reports 'equidistant' occur. With the arc 30 cm. distant from the eye, the breadth of this space is 1·5 mm.; that is to say, when the thread is moved along the arc so that the apex of the weight remains on the black surface, the report 'equidistant' *is always returned.* The report continues similarly unaltered so long as, while moving along the radii, the thread remains within the black area. The area narrows towards the fixation centre. At the arc 33 cm. from the eye, it is only 1 mm. broad—measured along the arc. At the arc 34·5 cm., it is only 0·5 mm. broad; at the arc 34·8 cm. only 0·3 mm. broad. The sensitiveness for the positions of 'equidistance' is therefore extraordinarily delicate[1]. *Any position within the curve* is always reported *nearer*, any

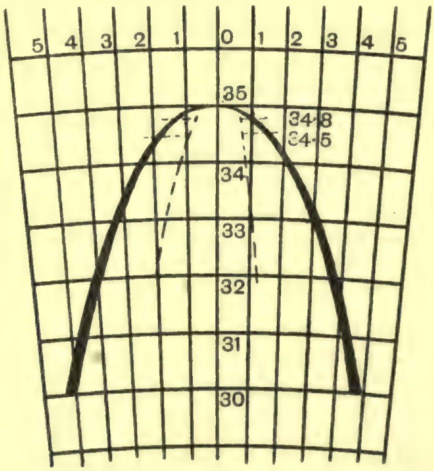


FIG. 1.

position *outside* the curve *further.* That is to say, when the eye fixates the middle of the central thread, and when the laterally seen thread is so placed that the apex of the weight lies *outside* the curve, the lateral thread always appears to lie *further* away than the central. When on the other hand the thread is so placed that the apex of the weight lies *within* the curve, then the reply always is that the lateral thread is *nearer* than the central.

[1] In some later experiments with Miss R. who has very sharp vision, the width of the curve of equidistant position is sensibly smaller, i.e. the sensitiveness to the position of equidistance is more delicate than with Mr K.

Thus the geometrical distance of the threads from the eye is absolutely unimportant. For example, the point of intersection of the arc 31 with the radius 5 is described as lying further than the central fixation point at the distance 35 cm.; whereas the point of intersection of the arc 34 and the radius 1 is described as lying nearer than the fixation point.

7. Figure 2 shows the results of investigations when the fixation point was 45 cm. distant, i.e. beyond the range of accommodation. The relations here are quite analogous to those of Fig. 1. The breadth of the surface, showing the range of the reports 'equidistant,' is somewhat greater in linear measurement, but the angular breadth is unchanged. At 40 cm. the breadth of the area amounts to 2 mm.; at 44 cm. the

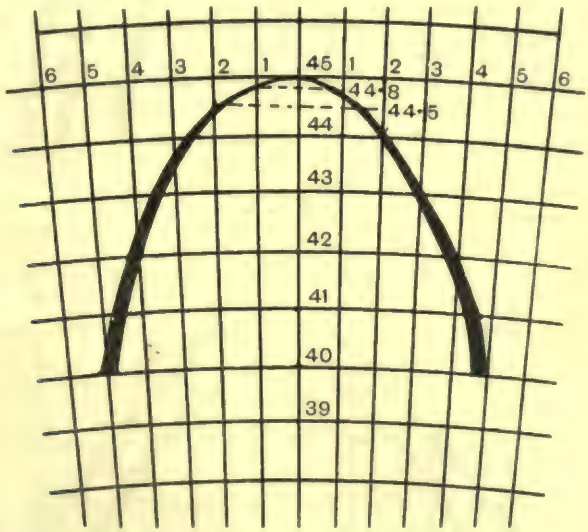


FIG. 2.

breadth of the area is only 1 mm. All points within the curve are described as lying nearer, all points without the curve as lying further, than the central fixation point.

8. Interesting results were obtained from the subject of these experiments when the fixation line was 60 cm. and 80 cm. distant (see Figures 3 and 4). In place of one curve we have three curves, having the same significance. Here too the curves are really areas, lineally broader than those of Figures 1 and 2, but as regards angular breadth of approximately the same size. All judgments, concerning points *within the curve I and outside the curve III*, are unequivocal; thus, for

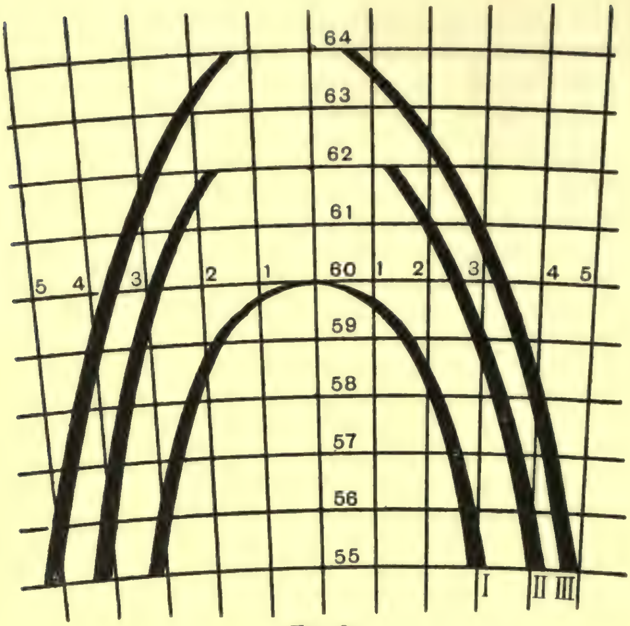

64
63
62
61
5 4 3 2 1 60 1 2 3 4 5
59
58
57
56
55
I II III

Fig. 3.

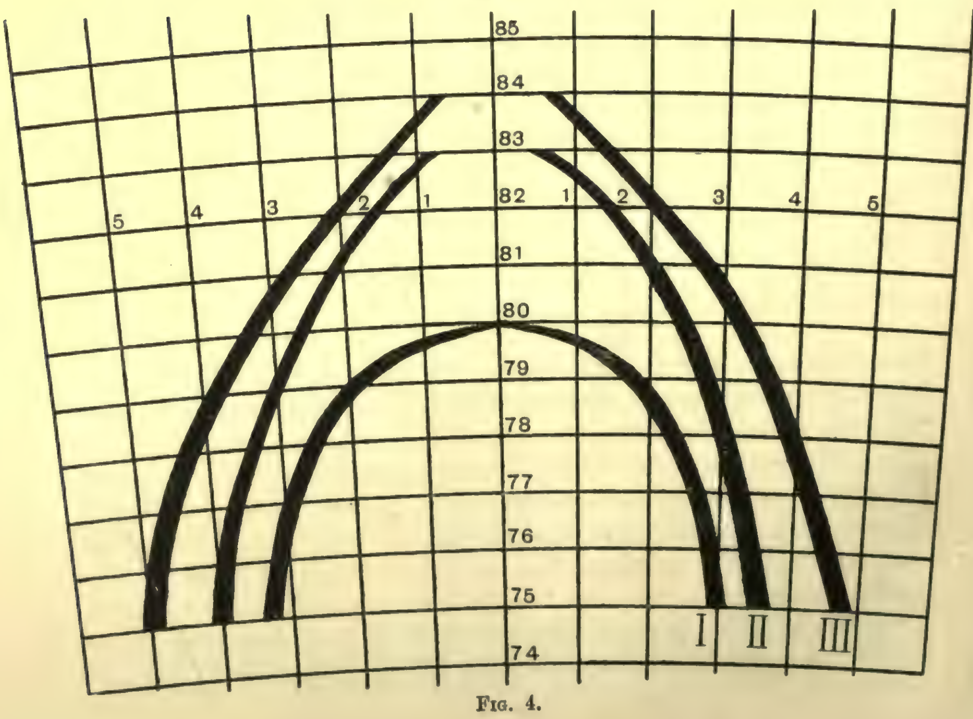

85
84
83
5 4 3 2 1 82 1 2 3 4 5
81
80
79
78
77
76
75
74
I II III

Fig. 4.

points within the curve I the report *nearer than the fixation point*, while for points outside the curve III the judgment 'further than the fixation point is always given.' In the spaces between curves I and II and between curves II and III, the reports are irregular, being sometimes 'nearer,' sometimes 'further.' The nature of the reports usually depend upon the direction of procedure. Thus, starting from the curve II and moving outwards, we obtain the report 'further,' and moving inwards the report 'nearer,' until the next curve is reached, whereupon 'equidistant' answers are once again obtained. When, on the other hand, the lateral thread is moved irregularly in the space between the curves, the replies are irregular. Within the areas of the curves themselves there is *always* a uniform report of 'equidistance.'

9. When the subject attends to the central and lateral threads placed in the 'equidistant' position, a periodical displacement of their reciprocal positions is observed. Mr Kurtz describes these oscillations in the paper following.

10. It remains to make some general remarks about the facts that have been observed. I see very clearly that these experiments only serve as a starting point for future inquiries. On the other hand, some important conclusions may be deduced from the results here given. I should like also to point out certain general considerations which will guide me in future experiments.

A. The present investigation once and for all disproves the old assertion that monocular space-perception is only bidimensional. The immediate awareness of tridimensionality,—the accuracy with which the relative position of the threads is perceived,—affords strong argument for reversing the previously accepted opinion. All the subjects investigated[1] agree in maintaining that the tridimensionality of monocular space is an immediate datum of perception. No essential difference then exists between monocular and binocular space-perception; both are tridimensional. The only difference between them is that near objects appear more plastic in binocular than in monocular vision. But with distant objects this difference does not exist. The facts observed are evidently inexplicable either by Hering's or by Helmholtz's theory of the relation between geometrical space and perceived space.

B. The direction, in which the explanation of the facts here given is to be sought, is indicated by the very nature of the facts. Our investigations started from the observation that points which, with a

[1] We began a series of experiments upon other individuals, which are not here described.

given accommodation of the lens, form sharp images on the retina, are simultaneously perceived as lying equidistant[1]. In the experiments which have been here described the converse method was adopted of determining the position of those points which appeared to be equidistant. If we may suppose the line formed by these latter points is identical with the line formed by those points which produce a well-defined image on the retina (this hypothesis, likely as it seems, has yet to be proved[2]), then we reach the following fundamental conclusion.

Points in space, which in a given state of accommodation of the lens in monocular vision form well-defined retinal images, represent a surface which serves as a fundamental surface in relation to which the position of other points in the third dimension is determined. The points outside such a surface are regarded as more distant, those within it as lying nearer.

The images of the points outside the surface are diffusion circles. With these diffusion circles such factors as the degree of diffusion, light-intensity, etc., will become of importance; and these factors contribute to the appearance of depth. In other language, the image formed on the retina by means of the human dioptric apparatus must include all those factors which correspond to the report of depth. An analogy may make this clearer. It is well known that, in a picture representing objects in spatial order, every object must have its local tone. It is this local tone, and not the geometrical perspective, which determines the position of the object in space. Any object not in the projected line does not fit into the space of the picture. Something analogous may be supposed to occur in the case of the image formed on the retina. Yet another example. The images of objects formed by means of a badly constructed system of lenses appear on a screen as ill-defined. Yet the whole image is characterised by an exaggerated effect of depth.

I think the time has not yet come to give further details. There are facts enough which appear to support the correctness of the train of thought which I have set forth. These however must be examined in regular order. The foregoing remarks serve to show the path I intend to follow in future experiments.

[1] Cf. above, p. 67 *fin.*

[2] It is impossible to investigate by the method used by Dr Loria the central part of the field of vision, but the curves, as obtained here, if prolonged to the periphery coincide with the curves furnished by the method of Dr Loria.

ON THE FLUCTUATIONS OF RECIPROCAL POSITION OF TWO POINTS IN THE MONOCULAR FIELD OF VISION.

By JAN KURTZ.

From the Psychological Laboratory, Cracow.

I. When two threads are so placed as to appear equidistant from the observer (on the curve of the equidistant position) as in the arrangement described in the preceding paper by Prof. Heinrich, it will be noticed that, after an interval varying from 20 to 60 seconds, the threads periodically undergo a change in their relative position. Inasmuch as the central thread always serves as the point of reference, the change manifests itself in the sense that the lateral thread abandons the 'equidistant' position. The problem was to investigate the periods of this change of position. The registration of the oscillations was effected by means of a contact peg which allowed of the moments of closure of an electric current being recorded on a rotating drum. Fine time-intervals were simultaneously registered on the drum by the aid of a clock marking tenths of seconds.

II. The fixation point being 35 cm. distant, the displacement of the lateral thread is always of such a kind that it is periodically displaced outwards.

These periods were—

(i) Nasal position of the lateral thread.

Distance of lateral thread	Times of equidistant position	Times of displacement
34·8 cm.	7·27 secs.	8·42 secs.
34·5	1·13	9·31
34·0	0·78	11·22
33·5	1·18	5·34

(ii) Temporal position of lateral thread.

Distance of lateral thread	Times of equidistant position	Times of displacement
34·8 cm.	2·54 secs.	9·36 secs.
34·5	1·48	6·99
34·0	3·09	4·75
33·5	2·56	4·81

The above figures give the average of the individual records. It is noteworthy that the deviations from the average are very important. The times of the outward displacement for positions that lie nearer than the fixation point are greater than for positions of the thread lying further.

III. When the lateral thread is moved to the inner side of the curve of the equidistant position, so that the thread lies nearer, the points can be determined by the outward displacement from which the lateral thread appears equidistant with the central thread. These points are shown in Fig. 1 (see preceding paper, p. 70) by a dotted line. The distance of the dotted line from the line of equidistant position increases towards the periphery; that is to say, the amount of displacement of position increases as the distance diminishes.

The relative displacements of the thread on the dotted line are not all equally great. The greatest are those which bring the line into the position of equidistance with the central thread. When the times and periods of this position of equidistance are recorded, the following results are obtained.

(i) Nasal position of the lateral thread.

Distance of lateral thread from the eye	Times of equidistant position	Times of unequidistant position
34·8 cm.	2·38 secs.	3·83 secs.
34·5	1·08	8·28
34·0	3·01	3·36

(ii) Temporal position of the lateral thread.

Distance of lateral thread from the eye	Times of equidistant position	Times of unequidistant position
34·8 cm.	2·78 secs.	3·88 secs.
34·5	2·44	7·71
34·0	1·77	2·86

From these figures it is evident that the periods of oscillation are greater (the times of the unequal position are greater), and that the times of equidistance are less than the periods of the oscillations in the preceding records.

IV. Special interest attaches to the variation of the spatial position in cases where the equidistances give rise to not one but three curves. If one takes the position of Fig. 3 (p. 72) where the fixation point lies 60 cm. distant, the following results are obtained.

When the lateral thread lies on the curve I the displacements occurring are of such a kind that the thread appears displaced outwards, very rarely inwards.

The periods of the displacement are as follows:

Distance	Times of equidistant position	Times of displacement outwards
59·5 cm.	3·69 secs.	7·59 secs.
59·0	3·81	6·76

When the lateral thread lies on the curve III only displacements inwards of the thread result. The corresponding periods of displacement are as follows:

(i) Nasal position of the lateral thread.

Distance	Times of equidistant position	Times of displacement
59·5 cm.	5·84 secs.	5·72 secs.
59·0	6·09	2·61

(ii) Temporal position of the lateral thread.

Distance	Times of equidistant position	Times of displacement
59·5 cm.	7·60 secs.	4·52 secs.
59·0	9·36	2·94

If the lateral thread lies on the curve II there ensues not only the outward but also the inward displacement of the thread. The phenomenon did not occur irregularly, but generally in series. We obtain series of subsequent record for like change of direction.

The following figures show instances of the displacements.

Series of inward displacements.

Distance	Times of equidistant position	Times of displacement
59·5 cm.	7·60 secs.	4·52 secs.
59·0	5·93	1·73

Series of outward displacements.

Distance	Times of equidistant position	Times of displacement
59·5 cm.	5·24 secs.	6·18 secs.
59·0	4·48	5·09

V. We cannot at present enter into an explanation of these data. It is evident that one can bring them into relation with the known fluctuations of the perspective spatial orientations of certain figures. (As investigated in the paper of Dr Wycholkowska, *Bulletin de l'Academie des Sciences de Cracovie*, 1900, and *Psychological Review*, Vol. 13, 1906.) But this will be done elsewhere.

THE INFLUENCE OF MARGINS ON THE BISECTION OF A LINE.

By W. G. SMITH and J. C. ROBERTSON MILNE.

The influence of a marginal space on the process of bisecting a line: the middle point is shifted towards the margin.—Earlier observations; recent work dealing with the effect of variation of margin.—The presence of a constant error in the division of unmodified lines.—The region of maximal modification due to margins. The application of the principle of confluence.—The connexion between these observations and other phenomena of confluence.

WHEN a sheet of paper, which has a marginal space marked off at one side, is divided by a vertical line placed exactly midway between the line indicating the margin and the opposite edge of the sheet, the division when subsequently observed appears unsatisfactory. Similarly, when a sheet of this kind has the middle point of its lines determined by subjective estimation, it is found by subsequent measurement that the point thus determined does not coincide with the objective middle point, but occupies a position lying towards the margin.

Such observations as these formed the starting point of the work which is described in this paper. Its aim has been to investigate the character and magnitude of the error or illusion which is thus occasioned by the presence of the margin. In all the sections of the work the procedure has consisted in securing from the subjects indications with pen or pencil of the point which was in each case judged to lie at the middle of the line on the sheet laid before them. There are certain disadvantages involved in this procedure. But it was considered that it offered us a practicable method of securing, with the means at our disposal, a sufficiently large series of observations. It is intended that the work shall be continued and developed as opportunity permits on this and related topics. We should like to express our indebtedness to those who have given their assistance in the various stages of the present investigation.

Observations on the bisection of a line were made by one of us and brought before the Biological Research Club of the University of Edinburgh in the summer of 1907. It was not however till this year that arrangements could be made for a more extended study of the subject. This later work consists partly in the examination of an extensive series of determinations made by members of a class in the University, and partly in a more detailed study of a large range of variations in the marginal space, with a smaller group of subjects.

In the earlier observations the material employed was ordinary essay paper already ruled and provided with a vertical red line indicating the margin. Such material has the advantage that when one asks the subjects to show how accurately they can divide the lines, speculation is not ordinarily aroused as to the ulterior purpose which may underlie the work. Each of the persons who gave their help at this stage made ten determinations, the marks indicating the middle point being made with pencil and the line which was divided being concealed by another sheet of the same paper as soon as it was marked. The length of the lines whose middle point had to be found was 183 to 184 mm.; the length of the lines in the marginal space was 26 to 27 mm. Observations arranged in this way were contributed by fourteen subjects, more than one half of whom were lecturers in science. The average displacement of the middle point towards the margin was found to be 5·4 mm., that is, nearly 6 °/ₒ of the half of the line which was being divided. In only one instance was the illusion absent.

The data contributed by the students were secured in the course of exercises on space perception in the Class of Experimental Psychology. In the other sections of work on spatial perception both determinations and measurements were completed by the students: in the work bearing on the present topic the measurements were carried out by the authors of the paper and the results were reported to the class at a later date. It hardly needs to be said that only after the determinations were completed was mention made of the likelihood of an error in a given direction. A line of 80 mm. was taken as giving a convenient length for the purposes of division; this length was kept constant throughout this and the next stage of the work, while, on the other hand, the length of the lines in the marginal space was varied. Sheets of foolscap paper, the lines on which were 8½ mm. distant from each other, were prepared for employment in the determinations[1]. One set of sheets possessed no margin; we will term these the 'normal' or unmodified sheets, containing 'normal' lines. The other sets of sheets possessed margins indicated by a narrow black line: the term 'marginal' will be applied to these cases where a margin was added. In one set the marginal lines were 40 mm. in length, in the others they were 80 mm. and 120 mm. respectively. The breadth of the sheets, while uniform in each set, was somewhat different in the different sets, but in each case it was such that at least one line could be left unmarked at top and at bottom. In addition, covering sheets, for the purpose of concealing each line when marked, were prepared in sets having approximately the same total length as the sheets they were meant to cover: those used for the sheets with the margin of 120 mm., while sufficiently broad to cover all the lines that were marked, were narrower than those for the other sets.

The subjects were instructed to place each sheet in turn directly in front on the table with the lines parallel to the edge: a clear space was to be kept round the sheet, and the right hand alone was to be brought into contact with the sheet during the determinations. As soon as the middle point had been marked, the covering sheet, which had previously been put in such a position as to give the needed space for the determinations, was drawn down symmetrically, thus presenting an unmarked surface for the next determination. Six lines were marked with pen on each sheet in the following order: (1) normal sheet—3 lines, (2) sheet with 80 mm. margin—6 lines, (3) sheet with 40 mm. margin—6 lines, (4) sheet with 120 mm. margin—6 lines, (5) normal sheet—3 additional lines. The class met in two sections, hence we were able without difficulty to arrange that one section containing 17 students, termed Division I, should have the sheets in the marginal cases placed with the margin on the left hand, while the second section containing 22 students, Division II, had the margin on the right hand.

The results of these determinations are presented in Table I. Here, and in the other tables which follow, the values given for the marginal cases are the averages for the portion of the divided line

[1] An order was given for cutting the sheets by machinery, but as those which were supplied were found to be insufficiently exact, a large number of fresh sheets had to be cut by hand, a procedure which was found unexpectedly tedious, and which delayed the proper work. It may be mentioned here that when the final calculations were adjusted, every sheet was measured by an instrument provided with vernier, and where the lines which had been set for division were found to deviate from the proper length—80 mm.—an appropriate correction was introduced in the calculations.

which lies next to the margin. Thus in Division I it is the left-hand portion, in Division II it is the right-hand portion of the line whose measurement is given. It is obvious that corresponding portions of the normal line must be chosen for comparison; thus for Division I it is the left-hand, and for Division II it is the right-hand portion of the normal line whose value is given. The magnitude of the illusory effect, where present, is measured by the extent to which the values in the normal case exceed those in the marginal cases. The general (statistical) mean variation, M.V., is also given for each of the cases in each division.

TABLE I.

	Normal	M.V.	40	M.V.	80	M.V.	120	M.V.
Div. I ...	38·8	0·9	38·0	1·3	38·0	1·2	39·0	1·0
Div. II...	41·0	0·7	38·8	1·1	39·2	1·1	39·5	0·7

The values in this table show clearly that in both divisions there is a distinct tendency to shorten the portion of the divided line which lies next to the margin; in other words, the subjective middle point does not coincide with the objective middle point, but is displaced towards the margin. We notice, also, especially when the data of the two divisions are combined, that the illusion tends to decrease as the margin increases from 40 mm. on to 120 mm.[1] These points can be proved also by taking account of the frequency of the illusion. Combining the figures for the two divisions we find that 85 °/₀ of the subjects exhibit its influence with a margin of 40 mm., while with 80 mm. the percentage is 83, and with 120 mm. is 68. The extent to which the individual averages differ from each other, as shown by the mean variation, is greater in the marginal cases than it is in the normal case.

It will be noticed that in both divisions a constant error, of the same direction, and of approximately the same magnitude, is exhibited in the bisection of the normal line, the middle point being subjectively displaced 1·2 mm. to the left in the first, and 1·0 mm. to the left in the second division. It seems clear that we should take this constant error as a persistent factor, operative both in the normal and in the marginal

[1] It is assumed in this and in the following discussions that the character of the marginal influence is for our present purposes sufficiently determined when it is referred to the length of the lines in the marginal space. The question how far a marginal space containing no lines would be similarly effective is a matter for additional investigation.

cases. When this is done, it is seen that the illusory effect in the marginal cases is much less in the first than it is in the second division, being, for example, 0·8 mm. in the former and 1·2 mm. in the latter division with a margin of 40 mm. In considering this difference it should be kept in mind that in the first division, where the margin lies on the left, the illusory tendency operates in the same direction as the constant error referred to above, viz. towards the left, while in the second division these two tendencies are in opposition. In the former case there is summation, in the latter there is interference.

We were at first inclined to accept the following view of the difference. Inasmuch as the halving of a line is an operation carried out with a relatively high degree of accuracy and a small average of variation[1], the deviation of the middle point, though heightened by summation in Division I, may be expected to be rather strictly limited in amount. In Division II, on the other hand, the illusory tendency, acting on the basis of a constant error of 1·0 mm. towards the left, can develop its strength much more fully in carrying the subjective middle point back towards the objective middle point and onwards to the limit of deviation on the right. With the aim of testing this view a further analysis of the results has been carried out. Seven subjects have been selected from the first division (Group I), whose constant error is relatively small, being within the limit of ± 1 mm.; the constant error being small in this group, the illusory tendency should have larger scope for development. In addition seven subjects (Group II) have been selected from the second division as having a large constant error; here again the illusion has full scope. Thus, if the view suggested above were correct, both groups should show a relatively large illusory effect.

TABLE II.

	Normal	40	80	120
Group I........	39·5	39·5	39·1	40·0
Group II	41·9	38·9	39·0	39·1

[1] It is ordinarily stated that there is no constant error in the bisection of a line with binocular vision. According to the results of Fischer, *Archiv f. Ophthalm.* Bd. 37, I Abth. SS. 104, 105, the average mean variation when a horizontal line was halved was 0·67 °/$_{0}$ of the half of the line. In the case of comparison of two adjoining horizontal lines the percentage was 1·31 °/$_{0}$ of the line: where the two lines were separated by an interval it was 1·22.

The analysis of Table II shows that while the view might be taken as holding true in regard to Group II, it has no support from the results of Group I, where with diminution of the constant error there goes a marked decrease of the illusory effect. We seem therefore to be left in the unsatisfactory position of simply accepting the different magnitudes of the illusion in the two divisions as an unexplained fact. At the same time the analysis indicates an interesting fact in regard to the connexion between illusion and constant error. In Group II where the constant error is large the illusion is large: in Group I both are small. The presence of this tendency in Group I may be shown also in another way. If we take the 8 subjects in this division whose constant error is large in amount, viz., 1·6 mm. or more, we have the following results for them:—

Normal	40	80	120
37·9	36·6	37·0	38·0

Here, though the constant error in the normal case amounts to 2·1 mm., the illusory tendency adds to this, for example at the 40 mm. margin, a further deviation of 1·3 mm. It may be pointed out that, though we have not found an explanation of the differences between the two divisions, we are enabled by the help of the constant error to gain from the second division what seems to be a true estimate of the magnitude which the illusory effect may reach. In the division as a whole the illusion reaches a magnitude of 2·2 mm. at the margin of 40 mm., while in the selected group (Group II) it reaches a magnitude of 3·0 mm. These values give as percentages of the half of the line 5½ and 7½. It will be remembered that in the earlier observations a value of nearly 6 °/₀ was reached.

That the constant error in the bisection of the normal line is, under our conditions of work, a persistent and decided feature can be shown even more clearly by a statement of frequency than by averages. Taking the two divisions together we find that the middle point is displaced in the direction already indicated by upwards of 90 °/₀ of the subjects. It is difficult to understand this feature of our results. If a constant error were to be found, we might have expected that the portion of the line lying on the right would be the shorter. Experience with the method of average error teaches that the usual result of a given direction of approach is a relatively early arrest of stimulus; now the hand in proceeding to the marking of the line will usually bring the pen by a more or less regular movement from the right towards the middle point[1]. As has been already noted, the usual statement is that the process of halving a line is free from constant error, the variations being indefinite in direction[2]. No doubt there is some special feature in our work which conditions its appearance here, but we are unable to make any satisfactory suggestion as to its nature. In addition to other questions raised in the course of this paper this will form the subject of further investigation.

The final part of our work consists in the more detailed study of the effect of variation of the marginal space. The length of the line which was to be divided remained the same as before, and the same number of determinations was made on each sheet. On the other hand the number of marginal cases was considerably increased. The following are the lengths, given in millimetres, of the marginal lines which were employed:—

5, 10, 20, 30, 40, 60, 80, 100, 120.

Our aim in multiplying the variations, particularly at the beginning of the series, was to discover if possible where the maximal effect of the illusion was to be found. We had already seen that the illusion tended to increase with diminution of the margin from 120 mm. to 40 mm.: we therefore arranged that the margins below this magnitude should be studied in greater detail and should include one, viz. 5 mm., through which the initial stage of modification might be observed. The sheets which were to be divided, while varying in length, had a uniform breadth of approximately 80 mm. In one section of the work the covering sheets for the last two marginal cases—100 mm. and 120 mm.—were less broad than the others; in the other section they had all a uniform breadth of about 56 mm.: the results of these two sections being identical in all points of importance they have not been separated in the following tables.

The following order of the various cases was laid at the basis of the experiments; in this instance for the sake of clearness the total length of the sheet including the margin is given:—80, 90, 110, 140, 200, 160,

[1] We are not aware that any one of the subjects was left-handed.

[2] It may be noted that in Münsterberg's study of *Augenmass* (*Beiträge*, H. 2, SS. 160 ff.) the left one of two horizontal distances limited by points was constantly overestimated: when lines were compared the error was lessened. In these experiments the compared distances were separated by an interval of 60 mm.

In experiments on bisection of a line of 100 mm. (*Cambridge Anthropol. Expedition to Torres Straits*, II. Pt. I. pp. 101 ff.) Rivers found that the right-hand half of the line was given the following values:—Murray Island men 48·5, Murray Island boys 49·9, Students 50·9, Girton children 51·1.

120, 85, 100, 180. With this as a basis the successive determinations were carried out as follows:—(*a*) Three lines marked on each sheet in the order indicated above, the margins being placed in each case on the left; (*b*) three lines marked on fresh sheets in the same order, the margins being placed on the right; (*c*) the sheets employed in the first series completed by having three more lines marked, the order being reversed, i.e. proceeding from 180 to 80, and the margins lying on the left; (*d*) the sheets employed in the second series completed in reverse order with margins on the right. At the same time in order to heighten the trustworthiness of the normal estimation, which forms the control in these experiments, an additional sheet of 80 mm. was introduced in the course of these determinations; the first three lines were marked on it towards the close of the second series, and the second three lines were marked towards the middle of the third series. These determinations were in all cases completed at one sitting, while in no instance was more work done on one day. Thus the work of each day includes eighteen determinations of the normal line, and six determinations for each marginal case in each of the two positions, right and left[1].

We are able here to present results from eight subjects. The largest series of determinations were contributed by the authors of this paper—S. and M. They suffer from the disadvantage that complete knowledge of all details of the work was possessed by both, and the work of preparing the sheets was shared by both from the beginning. We should not, however, have been able to present examples of results in which the individual averages were based on a substantial number of determinations, had we not utilised our own work[2]. We do not consider that this knowledge on our part has materially influenced the results; in any case the new information supplied with regard to the point of maximal effect is entirely free from suspicion. In the case of the other six subjects no previous knowledge or practice comes into play. While the results given for S. and M. represent the work of three days, those for the other subjects are based on one day's work. Since the number of determinations contributed by each of these six subjects is thus limited, it has seemed sufficient to give the combined general averages; consequently these results are presented in the following tables as the results of the "Group."

[1] Two of the subjects in the Group contributed 12 instead of 18 determinations of the normal line.

[2] It may be mentioned that as one of us, J.C.R.M., had to leave on his return to India very soon after the end of the summer term, our opportunities for joint work were definitely limited.

TABLE III.

		N¹	Normal	m.v.	N²	5	10	m.v.	20	30	40	m.v.	60	80	m.v.	100	120
S.	L.	54	39·1	0·4	18	38·5	37·6	0·5	38·2	38·2	38·6	0·4	38·7	39·2	0·6	39·3	39·6
	R.	54	40·9	0·4	18	39·7	39·5	0·5	39·1	39·2	39·1	0·4	39·4	39·6	0·5	40·0	39·9
M.	L.	54	40·2	0·6	18	39·9	38·9	0·7	39·6	39·2	39·3	0·6	39·7	39·8	0·5	40·0	39·9
	R.	54	39·8	0·6	18	39·3	38·7	0·5	38·9	39·0	38·8	0·3	38·9	38·7	0·5	38·9	39·0
Group	L.	96	40·1	0·6	36	39·2	38·4	0·9	38·5	38·3	38·9	1·1	39·4	39·2	0·9	39·8	39·9
	R.	96	39·9	0·6	36	39·5	38·6	0·8	38·1	38·7	38·7	0·6	39·1	39·1	1·0	39·3	39·3
M.V. of Group	L.	—	0·6	—	—	0·7	1·3	—	1·1	1·3	0·6	—	1·0	1·2	—	0·6	0·8
	R.	—	0·6	—	—	0·8	0·7	—	1·5	1·0	0·9	—	0·6	0·7	—	1·0	0·4

TABLE IV.

	Normal	5	10	20	30	40	60	80	100	120
S.	40·0	39·13	38·58	38·66	38·71	38·87	39·06	39·38	39·65	39·74
M.	40·0	39·60	38·80	39·26	39·08	39·04	39·33	39·23	39·44	39·44
Group	40·0	39·33	38·49	38·30	38·52	38·77	39·24	39·14	39·54	39·61

In Table III the data are arranged (1) in vertical columns according to the length of the marginal lines, indicated at the head of each column, (2) in horizontal lines according to the position of the margin on the right (R.), or on the left (L.). Throughout the tables the values given by the day's work are taken as the basis for the calculation of the final values of the average and the individual mean variation as they appear in the table: thus with S. and M. three of these daily values (averages and individual mean variations) are combined to give the final values in the table, while with the Group each subject contributes one day's values for the final set of values. The individual mean variation (m.v.) has been calculated for representative points in the series of cases, viz., for the normal line, and for the margins of 10, 40 and 80 mm.; it is given in the column immediately following that which contains the averages to which it refers. The general mean variation (M.V.) of the Group has been calculated separately for the set of determinations with margin on the left (L.) and for the set with margin on the right (R.), and the results are given at the foot of the table. Under N^1 are given the totals of separate determinations on which the normal averages are based: under N^2 are given the totals on which the averages in all the succeeding marginal cases are based.

The results given in Table III confirm the conclusions already reached regarding the direction of the illusory displacement, its prevalence, and the gradual decrease in the effect as the margin increases beyond 40 mm. They enable us further to see where the region of maximal effect lies. Thus the displacement of the middle point increases gradually from the cases where the marginal lines are longer than the main line to a region which, for the individuals concerned, lies between the margins of 10 and 20 mm.: after that the decline is rapid, as we see from the results secured with a margin of 5 mm. In each of the series of values in the table the values for the margin of 5 mm. represent an initial stage in the development of the illusion; they are thus of great interest as showing how quickly the maximal effect is reached. The variability in the determinations of the eight subjects, as indicated by the individual mean variation, is in general greater in the marginal cases (av. 0·6), than it is with the normal line (av. 0·5). There is to be noted also a parallel fact in regard to the variability of the Group: the general mean variation amounts to 0·9 on the average for the marginal cases, and to 0·6 for the normal lines. Thus the effect of the margin here, as in the results of

Table I, is to increase the deviation of the individual results from the mean value[1].

The general results stand out still more clearly when the data for the right and left positions of the margin are combined, as has been done in Table IV. This combination appears to be justified inasmuch as there does not seem to be any significant or persistent difference between the results secured in the two positions. The combined averages given in Table IV are based not on the figures of Table III, but on a separate calculation. In these values the accidental variations in the results largely disappear, leaving a clearer picture of the general rate of alteration in the illusory effect. It will be noted that in this combination the differences between the two portions of the normal line likewise disappear, and we have thus the modification starting from zero value of displacement of the middle point.

In the figure which follows the course of modification is presented

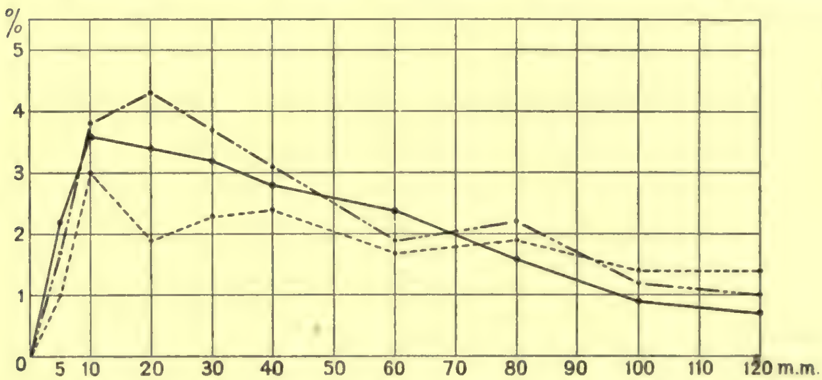


Fig. 1. Percentages of Error in Bisection due to varying Margins.

for S., M. and the Group on the basis of the data given in Table IV. The differences between the averages for each marginal case and the objective half of the divided line—40 mm.—have been expressed as percentages of this half, and transferred to the figure, in which the vertical distances correspond to percentages and the horizontal distances correspond to increasing magnitudes of the marginal lines. The continuous line

[1] These results may be compared with the data secured in work with the ordinary Müller-Lyer illusion, this *Journal*, II. pp. 23 ff. It was there found that only in the supplementary group of less exact workers was the general mean variation increased by the illusion. On the other hand it appeared that the individual mean variation was increased throughout by the illusion and that there was a certain degree of direct correlation between the general and the individual mean variation.

represents the data for S., the pointed line those of M., and the interrupted line those of the Group.

The results for the division of the normal line in this section of our work show no such marked constant error as appeared in the earlier section. Out of the eight subjects only three show an error of displacement towards the left. This approximate equality in the tendencies affecting the bisection of the line is in accordance with the general statement regarding this subject and tends to show that in this section at least the procedure by which the middle point was indicated has not given a bias to the results. This equality shown in the present instance, however, throws no light on the earlier constant error, and we have not noticed any special feature in the work of the subjects in the present section which might explain the absence of the constant error in their results.

In a paper dealing with spatial contrast and confluence, published in this *Journal*[1], a study was made of the lengthening which a line undergoes when it is accompanied by a longer line which is parallel to it, or is continued by another line from which it is separated by a distinguishing mark[2]. Our view is that the principle of confluence which was taken as the explanatory condition in these cases, in preference to contrast, is applicable also to the facts which we have been studying here, where again the main line is continued by another line. It may be true that Müller-Lyer expressed this principle in a too limited form[3]. But the central idea—that account is involuntarily taken of the surrounding space—seems to be a true expression not merely of the phenomena with reference to which it was originally stated, but of other cases of illusion, such as those mentioned above. The problem would then remain of stating in the various cases the special motives which lead us to take into account the space lying beyond the central object of perception. In the cases with which we are now dealing the

[1] Vol. II. p. 196. [2] Cf. Kiesow, *Archiv f. d. ges. Psychol.* VI. S. 296, 1906.

[3] Du Bois Reymond's *Archiv* 1889, Suppl. Bd. S. 266, "Man hält die beiden Linien für verschieden gross, weil man beider Abschätzung nicht nur die Linien selbst, sondern unwillkürlich auch einen Theil des zu beiden Seiten derselben abgegrenzten Raumes mit in Anschlag bringt." The question may be raised as to the stress which should be laid on *Abgrenzung*.

Compare with this the more general statement (*Zeitschr. f. Psychol.* IX. S. 3) where, speaking of confluence and contrast as modes of the interaction of neighbouring stimuli, he thus refers to the action of stimuli:—"indem sie entweder in der gleichen oder aber in der entgegengesetzten Richtung aufeinander wirken können." This seems too broad, even with the rule given on page 15 of the same paper.

motive seems to be found in the presentation of a line which, while partly interrupted by the marginal crossing line, is still continuous. If we attribute to confluence the apparent lengthening of a line through the presence of another which is continuous with it, we seem justified in referring the phenomena dealt with in this paper to the same principle. It seems reasonable to suppose that if a line is subjectively lengthened by the presence of a line continuous with it the position of the middle point will be altered. If, however, the lengthening thus produced were to apply equally to the whole line, involving an addition at both ends, the position of the middle point would not be altered: it would be capable of determination with as little difficulty, or error, as when the line is unmodified. We must therefore, in order to explain our present results, assume that the lengthening does not apply equally to the whole line, but that it affects mainly, if not exclusively, that part which lies nearest to the modifying line. In other words a certain length is subjectively added to one end of the line, the end next to the margin[1]. This being the case the point which seems to lie at the middle of the line will in reality occupy a position which is, objectively regarded, too near the margin.

In the former investigation to which reference was made above the maximal effect of the continuous lines, in the way of lengthening the main line (100 mm.), was found to appear in the earlier experiments (with which the present work is more nearly comparable) when the modifying line extended 20 to 40 mm. beyond the main line; in other words the marginal extension having the greatest effect was 0·2 to 0·4 of the main line with the two subjects then investigated. In our present work the maximal effect is found with a marginal line of 10 to 20 mm., i.e. 0·13 to 0·25 of the main line of 80 mm. It seems possible, however, that this divergence is more apparent than real. In the former investigation the modifying lines varied in the critical region, not as in the present instance by 5 mm. and 10 mm., but by 20 mm. It is therefore possible that the maximal would have appeared there, had opportunity been given, with modifying lines somewhat shorter than 20 mm. in the one case, and between 20 and 40 mm. in the other. And this supposition is rendered more probable by the form of the curves in that investigation; they are distinctly asymmetrical, and suggest that with more detailed work the apex would have been found with still smaller margins. All that is needed to bring the two sets of results into harmony is to suppose that the

[1] This point is of importance for the subsequent discussion of contrast.

maximal effect in the earlier work would, with more detailed examination, have been found continuing to a region where the modifying lines were 10 to 15 mm. and 20 to 30 mm. in length respectively. At the same time it is possible that the second of the factors which will be mentioned presently in accounting for the decrease in the illusory effect, viz. the decided concentration or limitation of attention in these experiments, may operate to bring the maximal point—the point beyond which the margin loses in effectiveness—nearer to the beginning of the curve.

While the primary increase of the illusion with the initial increase in length of the marginal line can be interpreted without difficulty as a phenomenon of confluence, the secondary decrease in the efficacy of the lines after their length exceeds a certain limit is a more complicated and difficult problem. It may be suggested that when the marginal line is lengthened to such an extent that its terminal point cannot easily be brought into direct relation with the rest of the figure, it will necessarily lose in effectiveness; it will tend to lose its connexion with the main line and be lost in the surrounding field. With a small margin, on the other hand, the whole figure—main line together with marginal extension—will form a more easily and completely related whole, and a definite motive will thus be supplied for the subjective extension of the main line. This limitation in the effectiveness of the longer lines will be supported and heightened by the limitation of the attention which is occasioned by the special conditions of the experiments. Instead of being free to range over the whole line the attention tends, involuntarily as well as voluntarily, to be concentrated on one definite point—the middle of the main line: the rest of the field will thus enter less readily into clear perception. As a final factor there may be noted a more directly sensory condition—the decreasing efficacy of peripheral vision with relatively extensive figures. Taking these three factors in combination, and noting that they operate more strongly with increasing length of the marginal lines, we seem to have a sufficient explanation of the decrease in the operation of confluence. We are only able, however, to indicate their joint result, the fall of the curve after a certain distance has been reached: we have no means at present of estimating their relative efficacy in the total result.

It does not seem possible to apply the principle of contrast to the results of the present investigation. The lengthening of the main line, taken by itself, might be explained on the basis of contrast. The lengthening which contrast is supposed to produce is, however, understood usually as applying generally to the line whose length appears to be increased by contrast; it is the line as a whole which is supposed to be lengthened. But, as we saw a short while ago, the lengthening with which we have to do in the present instance is one which applies mainly if not solely to one end of the main line, viz. that which lies next to the margin: only on this supposition can the displacement of the middle point in a constant direction be explained. A local operation of contrast, on the other hand, producing a lengthening of one portion only of the main line, seems an inadmissible hypothesis. If we take the case of the marginal line of 40 mm., for example, where the marginal error is still quite distinct, we should have to suppose, on this hypothesis, that the half of the main line, viz. 40 mm., was subjectively lengthened by the presence of a marginal line which was of the same length. It would seem then that here also the conception of contrast must be dismissed.

It was pointed out in the former discussion of confluence[1] that the modification caused by continuous lines was identical in principle with that apparent in the ordinary Müller-Lyer illusion, the angles formed by the accessory lines with the main line being supposed to increase continuously till an angle of 180°, i.e. a straight line, appeared. It is interesting in this connexion to note that the modification in the position of the middle point, which we have just been studying, is parallel to one of the features of the illusion referred to above. It has been shown by Judd[2] that the operation of the accessory lines can be traced in its effect on the division of the space, both beyond the termination of the main line in the figure and within the figure itself. We have here an additional proof of the unity of the principle underlying both the Müller-Lyer illusion and the phenomena which have been dealt with in this paper. Reference may be made also to the fact that, as has been shown by Pearce, an illusion corresponding to the Müller-Lyer visual form is found in the tactual perception. At the same time this investigator has shown that both the localisation of a tactual stimulus and the apparent magnitude of a tactual distance are modified by an accessory stimulus (*Nebenreiz*) separated from the main stimulus by an interval. We may reasonably bring this modification by accessory tactual stimulation into relation with the influence of marginal lines which we have been studying. It is to be noted, however, that the law according to which the modification varies seems quite different in the two spheres.

[1] *Loc. cit.* p. 213. [2] *Psychol. Rev.* VI. p. 242.

It may be noted in conclusion that our aim in these more theoretical considerations has not been to enter into the wide field of discussion of theories of illusion: our object has rather been to find the principle which most closely and directly expresses the facts which have been under review.

GENERAL CONCLUSIONS.

1. The presence at the end of a given line of another, marginal line, which is continuous with it, influences the bisection of the former in such a way that the subjectively determined middle point is shifted in the direction of the margin.

2. The magnitude of the illusory effect varies with the length of the marginal line. It increases rapidly with increase of the marginal line up to a length, the ratio of which to the whole of the divided line is 1 : 8 to 1 : 4 with the subjects now investigated. It then gradually decreases, but is found still operative when the ratio of marginal to main line is 3 : 2. In other words the curve of the modification is distinctly asymmetrical, having its highest point in the terminal portion representing the shorter marginal lines.

3. The illusion is found in varying strength in upwards of 85 °/₀ of the individuals examined, and may amount to 5½ °/₀ or more of the half of the line which is being divided.

4. It is considered that this modification of visual space presents another example of the operation of confluence, and that it is intimately related to the confluence shown in cases where the length of a line is modified by adjoining continuous lines.

5. It is found that when sheets bearing lines, 80 mm. in length, have the middle point of the lines indicated by means of subjective estimation, a marked constant error appears, which consists in a displacement of the subjective middle point towards the left. This error amounts, on the average of a large group of subjects, to upwards of 2·5 °/₀ of the half of the line which is being divided, and it is exhibited in the results of upwards of 90 °/₀ of the members of this group.

EXPERIMENTAL TESTS OF GENERAL INTELLIGENCE.

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I. *Origin and nature of the investigation.*

II. *Principles governing the selection of reagents and tests, and the application of the tests to the reagents.*

III. *Methods of provisionally estimating the intelligence of the subjects, and of mathematically deducing its relations to the tests.*

IV. *Apparatus, procedure, and results of the several tests (Touch, Weight, Sound, Lines; Tapping, Card-dealing; Card-sorting, Alphabet-sorting; Memory, Mirror, Spot Pattern and Dotting Apparatus).*

V. *Conclusions as to the possibility of the diagnosis, analysis and inheritance of intelligence.*

Origin and Nature of the Investigation.

The experimental determination of the mental characters of individuals is admittedly a problem of wide theoretical interest and of vast practical importance. The particular mental character which in importance is perhaps above all supreme, is that traditionally termed 'General Intelligence.' Yet the notice it has received from psychologists has been in proportion astonishingly scant. First suggested by Galton's *Inquiries* in 1883, first actually attempted upon some fifteen hundred American school-children in 1891, then assiduously prosecuted during the next ten years in America, in Germany and in France, eventually discredited by the hopelessly discordant results of the various researches, the investigation of Intelligence now seems once more to be attracting scientific attention. The success of anthropometry in statistically establishing relations between physical characters renders it not improbable that the discrepancies and failures of previous investigators of the relations between mental characters were largely due to their reliance for the discovery of correlations upon mere inspection of the data they obtained, instead of upon quantitative determination and mathematical deduction. On this presumption, the mathematical methods of biometricians have recently been adapted and applied by Dr Spearman to psychological data obtained by himself, by co-workers, and by earlier pioneers, with positive and encouraging success. The experimental methods adopted for such investigations, the 'Test-Methods,' have been debated at length, and at last rehabilitated, in the latest and completest work on *Experimentelle Pädagogik*, by Professor Meumann, who had previously contributed some original experiments of his own. To the publications of these two writers reference may be made for a detailed history of the subject and for a summary of its literature[1].

The investigation reported in the following pages was commenced with a view to testing in practice the mathematical methods of Dr Spearman, and to verifying the experimental results both of Dr Spearman and of Prof. Meumann. For comparison with these some new experiments were also attempted, similar in aim but different in nature. The essential purpose of these was to determine whether higher mental functions would not show a yet closer connection with 'General Intelligence' than was shown by simpler mental functions, such as sensory discrimination and motor reaction, with which previous investigators had been so largely engrossed. The experiments were carried out—thanks to the extreme courtesy and kindness of Mr Claude Moore, Headmaster of the Central School, Oxford, and of Mr C. C. Lynam, Headmaster of the Oxford Preparatory School—mainly in the premises and upon the children of their respective schools during the autumn of 1907 and the spring of 1908. Throughout almost the whole period of the experimental part of the work I enjoyed the invaluable co-operation of Mr J. C. Flügel, of Balliol College, Oxford, whose results are here recorded with my own; while the mathematical part of the work is especially indebted to the generous advice and assistance of Dr Spearman. The whole research, however, owes its origin to the suggestion of Mr W. McDougall and its completion to his constant encouragement and advice. In conjunction with Mr Keatinge, Reader in Education in the University of Oxford, and Mr A. M. Hocart,

[1] C. Spearman, "General Intelligence Objectively Measured and Determined," *Amer. J. Psychol.* 1904, Vol. xv. pp. 202–292 (literature, p. 206 ff.). Krueger u. Spearman, "Die Korrelation zw. Verschiedenen geistigen Leistungsfähigkeiten," *Zeitschr. f. Psychol.* Bd. 44, S. 50 ff. E. Meumann, "Intelligenzprüfungen an Kindern der Volkschule," *Die Experimentelle Pädagogik*, 1905, 1 Bd. Heft 1/2 (literature *ad init.*). E. Meumann, *Experimentelle Pädagogik*, 1907–8, Vol. I. p. 386 ff. (Literaturverzeichnis, p. 552).

Mr McDougall had begun experiments on similar lines in his laboratory. Their results were not extensive enough for statistical treatment, but their experiments worked out some of the new tests to be described below, and proved their applicability to school-boys. The work of Mr Flügel and myself was thus considerably facilitated when we took up the investigation *de novo*. Both apparatus and rooms of the new Psychophysical Laboratory at Oxford were also lent most generously for our use. Dr Spearman and Mr McDougall have been kind enough to read through my paper in manuscript, and to allow me to make use of their criticisms and embody their suggestions.

To all those who in these and many other ways aided my work I take this opportunity of expressing my inadequate thanks.

Any attempt to elaborate a method for the determination of the presence of a given character in the mental constitution of individuals will at the same time throw light upon the nature and upon the development of that character. An investigation of 'General Intelligence' thus leads to three main enquiries:—(i) can its presence be detected and its amount measured? (ii) can its nature be isolated and its meaning analysed? (iii) is its development predominantly determined by environmental influence and individual acquisition, or is it rather dependent upon the inheritance of a racial character or family trait?

To decide between the possible answers to these three enquiries, or at least to contribute towards the definition of the problems involved and the methods available for their solution, seems a task of peculiar urgency for experimental psychology. For general psychology has not established the nature, nor individual psychology the measure, nor social psychology the transmission of 'General Intelligence.' The meaning of the term is assumed in pedagogical and sociological theory as generally understood; the property denoted by it is recognised in practical life by miscellaneous symptoms, such as physical and physiognomical characteristics, 'general impression,' or examination results. But the signs of intelligence are notoriously fallible, and its significance has either been ignored by introspective psychology, or else baffled its analysis. And so, with the current failure of theory in analysis, and of diagnosis in practice, the field is open for special experimental research. Whether Intelligence consists of a single elementary faculty; whether it is the complex resultant of a number of faculties, all working in co-operation; or whether there is really no such thing as 'General Intelligence'—the substantive being but the hypostatisation of an attribute applied to effects, apparently similar and practically equivalent, but arising indifferently from a variety of alternative processes, which may operate independently on various occasions and in various individuals—these are controversies still awaiting the evidence of experiment. Further, if Intelligence consist only of some single, relatively simple function, such as adaptation of attention (suggested by Binet and others), or general sensory discrimination (suggested by Titchener and others), then Intelligence will presumably be recognisable by the success with which some simple task, demanding little but rapid adaptation of attention or fine acuity of sensory discrimination, is performed; on the other hand, if it be a more composite function, then it will reveal itself in tasks involving mental process of a higher level and more complicated type; if a central or general faculty be a figment, if excellence in any one function or group of functions will serve as well as excellence in any other (as once suggested by Dürr[1]), then recognised intelligence will not associate itself uniformly with proficiency in one special direction, but will disclose itself, in some individuals by peculiar success in this or in that one particular direction, in other individuals by moderate success in several directions at once; but again the possibility and nature of any practicable tests can only be decided by means of experiment. Once devised, once demonstrated to measure a general, innate endowment, as distinguished from special knowledge and special dexterities, that is to say, from post-natal acquisition, such tests would find yet a third direction for experimental investigation, namely the enquiry how far the capacity thus measured varies with Age, with Education, with Parentage and with Social Rank; and this further application of the methods of the 'tests' would provide at once an illustration of their practical importance and a corroboration of their theoretical validity.

In attacking experimentally the several problems thus indicated the following course must be pursued: having selected suitable reagents and appropriate tests, to apply the tests selected to the selected reagents; to estimate the degree of correspondence between the results of the respective tests and the presumable intelligence of the reagents; to discover the highest common psychological factor that explains the various correspondences revealed; and to estimate the degree of correspondence between the native ability of the reagents—as

[1] *Zeitschr. f. Psychol.* 1906, Vol. XLI. April: "dass, was man im Leben 'Intelligenz' nennt, sich wissentschaftlich ebensogut als zufällige Verbindung einiger glücklicher Dispositionen verstehen lässt." Dürr, however, no longer endorses this view, and has retracted the chief criticisms founded thereon.

shown by those experiments that appear the better tests of unacquired intelligence—and the presumable intelligence of the reagents' parents. It is clear that a single set of experiments may thus be made to elucidate the threefold enquiry enunciated above.

The Selection of Tests.

The 'Tests' were psycho-physical tasks selected as readily and rapidly yielding comparable quantitative results, and as standing in possible functional relationship with the capacity called 'General Intelligence.'

To fulfil the first condition and thus subserve the practical diagnosis of Intelligence, it was necessary that the tasks should be fairly simple in character, dispensing as far as possible with elaborate or expensive apparatus; otherwise the time and cost entailed would preclude the subsequent application of the methods, if they proved promising, upon any extensive scale. For experiments upon young and untrained subjects such as ours, there is a further advantage in using none but the simplest apparatus. To boys, strange apparatus is distracting. Clock-work mechanism arouses irrelevant interests. Electric wires and keys inspire needless apprehensions. Consequently, in dispensing with elaborate instruments, the sacrifice of the mechanical regulation of objective conditions is often more than compensated by the exclusion of subjective irregularities and unstable attitudes of mind.

To fulfil the second condition, and thus contribute to the theoretical analysis of Intelligence, it was necessary that the tests should represent as far as possible the various main aspects and levels of mental process. The simplicity and practicability of the tests actually employed by us may be judged from the ensuing descriptions. Their representative character and limitations may be exhibited by the following classification (necessarily a somewhat arbitrary and schematic one) which attempts to arrange them according to the type of mental process predominantly involved in each.

List of Tests.	*Nature of process tested.*
I. Sensory Tests:	
(1) Discrimination of two points upon the skin (2) Discrimination of lifted weights (3) Discrimination of pitch (4) Comparison of length of lines by eye	Perceptual discrimination.

List of Tests.	*Nature of process tested.*
II. MOTOR TESTS:	
(5) Tapping (6) Card-dealing	Simple reactions.
III. SENSORI MOTOR TESTS:	
(7) Card-sorting	Reactions complicated by discrimination.
(8) Alphabet-finding	Reactions complicated by recognition.
IV. ASSOCIATION TESTS:	
(9) Immediate retention of (*a*) Concrete words (*b*) Abstract words (*c*) Nonsense syllables	Immediate memory.
(10) Mirror test	Formation of associations during motor activity (progressive process of 'Trial and Error').
(11) Spot pattern test	Formation of associations during perceptual activity (progressive process of 'Apperception').
V. TEST OF VOLUNTARY ATTENTION:	
(12) Dotting irregular dots	Maximal effort of sustained attention.

Of these, the Alphabet Test, the Mirror Test, the Spot Pattern Test, and the Dotting Test are believed to be, at any rate in this connection, new.

THE SELECTION OF REAGENTS.

The interdependence of mental processes is far more complex and far more intimate than that of physical processes. Thus the prime difficulty in a psychological research is the elimination of the factors that are irrelevant. In investigating General Intelligence by means of experimental tests the essential relations between the functions to be observed are liable to be distorted or obscured by such accidental conditions as the personality of the conductor of the experiments, the age, sex, social status, education, zeal and practice of the subjects of the experiments. Such sources of confusion may be reduced in three ways: their introduction may be evaded at the outset by circumspection in the choice of reagents; their interference may be nullified during the actual experiments by appropriate procedure and by repetition; their effects may be estimated in the results and discounted mathematically by subsequent calculation.

The differential influence of age, sex, education and social status may be minimised by selecting the groups of reagents from persons as far as possible similarly equipped in these several respects.

The reagents for the following investigations form two groups chosen from two Oxford schools,—a superior Elementary School and a high class Preparatory School. Both were exclusively boys' schools. Difference of *Sex* was thus at once ruled out. In *Social Status*, the boys of the Elementary School were of the lower middle class, sons of local tradesmen, paying a fee of 9*d*. a week. The boys from the Preparatory School were being prepared for scholarships at one or other of the great Public Schools, and were in nearly every case sons of men of eminence in the intellectual world, that is to say, of Fellows of the Royal Society, University Professors, College Tutors and Bishops. Between the two schools there was thus a considerable difference of parentage. Such a difference should enable us to discover, not only if an application of the same tests to children of totally different types would reveal the same relations between the mental capacities tested and general intelligence, but also how far the tests were applicable to the problem of the inheritance of intelligence as well as to its analysis and diagnosis. Within the two schools, however, the social status of the boys was unusually uniform. Out of each school a further selection was eventually[1] made with a view to avoiding the serious influences arising from difference of *Age*. After several trials and special consideration it was decided to restrict the reagents to boys of a single year and to choose as limits the ages of 12 years 6 months and 13 years 6 months; older children had already come under the selective influence of superannuation regulations or scholarship examinations, while younger children were found to be scarcely equal to co-operating in prolonged and careful experimentation. In the Elementary School, 30 children fell within these age-limits; and in the Preparatory School, 13. For purposes of corroboration a dozen cases are sufficient (as will later be seen) to establish statistically a significant correlation; while within the limits feasible in the present investigation an increase of the main group of cases beyond about 30 would not advantageously diminish the probable errors. These 43 children, therefore, were selected as the final reagents.

Application of the Tests to the Reagents.

Irrelevant factors that could not be excluded in selecting the reagents,—such as *Personality*, both of operator and of subject,—we sought to exclude in selecting the methods to be adopted in the application of the tests. Before applying them to the selected reagents, most of the tests were first executed upon other boys taken either

[1] After the experiments recorded on p. 140.

from the same school or from a third school[1]. We were thus enabled to discover the errors incidental to the respective tests, to ascertain the more satisfactory modes of procedure with young subjects, and to habituate ourselves in the use of such procedure when adopted.

From the outset it was determined to examine the boys individually and personally rather than by class experiments or through the instrumentality of their teachers—thus differentiating our procedure from many, if not most, of our predecessors. Many of our tests could not have been carried out upon a number of subjects at once without needlessly multiplying apparatus and superintendents. Again, the writer had for a different purpose compared the results of tests carried out at one of these schools by mass-experiments with those of individual experiments; and there appeared no doubt that in the mass-experiments, even with the most rigid discipline, a number of undesirable factors intervened which could be ruled out or reduced in individual experiment. He ventures to think that high correlations obtained by the former method between imputed intelligence and particular tests—e.g. tests of visual and auditory discrimination—indicate that the more intelligent children will execute an unfamiliar, non-scholastic task (whatever its intrinsic nature) with the greater amount of interest, attention and care, rather than that their apparently higher thresholds measure their supposed greater acuteness in the mode of native sensory discrimination thus investigated, or that this acuteness is a component or a sample of general intelligence. Indeed, it might plausibly be argued that the complete theoretical correlation held to subsist between imputed intelligence and general sensory discrimination (i.e. the factor assumed to be common and fundamental to all sensory tests), really represents the correlation between imputed intelligence, and intelligence in the sense of power of attentively and carefully applying the mind to something relatively novel; for intelligence in this sense is inevitably manifested in all unfamiliar processes where much is left to the activity of the child; and may really have been the highest common factor in such tests.

The children whose performances are recorded in the sequel were therefore examined, not in class, but individually. They were also examined, not through the medium of their teachers or of other experimental conductors, but by ourselves personally. Here again the restricted numbers with which we had to cope enabled us to depart from the procedure of many of our predecessors. We found that, except

[1] We are much indebted to the Headmaster of St Philip's School, Oxford, for permitting us to perform some of these trial-series upon his pupils and at his school.

perhaps after prolonged collaboration, no two investigators adopt precisely the same procedure in tests where much depends upon the management of the operator; and that these variations in the details of procedure affect the measurement accepted as the result of the experiment, especially with children, to an undesirable extent. Since these and other consequences of the personal equation could not be completely avoided, it was resolved that as far as possible they should at least operate upon all alike.

In the actual course of the experiment the personality of the operator was relegated to the background by making our procedure as mechanical as could be, using a written form of instructions to be recited to each boy at the beginning of every new test. But since the same formula has not the same intelligibility or the same meaning for different children, this was always supplemented by asking if the reagent had any questions or difficulties as to the nature of his task. To make it possible to neutralise the personal factor by means of mathematical manipulation of the results nearly every test was applied, not only by the writer, but also by Mr Flügel.

Irregular influences arise not only from the personality of the operator, but also from that of the child. One of the most prolific sources of erroneous psychical measurement is difference of *Zeal*. The discipline of boys of a superior Elementary School and the good-nature of boys of a high class Preparatory School rendered our subjects surprisingly and uniformly good experimental reagents. Of the two groups, the Preparatory School boys were perhaps slightly superior in conscientious steadiness and care; while the Elementary School boys (who were markedly pleased at the interruptions of their regular routine, and were further fortified in their specially prolonged examination by the promise of a prize for the best) were perhaps slightly superior in spontaneous interest and attention. But in both groups attention, interest and goodwill were excellent, and consistently maintained.

Other sources of error lying in personal differences between the subjects were differences of *Practice* at similar tasks prior to serving as reagents, and differences of susceptibility to practice during the actual experimentation. The former affects only one or two of the tests applied, and will be noticed in connexion with these. The latter must vitiate in some degree every test no matter what may be its special nature. The ideal method of combating it would have been to insist on a maximum amount of practice in every case, to train each boy in each test at successive sittings, and accept only his final records as furnishing the measurement required. This, however, was needless for the scope of the present enquiry, and would be impossible in any anthropometrical survey upon a large scale[1].

The customary procedure in other researches has been to adopt a medium and limited amount of practice, such as a "quarter of an hour's fore-exercise." In our preliminary trials we found that this had a highly differentiating effect upon the boys. Some reached their most favourable disposition almost at once, while the comprehension and interest of others were awakened much more gradually. In any case, a quarter of an hour at a task whose intrinsic interest for the reagent was small (such as a sensory-discrimination experiment) tends to prove tedious for him; and if immediately followed by the crucial investigation, which may last for another 20 minutes, the whole experiment usually resolves itself into a test either of his power to withstand boredom or of his tendency to get rapidly confused. Accordingly, with the exception of two tests involving elaborate apparatus, all express fore-exercise was, where possible, discarded; with the few boys, or on the few occasions, that needed special training, an interval for rest and recovery was allowed; otherwise only such training was given as was involved in the procedure of the actual test. Our procedure in regard to practice thus aimed at reducing it to a minimum.

The remaining factors likely to disturb the accuracy of the measurements, such as fatigue, variation in health, nature of the weather, time of day, and incidents of the occasion generally (all of which were found

[1] Those who see in tests of General Intelligence by psycho-physical experiment mainly a possible substitute for the present examination system commonly suggest that the chief and fatal objection to their introduction for practical purposes is that, once introduced, it would be impossible to prevent schoolmasters from training their pupils to their highest possible efficiency in the various tests just as they now tend to cram their pupils to their highest possible capacity with material for the examination, and that such special training would frustrate the intention of the new system just as it has frustrated the intention of the old. The defect of cramming in the examination system, however, is that it converts an instrument intended to test the subject's power of intelligently applying what he has learnt, into a mere test of the contents and retentiveness of his memory, and that these are very fallible symptoms of intelligence. The result of special practice in the experimental tests, on the other hand, would only be to convert a measurement of a special mental process on its first appearance into a measurement of that process at its maximum efficiency; and as this must form a far truer measurement of that process itself, it presumably would also form a superior test of the intelligence correlated with it. Simpler and quicker, such a course of special training would be far more generally available than a course of special cramming. And thus, by rendering the subjective conditions of the final test more constant and more universal, it would further and not frustrate their original design.

to exert a slight but observable influence), were more or less neutralised by repeated sittings. As has been mentioned, a second series was kindly undertaken for most of the tests by Mr Flügel. In the case of the more important tests a third series was again worked through by the writer, and in one or two instances a fourth and a fifth; but it appeared that little or nothing was gained in the average by more than three applications. At the Elementary School the repetitions were usually made at intervals of about a week, and at the Preparatory School at intervals of two days. Several tests were in execution during the same period, so that the same test might be applied to the various boys during the same portion of the same school "hour." But it was unfortunately impossible to ensure this in the case of all.

Both in the selection of reagents and in the application of the tests the principles of the present investigation have departed from those of previous ones. In endeavouring to abstract from accidental conditions, previous investigators of General Intelligence have commonly thrown together the performances of several hundreds of subjects, and trusted to numbers to neutralise in the average the manifold variety thus courted; or else they have taken haphazard a few available subjects, and relied on purely theoretical calculations for estimating and discounting the probable effects, whether of individuality or of occasion, that were irrelevant to the main issue. In the present investigation the subjects were as nearly as possible alike in all important respects save those investigated, and their number was small enough for each individual to be examined by the same operators and for each examination to be repeated at least once. To these features such positive results as the present investigation has attained are believed to be largely due.

Method of provisionally estimating Intelligence.

To determine the degree to which the various tasks might be considered satisfactory tests of General Intelligence, it was necessary to obtain an independent estimate of the relative intelligence of the reagents tested. For this, recourse was had at the conclusion of the experimental part of the work to their headmasters, their teachers, and their schoolfellows, who undertook to draw up on the basis of their general experience of the examinees independent lists, grading them in order of General Intelligence. No *a priori* assumption was made in the projection of this investigation as to what kind of mental capacity may with the greatest propriety be termed 'General Intelligence,' since it is part of the aim of such investigations as the present empirically to examine the various capacities having a claim to this title and to ascertain their relations to one another. Hence, no determinate definition of 'General Intelligence' could be given to the compilers of the lists; rather it was presumed that the schoolmaster was the proper person, if any, to know the original meaning of intelligence, to recognise it in the concrete, and to compare its various degrees, even though the psychologist might prove the proper person subsequently to find for that meaning adequate expression, and to analyse and describe in technical terminology the nature of the capacity denoted by it. Accordingly, the serial classification of the children in order of intelligence, which serves as our provisional criterion in evaluating the experimental tests, was carried out by the compilers from a practical point of view with virtually no interference whatever from the experimenters in their capacity as psychological theorists[1]. And, of course, during the progress of the compilation and of the experiments, both compilers and experimenters remained in ignorance of the lists of each other.

The actual procedure of the compilers in arranging the names in the required order seems to have been somewhat as follows:

In the case of the children from the Elementary School, the Headmaster made three lists of the 30 boys who had served as subjects in these experiments, according to the class-lists of the three standards to which they respectively belonged; these three lists he connected into one by carefully intercalating the bottom boys of the upper standards with the top boys of the lower standards. He then thoroughly scrutinised the order and further re-arranged it from his private knowledge of the boys, with each of whom he was personally familiar. After an interval of several weeks, during which he frequently took lessons with the standards in question, he again revised the list. Where in doubt as to the relative position of two or more boys, his test-question was: "Which boy is the quickest at seeing the point of anything?" From his reputation as a judge of character, from his long personal experience of the boys concerned, and from the special interest, care, and conscientiousness with which he performed the task, there can be little doubt that the grading is as nearly perfect as a grading based on personal impression could be.

[1] It may be remarked that all who were kind enough (at the cost of considerable trouble, time, and care) to draw up these lists recognised, in one way or another and in their own phraseology, the distinction between successful mental activity due to sheer intelligence, independent of special experience of the particular subject-matter of that activity, and successful mental activity due to "learning" in one form or another, i.e. to acquired knowledge or to habitual practice; and were reminded that the preservation of this distinction was the most important and the most difficult part of their task.

For the mathematical elimination of observational errors by means of the formula presently to be cited, it was necessary to have at least two gradings for the same capacity, as far as possible independently obtained. A second grading of the boys, therefore, was obtained from their class masters. A third grading was furnished by their school-fellows,—two competent and impartial boys, not themselves among the thirty, being selected for the purpose. The boys were asked: 'supposing you had to choose a leader for an expedition into an unknown country, which of these 30 boys would you select as the most intelligent? Failing him, which next?' And so on.

In the case of the 13 boys from the Preparatory School, the Head-master's estimation was again based on the class-orders. By revising these, he produced two lists in order of Literary and Mathematical Ability respectively; and from an amalgamation of these a final order of general intelligence was derived. A supplementary grading for the purpose of mathematical correction was also obtained from two boys holding responsible positions in the school, not themselves on the list.

The Method of Calculating the Correlations.

The tendency to concomitant variation between two mental characteristics, such as General Intelligence and proficiency in some experimental test, may be best expressed by means of a coefficient of correlation. A coefficient of correlation is a single figure so calculated from a number of individual measurements as to represent with quantitative precision that degree of relationship between two variable qualities of a group, from which all the measurements actually observed might have arisen with least improbability. A relationship may be either absent, or present; and if present, either negative (i.e. inverse), or positive; and either complete, or more or less incomplete. Accordingly, the coefficient expressing it may have all possible values, from -1 through 0 to 1; or, in percentage, from $-100\,\%$ through $0\,\%$ to $100\,\%$.

A method of calculating correlational coefficients, convenient and of sufficient accuracy for the present purpose, has been devised, and used in investigations similar to the present, by Dr Spearman. It is called by him the 'foot-rule' or R-method. This method is a simplified application of the standard, 'product moments,' or r-method, elaborated by Bravais, Galton and Pearson. The actual measurements of the reagents' thresholds (or other capacities) forming the two series to be compared are first converted into comparable, i.e. homogeneous, values by substituting for them the numbers denoting the relative rank or position of the several reagents as arranged in order of proficiency for the two tasks.

The sum of the discrepancies in rank between the two series is then found. This is most readily done by subtracting the number representing the position in rank of each individual for the second test from the number representing his position in the rank for the first test, wherever the former is smaller; the remainders, representing the number of places gained by the individuals better at the second test, are added together, and multiplied by two; for the number of places gained by these individuals must necessarily be the same as the number of places lost by the rest, who are worse at the second test; and thus the sum of discrepancies between the two tests will be equal to twice the sum of gains in the second test.

The sum of the discrepancies between two series of the same length to be expected on an average by mere chance is then to be obtained by squaring the number of the members, subtracting 1 from the square, and then dividing the remainder by 3.

The ratio is next found between the sum of the discrepancies in rank between the two series observed and the sum of rank discrepancies to be expected by mere chance; and the resultant fraction is subtracted from unity. This gives the coefficient for the correlation between the two series in terms of the 'foot-rule'[1]. (For illustration and proof, see Spearman, this *Journal, loc. cit.* p. 107.) In algebraic notation, if R denote the coefficient of correlation, Σd the observed sum of dis-

[1] C. Spearman, *Brit. Journ. of Psych.* 1906, Vol. II. Pt. 1, p. 89, "'Foot-rule' for Measuring Correlation." Cf. also *ibid. Am. Journ. of Psych.* XV. 1904, pp. 72–101, "The Proof and Measurement of Association between Two Things"; also *id. ibid.* pp. 202–292, "General Intelligence Objectively Determined and Measured." The validity of the formulae given in the *American Journal* was challenged by Karl Pearson in *Biometrika*, III. p. 160; detailed mathematical proofs, however, have since been furnished, *Am. J. Ps.* XVIII. p. 161, and *Brit. J. Ps. l.c.* A critical discussion of the Foot-rule method by Karl Pearson will be found in *Biometric Series*, vol. II. (Drapers' Company Research Publications), and of the Correction formulae by William Brown in the reports of the *VIme Congrès Internationale de Psychologie, Genève*, 1909 ('Some Experimental Results in Correlation'). In this article I have nowhere discussed at length the advantages and limitations of Dr Spearman's formulae: instead of burdening a lengthy paper with further mathematical technicalities, I have preferred to leave the merits and defects of the methods I have endeavoured to test and illustrate to be inferred from the results they yield by those more competent to form an opinion on mathematical questions than myself. I may, however, add that most of the figures essential to my general psychological conclusions have been checked by the more elaborate and better accredited formulae, and the discrepancies have been found to be practically negligible.

crepancies, Σg the observed sum of gains, and n the number of cases in each series, then

$$R = 1 - \frac{\Sigma d}{\frac{n^2 - 1}{3}} = 1 - \frac{\Sigma g}{\frac{n^2 - 1}{6}}.$$

Thus, when there is complete correspondence between the two series of measurements and consequently no discrepancies of rank, the correlational coefficient will obviously be 1; when there is no correspondence the number of discrepancies will presumably be approximately the same as the number of discrepancies expected by pure chance, and the coefficient will consequently be 0 or nearly 0. When there is complete inverse proportionality the coefficient should be -1. This is actually the case when the coefficients are calculated by the standard or r-method, the method of product moments. But when calculated by the foot-rule or R-method the positive values obtained, especially midway between 0 and $+1$, are smaller than those obtained by the standard method, and the negative values are all larger. An inverse or negative correlation can usually be evaded by reversing the ranks of one of the series compared; while the positive values for R can readily be converted into terms of r by the empirical table given by Spearman (*B. J. P.*, *l. c.*, p. 104)[1]. On account of its convenience the R-method has been preferred in the present work for calculations, and it has not been thought necessary to evade by re-calculation the exaggeration of negative values. But as coefficients obtained by the standard method are more familiar to statisticians and admit more readily of the application of correction formulae, the translation from 'foot-rule' values has always been made, and the figures printed represent throughout correlations in terms of r.

In determining the relationship between two qualities from their manifestations in a limited group or sample, instead of from a complete investigation of the entire class—an investigation we never could accomplish—we are admitting a determinable source of error. The expression representing this source of error is called the Probable Error. It must obviously decrease in some way with the increase of the number of cases employed. For the correlations given below the probable error may, legitimately enough for the present purpose, be calculated directly from the more complete formula of the r-method; since the probable errors of the two respective methods are approximately the same in size,

[1] Or by the formula since mathematically deduced by Prof. Pearson (*Biometric Series*, *l.c.*). The average discrepancy, however, between the values given by Prof. Pearson's formula, and those given by Dr Spearman amount to less than 0·01.

(On the existing formulae for the probable error for coefficients obtained by Spearman's methods, see Karl Pearson, *loc. cit.*)

and the coefficients are everywhere converted into terms of r[1]. The formula for the r or standard method is taken as

$$\text{p. e.} = \frac{\cdot 6745}{\sqrt{n}}(1 - r^2).$$

Thus, if $r = \cdot 50$,—where $n = 30$ (number in Elementary School group), $\text{p.e.} = \frac{\cdot 6745}{\sqrt{30}}(1 - \cdot 50^2) = \cdot 09$; where $n =$ only 13 (number in Preparatory School group), $\text{p.e.} = \frac{\cdot 6745}{\sqrt{13}}(1 - \cdot 50^2) = \cdot 14$. A coefficient of correlation has little or no significance unless it is at least two to five times as great as its probable error. A coefficient five times as great as the probable error occurs by chance only once in 1000 trials; accordingly where a high correlation, such for instance as would give a coefficient $r = .50$, obtains between two functions, its existence may be satisfactorily demonstrated by about a dozen cases; and below this number none of the groups here investigated falls. A coefficient only twice as large as the probable error occurs about once in six times by mere chance. Hence such small coefficients can but suggest, not prove, the existence of real correspondences.

Besides those errors inevitable to the process of sampling and expressed by the 'probable error,' there are others incidental to the

[1] Dr Spearman has furnished me with the following proof that the probable errors of the two correlational methods are about equal:

Let R_R denote the p.e., expressed in terms of R, when using the R-method. Then

$$R_R = \frac{\cdot 43}{\sqrt{n}}. \qquad \text{(*Brit. J. Psychol.* Vol. II. p. 106.)}$$

Next let r_r denote the p.e., expressed in terms of r, when using the r-method. Then

$$r_r = \frac{\cdot 6745}{\sqrt{n}}(1 - r^2).$$

But it has been shown (*Brit. J. Psychol.* Vol. II. p. 102) that

$$r_r = \sin\left(\frac{\pi}{2} \cdot R_r\right)$$

(where R_r is the value corresponding to r_r in terms of the R-method); the latter expression, $\sin\left(\frac{\pi}{2} \cdot R_r\right)$, when, as here, small, is known to be approximately equal to $\frac{\pi}{2} \cdot R_r$.

Hence $$r_r = \frac{\pi}{2} \cdot R_r,$$

so that $$R_r = \frac{\cdot 6745}{\sqrt{n}}(1 - r^2)\frac{2}{\pi};$$

and, neglecting r^2, $$= \frac{\cdot 6745}{\sqrt{n}} \cdot \frac{2}{\pi} = \frac{\cdot 429}{\sqrt{n}} = \text{very approximately } R_R.$$

determination of correlations which are also susceptible of mathematical treatment. Errors traceable to definite and controllable sources have been largely minimised empirically by the principles governing the selection of reagents and the application of the tests; the remainders of these, together with errors creeping in from miscellaneous uncontrollable sources, constitute Observational Errors; such are inevitable to all measurement. All observations are liable to fluctuations, due to the fact that our figures measure not purely and directly the quantity they are intended to measure, but that quantity as modified by irregularly varying observational conditions; and psychological observations with children are peculiarly difficult and indirect. But upon repetition of the measurements, and appropriate calculation, such fluctuations may either neutralise each other in the average, or themselves be estimated and corrected in the final result.

As has been stated, two or three series of applications were made with nearly every test. The simplest method of eliminating observational error is to find the average of the two or three measurements actually observed for each individual, and work out the correlation between the two series of averages. The coefficient thus obtained is called the Coefficient of the Amalgamated Series.

But in so doing we have clearly sacrificed an additional datum supplied by the actual figures as experimentally obtained, namely, the amount of variability of these original figures about the calculated average. A set of averages derived from figures showing but small fluctuations, and consequently involving but slight errors of measurement, is more reliable than a set of averages derived from figures exhibiting large fluctuations and thus involving relatively large errors of measurement; such wide deviations and such unreliable averages must evidently tend to obscure a correlation when present and to diminish its apparent coefficient. The amount of fluctuation to which a given mode of measurement is liable can be determined by finding the correlation between the two sets of experimental figures obtained on different occasions or by different observers for one and the same measurement, or the average correlation if the measurement was applied more than twice. The coefficient then obtained is called the Reliability Coefficient, and measures inversely the reduction of apparent correlation caused by errors of observation incidental to that measurement. To eliminate such illicit reduction in the correlation between two such modes of measurement, the arithmetical mean of the several correlations between each of the various series repeated with one mode of measurement, with each of the various series repeated with the other, is divided by the geometrical mean of the two reliability coefficients for each of the two modes of measurement. Take for instance the determination of the true amount of correspondence between some mental test on the one hand and the estimation of intelligence by teachers on the other. Let us suppose that the test has been applied several times to a group of school children, and that these children have also been ranked according to intelligence by several school teachers. Let us denote the average reliability correlation between the several series of applications of this one particular test by $r(TT)$; the average reliability correlation between the orders for intelligence drawn up by the different teachers by $r(II)$; the average crude correlation of each of the series for the test with each of the orders for intelligence by $r(TI)$; and, finally, the required Corrected Coefficient of pure correlation between capacity tested and Intelligence by $r'(TI)$: then[1]

$$r'(TI) = \frac{r(TI)}{\sqrt{r(TT) \,.\, r(II)}}.$$

When the coefficient of correlation is raised by such correction the probable error of the coefficient will necessarily rise as well. A fair approximation for the probable error of the corrected coefficient may be gained by the aid of the probable error of the amalgamated series and the use of the following formula:

$$\frac{\text{p. e. of corrected coefficient}}{\text{p. e. of coefficient of amalgamated series}} = \frac{\text{corrected coefficient}}{\text{coefficient of amalgamated series}}.$$

Suppose, for instance, that the coefficient of the amalgamated series comes to ·48, and the corrected coefficient to ·55, and that the number of members in the series correlated is 30, then the probable error of the amalgamated series

$$= \frac{\cdot 6745}{\sqrt{30}} (1 - \cdot 48^2) = \cdot 096;$$

hence the probable error of the corrected coefficient

$$= \cdot 096 \times \frac{\cdot 55}{\cdot 48} = \cdot 11.$$

[1] C. Spearman, *Am. J. Psychol.* 1907, Vol. XVIII. p. 161. See, however, criticisms by W. Brown, *loc. cit. sup.*, and comments below pp. 137, 160.

From this it is evident that nothing but an illusory result can be obtained by applying a big correction to a correlation having a big probable error. Mathematical correction can only be employed to deduce theoretically the probable amount of the pure correlation when the size of the crude coefficient, being at least twice its probable error, indicates that the observed correlation actually exists.

Non-Experimental Correlations for Intelligence.

The correlational results for the empirical estimation, furnished by masters and boys, of the reagents' relative intelligence may be given first. The reliability coefficient for the Elementary School group is ·88; for the Preparatory School group ·91. The estimates made by the Head-masters, especially for the larger group, were without doubt unusually reliable. Nevertheless the reliability coefficients are probably a little too high. This may, perhaps, be due to the fact that all who drew up the lists in question were more or less aware of the official class order, and may consequently have been influenced consciously or unconsciously by its arrangement, especially perhaps where their own personal impressions were deficient. If so, their gradings would to that extent fail to represent two series of observations completely independent.

No exact scholastic data were obtained to supply an absolute, as well as a relative, empirical evaluation of our boys' level of intelligence. At the Elementary School I was kindly allowed to set a special examination to the boys in Arithmetic, Composition, and other school subjects; but it did not prove practicable to set a similar examination to the boys of the Preparatory School, the results of which should be comparable to those of the former. The examinations referred to in the sequel, therefore, are the annual school examinations. The correlations of these with intelligence, as estimated by the masters' impressions, are ·81 at the Elementary School, and ·78 at the Preparatory School. The reliability coefficients of such examinations is between ·60 and ·80.

Though irrelevant to the present investigation, the following miscellaneous correlations obtained from the Elementary School group during our work there may be of interest to some. With them are printed, for purposes of comparison, coefficients obtained by Prof. Karl Pearson from a large number of schools, mainly secondary, taken from *Biometrika*, 1906–7, p. 127:

	Oxford Central School	Secondary and other Schools
Intelligence (Headmasters' estimate) and fairness of hair	·32	·10
Health (Masters' estimate)	·24	·17
Athletics ,,	·37	·20
Conscientiousness (Masters' estimate)	·54	·46
Age	·28	·05
Handwriting	·41	·28
Suggestibility	·28	
Repetition (Special Examination, 20 to 26 boys only)	·51	
Arithmetic ,, ,, ,,	·62	
Mental Arithmetic ,, ,, ,,	·73	
Dictation ,, ,, ,,	·77	
Composition ,, ,, ,,	·79	
Amalgamated result ,, ,, ,,	·74	

Suggestibility (Binet's line test) correlates with Conscientiousness to the extent of ·30. With reference to the supposed racial character of fair hair it may be added that over 75 °/$_\circ$ of the Preparatory group, and under 25 °/$_\circ$ of the Elementary group, appeared typically fair.

NATURE AND RESULTS OF THE SEVERAL TESTS.

At the risk of tediousness the apparatus and methods employed for the various tests are described with some detail, in order to enable anyone from the mere description to repeat the experiments with sufficient similarity for comparison with the results here recorded, or with the improvements suggested by our own experience for the achievements of results more accurate still.

The results—reliability coefficients, coefficients of amalgamated series, average raw coefficients, and corrected coefficients, together with the observational data in summary form—are first recorded and discussed under the headings of several tests; a comparative survey of the entire series may be obtained from the tables given later with the general conclusions.

For the sake of brevity, the observational data are recorded not in the form of the actual measurements experimentally obtained, but in the form of averages for each of the groups (calculated from the average performances for each of the members)—the distribution of the measurements within the groups being indicated by the mean variation of the performances of the individuals[1], and their range by the two extreme measurements, i.e. by the average performances of the best and worst individuals in each particular test. A comparison of the respective data for the two groups will yield a conception of the capacities—both special and general—of the boys of superior parentage relative to that of the boys of ordinary parentage. The boys of the larger group, when arranged in order of intelligence by the Headmaster, seemed to him to fall naturally into three sections, comprising 7 "clever" boys, 14 "average" boys, and 9 boys "below the normal"; the average performances for each of these three sections is, accordingly, also given, since they not only afford a simple corroboration of correlations with intelligence, but also render the comparison between the two groups more definite. At the same school there was found to be a boy congenitally weak-minded; his age at the time of the commencement of the investigation was 13 years 10 months; he, therefore, did not fall within the group selected, but where it proved possible to apply the tests to him the results are recorded, since his intelligence was presumably minimal, and his case therefore furnishes a useful negative instance.

SENSORY TESTS.

(1) Touch Discrimination.

Apparatus and Procedure.

Tactile acuity was measured by the threshold for simultaneous discrimination of two points upon the skin. The area of skin tested was the middle third of the volar surface of the right forearm. The instrument used was the aesthesiometer devised and used by Dr Spearman[2]. Being constructed of aluminium with blunt celluloid tips, it is peculiarly light (25 grs.). It carries a millimeter scale and vernier for adjusting the distance between the two parallel points, and a third point for single stimulation. In preliminary trials upon other subjects the compasses were also employed, and found practicable, but not so convenient; the thresholds given were similar, but more variable.

[1] Designated, in accordance with Rivers' suggestion (*Brit. J. Psychol.* Vol. I. Pt. 4, pp. 354, 355) by the capital letters M. V., to distinguish it from the mean variation as a measure of the variability of observations designated by small letters, m. v.

[2] C. Spearman, *German Congress for Experimental Psychology*, 1904. K. Krueger u. C. Spearman, "Die Korrelation zw. verschiedenen geistigen Leistungsfähigkeiten," *Zeitschr. f. Psych.* Bd. 44, S. 69.

Each boy was allowed to watch and to emulate the performance of his predecessor. If it was his first occasion, the nature of his task was also briefly demonstrated to him. And then, the boy being seated, with his head averted, and his arm lying comfortably on a small table, screened from surreptitious glances, the points of the instrument were applied to the skin with firm pressure lasting nearly a second. It was found advisable to warn the subject by saying "Now" about one second before each application. The single point was applied almost as frequently as the two points. The subject was told to reply "One" or "Two" according as he judged that one or two points touched his arm. And his judgments were recorded (where possible) by a third person.

The threshold sought was the smallest distance at which two points yield a sensation perceptibly double, in order as far as possible to avoid the influence of inference from secondary sensory factors other than distinct twoness[1]. To take as the threshold the smallest distance at which two points yield a sensation merely different from that yielded by a single point, tends (unless all the reagents are first subjected to prolonged training) to handicap favourably those boys whom Binet[2] would class as *interpretateurs* at the expense of the *simplistes* and the so-called *distraits*; such a classification of subjects according to their different methods of judgment in this test was found in preliminary experiments to correlate with intelligence. The procedure followed was a combination of the Method of 'Minimal Changes' with the Method of 'Right and Wrong Cases'—a procedure devised and adopted by Mr McDougall for the investigation of the delicacy of Tactile Discrimination among the Murray Islanders[3].

Each series was commenced with applications of the two points at a distance well above the probable threshold of the subject (usually about 70 mm.), and proceeded by gradually diminishing this distance by successive stages till it had descended well below the threshold. So long as the boy's answers evinced no genuine errors, five applications of the two points were made at each stage, the interval between them was

[1] On this cf. especially W. H. R. Rivers, *Brit. Journ. of Psych.* Vol. I. Pt. 4, pp. 366 sq., 364 and 390.

[2] *L'Année Psychologique*, 1897, III. p. 225. In the two groups whose achievements are recorded below, there were noticed but two *interpretateurs*, and but three or four *distraits* (owing to imagination or suggestibility rather than inattention),—all at the Elementary school.

[3] W. McDougall, *Cambridge Anthropological Expedition to Torres Straits*, Vol. II. Pt. 2, p. 189.

diminished by 5 mm., and the boy was told whether his answers were right or wrong; this modification of the usual procedure was eventually adopted as the quickest method of leading the boy to feel confidence in his own judgments without developing and practising a tendency to speculate and infer. As soon as the boy's answers exhibited a genuine error, ten applications of the two points were given at each stage, and the distance was reduced by 2·5 mm., till a large percentage of errors was obtained. The stage at which the boy first began to make 20°/ₒ errors, i.e. two wrong judgments in ten applications of the double[1] stimulus, was accepted (unless subsequent recovery in the continuation of the experiment indicated temporary inattention) as his threshold of tactile discrimination, and the distance in millimeters of the two points at this stage was accepted as the measure of his threshold. It may perhaps be argued that strictly thresholds are only comparable when, not the actual measurement of absolute distance just discriminable, but the ratio of this distance to the length of the arm of the subject, is taken as the measure of the threshold[2].

Results.

Three series of observations upon Tactile Discrimination were obtained from the 30 boys of the Elementary School, by the writer, by Mr Flügel, and by the writer again, at intervals of about a fortnight. Each observation occupied about 20 minutes, and consequently each series extended over several days. The influence during this period of changes of weather and of health, of the time of the day and of the week, was noticeable, but not considerable; and when any extraneous factor appeared to be influencing the work of a particular boy to a marked degree, manifesting itself in a suspiciously high threshold or (more commonly) in suspiciously irregular judgments, the sitting was adjourned. Two series of observations were obtained from the 13 boys at the Preparatory School, one by each of us, at an interval of four days. The number of subjects being here far smaller, variations of weather, fatigue, &c. were unmarked during the relatively short duration of the

[1] According to Mr McDougall's original procedure. Dr Rivers in adopting it prefers to use the errors in the single stimulations, not merely as a rough precaution and guide, but also as indicating the proper estimate of the threshold, which he defines as that distance at which two mistakes in ten occur with *each* kind of stimulation (*loc. cit.* p. 364).

[2] The forearms of the boys of the Elementary School varied considerably in length; consequently their thresholds would perhaps be more strictly comparable if they were first reduced to terms of an arm of average length. When this is done the correlation is slightly diminished, viz. to ·08.

observations. The reliability coefficient for the three Elementary School series was ·73; for the two Preparatory School series, ·75. The lower coefficient in the larger group may be due to the peculiar variability commonly evinced in sensory tests by about 20 °/₀ of any group of subjects, and actually traceable in the experimental figures yielded by this group, since for the above reasons this variability probably had a slightly freer opportunity to manifest itself at that school. But a difference of ·02 is in itself not a significant one. Excepting these characteristically variable subjects, and considering what at the time of experimenting we took to be the peculiar difficulties of applying sensory tests to school-children—the thresholds independently arrived at prove in the majority of cases to be strikingly uniform. Nor is there any significant difference between the reliability coefficients of two series obtained by two different operators and those of two series obtained by the same operator.

In an investigation into the *Korrelation zwischen verschiedenen geistigen Leistungsfähigkeiten* by Krueger and Spearman the reliability coefficient between the two series obtained by the two different operators upon eleven German subjects tested with similar apparatus upon the right hand, and the right and left cheeks, is given as only ·42. From this we may perhaps infer the relative superiority of the psycho-physical method devised by Mr McDougall and employed in the present investigations to the more rapid psycho-physical procedure employed by Krueger and Spearman.

The coefficients of correlation between the amalgamated series of average individual thresholds and the orders of Intelligence given by the Headmasters are for the two schools ·13[1] (p.e. ·12) and − ·06 (p.e. ·20) respectively. The average of the uncorrected correlations between the several original series and the various gradings for Intelligence is ·14 for the Elementary children and − ·14 for the Preparatory children. Corrected by the above reliability coefficients these would give pure correlations of ·18 and − ·17; correction, however, would here be scarcely valid, as the average raw correlations are not twice the probable errors.

The correlations obtained by Krueger and Spearman between tactile thresholds and tests which we may regard as indicating the relative intelligence of their subjects—adding figures, Ebbinghaus's *Kombinations-Methode*, and learning rows of numbers—were correspondingly small, namely, 0·19, 0·00, and − 0·13 (Probable Error,

[1] *Loc. cit.* S. 77.

0·19 to 0·25)[1]. Many observers have argued for a close connection between tactile discrimination and intelligence. Of these the most recent is M. Schuyten. In his experiments upon the cheeks of Antwerp school children, he finds that the intelligent half of each group tested has a decidedly acuter average threshold than the unintelligent half[2]. With him, however, "les expériences, une minute par élève," were extremely short. Such rapid methods with the aesthesiometer may yield (and apparently often have yielded) a correlation with intelligence; but it cannot yield a valid measure of sensory discrimination.

We may accordingly conclude that a genuine threshold for successive discrimination of two points upon the skin has extremely little or no connection with General Intelligence. This is corroborated by the observation that the congenital imbecile seemed to have the best threshold of any examined, namely less than 30 mm. according to one observer (Burt) and 10 mm. according to another observer (Flügel), even though our psycho-physical procedure was particularly unfavourable to such a subject. Though too small as compared with its probable error to be significant by itself, the positive correlation in the larger group accords with the fact that the three sections comprising clever, average and infra-normal boys had average thresholds of 31·3, 37·3, and 38·3 mm. respectively[3]. But this small correlation, if really existent at all, is probably to be attributed to the more intelligent attitude adopted by the more intelligent boy in submitting himself to the test, and to his consequent smaller percentage of accidentally erroneous judgments and higher percentage of correct inferences—a difference among the subjects which was scarcely observable among the Preparatory boys.

The observational data for comparing the Tactile Acuity of the two groups are as follows. Average threshold for discrimination of two points upon the forearm of boys tested at the Elementary School 36·2 mm. (Mean Variation 9·0, lowest threshold 19 mm., highest threshold 58·3 mm.); at the Preparatory School 38·9 mm. (Mean Variation 11·0, lowest 12·5 mm., highest 63·7 mm.). The average threshold at the latter being actually duller than the average threshold of the dullest group at the former (38·3 mm.), it is clear that sensory discrimination upon the skin is acuter at the Elementary School. Perhaps this may be plausibly attributed to an endowment making for readier and sharper sense-perceptions and perceptual 'inferences,' currently credited to the children of lower social status as compared on the

[1] *Loc. cit.* S. 75. [2] *Revue de Psychiatrie*, 1908, p. 185.

[3] Or, reduced to terms of a forearm of average length, 31, 37, and 37 mm.

whole with the steady and more cautious child of the thoughtful classes. Confirmation of this suggestion, and of the data upon which it is based, may be found in measurements recently recorded by other observers. Yasousabouro Sakaki has tested the tactile discrimination upon the cheek of children at Tokio. He finds "la normale physiologique est pour les écoles primaires de filles 11,6 mm., pour les écoles primaires de garçons 12,3; pour les écoles supérieures de jeunes filles 12,1; pour un athenée 13,2." And again, he finds as the mean threshold of sons of 'petit négociants' 12,2 mm.; of sons of 'savants, professeurs' 13,0 (that of sons of 'banquiers,' however, is yet higher, namely 13,4)[1]. The figures for schools of different social status may be paralleled by figures for races of different culture. Arranging the available figures[2] for thresholds upon the forearm in order of magnitude, commencing with thresholds of greatest acuteness, we have the following series:

TABLE I.

Tactile Discrimination among Groups of Different Cultural Status.

Group	Observer	Threshold
Papuan boys	McDougall	15·0 mm.
Papuan men	McDougall	19·8 mm.
Dayaks	McDougall	35·0 mm.
Toda boys	Rivers	35·0 mm.
English Elementary boys	Flügel and Burt	36·2 mm.
English Preparatory boys	Flügel and Burt	38·9 mm.
English village children	Rivers	43·0 mm.
English men ('mostly of working classes')	McDougall	44·6 mm.
English men ('of the educated class') ...	McDougall	'rather higher'
Toda men	Rivers	45·5 mm.
English men (Cambridge graduates and undergraduates)	Rivers	56·5 mm.

In obtaining these figures, the procedures used by Mr McDougall, Mr Flügel and myself were the same; that used by Dr Rivers yields (as has been noted above) thresholds slightly higher than the procedure suggested by Mr McDougall. If, therefore, we made allowance for this difference, the series would rearrange itself, and the groups would fall in an order showing a complete inverse correspondence with that of

[1] *Revue de Psychiatrie*, 1908, pp. 140, 142.

[2] W. McDougall, *Anthropological Expedition to Torres Straits*, Vol. II. pp. 191, 192; W. H. R. Rivers, "Observations on the Senses of the Todas," *Brit. Journ. Psychol.* 1905, Vol. I. Part 4, p. 369.

cultural development. For, as Dr Rivers observes "in order of cultural development the Dayaks and Todas occupy an intermediate position" between Papuans and English; the Todas, I understand, are perhaps a little higher than the Dayaks; while the respective culture of the various English groups is implied in their description.

This, then, confirms our observations, that among groups differing considerably in cultural status, and so far presumably of different innate intelligence, the less intellectual group tend in average tactile discrimination to be the more acute; while among individuals of the same cultural class, any apparent positive correlation between tactile discrimination and intelligence, is probably illusory, and due to the interpretative quickness of the more intelligent.

(2) Weight Discrimination.

Apparatus and Procedure.

Capacity to discriminate Weights was investigated by means of a graduated series of some 21 weights, constructed by the Cambridge Instrument Company on Galton's convenient cartridge pattern, ranging in weight from 80 to 120 grams. These were lifted by the subject with the thumb and finger of the right hand through a vertical distance of 17·5 cm.—the height of the lift being regulated by a tape stretched horizontally between two uprights. The standard, a constant[1] weight of 100 gms., was lifted first; the variable weight, placed on the right of the standard weight, was lifted second. And the boy was instructed to say, after once simply raising and replacing each of the pair of weights set before him, whether the second weight was 'lighter' or 'heavier[2].'

The psycho-physical method adopted was analogous to that described as used in testing discrimination of two points. As before a 'descending series' alone was taken, and at the crucial stages the procedure was

[1] To ensure that the judgments were immediate, and not illegitimate inferences based on surmises as to the nature of the psycho-physical method (e.g. when there was reason to suspect that a boy recognised the variable weight as the same during its repetition at a given stage from accidental marks upon the cartridge cases), it was sometimes necessary to alter the standard slightly, and make a corresponding alteration in the variable weights.

[2] It may be of interest to note, as bearing on the psychological theory of Comparison of Sense-impressions, that the natural tendency of the boys seemed invariably to be to indicate, by pointing or by naming, the heavier of the two weights rather than to pronounce a judgment directly expressing an 'absolute impression' of the heaviness or lightness of the last lifted.

'without knowledge' of the correctness of the judgment. The subject commenced with an easily distinguishable positive difference, viz. 18 gms. (100 gms. standard and 118 gms. variable), and the difference was reduced by successive steps of 2 gms. An equal number of equal negative differences, i.e. differences below the standard, were irregularly interspersed at each step to obviate a bias in favour of the judgment that the second was generally heavier. The point at which the subject first made 20 °/ₒ errors upon the positive differences without subsequent recovery was accepted as his threshold.

Results.

In successive discrimination of lifted weights, three experimental series were carried out at the Elementary School and two at the Preparatory School, as for Touch. At the Elementary School the determination of the threshold for weight gave far less trouble to both subjects and operators than that for touch. But by nearly all the Preparatory boys it was much more confusedly performed. The reason is difficult to assign; observation of their manner when judging suggested that they found greater difficulty in associating difference of mass (unless very distinct) with objects of equal size and similar appearance. The fact reappears conclusively in the reliability coefficients; these are for the one school ·86, and for the other ·51.

The correlation between the average thresholds and the Headmasters' grading is − ·13 (p. e. ·12) and − ·19 (p. e. ·19); the average crude correlation − ·01 and − ·14; the corrected coefficients would work out at − ·01 and − ·20, but correction is again scarcely valid.

The standard being 100 gms., the average threshold was at the Elementary School 8·75 gms. (M. V. 1·5, extremes 6 and 16 gms.); at the Preparatory School 9·3 gms. (M. V. 1·6, extremes 5 and 11·5 gms.)—the three sections of the former group averaging 8·5 gms. (clever boys), 9·5 gms. (average boys), 8·1 gms. (infra-normal boys); and the imbecile boy 4 gms.

Hence, connexion between Weight-Discrimination and Intelligence seems to be either zero or even slightly inverse; and boys of superior cultural status are hardly as acute in distinguishing fine differences of weight as those of lower social status.

The latter conclusion is in harmony with the results of anthropological investigation. Mr McDougall found the power of discrimination of small differences of weight rather more delicate in the Murray

Islanders than in Englishmen, the average least perceptible difference among the former being 3·2 °/₀ of the total weight, among the latter 3·9 °/₀[1]. But the latter conclusion seems at first sight inconsistent with the results obtained by Dr Spearman. At a village school the correlations between weight-discrimination of the 24 eldest children individually tested and two gradings for "Common Sense" and one for "School Cleverness" were (raw) ·38, ·27, and ·38; (corrected) ·43 (prob. error, about ·10). In a high class Preparatory School the correlation between a collective test of weight-discrimination and amalgamated school place, modified to eliminated age, was ·12 (prob. error ·09). Calculated from his figures, given in terms of 200ths of the standard, the average thresholds prove to have been, at the former, 11 (M. V. 4·4, extremes 4 and 28); at the latter, 12 (M. V. 4·1, extremes 4 and 28),—the apparatus used having been cartridge weights of 1000 grains and upwards, the heavier weights successively increasing in geometrical proportion[2]. It seems evident that the threshold determined in the two different investigations was not the same kind of threshold. The main differences in the method of determination (besides the fact that the subjects required differences of weight four times as fine as ours, *loc. cit.* p. 246) seem to have been that, in the case of his Preparatory School, the subjects were examined collectively, so that "it was impossible to control whether they all handled the apparatus in the same manner"; and, in the case of his village school, "the beginning of the test was devoted...to quietly affording the reagent a maximum of fore-exercise," and "before taking down each reply, a chance of reconsideration was given by repeating the test in such a manner as to reverse the constant error of time and space." The comparative lowness of the thresholds recorded by Dr Spearman is thus explained by the fact that they were obtained by "only recording those answers which were given under the most favourable conditions[3]," whereas our thresholds were obtained under conditions which were unfavourable to the objective truth of the judgments because they aimed at excluding every ground for the judgments except subjective sensations obtained in a manner as nearly as possible the same for all. Whether rigidity and limitation of condition is, more nearly than freedom and favourableness, 'the same for all' subjectively

[1] *Loc. cit. sup.* p. 198. Dr Spearman's village children were also slightly more acute at weight-discrimination than his preparatory children.

[2] *Amer. Journ. Psych.* Vol. xv. p. 286 sq.

[3] *Loc. cit.* pp. 249, 247; the last quotation in its context refers only to the group tested individually.

as well as externally is hard to decide. In a prolonged investigation into absolute thresholds, variety of conditions as to 'time' and 'space' would be indispensable; but in a comparative test lasting but 15 minutes differences of thresholds obtained with opportunities unrestricted in these respects probably indicate differences in power of inference or in rapidity of practice as often as differences of mere sensory acuity. In this way it seems possible to explain the apparent divergence between our respective results with this test[1].

(3) Sound Discrimination.

Apparatus and Procedure.

This test was introduced as an after-thought, somewhat late, into the programme. It was in consequence impossible either to repeat the observations on this test or to work through a preliminary set of experiments with a view to determining the most satisfactory instrument and the most satisfactory procedure for the purpose. It seemed that at least the results would be more comparable with those of the most recent and most successful employers of this test, if the form of the instrument and the form of the problem employed by them were adopted for the present series.

The tones, therefore, were produced by plucking successively the two wires of the dichord devised by Spearman, and the subjects were required to state whether the second note was "higher" or "lower" than the first.

The constant wire was tuned to E above middle C ($e' = 320$ vibs.); the variable wire was tuned to the same pitch by adjusting its tension till no beats were perceptible between the note emitted by it and the note emitted by the constant wire. Its length was then varied by moving the sliding clamp. This clamp is furnished with a vernier, moving upon a millimetre scale, so that the length of the wire vibrating

[1] Commenting on this passage Dr Spearman has written to me: "It seems to me that your results are in perfect harmony with that obtained at my high class preparatory school, as the correlation there was less than twice the size of the probable error, and therefore without significance. But at my village school, unlike you, I tried to include as much practice as possible, and this I think amply explains our at first sight discordant results; I have found over and over again that practice may greatly increase tendencies to correlate." By 'practice' is here apparently meant practice during the *same* sitting, or 'fore-exercise.' Practice in *successive* sittings seems to have the reverse effect on intelligence correlations in tests whose procedure is readily grasped. Cf. p. 168.

can be adjusted correctly to 0·1 mm. In the region of the once-accented octave a difference of 1·0 mm. corresponds to difference of about one vibration. The procedure was again the combination of Right and Wrong Cases with Minimal Changes,—commencing from an easily noticeable supra-liminal difference of about 10 vibration differences, and descending by steps of 1 vibration difference,—and 'without knowledge' at the critical stages.

Results.

One series was worked through at each school by the writer; and at the Elementary School 18 of the boys were also tested by Mr Flügel. For the two series thus obtained from these 18 the correlation coefficient is ·67; this figure is accordingly used as the measure of reliability both at the Elementary and at the Preparatory School. The reliability coefficient for the differential thresholds for pitch obtained by Krueger and Spearman from their eleven adult German subjects was much higher, viz. ·87[1]. The low 'reliability' of the results obtained at the schools seems due to two main causes. Though admirably adapted to collective work, or individual work with small groups, for which it had previously been employed, the form of dichord used by us was not so well adapted to the individual testing of groups so large that the experiments lasted over periods of several hours on several successive days, since the tension of the wires, and consequently the pitch of the notes, was found to vary appreciably from hour to hour and from day to day[2]. It was found, too, that successful judgments of *direction* of differences of pitch are not only intrinsically more difficult than judgments of mere difference, but also presuppose a knowledge of, and familiarity with, the meaning of the terms 'higher' and 'lower' as applied to the musical scale,—a knowledge which is not always available in children who have not enjoyed some degree of musical training. Owing to the late introduc-

[1] *Zeitschrift für Psychologie*, Bd. 44, p. 77.

[2] On finishing the experimental work recorded in the present section we were inclined to believe that tuning-forks would have given more reliable results; but subsequent experience with these convinces me that the rise in pitch of the tuning-fork after being sounded, unavoidable unless complicated and expensive accessory apparatus is used, renders its tones even more unsatisfactory for work such as the present than the ordinary dichord, while the manipulation of the dichord is easier to acquire. In his report upon "Apparatus and Methods for the Experimental Investigation of Sensations of Tone," for the American Psychological Association, Dr Spearman proposes several modifications of the original form of his instrument which will minimise or abolish these and other slight imperfections that he notes.

tion of this test into our programme we were unfortunately unable not only to repeat the series for the mathematical elimination of errors otherwise unavoided, but also to make preliminary experiments which would have led to an earlier discovery of avoidable sources of error, such as the two just mentioned.

The correlation between the thresholds for pitch (amalgamated where possible) and the Headmasters' order is, for the Elementary School ·40 (p.e. ·10), and for the Preparatory School ·37 (p.e. ·17). The raw correlations average ·40 and ·35. These being distinctly more than twice the size of the Probable Error, we may legitimately apply the correction formula, when we obtain pure coefficients of ·52 and ·41. The correlations obtained by Krueger and Spearman between Pitch-discrimination and Adding, the *Kombinations-Methode*, and Memory were (av. raw) ·67, ·59, ·17; (corrected) ·68, ·64, and ·00 respectively. At Spearman's village school the correlations of Pitch-discrimination with "Common Sense (A) and (B)" and "School Cleverness" were ·44, ·41, and ·25, yielding a 'pure' correlation with General Intelligence amounting to ·71. At the high class Preparatory School the correlation of Pitch-discrimination with Average School place was ·33, and with proficiency in four branches of study ·51 (av. raw); omitting non-musicians and 'correcting' ·87. These results harmonize with our own. The higher coefficients may be due either as before to a slight difference of psycho-physical method affecting the practice or procedure of the subjects, or perhaps to a more successful manipulation of the instrument[1].

The average threshold at the Elementary School was 6 v. d. (M. V. 1·9 v. d.; extremes 1 and 11·5 v. d.). The average thresholds of the three sections were 4·6, 5·6, and 7·1 v. d. The threshold of the imbecile was not, on the most generous estimate, lower than 10·0 v. d.; he was, however, peculiarly difficult to test. One or two boys who were known as "hard of hearing," that is, unable to hear sounds of low intensity, proved to have a high threshold also for pitch; they usually ranked as somewhat dull scholars. The average threshold at the Preparatory School was 3·5 v. d. (M. V. 2·2 v. d.; extremes 0·3 v. d. and 8·0 v. d.). The average thresholds of Krueger and Spearman's 11 German adults was 7·5 v. d. (M. V. 2·2; extremes 1·4 and 25 v. d.), of Spearman's 24 village children 9·8 v. d. (M. V. 6·0; extremes 1·3 and 30 v. d.), of his Preparatory boys 4·6 v. d. (M. V. 3·0;

[1] Dr Spearman writes: "Myself, I should attribute it to the same cause as before, viz. more fore-practice."

extremes 0·6 and 20 v. d.)[1]. As to hearing among savages, the available data "so far as they go point to inferiority rather than superiority in Papuans and Todas[2]." The average thresholds for pitch-discrimination among the Papuans tested by Dr Myers were 15·4 v. d. (adults, first sittings), and 12·5 v. d. (children, second sittings); those of the inhabitants of Aberdeenshire tested by him were 7·6 v. d. (adults, first sittings) and 4·7 (children, second sittings)[3]. The Todas were tested by Dr Rivers only for auditory acuity, not for pitch-discrimination. The present results, therefore, harmonize with those of previous investigators in finding an acuter judgment of pitch among groups of superior cultural status. This difference was not due to superior practice on the part of the Preparatory boys, since in this respect the Elementary boys were equally, if not more, fortunate. All of them enjoyed musical instruction; more than half were choristers; many learnt some musical instrument; whereas five out of the thirteen Preparatory boys neither sang nor learnt music, and had to be told the meaning of the terms 'higher' and 'lower.' Beyond the fact that this unfamiliarity with the nature and nomenclature of their task seemed in several cases unfavourably to handicap them, lack of practice seemed to make little or no difference. The average threshold of the boys who neither sang nor played was 3·4 v. d., that of those who played 3·5 v. d. (At Spearman's Preparatory School, however, the median threshold of non-musicians was 5·0 v. d., that of musicians 2·3 v. d.) This harmonizes with Dr Spearman's opinion that superior auditory acuteness is due to 'general culture' more than to special practice[4].

(4) Comparison of Lines.

Apparatus and Procedure.

The only test essentially involving vision was a test of the discrimination of the lengths of lines by an active method of comparison.

The apparatus used was that devised by Dr Rivers[5]. It consists of an oblong board, of such a size and weight that it can be conveniently

[1] Calculated from figures given, *Zeitschr. f. Psychol. l.c.* p. 72, and *Amer. J. Psychol. l.c.* pp. 286, 290.

[2] W. H. R. Rivers, "Observations on the Senses of the Todas," *Brit. J. Psychol.* 1905, Vol. I. Part 4, p. 391.

[3] *Cambridge Anthropological Expedition, l.c.* p. 168.

[4] *Amer. J. Psychol. l.c.* pp. 29, 30.

[5] W. H. R. Rivers, "Observations on the Senses of the Todas," *Brit. J. Psychol.* 1905, Vol. I. Part 4, p. 349.

held with the left hand, perforated with two pairs of holes, all four in the same horizontal straight line. Through either pair of holes runs a loop of string kept taut by a spring at the back. Half the length of each of the two strings is coloured white, and the rest black. They thus present the appearance of two straight horizontal white lines on a black surface, whose length can be varied from 0 to 20 cm. by moving the loops. In the following experiments the left-hand line was kept at a constant length, viz. 10 cm., to serve as the standard; and the subject was required to adjust the right-hand string, holding the board in his left hand and adjusting the variable line by moving the loop from behind the board with his right, and thus diminishing it from a greater length, or increasing it from a less, till he could no longer distinguish any difference of length between the two lines. The length of the lines formed by the white portions of the strings was then measured by a T-square furnished with a millimeter scale.

The procedure was 'without knowledge.' Each boy gave two sittings to this test, one to each operator. Ten crude observational figures were obtained from each boy on each occasion, five by the shortening or 'descending' procedure, five by the lengthening or 'ascending' procedure. The average, taken regardless of sign, of the differences between these ten figures and the standard was taken as the measurement of the boy's Average Crude Error. The boy's average determination was calculated from the ten figures taken together and the difference between this average, and the length of the standard was taken as the measurement of his Constant Error. The arithmetical mean of the deviations about this average was taken as the measurement of his Mean Variable Error.

Results.

The correlations with Intelligence and the group-averages for these three measurements are given in Table II. As the differences between the two operators' results are striking, they have been calculated separately as well as in amalgamation. The Constant Error may be either positive or negative; in using it as a basis for further calculations, determining its average for a given individual, or comparing its measure as obtained by two different operators, the magnitude of the error may alone be considered, or its direction may also be taken into account, reckoning negative amounts as amounts below zero. Both methods have been used in computing amalgamation and reliability coefficients.

TABLE II. *Comparing Lines.*

Averages of ten.

	Elementary School	Preparatory School
(1) CONSTANT ERROR:		
(*a*) Reliability coefficient		
(i) Regarding only size of error	·34	– ·05
(ii) Regarding both size and sign	·22	·07
(*b*) Correlation between F's series and Headmaster's order	·14	– ·19
(*c*) ,, ,, B's ,, ,, ,,	·00	·51
(*d*) Correlation of amalgamated series and Headmaster's order		
(i) Individual averages calculated regarding only size of error	·14	·28
(ii) Individual averages calculated regarding both size and sign	·14	·05
Average size of error for group (F's series)	·26 cm.	·28 cm.
(B's series)	·32 cm.	·18 cm.
(amalgamated)	·29 cm.	·28 cm.
(2) AVERAGE CRUDE ERROR:		
(*a*) Reliability coefficient	·50	·54
(*b*) Correlation between F's series and intelligence (Headmaster's order)	·28	– ·07
(*c*) Correlation between B's series and intelligence ...	·17	·54
(*d*) Correlation of amalgamated series and intelligence ...	·29	·17
Average for group (F's series)	·41 cm.	·50 cm.
(B's series)	·40 cm.	·28 cm.
(amalgamated)	·41 cm.	·39 cm.
(3) MEAN VARIABLE ERROR:		
(*a*) Reliability coefficient	·23	·56
(*b*) Correlation between F's series and intelligence ...	·31	·01
(*c*) ,, ,, B's ,, ,, ...	·13	·56
(*d*) Correlation of amalgamated series and intelligence ...	·31	·35
Average for group (F's series)	·33 cm.	·40 cm.
(B's series)	·26 cm.	·23 cm.
(amalgamated)	·30 cm.	·31 cm.

The correlation between this test and Intelligence is highest in the case of the Mean Variable Error, and lowest in the case of the Constant Error. But in both these measurements the results of the two operators differ considerably, and the difference is itself different in the case of the two different schools. In the case of the Average Crude Error—the divergence, especially at the Elementary School, is not so great; the reliability coefficients are high, and about equally high, at both schools. The anomalies thus seem to be brought out by the further calculations; and consequently the Average Crude Error is taken throughout as the least precarious measurement of proficiency at this test for the purposes of the present investigation.

The correlations of the amalgamated A. C. E's. with Intelligence is ·29 at the Elementary School and ·17 at the Preparatory School. The average raw correlation between the two gradings for the A. C. E. and the various gradings for Intelligence proves to be somewhat higher, namely, ·34 at the Elementary School and ·31 at the Preparatory School, correcting to ·51 and ·44 respectively. As to the comparative merits of the two schools in the two tests, the amalgamated A. C. E. show a very slight difference in favour of the Preparatory School; the group averages being ·41 (M. V. 107) and ·39 (M. V. 179); but if we take the more satisfactory of the unamalgamated series for each school, then the difference is decided. So far as I am aware, no measurements have been made by means of this apparatus among savages. In estimating length of lines by eye with other methods, the Murray Islanders seem to have been slightly inferior to English students and children. The visual test employed by Dr Spearman was discrimination of luminosity. Figures obtained by previous workers, therefore, are here not available for purposes of comparison.

The origin of the curious divergences between the figures for the two operators and the two different schools is a matter for speculation. Such speculations, however, would be relevant rather to a discussion of the methodology and psychology of the test, than to a discussion of its relations with Intelligence. I append, therefore, a further table of

TABLE III. *Comparing Lines.*

Averages of five.

	Number tested	Average			M. V.			No. of average determinations	
		Flügel	Burt	Amal.	Flügel	Burt	Amal.	above 10	below 10
Shortening.									
Elementary boys ...	30	10·11	10·06	10·08	·276	·211	·225	37	23
Preparatory boys ...	13	10·23	10·07	10·15	·250	·203	·226	16	10
Cambridge adults ...	20	(Rivers) 10·35	—	—	(Rivers) ·168	—	—	16?	4?
Lengthening.									
Elementary boys ...	30	9·95	9·91	9·93	·235	·216	·227	25	35
Preparatory boys ...	13	9·80	9·83	9·82	·204	·180	·192	13	13
Cambridge adults ...	20	(Rivers) 10·12	—	—	(Rivers) ·144	—	—	16?	4?

calculations, showing the differences between the Averages and Mean Variations when calculated, not for the ten determinations together, but for the five determinations by the shortening and lengthening procedures respectively, and leave inferences to the reader. I have printed Dr Rivers' own figures as obtained with this test by a similar method upon twenty English adult observers (graduates and undergraduates of Cambridge)[1].

It may be mentioned that there was certainly a difference in the several experimental series as to the nature of the instructions to the reagents and the way in which they were carried out. In Dr Rivers' experiments the observer was apparently asked to shorten or lengthen the variable line "till it appeared to him to be of the same length [as the standard line]. Fine adjustment was allowed when the lines were judged to be approximately equal" (cf. *l.c.* p. 340). The boys on the other hand were told gradually to shorten or lengthen the variable line till they could see no difference. To forbid fine adjustment, however, was with the subjects and apparatus in question, rather a counsel of perfection: and the strictness of the two superintendents, and the strictness at the two schools, was probably not the same. These differences in procedure are probably the key to instructive divergences between the results of Dr Rivers and our own, between the results of Mr Flügel and myself, and between the results of the two schools. It will be noted that the under-estimation of the variable line when lengthened is on the whole not so great as the over-estimation of the variable line when shortened. This seems to indicate that the tendency to over-estimate the variable line, noted by Dr Rivers as operating with both ascending and descending methods, still operated with both methods in our experiments, though partially obscured by the constant error of stopping too soon. "The explanation of this general tendency," says Dr Rivers, "is doubtful." I should venture to suggest that it may be due to the fact that the centre of the board is not always held in the direct line of sight. There is a tendency to hold the variable line—left or right horizontal, according to the nature of the experiment—opposite the eyes; in consequence of this the standard line is seen somewhat foreshortened. Doubtless the larger discrepancies in the difference between the constant errors as obtained by the two observers at the two different schools are due to variations in the strictness with which the boys were made to hold the board exactly in front of the face. This general lack of pre-arranged uniformity in the procedure of the test is probably

[1] *Brit. Journ. Psychol. l.c.* p. 350.

the reason of the occasional low reliability coefficients, rather than any serious unreliability inherent in the nature of the apparatus or test itself. But the reliability at its best is in this test nowhere very high.

Conclusions (Simple Sensory Tests).

On comparing the correlation coefficients, we have the following generalisations with regard to the relations between Sensory Discrimination in various departments, and Intelligence. There appears to be no general connexion between Intelligence and capacity to discriminate Weights; any general connexion between Intelligence and Tactile Discrimination, if it exist, is of the slightest; there is considerable general connexion between Intelligence and Pitch Discrimination; and an undoubted general connexion between Intelligence and Visual Discrimination of lengths, though not to such an extent as in the case of Pitch Discrimination. Owing, I am inclined to suggest, to some participation of intelligence in his subjects' general attitude and procedure, as well as in the function specifically tested, the correlations obtained with school-children by Dr Spearman were larger than those obtained in the present investigation; nevertheless, they follow the same order, namely, Weight, Vision, and Sound[1].

It is to be observed that absence of general connexion between Sensory Discrimination and Intelligence in any particular department of sense is compatible with, and not refuted by, cases in which the general fineness of nervous organization, manifesting itself at higher levels as Intelligence and as superiority in more complex tests, is also shared by lower levels of psycho-physical constitution. Thus the most brilliant of the Preparatory group, a son of a mathematician and scientist of international repute, and therefore presumably inheriting the high order of intelligence his work at school displayed, was third in the Lines test, second in the Skin test, and an exceptional first in both Weight- and Pitch-discrimination; and these performances seemed to the onlooker to be due rather to peculiarity of general sensory organization than to peculiarity of attention or other feature of general mental attitude. The superiority of the congenital imbecile could certainly not be attributed to any such central factor. Accordingly, the correlation found between Intelligence and Pitch Discrimination and—to a less extent—

[1] The correspondence is not due to suggestion, as at the time of our actual experiments I had not noticed the order obtained by Dr Spearman, and before actually calculating the coefficients believed we were finding no correlation throughout the sensory region.

between Intelligence and Visual Discrimination both by Dr Spearman and by the present investigators are probably to be explained not as manifestations of a fundamental identity between Intelligence and General Sensory Discrimination, but rather historically, namely, by the large dependence of the development of intelligence in mammals upon visual acuity, especially in relation to the perception of space, and by the yet larger dependence of the development of intelligence in man upon power of speech, and of this in turn upon auditory acuity.

General Intelligence, then, shows little or no relation to senses which to civilised man are of low cognitive value; but it shows a marked relation to those senses which aid the perception of relations or formation of concepts, and are of high cognitive value.

Motor Tests.

Motor ability has commonly been tested by means of Reaction-Time apparatus—apparatus too costly and elaborate for extensive use at schools and unsuited to testing the easily distracted minds of boys. Among the motor tests not requiring unusual apparatus, those claiming to yield the most successful results are the Tapping test introduced by Binet, and the Card tests introduced by Jastrow.

(5) Tapping.

Apparatus and Procedure.

Binet's test of "Petits Points," which has been adopted also for this purpose by Bagley and Kirkpatrick, consisted of tapping rows of dots with a pen or pencil, thus recording the number of taps executed in a given time. I found, however, an undesirable amount of variation in the way in which the boys used the pen or pencil—some being accustomed to hold them in a way that rendered successful 'tapping' almost impossible, others remedying the tendency of these instruments to leave indistinct marks by moistening the point of the pencil or scratching in the dot with a circular movement of the pen. For the boys whose work is recorded here I therefore substituted a blunt needle mounted on a holder. They were instructed to hold the needle almost vertical, and to prick at maximum speed a line of holes in a sheet of paper laid over a thick cloth. Each was allowed a preliminary practice-row; the number of perforations pierced in 15 secs. was taken as the measure of his rapidity.

Two experimental series were carried out at the Elementary School and one at the Preparatory School.

Results.

The reliability of this test was not high; the correlation between the two series, the operator being the same for each, was ·51.

The correlation of the average rate of Tapping with Intelligence was ·47 (p. e. ·09) at the Elementary School, and the corresponding correlation at the Preparatory School ·41 (p. e. ·16). The average of the raw correlations between the gradings for tapping and for intelligence were ·44 and ·28 respectively; correcting to ·65 and (assuming at the Preparatory School the same reliability coefficient as at the Elementary School) ·41.

The average speed of the Elementary group was 80·5 taps in 15 secs. (M. V. 8; extremes 59 and 97 secs.), of the Preparatory group 95 secs. (M. V. 11; extremes 66 and 119 secs.). The correlation of Tapping with Athletics according to rough gradings by the masters was ·35 at the former school and ·14 at the latter; but its correspondence with athletic constitution, as distinguished from athletic interests or achievements, and estimated roughly by eye and by the information contributed by the masters, seemed much closer, particularly at the Preparatory School.

The reliability coefficient is perhaps lower, and the correlation with intelligence perhaps higher, than might have been anticipated. Subsequent experiments in 'tapping' carried out by means of a rotating cylinder, which carries the padded paper past at a uniform speed so that the only movement the subject has to make is the movement of pricking itself, have yielded distinctly higher reliability coefficients. Apparatus was not devised or used for the experiments upon the school children for tests in which it could be dispensed with. Consequently in the Tapping test the child besides pricking had also to move his arm along the paper, and sometimes back again, *βουστροφηδόν*. In this way there was introduced into the movements the need of more accurate co-ordination. And to this both the slight peculiarities in the results seem attributable.

(6) Dealing.

Apparatus and Procedure.

The apparatus used consisted of playing-cards[1] of the kind made by De La Rue and Co., called 'Pneumatic Cards,' the backs being roughened by grooves, which allow air-spaces between the cards when the pack is held in the hand, and thus facilitate dealing and prevent misdeals. The subject was required to deal 50 cards into five heaps in the ordinary manner, and the time was recorded by a stop-watch.

Three series were made with the larger group, and two with the smaller—Mr Flügel again undertaking one at each school.

Results.

The reliability coefficients of these series were high, namely ·88 and ·80. The coefficients of the amalgamated series when correlated with Intelligence were ·44 (p. e. ·10) and ·29 (p. e. ·18); average raw correlations ·48 and ·05; corrected coefficients ·54 and ·06.

The average time taken to deal the pack of cards was at the Elementary School 31·6 secs. (M. V. 5·9; extremes 20·3 and 46 secs.); the averages of the three sections of the group were,—clever boys 26, average boys 31·6, dull boys 35·6, the time taken by the imbecile boy 46, secs.,—thus confirming the presence of a marked correlation. The average time at the Preparatory School was 27·3 secs., or nearly as fast as the 'clever' section of the Elementary group (M. V. 1·8; extremes 20 and 34 secs.).

On plotting the 'surface of distribution' of the individual figures for the two groups, the curve limiting the surface of distribution for the Elementary group was found to be fairly symmetrical, and both 'median' and 'mode' (most frequently recurring speed) coincided with the average; while the curve for the Preparatory group was found to be decidedly asymmetrical or 'skew,' and the median (26·5 secs.) and mode (26 secs.) both distinctly below the average, there being but one figure still smaller than the mode. A small number such as that of the Preparatory School group naturally tends to evince irregularities and asymmetry in the curves constructed for that group. But a difference in symmetry so marked, together with the difference between the respec-

[1] The method of studying reaction-times by playing-cards was introduced by Jastrow (*Science*, Vol. VIII. p. 237), and has been successfully used by Bergström (*Am. J. Psych.*, v. p. 356). For an application of these two forms cf. also W. G. Smith, this *Journal*, Vol. I. Part 3, p. 244. Their application to testing intelligence is, I believe, new.

tive correlations with Intelligence, seems to indicate the presence of some extraneous factor not appreciably influencing the Elementary boys, but influencing the other group so as almost to antagonise any tendency to correlate with Intelligence. Such a factor naturally suggested itself in previous practice. It was conceivable that in the main only the more intelligent subjects at the Elementary School habitually played cards and dealt for themselves, and that only the less intelligent of the Preparatory boys spent their time in such indoor recreation. Enquiry was accordingly made of the boys as to the amount of their familiarity with cards, and they were graded in rank on the basis of their replies. This rough grading correlates with the experimental results to the extent of ·44 in the Preparatory School, and of only ·28 in the Elementary School. Here then was the cause of the difference in distribution frequency of the observational data, and of the correlational coefficients; its operation would be explained if at the lower class school the brighter boys and at the higher class school the duller boys were the chief regular card-players. There proved, however, to be no reliable positive correlation between Intelligence and familiarity with cards at the Elementary School, nor any negative correlation at the Preparatory School. The difference of incidence therefore operated in some other way than that expected. It transpired that at the Elementary School the card-players played at home "in the winter evenings," "during the holidays," and thus did not play regularly and did not necessarily deal for themselves. At the Preparatory School the more frequent card-players were boarders, who played at school "on most wet afternoons" and necessarily dealt for themselves. Thus at the latter school practice had simply had a far more profound influence, and (if we disregard the possibility of some abnormal disturbance in this test at this school—and of this there were no signs) this profounder practice probably determined the results of the experiment so completely as to overlay and obscure the original connexion between Motor Rapidity and Intelligence.

Conclusions (Simple Motor Tests).

Of the two forms of simple motor test, Tapping seems a more satisfactory method than Dealing, especially as its defects could largely be remedied by improved apparatus, while those of Dealing cannot. Motor tests seem to have a higher correlation with Intelligence than

Sensory tests. But where motor rapidity is due to frequent practice[1], as in the Dealing of the Preparatory group, the correlations with Intelligence and other tests are reduced, abolished, or inverted. Thus so far as motor rapidity is the function of temporary 'facilitation' of the paths of neural discharge it appears also to be a function of intelligence, while so far as it is a function of permanent 'canalisation' of those paths it but slightly or inversely related to intelligence. Facilitation, however, is a function of operative attention; while canalisation, though due to the operation of attention in the past, corresponds with diminution or absence of attention, as the adaptations of the past become the habits of the future. Here, then, the correlation between the tests and Intelligence seems more direct, and more likely to be the outcome of a central factor, than in the case of the sensory tests.

Sensori-motor Tests.

In the two following tests the reaction was complicated by sense-perception, or by recognition.

(7) Card Sorting.

Apparatus and Procedure.

Fifty pneumatic playing-cards, consisting of ten from each of five different packs with backs of five different colours, were arranged in a haphazard order, special care being taken to avoid favourable or repeated sequences. The subject was then required again to deal them out into five heaps against time, now distributing them according to the five colours. The positions and colours of the heaps thus to be formed were indicated for him by five similar cards placed conveniently in a single row on the table. In Sorting and in Dealing the boy was instructed not to correct mistakes in allocation or misdeals; if made, they were counted as so many seconds to be added to his time, according to his average speed per card.

Three experimental series were undertaken at the Elementary School and two at the Preparatory School as before.

[1] The correlations for the two successive series of experiments in Tapping (given in Table VII) are at first sight not consistent with this inference. But the effects of a single attempt at 15 seconds Tapping scarcely constitutes practice in the sense implied above; and the general effect of repetition not only with Dealing, but with most of the other tests, seems to confirm what is there said.

Results.

The reliability coefficients were ·84 and ·38; the correlations with Intelligence ·52 (p. e. ·09) and ·56 (p. e. ·13) for the amalgamated series, ·45 and ·60 raw, and ·53 and 1·63 corrected. A correlation larger than unity is, of course, impossible. The last coefficient, therefore, contains an error of at least ·63,—an error nearly three times as large as its 'probable error,' which works out at ·23[1]. This, however, proves not so much that 'correction' is invalid, as that the formulae are at present sometimes inadequate. The low reliability coefficient and high intelligence correlation which, as compared with those of the Elementary School, characterise the Preparatory, are possibly due to the fact that the relative novelty involved in the process of sorting was for them greater, since the process of dealing was to them more habitual than at the other school. The correlations between Sorting and Dealing (amalgamated series) is at the Elementary School ·72; at the Preparatory School it is only ·02. The correlation between Sorting and previous practice in handling playing-cards proves to be even slightly inverse, namely,—·11 (Elementary School)[2]. Sorting for the habitual dealer doubtless involves not merely the acquisition of the complex tendency to lay a given card upon the heap of similar colour, but also the inhibition of the simpler and older tendency to lay it on the heap next in order after the heap on which the preceding was laid. Hence, among the more practised card-players of the Preparatory boys there was probably room for a greater play of at least one of the functions credited to 'intelligence,' and at the same time for a greater diversity and a greater irregularity of operation on the part of this additional influence. When projecting the entire series of tests it was hoped that it would be feasible to discount by subtraction the element of practice with cards from the results of the experiments in Sorting by means of the results of the experiments in Dealing. But it became evident in the course of the observations and calculations that the processes are involved in a way which is scarcely so simple as to warrant such a procedure; indeed we have already noted that experimental Dealing is no invariable measure of practice in handling cards.

[1] According to the formula for p.e. of corrected coefficients given on p. 111.

The six other corrected coefficients printed on pp. 176, 177 as 1·00, also actually work out to figures considerably more than unity.

[2] I find that Miss Thompson noted a similar fact in her experiments with Jastrow's Card-sorting upon students at Chicago: "Those who made the best records, both men and women, were people who played cards little or not at all." *Mental Traits of Sex*, p. 16.

At the Elementary School the average speed in this test was 48·1 secs. (M. V. 6·3). The fastest was 34 and the slowest 61·6 secs. The average speed for each of the three sections 41, 49·3, and 51·5 secs., and the speed of the imbecile child 87 secs. At the Preparatory School the average speed was 41·3 (M. V. 4·1)—again nearly as fast as that of the brightest section of the Elementary boys. The fastest was 36·5 and the lowest 49·5 secs. Thus whereas in Dealing the fastest speed achieved at the Preparatory School was slightly greater than that attained at the Elementary School, in Sorting it was considerably less; this seems to confirm the supposition that the complication of the afferent part of the reaction by the introduction of the element of Sorting as such entailed a greater interference with the reaction process for the Preparatory boys than for the Elementary boys.

(8) Alphabet Sorting.

Apparatus and Procedure.

This test is believed to be entirely new, and was devised by Mr McDougall. The apparatus required consists simply of a stop-watch, and two complete alphabets of childrens' cardboard letters, such as are sold for the game of 'Word-making and Word-taking.' Each letter of the alphabet is boldly printed upon a single white card about 20 mm. square. The fifty-two cards were laid before the subject upon a table, placed right way up, but arranged in an irregular or 'chance' order in three rough rows. The subject was not allowed to see them until he commenced the experiment; and was instructed to pick out in order, and arrange in sequence in two rows below, one complete alphabet from the two before him. He was directed to work through the alphabet in order continuously, not to pick out letters as his eye fell upon them, and was started by a pre-arranged signal. The task was, as before, to be performed at maximum speed; and the time occupied was recorded by the stop-watch.

Three series were executed at each school, one of each set being as before undertaken by Mr Flügel.

Results.

The average reliability coefficients, ·60 and ·48, are not quite so unequal as for sorting, but still both somewhat low. This is probably due to the opportunity afforded by the nature of the test for slight differences in the operator's method of arranging the letters and carrying out the test, and for the operation of chance generally. Probably for the same reason the several raw correlations of the three successive series are somewhat widely discrepant (cf. p. 168).

The average raw correlations with Intelligence are ·50 and ·61 (correcting to ·68 and ·91), being thus at first sight much the same as those for Sorting. But the correlation relations between the amalgamated or average results show a far more marked improvement upon these than do those for Sorting; they rise to ·61 (p. e. ·08) and ·80 (p. e. ·07). The correlation with Intelligence (Headmaster's estimate) is found, too, to increase on each successive occasion on which the best was performed. Thus on the first occasion of the test (operator B.) the coefficient was ·45; on the second (operator F.) ·70; on the last (B. again) ·74. The separate reliability coefficients similarly improved. There was no such improvement observable in the case of Sorting. It would thus appear that the Alphabet experiment, involving as it does a similar type of reaction, tests much the same capacity as the Sorting experiment; and so far as it does differ from the latter, is on the one hand somewhat more closely connected with Intelligence than Sorting, particularly where repetition is possible to eliminate the element of chance, but on the other hand somewhat less reliable than Sorting, particularly where the differential influence of card-practice does not complicate the element of dealing. The Alphabet test also unfavourably handicaps those who have a difficulty in reading and therefore do not readily recognise letters; and the Sorting test unfavourably handicaps those who are more or less colourblind, and therefore do not readily recognise the colours on the backs of the cards; while both are unfavourable to those who are weak in the arm and often to the left-handed. Specimens of each defect will usually be present in groups as large as the larger of our groups.

The disadvantages of this test are thus not serious; similar disadvantages are inevitable in any test. The defects which seem to have lowered the reliability coefficients could largely be improved upon by devising stricter conditions for future applications. The test is a very quick and simple one, and so could easily be repeated often enough to yield results of considerable reliability, and therefore, even without correction, of very high correlation. Even in our own use of it, the results obtained were quite remarkable: on correction, it furnishes in both schools a higher average correlation with Intelligence and the other tests, without any countervailing error of disastrous size, than any other test at the Elementary School, or any other test except Dotting at the Preparatory School[1].

At the Elementary School the average speed of the group was 91 secs. (M. V. 15·4, extremes 50 and 138 secs.), the three sections averaging 75·6, 90·5, and 103 secs., and the imbecile boy occupying a little over five minutes on one occasion, and six on another. At the Preparatory School the average speed was 74 secs. (M. V. 13, extremes 48·5 and 119 secs.), actually superior to that of the brightest boys of the Elementary School group.

Conclusions (Compound Sensori-Motor Tests).

Depending as they do for their performance upon processes of a more complex nature and a higher mental level, tests combining perception with motor reaction[2] seem to involve Intelligence to a still higher degree than relatively simple sensory or motor tests. Of the two above discussed, the Alphabet seems to be in practice far the more efficient.

At the outset of the entire investigation, the last three tests (the Dealing, Sorting and Alphabet tests) were applied to 63 boys varying from 10 to 16 years of age with a view to determining the nature of the correlations of the tests with Age. They prove to be ·14, ·45 and ·29 respectively, the probable error of a series of this size being about ·07. The peculiar nature of these correlations is probably to be explained by the character of the selective influences that had been at work among the boys at upper and lower ends of the scale of age. From these three samples it was concluded, firstly, that the correlations of our tests with Age would be sufficiently large to introduce an undesirable factor disturbing the estimation of their correlations with Intelligence; and consequently it was determined to eliminate this influence at the beginning of the work by a suitable choice of reagents; secondly, that the correlations were not large enough to vitiate the assumption that such tests as should prove to be correlated to a high degree with imputed Intelligence, such as the Alphabet test, might be regarded to a large (though not perhaps an equal) extent as correlated also with, and therefore tests of, *innate* ability; and, thirdly, that the only way of proving this assumption empirically would be to take the *same*

[1] See Table IX at end (pp. 176, 177), and Tables V and VI (pp. 161, 162).

[2] It perhaps may be doubted whether the Dotting test yet to be described should not also be classed under this head, since in the general nature of the process and in results it may be considered to resemble the Alphabet test.

reagents at a later period of their educational career and test them again with the same tests.

ASSOCIATION TESTS.

(9) Immediate Memory.

Apparatus and Procedure.

The Memory of the children was investigated according to the method employed by Prof. Meumann[1], who investigated the immediate memory of some 800 Zurich school-children by determining by a series of mass-experiments the maximum number of words which each child could reproduce after once hearing them read in class by the class-teacher in groups of 4, 5, 6, 7 and 8 words at a time. Two different sorts of words were used: namely, (1) words of *concrete* significance chosen from the children's circle of ideas, *e.g. Papier, Strasse, Ofen, Feder*, &c.; (2) words of *abstract* significance, less known to the younger children, such as *Menschheit, Gesetz, Masse, Organ, Anziehung*, &c. As a result of his experiments, confirming the casual observations which had suggested them, he concludes that the degree of mental development of the children is determined by their capacity of abstraction, and can be recognised by their retention of abstract words, since such retention is essentially dependent upon their power of understanding the words.

In applying this method to our purposes, the procedure used by Meumann was modified in the following respects. The children were, as always, tested, not by a class experiment, but privately and individually; and the test was conducted, not by the master, but by ourselves personally. The words chosen were all of one syllable, and, in addition to concrete and abstract words, a third series consisting of 'nonsense' syllables was used. The children not only heard the words, but also saw and spoke them—thus in some degree obviating differences of Imagery-type. In presenting the words to be read by the subject, no use was made of the machines usually employed in memory experiments, since the distraction caused by such pieces of apparatus, especially when unfamiliar to the child, more than counterbalances the accuracy gained by mechanically regulating the tempo, &c.[2] The

[1] E. Meumann, "Intelligenzprüfungen an Kindern der Volksschule," *Die Experimentelle Pädagogik*, 1 Band. Heft 1/2, 1905.

[2] This was found to be the case by Messrs W. McDougall and A. M. Hocart in some unpublished investigations on memory undertaken at the same school in 1905 with Müller's rotating cylinder.

words were printed by hand in 1 cm. Roman Capitals upon large cards in five vertical columns of 4, 5, 6, 7 and 8 words respectively, one card containing 30 abstract words, another 30 concrete words, and a third 30 nonsense syllables. In explaining to the boy beforehand the way in which he was to memorize and reproduce the words, he was warned to be careful to observe as far as possible in his reproduction the order in which the words were shown; and before each column he was told the nature and number of the words it would be found to contain. Over the card to be learnt was laid another and larger cardboard sheet, having an oblong aperture near its centre of such a size as to expose one and only one word at a time. By sliding this screen at a favourable rate over the printed card towards the boy, the words of each column were successively revealed through the opening and thus presented one by one to the view of the boy, who read them aloud as he saw them, and simultaneously heard them pronounced by the operator. As soon as a column had been presented in this way, the words contained in it were scribbled down by the boy from memory on paper previously ruled into appropriate columns. In this way 30 monosyllabic concrete nouns, 30 monosyllabic abstract nouns, and 30 meaningless monosyllables were seen, heard and read, 4, 5, 6, 7 or 8 at a time; and, as far as possible, reproduced by the boy in their original order, column by column.

The boys' papers were subsequently marked according to a system based upon Meumann's system of marking: each word correctly reproduced in its correct place counted 4, a correct word misplaced counted 3 if its position was altered by only one place, and 2 if removed by more than one degree; an incorrect word, if either initial consonant sound, or final consonant sound, or medial vowel sound alone was incorrectly altered, counted 3, if in its right place; 2, if one place removed, and so on; if two such components were altered, it counted 2 if rightly placed, and 1 if wrongly placed; the omission of a word, or the substitution of an extraneous word, counted 0[1].

Results.

The following are the figures for the three complete series made at the Elementary School, and for the two complete series made at the Preparatory School, based upon the total marks obtained at each sitting

[1] Meumann's full mark was 3, instead of 4, for each correct word correctly placed. To ensure that the method of marking should throughout be the same for all, the entire series was kindly marked by Mr Flügel.

by each individual for 'Concrete,' 'Abstract,' and 'Nonsense' Memory taken together:

TABLE IV. *Memory.*

	Elementary School	Preparatory School
Reliability coefficients	·70	·93
Correlation of amalgamated series:		
with Headmaster's order for intelligence	·57	·78
with Examination order (literary subjects)	·67	·82
(mathematical subjects)		·69
Average correlation of unamalgamated series with the various		
gradings for intelligence (Headmaster's)	·53	·80
(Assistant Masters')	·43	—
(Boys')	·46	·72
(Average)	·48	·76
Corrected correlation	·60	·82
Average no. of marks gained by boy per sitting	134·2	216
Mean Variation for group	27·7	29
Extremes (highest boy's average)	223·3	265·5
(lowest boy's average)	68·6	153·5
Average for the three sections: 'clever' boys	167	—
'average' boys	135	—
boys 'below normal'	134·2	—

From these figures it is clear, in the first place, that Immediate Memory is correlated to a considerable, but not a high, degree with Intelligence as estimated by the Headmaster's grading; to a slightly lower degree with Intelligence estimated by the general impression of others than the Headmaster; and to a significantly higher degree with Intelligence as estimated by the results of examination—particularly of the examination in 'literary' subjects (Classics, &c.). The correlation between the Headmaster's estimate of Intelligence and the examination order (on which, it is to be remembered, the Headmaster's estimate was with subsequent modification originally based) was, as we have seen, ·81 at the Elementary School, and ·78 (actually the same as the correlation with Memory) at the Preparatory School. The correlation between all other tests of intelligence and the results of the examination is much lower. Thus we seem to have scientific proof of what on *a priori* grounds has commonly been surmised, namely, that the present examination system tends to test mainly that aspect of intelligence which manifests itself in memory, to the neglect of other manifestations of intelligence, and to the inclusion of other factors of memory which distort even this manifestation of intelligence. There is clear evidence also that at both schools the Headmaster's estimate is also biassed towards memory, since the correlations of the Memory test with the other provisional estimates of Intelligence are much lower.

This conclusion is corroborated when we attempt to trace the divergences between the figures for the two schools to their probable cause. The Elementary School which supplied our subjects, in common with many other elementary schools, had followed the reaction against excess of rote-work in its instruction to a degree which, in the opinion of the Headmaster, was even extreme; in its examinations, too, it aimed rather at setting problems, directly involving intelligence for their solution, than at demanding facts or formulae, primarily involving memory for their reproduction and intelligence only indirectly; and these examinations thus correlate more closely with the Headmaster's order and less closely with Memory than at the other school. A preparatory school, on the other hand, preparing boys for scholarship examinations at the great public schools, necessarily trains and disciplines to a high degree the memories of its scholars, particularly of its more intelligent scholars. This superior power of memorization on the part of the Preparatory School boys unmistakeably betrays itself in our experimental investigation. In no other test do the observational figures show such a complete superiority to those of the Elementary boys; the marks obtained even by the weakest boy in the former group was not much below the average marks obtained by the seven 'cleverest' boys of the larger group. The peculiarly high reliability coefficient measures the remarkable accuracy and steadiness of their work, which was also conspicuous during the actual experiment. For while the Elementary boys could not conceal their dislike for the Memory test, the Preparatory boys were here evidently most at home. In these boys—preparing as they were for examinations—susceptibility to the irrelevant factors liable to hinder subjects applying themselves to the assimilation and reproduction of a maximum amount of material in a limited amount of time had probably been largely reduced by previous discipline. Hence the results of their work would naturally correspond more closely with their Intelligence—particularly if, as seems possible, the efficient factor in Memory and Intelligence alike be attention manifesting itself in different guises. The fact that in this test attention appears to have been more efficient among the Preparatory boys than among the Elementary boys, seems to have made their reproduction of the associations between the words (as opposed to their reproduction of primary memory images of the words themselves) more accurate[1]. The masters who drew up the grading for Intelligence at this school may have also been specially biassed in favour of an accurate and capacious memory as a symptom of Intelligence.

The coefficients of correlation between 'Concrete' Memory, 'Abstract' Memory, and 'Nonsense' Memory, taken separately with Intelligence, are, when the results of the successive sittings are amalgamated, ·58, ·48, ·43 respectively at the Elementary School, and ·84, ·78, ·75 respectively at the Preparatory School; thus the memory for abstract words does not show a higher, but a lower correlation, with Intelligence. The average marks were 60·7 (M. V. 11·4), 49·1 (M. V. 10·5), 25·2 (M. V. 7·5) respectively at the Elementary School, and 87·5 (M. V. 9), 76 (M. V. 11) and 52 (M. V. 12) at the Preparatory School; and thus the introduction of difficult vocables, whether abstract nouns or meaningless syllables, proves in both groups to be on the whole a distracting element. Indeed the only three cases where Abstract Memory gained better marks than Concrete Memory were those of boys, who so far from being boys of superior Intelligence, were placed 15th, 26th and 29th in the Headmaster's order. Hence so far as concerns children of the age and station examined by us, Meumann's claim that superiority of 'abstract' memory to 'concrete' memory is a strong mark of Intelligence is not confirmed[2].

(10) Mirror Test.

Apparatus and Procedure.

The 'Mirror' test is a new[3] test intended to measure the child's adaptability, i.e. his power speedily to acquire new co-ordinations of

[1] This point is of importance in considering the relations of the Memory test to the Sensory tests; fuller discussion of it is therefore postponed till the section dealing with the interrelations of all the tests (p. 167).

[2] In the children of the Volksschule investigated by Meumann, up to the age of 12 the actual errors made in reproducing the abstract words were in general greater than in reproducing the concrete words; with children of 14 and 15 years of age the relation reversed itself. *Exp. Päd. l.c.* p. 70.

[3] The suggestion of writing before a mirror is due to Mr Keatinge. For the present imperfect form of the test the writer is responsible. A similar experiment for a different purpose has since been published in Judd's *Laboratory Manual of Psychology*, Vol. II. p. 49. A prolonged investigation into adaptation to mirror-vision is described by G. M. Stratton, "The Spatial Harmony of Touch and Sight," *Mind*, Vol. VIII. p. 492, 1899. Mirror-drawing sometimes appears as a parlour pastime; and this may occasionally vitiate the test, as in the case of several of our Preparatory boys. One of the Elementary boys had also served as subject in a series of apparently analogous experiments carried out a couple

movement appropriate to circumstances relatively novel. The reagent traces over a geometrical pattern a number of times successively, both the pattern and his hand being seen by him, not directly, but in a mirror. The visual and motor factors at work in such a task involve in themselves nothing specially new or highly complex; but the relation between them is disturbed, and must be readjusted. The gradual improvement in the process of readjustment is indicated by a progressive reduction of the time required to make one complete tracing.

The apparatus employed was of the following nature. A sheet of paper was laid upon a thick cloth covering a firm table, and upon this paper was pinned a sheet of stout millboard pierced with eight holes arranged in the form of an octagon or circle 20 cm. in diameter, with a ninth hole in the centre. On piercing with a blunt mounted needle or 'seeker' through the holes in the card and through the paper beneath, a pattern will be left upon the paper similar to that upon the card. In this experiment, as in 'Tapping,' pricking holes with a seeker has several advantages over marking dots with a pen or pencil: it avoids accidents with the nib or pencil-point; it ensures that the method of holding and using the instrument shall be practically the same with all subjects; it leaves a comparatively infallible record of the subject's performance, and by simply renewing the paper a separate record for each subject can be obtained. Behind the pattern to be traced and facing the subject is fixed a mirror, not quite upright, but tilted towards the subject at an angle of 85° to the horizontal card; and the pattern is concealed from the subject's direct view by a horizontal screen, supported on a frame, underneath which his hand can freely move. Thus the holes, the needle, and the subject's hand can only be seen by reflection in the mirror. When about to commence the experiment, the subject holds the mounted needle in his right hand underneath the screen, and the point is inserted for him in the hole at the centre of the pattern. He is instructed to start at a given signal by piercing through this hole; then to find a certain hole on the circumference (marked with green), to pierce this; then to go to the hole next it (marked with red), and to pierce this; and so on in the same direction round the circumference of the circle till he reaches the green hole, when he is to return from this to the centre again. The time occupied in so doing is recorded by a stop-watch.

of years before at St Barnabas School, Oxford. According to his account, the test then consisted of pricking out letters of the alphabet as fast as possible; it was conducted by a stranger, whose identity and design I have been unable to discover.

At the same sitting, but with intervals of a few seconds, this operation is repeated till it has been achieved six distinct times. Such a six-fold series was obtained at both schools. Twelve weeks after the six-fold series, a second series of sittings was obtained at the Elementary School; in this series each boy's sitting included only two consecutive attempts.

Results.

In the six-fold series, six figures, recording the respective durations of the six successive tracings, were by the above procedure obtained for each individual. These figures provided the data from which to extract some comparable measure of his relative improvability so far as revealed in rapidity of adaptation during the entire sitting. A simple and satisfactory method of mathematically treating these raw figures, in order to obtain some such measure, is difficult to discover. Various procedures suggested themselves: to subtract the last and lowest figure from the first and highest; to subtract the average of the last three from the average of the first three; to express either of these remainders as a fraction of the original speed, or of the total time of the sitting; to reduce all sittings to terms of a common original speed, and calculate the coefficient of their curves; or simply to take the sum or average of the six experimental figures. Of these, the last method gives the highest correlation with Intelligence; while the others are in practice not so satisfactory, and are all more or less open to obvious objections on *a priori* grounds.

The correlation at the Elementary School between the total results of the first series of sittings and those of the second was ·52. From its very nature the Mirror experiment cannot strictly be held to test quite the same capacity on repetition. The observed figures indicate considerable retention by the subjects of the effects of the first sitting; and doubtless the degree of retention is not the same for all. Indeed the second series was undertaken partly with the hope that it might furnish a test of the retentiveness of improvability rather than of improvability itself. No method, however, presented itself of isolating this second capacity, and of differentiating the subjects accordingly. In any case the difference of retentiveness does not seem to have been great. And of the various substitutes for a reliability coefficient that one might devise, the figure cited above probably gives the best approximation for these particular experiments. In lieu of a better it is therefore employed at both schools for purposes of correction.

The correlation of results of the six-fold series with the Headmaster's estimate of Intelligence was ·67 (p. e. ·07) at the Elementary School, and ·54 (p. e. ·14) at the Preparatory School. The correlation of the two-fold series was ·22 (p. e. ·13). The average of the raw correlations with the various empirical estimates of Intelligence was ·50, and ·47, correcting to ·74, and ·68 respectively.

The average total time for the six-fold series was 388·9 secs. for the Elementary boys (M. V. 90·1; extremes 181 and 747 secs.; averages of the three sections 347, 383·7, and 429·8; time occupied by the imbecile boy 2464 secs.). The average for the Preparatory boys was 257 secs. (M. V. ·54; extremes 156 and 526 secs.). The divergence between the two schools is largely due to the fact that 4 out of the 13 Preparatory boys had had previous practice at an analogous task in the form of a not very common parlour pastime. Only one of the 30 Elementary boys had done any similar exercise before. The divergence might also be in part attributed to a greater familiarity with the use of the mirror among boys of the higher classes as compared with boys of a lower status. A similar factor apparently operates when the test is applied to children of the opposite sex, though subsequent applications to very young children, and to adults, have led me to wonder whether here we are not dealing with one of the uninvestigated innate differences between the two sexes[1]. The unexpected degree to which the effects of practice may persist was demonstrated in the later double series at the Elementary School. The average speed in this group was 103 secs. for the first tracing, 39·6 secs. for the sixth[2]; 34·5 secs. for the seventh tracing (i.e. after 12 weeks interval), and 27·4 for the last. The individual boys resumed the task with a speed in no case much slower, and in the case of 16 boys out of 26 actually faster, than that with which they had left off three months before. This furnishes a striking experimental parallel to the dictum quoted by Prof. James: that our brain learns to swim during the winter and to skate during the summer[3]. If the four practised boys are omitted from the Preparatory results the average speed is still somewhat greater, namely 291·8 secs., and the correlation with intelligence rises distinctly, namely to ·59 (p. e. ·15).

[1] Miss Smith, of Cherwell Hall, Oxford, who has since applied this test to women students and school girls, has also found a higher speed.

[2] The figures are the same for the 30 boys as for the 26 who alone sat for a 7th and 8th attempt, if decimals are disregarded. The corresponding figures for the Preparatory School are 63·8 (first tracing), 32·9 (sixth tracing).

[3] James, *Principles of Psychology*, Vol. I. p. 110.

Another defect in the form of the test adopted for these experiments revealed itself on plotting curves to represent graphically the course of the improvement in the various individuals; the character of these curves seems to indicate that the measurements on which they were based in some cases really measured a development due to at least two independent factors. Introspection suggests that besides the process of building up a new system of acquired co-ordinations between eye-movements and hand-movements by trial and error under the guidance of attention, there may also come into play simple kinaesthetic memory, i.e. the process of forming a disposition, purely motor, to move the hand over the pattern to be traced independently of visual guidance and of attention to the objects reflected in the mirror. In some subjects (not perhaps in children as young as those concerned in the present investigation) the movements may be actually directed by reflective inference as to the nature of the properties of reflection by plane mirrors, based either on previous knowledge or on observation during the course of the actual experiment.

Many of these defects might be remedied by further improvements in the form of the test, such for instance as the adoption of more irregular patterns to trace, and of different patterns of equal difficulty for the practice-attempts intervening between the first and last of a given sitting, and of a more adequate mathematical method of treating the experimental figures so yielded. It seems at any rate clear that in this test, apparently new, we have one both capable and worthy of further improvement and use[1].

[1] With a fresh set of 30 boys from the same school and also 12½ to 13½ years old at the time of experimentation, I have recently tried the following method of using the Mirror. The patterns contain 24 holes arranged in a circle 15 cm. in diameter. The holes are connected by straight lines forming chords of the circle, drawn two from each hole at irregular angles. Lines parallel to each other or to the edges of the card are avoided. The lines thus form an irregular zigzag route within the circle, returning on itself, and touching each hole upon the circumference once and once only. In pricking out the pattern the boy goes from hole to hole, not in order round the circle, but backwards and forwards across, according to the route thus traced by the lines. Each boy starts by pricking for the same period of time, namely two minutes; and the operator notes the number of holes pricked in this time. The subject then practises upon another similar pattern for five minutes. He then returns to the first pattern, pricks the same number of holes as on the first occasion, and his performance is now timed. The amount of improvement is measured by subtracting the time of this final performance from the time of the first, viz. from 120″; his retentiveness may be similarly measured. (Every boy to prick the same number of dots as the rest for his first attempt, and, for his final attempt, to prick for the same length of time as this given number originally took him, the additional number of dots then pricked to measure his improvement, would

(11) Spot Pattern.

Apparatus and Procedure.

The 'Spot Pattern' test was devised for the present purpose as a test of scope of apprehension by Mr McDougall; and is now being used by Dr Schuster as a mental test in the Anthropometric Laboratory at Oxford.

The chief piece of apparatus employed in this test is a Portable Tachistoscope, also devised by Mr McDougall. This consists of a vertical stand of wood attached to a horizontal base by a hinge,—which allows it to fold forward upon the base when not in use,—and by a detachable spring,—which keeps it upright when hooked to it at the back. In the upright stand is cut a circular aperture, about 7 cm. in diameter; and at the back of this is screwed a self-setting photographic time-shutter, made by Bausch and Lomb[1]; while the front—for ordinary tachistoscopic experiments—is covered with a semi-transparent card, sliding in grooves fixed on the face of the stand, and bearing printed on its hinder surface the object to be shown by means of the tachistoscope. The card is illuminated for a small fraction of a second by transmitted light from a lamp placed behind the stand, the duration of the exposure being regulated by the shutter.

In the Spot Pattern test the cards used were of a different nature. The objects to be shown consisted of patterns made by piercing 7, 8 or 9 holes in opaque squares of millboard; each card was covered with a sheet of white paper, pasted on the surface presented to the subject, so that the patterns could only be seen when light from behind was thrown upon them through the shutter. The nature of his task was first explained to the subject by reading to him a written form of instructions accompanied by a brief demonstration; and the test pattern was then shown to him in a dark room by means of the tachistoscope. The length of exposure chosen was $\frac{1}{25}$th sec. After seeing it five times at intervals of about $1\frac{1}{2}$ secs. the subject attempts to reproduce a copy of the pattern by mapping it on prepared sectional paper (see figure 1).

also embody the same principle, though the differentiation thus obtained is not quite so minute.) In this way, many of the defects mentioned in the text seem largely evaded; and the correlations with Intelligence prove to be slightly higher,—viz. improvability, ·73, retentiveness, ·48.

[1] A cheaper shutter is used by Dr Schuster, viz. a No. 6 Packard-Ideal Shutter with a $2\frac{1}{2} \times 4\frac{3}{4}$ in. aperture, set for instantaneous exposures.

The sectional paper is divided into large squares about the size of that figured below, each containing 36 (6×6) small squares; and the pattern to be copied is so pricked in the card that when reproduced the whole design will fall completely within the area of a single large square, while each of the spots of which it is composed will fall at the corner of a small square, i.e. at a point of intersection of two sectional lines. The advantage of this arrangement is that each of the spots in the boy's reproduction is definitely either right or wrong, and his performance is rendered susceptible of numerical evaluation. If the subject fails to reproduce the pattern correctly at the first attempt, he is again shown it five times as before, and then makes a second map in another large

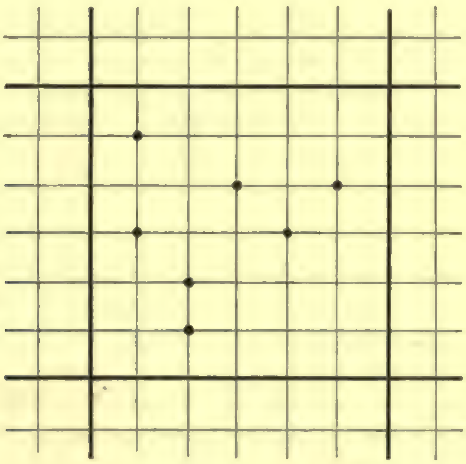


FIG. 1. Map of Spot Pattern as reproduced by subject.

square. This is repeated till the pattern is correctly reproduced. The total number of flashes or exposures required before he succeeds is taken as an inverse measure of the subjects' ability in this test, as shown by his proficiency with a pattern of stated difficulty, the difficulty of the pattern increasing with the number of spots, and with the irregularity of their arrangement[1].

Results.

The tachistoscope was found to require a larger amount of experience on the part of both subjects and operators than any of the other tests,

[1] For a brief account of the psychological nature of the process involved in such an experiment, cf. W. McDougall, *Physiological Psychology*, pp. 129, 130.

except perhaps those involving sensory discrimination. Six series were actually carried out at the Elementary School, four with patterns of seven spots, one with a pattern of eight, and one with a pattern of nine. The first series of all had to be rejected as worthless, owing partly to the irrelevant excitement aroused in the subjects by the 'electric flash' as the boys named it, and partly to the intervention of the holidays. The experiments with eight or nine spots were too difficult for many of the Elementary School boys, and as their inclusion slightly decreases the reliability coefficients and correlations they too have been omitted. There remain therefore three series, conducted successively by the writer, by Mr Flügel, and by the writer again; the reliability coefficient given by these is ·55. Three series were carried out at the Preparatory School, Mr Flügel using a card of seven spots, and the writer cards of seven and eight spots. At this school we were not able to obtain the complete darkness and silence procured at the other in our extemporised dark-room, and consequently the reliability coefficient and the raw correlations with Intelligence are not so high. The reliability coefficient here was ·50.

The element of unreliability in the separate series with this test seems very largely eliminated in the average of about three series; after this further experiments leave the averages much as before. The correlations of such amalgamated series with Intelligence, viz. ·76 and ·75 (p. e.'s ·05 and ·09) are fairly high and nearly the same at both schools. The average raw correlation at the Elementary School is, however, considerably higher than the average raw correlation at the Preparatory School; they measure ·64 and ·44 respectively; correcting to 1·00 and ·66 respectively.

The average number of exposures required to copy a pattern correctly by the boys of the Elementary group was 45·3 (M. V. 21·1; extremes 10 and 157; three sections, 19·5, 41, 71·6 respectively; weak-minded boy, 155 on one occasion, 220 on a second). In the Preparatory group the average was 38·6, where the series was made with an 8-spot pattern, since the 7-spot cards were found scarcely difficult enough to differentiate the better boys of this group (the M. V. being 12·5; the extremes 11 and 86·6); calculated on the basis of the series with 7-spot cards only, the group average for these boys is still better, namely, 30.

(12) Dotting Apparatus.

Apparatus and Procedure.

The apparatus here termed the 'Dotting Apparatus' is a machine for testing and graphically recording continued maximal voluntary concentration of attention. The method was devised by Mr McDougall; and an improved form of his apparatus has been suggested by Dr Rivers, and constructed by the Cambridge Scientific Instrument Company[1]. In an experiment conducted by means of this machine the task of the reagent is to mark with a pencil or stylographic pen[2] an irregular zigzag row of dots, lithographed in red upon a paper tape, carried past the field of view at an adjustable speed by a small wooden drum rotated by clockwork. The clockwork is driven by a heavy weight (some 9 kilos) pulling on a cord wound round a metal cylinder, which transmits, by means of a system of cog-wheels, a slow motion to the wooden drum and a faster motion to a specially designed friction-governor. The metal cylinder, cog-wheels and drum are covered by a low desk, which together with the weight is supported by a small table. In the lid of the desk is an oblong aperture or window, 5 × 10 cm., through which the dots are visible as the paper tape is carried past it by the wooden drum immediately beneath. The friction-governor is fixed at the side of the desk, accessible to the operator. The friction of a pair of revolving blunt brass points, pressed by the centrifugal force of a pair of weights against the rim of a metal disc around which they revolve, retards the motion of the machine. The amount of this friction is increased by increase in the rate of the motion of the machine (since this increases the centrifugal tendency of the weights), and thus automatically keeps the speed of the machine constant; the amount of friction is also regulated partly by the mass of the pair of weights, which are changeable at will and which thus provide a coarse adjustment of the speed; and

[1] The earlier form of the same apparatus is described in the *British Journal of Psychology*, Vol. II. No. 4, p. 435, W. McDougall, "On a New Method for the Study of Concurrent Mental Operations and of Mental Fatigue (Preliminary Communication)." A description of Dr Rivers' improvements will be found in his book on *The Influence of Alcohol and other Drugs on Fatigue*, Appendix II, p. 125.

[2] I venture to suggest that work with this apparatus would be yet more accurate if, instead of a pencil or stylo, a blunt mounted needle were used, and if the surface of the wheel or drum carrying the paper tape, instead of being hard and smooth, were covered with cloth or felt, to allow the needle to prick the paper. The considerations which led me to adopt the needle for the Tapping and Mirror tests (cf. pp. 132, 146) seem on consideration to apply with even greater force to the Dotting test.

partly by the pressure of a spring counteracting the pressure of the weights and regulated by a screw with a graded head, which thus provides a fine adjustment. By means of the fine adjustment the speed of the machine can gradually be changed by the operator, without stopping either the motion of the machine or the work of the reagent, from a speed of about 70 dots per minute to a speed of about 109 dots per minute with medium weights; with heavier or lighter weights similar ranges of speed can be obtained below or above these limits. The row of dots upon the paper band (see figure 2) is carefully designed so that the succession shall be as irregular as possible, the horizontal distance, however, of each dot from the last (i.e. the interval in the direction of motion) being always 5 mm., the extreme lateral deviation of the dots being 15 mm., and no dot deviating by more than 7 mm. from the line of its predecessor.

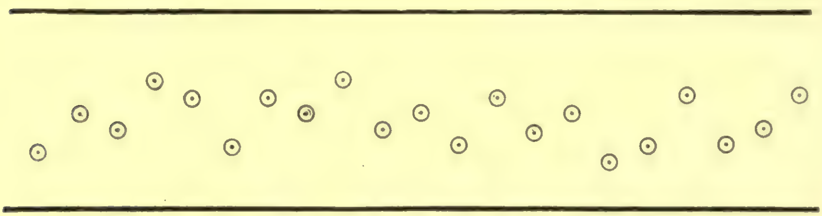


FIG. 2. Portion of Tape to be marked in Dotting test.

Sitting at the table, resting his wrist upon the desk, the reagent watches and marks the dots as they appear through the window, and are carried past towards his left. Each act of dotting constitutes a discrimination reaction, and a spell of dotting constitutes a series of such reactions performed at full, or nearly full, speed. Throughout the marking of a series moving at a given rate the subject's task is one of uniform difficulty, and the difficulty of the task depends upon the velocity at which the tape of paper moves. As he has the same fraction of a second for the accomplishment of each hit, he has to make a rhythmical series of strokes with the pencil; but as the position of each dot is unknown till it is seen, each stroke has to be aimed. This requires a sustained effort of attention, the degree of effort depending upon the rate of the rhythm of the strokes, and therefore measured by the rate of movement of the dots upon the paper tape. When marked, the paper furnishes a permanent graphic record of the maintenance of the effort, failure of continuity of attention being indicated by the presence of pencil-marks unaimed, or of red dots unmarked.

In the present investigation the measurement sought was the maximum rate at which each boy could mark the dots correctly. He was first given half a minute's practice, and then, after a short rest, commenced marking at the rate of about 80 dots per minute; this was continued for 10 secs., and, if the boy was fairly successful, the rate was increased to about 86 dots per minute; after another 10 secs. it was again increased by a similar increment, and so on till the boy completely failed to aim his strokes. His record was subsequently examined, and the speed at which he first made three or more omissions in 10 secs. (or their equivalent according to the system of marking unaimed hits, badly aimed hits, double hits, &c., suggested by Mr McDougall[1]) was accepted as the upper limit of his power of sustained attention. Where a boy went on for 50 secs., i.e. till he reached a speed of 110 dots per minute, he was given a second attempt beginning at a faster rate, as a higher range of speed is only attainable by stopping the machine, and altering the revolving weights of the friction-governor. This also evaded the highly fatiguing effects of a prolonged sitting.

Results.

It was not possible to obtain the Preparatory boys at the Laboratory (where this particular test was carried out) on more than one occasion, but that occasion was the same for every boy, viz. after morning school. Hence no reliability coefficient was obtainable for the Preparatory School, though the reliability was probably high. Each of the Elementary children was able to come twice. The reliability coefficient for the two series thus obtained for them was ·86. Part of the manipulation of the apparatus while the subject is actually at work devolves, it will be remembered, upon the superintendent of the experiment; so that the personal equation is in this test likely to be appreciable, though perhaps small; accordingly, as the series were all superintended by the same operator, the reliability coefficient is probably slightly too high. The Elementary group, partly owing to its size, was not able to work through the test at the same period of the day or week. The differences in fatigue this entailed were naturally slightly unfavourable to accurate work with an apparatus originally devised to test mental fatigue. Further, the right arms of three of the boys were either paralysed, recently broken, or recently dislocated. In consequence, the correlations with Intelligence are somewhat lower at the Elementary

[1] *Brit. Journ. of Psych. l.c.* p. 439.

School than at the Preparatory,—being ·60 at the one, and ·84 at the other, when calculated between the amalgamated series and the Head-master's estimate; ·65 and ·84, when the raw correlations with the various estimates of Intelligence are averaged; and ·75 and ·96, when corrected by means of the Elementary School coefficient. In contrast with most of the other tests, little seems to be gained in this test by a repetition of the experiments.

The average speed[1] of the Elementary group was 12 (M. V. 1½; extremes 17½ and 10¼; average of the three sections 14½, 13, 11¾; speed of the imbecile 8½). The average speed of the Preparatory group was faster than that of the brightest section of Elementary boys, viz. 15 (M. V. 2; extremes 21 and 10).

As these figures suggest, the method of computing achievements at this test does not permit such thorough differentiation between every subject as do some of the other tests. It yields the largest portion of ties. This has necessarily lowered its coefficient of correlation with intelligence. Nevertheless, although the classification which this test can furnish is not minutely discriminative, yet its general character follows the order furnished by the masters with remarkably few contradictions. Since the average raw correlations for the separate series are as good as the correlations when these series are amalgamated into one, it is evident that the results of a single application of it may be relied upon without corroborative repetition.

Conclusions (Association and Attention Tests).

According to the corrected coefficients, these last four tests—Memory, Mirror, Spots and Dotting—have pure correlations with intelligence, on the whole, the highest of all. The Sensori-Motor tests, it is true, are close competitors with the Mirror and Memory; but the reliability coefficients of the Preparatory series, where the corrected correlations are so high as to raise the averages of the Sensori-Motor tests above those of the Mirror and Memory, are both below ·50. In general purpose, as well as in results, these last four tests seem to fall together. The Dotting test was devised specifically to test Attention. The three other tests have been classed as tests of Association. Each of them for

[1] The speed is given in terms of units of the machine, i.e. in terms of the scale upon the fine adjustment wheel with medium weights on the friction-governor (where the light or heavy weights were actually used, the necessary conversion of the observed readings has been made to keep the figures comparable). A speed of 12 represents a speed of about 90 dots a minute; 15 represents a speed of about 110 dots a minute.

most individuals must involve, as an integral part of the process whereby they are carried out, the building up, under the guidance of attention, of new associations between one set of stimuli and another, between one set of movements and another, or between stimuli and movements. Hence, in these tests also, Attention is a particularly essential factor. The fact that the results of the Sensori-Motor tests approximate so closely to the results of these last four Tests is readily explicable if the general account already given of the former be taken in conjunction with the general account here given of the latter.

GENERAL CONCLUSIONS.

THE DIAGNOSIS OF INTELLIGENCE.

We may now bring these results and inferences to bear upon the three main problems, with which, as noted at the commencement of the article, the experimental investigation of General Intelligence has to deal.

For practical deductions, the coefficients of the amalgamated series (and perhaps the average crude correlations) are of more significance than their theoretical corrections. Of the twelve tests, six furnish coefficients below ·50 and six furnish coefficients above ·50. The former six—the simple sensory and motor tests—are thus of little use in the empirical diagnosis of intelligence. Among the latter six, no single test, at any rate in its present form, can be claimed as a self-sufficient instrument for measuring and detecting ability in individuals. But they indicate the direction in which such a test may hopefully be sought. Particularly promising are the four new tests. Of these Mr McDougall's 'Dotting Machine' seems to be the most scientific. Where the external conditions of the experiment could be kept most uniform, namely with the small Preparatory group, both the amalgamated and the average raw coefficients reached ·84. Such uniformity is difficult in more extensive work, and the ensuing variety in attention and fatigue affect the performances with this test. Moreover, its figures are less discriminative than either of the other three. By increasing the number of spots in the patterns, the Tachistoscope test may be made to differentiate with almost any degree of minuteness. The apparatus, too, is portable. It is a slow test, however, and without repetition scarcely reliable. And it calls for some experience both on the part of the boys to grasp the nature of the task, and on the part of the experimenter to manipulate the apparatus with regularity. These difficulties might perhaps be obviated by improving the instructions to be given to the boy, and the design of the cards shown, so as to make the technique simpler. Unlike the Dotting Machine and the Tachistoscope tests, the apparatus for the Mirror test can be procured with but little trouble or expense, and needs no trained superintendent. It too, however, requires further improvements, especially in procedure and calculations, to eliminate the influence of possible previous practice, and to elicit more completely the significance of the figures observed. If called upon to recommend a simple test for immediate use on untrained subjects, I should be inclined to advocate the Alphabet test as perhaps the simplest and most satisfactory of all.

Hitherto, we have considered each test in isolation. Let us now observe the effect of amalgamating the results of the best. On making a grand average of the six gradings from those that give coefficients above ·50 and arranging the boys accordingly, we obtain a list correlating with the Headmaster's order to the extent of ·85 at the Elementary School and ·91 at the Preparatory.

The lower correlation at the Elementary School is mainly due to five large discrepancies, of 5 to 8 places, between the amalgamated test order and the Headmaster's order. One of these is due to error in the test results, and is the case of a boy slightly incapacitated for them by a semi-paralysed right arm. One probably is due to overestimation by the master of a boy who, though in other tests inefficient, appeared to possess a remarkably good memory. The other discrepancies may possibly be due to a contrast effect which seems to have operated in the master's attempt to dovetail boys of different standards together; boys at the top of a lower standard have, I fancy, been placed too far above boys at the bottom of a higher standard. Most of these discrepancies in the Headmaster's list are corrected in the boy's and assistant master's lists (themselves not so reliable as the Headmaster's) in the same direction as the results of the tests. At one of the schools, the comparison of the final test results with those of the Headmaster drew his attention to a reticent boy who had apparently been allowed to take a lower position in the school than, according to the results of the tests, he appeared to deserve. This interesting point I owe to the kindness of the Headmaster himself, who subsequently told me that the boy was in consequence tentatively promoted and that this promotion was afterwards remarkably justified.

By means, then, of some half-dozen tests, we are able independently to arrange a group of boys in an order of intelligence, which shall be decidedly more accurate than the order given by scholastic examinations, and probably more accurate than the order given by the master, based on personal intercourse during two or three years, and formulated with unusual labour, conscientiousness and care.

THE ANALYSIS OF INTELLIGENCE.

The results of the investigation, then, are so far positive; the empirical measurement of individual intelligence appears to be at least feasible. Towards the explanatory analysis of the general nature of intelligence their contribution may seem more indefinite.

Two questions are involved: Is any single explanation possible? if so, what is it? The only previous answers based on methods similar to the present are those of Dr Spearman's. His main conclusions were as follows: "Whenever branches of intellectual activity are at all dissimilar, then their correlations with one another appear wholly due to their being all variously saturated with some common fundamental Function (or group of Functions)"..."All examination in the different sensory, school, or other specific faculties may be considered as so many independently obtained estimates of the one great common Intellective Function." Hence "there exists a something we may provisionally term 'General Sensory Discrimination,' and similarly a 'General Intelligence,' and...the functional correspondence between these two is not appreciably less than absolute[1]."

The first of Dr Spearman's propositions, the "Theorem of the Universal Unity of Intellective Function" is tested by a corollary logically issuing from it, called that of the "Hierarchy of the Specific Intelligences[2]." Its principle may be most briefly expressed as follows:

$$\frac{r(A,P)}{r(B,P)}=\frac{r(A,Q)}{r(B,Q)},$$

where A, B, P, Q represent any four capacities not obviously akin[3].

Suppose, for instance, performance A correlates twice as highly as performance B with performance P; by hypothesis, this is because some

[1] *Loc. cit. supra*, pp. 72, 73. *Amer. J. Psychol.*, see however Dr Spearman's comment quoted below, p. 165, note 1.

[2] *Amer. J. Psychol. loc. cit.*

[3] This equation is immediately deducible from equation (f), *Zeitschr. f. Psychol. loc. cit. supra*, Vol. XLIV. p. 85.

central performance X, say discrimination, plays twice as great a *rôle* in A as in B. Then, for precisely the same reason, the correlation of A with a further performance Q will be twice as great as that of B with Q. Take now a series of functions, let us say reactions of varying complication, such as Dotting, Alphabet-sorting, Card-sorting, Card-dealing, Tapping, etc.; let us for illustration assume that these are specific manifestations of one common process, say Motor Co-ordination, more or less essential to all of them, and therefore connected with them in various degrees; then, if Dotting correlate with Card-sorting, Dealing, etc. in progressively diminishing degrees in that order, any other function of the same series, such as Alphabet-sorting, will also correlate with Card-sorting, Dealing, etc. in progressively diminishing degree in the same order; and similarly, if the correlation of Card-sorting with Dotting be higher than that of Card-sorting with Alphabet-sorting, then the correlation of Dealing with Dotting will also be higher than that of Dealing with Alphabet-sorting; so of Tapping, and similarly throughout the series in either direction. The system of correlations between each possible pair in such a series is called 'a hierarchy.'

The correlations between the various modes of investigating Intelligence described in these pages have been arranged as a hierarchy in Tables V and VI. The correlations are given in terms of the coefficients for the amalgamated series obtained at the two schools, since these are least open to criticism; analogous results hold good for average raw, corrected, and average corrected coefficients; hierarchies for these may readily be compiled from the complete list of figures at the end (p. 176). It will be found, however, that they are not so perfect. Dr Spearman and Prof. Krueger imply that satisfactory hierarchies are exhibited only by the 'pure' or theoretical coefficients; but it appears that those based on amalgamated measurements are better than those based on theoretical 'correction,' if the experimental conditions are carefully controlled. Each of the 13 tests, etc., furnishes 12 coefficients, one with each of the others; the sum of these 12 coefficients is taken as measuring its general tendency to correlate, and therefore provisionally determines its order in the hierarchy. The numbers are so printed that each shows the correlation between the function named vertically above it and that named horizontally to the left. Hence, if the functions measured are all specific forms of one common, fundamental function, then the values should always become successively smaller as the eye travels across to the right or downwards to the bottom.

TABLE V. *Hierarchy of Coefficients.* (*Amalgamated Series.*)

(A) Elementary School.

		Dotting apparatus	Alphabet	Sorting	Imputed intelligence	Dealing	Spot pattern	Tapping	Mirror	Sound	Lines	Touch	Memory	Weight
DOTTING APPARATUS	Observed coefficient	—	77	67	60	69	57	57	50	52	48	38	20	16
	Theoretical value ...	—	80	73	72	72	67	63	49	45	33	28	27	05
	Deviation	—	03	06	12	03	10	06	01	07	15	10	07	11
	P. e. of coefficient ...	—	05	07	08	06	08	08	09	09	09	11	12	12
ALPHABET	Observed coefficient	77	—	74	61	66	59	54	29	52	16	62	31	07
	Theoretical value ...	80	—	69	69	69	65	60	46	43	32	26	25	05
	Deviation	03	—	05	08	03	06	06	17	09	16	36	06	02
	P. e. of coefficient ...	05	—	06	08	07	08	09	11	09	12	07	10	12
SORTING	Observed coefficient	67	74	—	52	72	45	61	34	52	14	22	19	23
	Theoretical value ...	73	69	—	62	61	59	54	42	39	28	24	23	04
	Deviation	06	05	—	10	11	14	13	08	13	14	02	04	19
	P. e. of coefficient ...	07	06	—	09	06	10	08	11	09	12	11	10	19
IMPUTED INTELLIGENCE	Observed coefficient	60	61	52	—	44	76	47	67	40	29	13	57	−13
	Theoretical value ...	72	69	62	—	69	58	53	41	39	28	23	23	04
	Deviation	12	08	10	—	16	18	06	26	01	01	10	34	17
	P. e. of coefficient ...	08	08	09	—	10	05	10	07	10	08	12	08	12
DEALING	Observed coefficient	69	66	72	44	—	76	47	67	40	29	13	57	−13
	Theoretical value ...	72	69	61	60	—	58	53	41	39	28	23	23	04
	Deviation	03	02	11	16	—	07	12	01	05	19	00	04	03
	P. e. of coefficient ...	06	07	06	09	—	10	07	11	12	10	11	12	12
SPOT PATTERN	Observed coefficient	57	59	45	76	51	—	41	41	47	25	03	26	11
	Theoretical value ...	67	65	59	58	58	—	48	37	35	35	26	21	04
	Deviation	10	06	14	16	07	—	07	04	12	01	18	05	07
	P. e. of coefficient ...	08	08	09	05	09	—	10	10	10	11	12	11	12
TAPPING	Observed coefficient	57	53	61	47	65	41	—	41	47	08	26	−05	22
	Theoretical value ...	63	60	54	53	53	48	—	36	34	25	20	20	04
	Deviation	06	06	07	08	12	07	—	05	13	18	06	25	18
	P. e. of coefficient ...	08	09	08	10	08	10	—	10	10	12	11	12	12
MIRROR	Observed coefficient	50	29	34	67	40	45	45	—	34	16	08	05	−05
	Theoretical value ...	49	46	42	41	41	37	36	—	25	19	15	15	03
	Deviation	01	17	08	26	01	04	05	—	09	03	07	10	08
	P. e. of coefficient ...	09	11	11	17	10	10	10	—	10	12	12	12	12
SOUND	Observed coefficient	52	52	52	40	34	47	47	34	—	−07	−01	01	−13
	Theoretical value ...	45	43	39	39	39	35	34	25	—	17	14	14	02
	Deviation	07	09	13	01	05	12	13	09	—	24	15	13	15
	P. e. of coefficient ...	09	09	09	10	17	10	10	12	—	12	13	12	12
LINES	Observed coefficient	48	16	14	29	47	25	08	16	−07	—	26	06	19
	Theoretical value ...	33	32	28	28	28	26	26	25	17	—	10	10	02
	Deviation	15	16	14	01	19	01	17	03	24	—	16	04	17
	P. e. of coefficient ...	09	12	12	08	10	11	12	12	12	—	11	12	12
TOUCH	Observed coefficient	38	62	22	13	23	03	26	08	−01	26	—	16	29
	Theoretical value ...	28	26	24	23	23	21	20	15	14	10	—	08	01
	Deviation	10	36	02	10	00	18	06	07	15	16	—	08	28
	P. e. of coefficient ...	11	07	12	12	12	12	11	12	12	11	—	12	11
MEMORY	Observed coefficient	20	31	19	57	19	26	−05	05	01	06	16	—	05
	Theoretical value ...	27	25	23	23	23	21	20	15	12	10	18	—	01
	Deviation	07	06	04	34	04	05	25	10	13	04	08	—	04
	P. e. of coefficient ...	12	10	11	10	12	11	12	12	12	12	12	—	12
WEIGHT	Observed coefficient	16	07	23	−13	01	11	22	−05	−13	19	29	05	—
	Theoretical value ...	05	05	04	04	04	04	04	04	03	03	01	01	—
	Deviation	11	02	19	17	03	07	18	08	15	17	28	04	—
	P. e. of coefficient ...	12	12	12	12	12	12	11	12	12	12	11	12	—

Average deviation = ·100. Average p. e. = ·101.

TABLE VI. *Hierarchy of Coefficients.* (*Amalgamated Series.*)

(B) Preparatory School.

		Dotting apparatus	Alphabet	Imputed intelligence	Mirror	Memory	Spot pattern	Tapping	Sorting	Sound	Lines	Weight	Touch	Dealing
DOTTING APPARATUS	Observed coefficient	—	84	84	71	69	62	48	73	48	25	07	03	−03
	Theoretical value ...	—	85	80	76	70	66	66	60	48	39	14	−07	−13
	Deviation	—	01	04	05	01	04	16	13	00	14	07	10	10
	P. e. of coefficient ...	—	06	06	10	12	12	16	10	16	19	20	20	20
ALPHABET	Observed coefficient	84	—	80	48	84	67	57	76	34	22	−14	−28	45
	Theoretical value ...	85	—	78	74	68	64	64	58	51	37	14	−07	−12
	Deviation	01	—	02	26	16	03	07	18	17	15	28	21	57
	P. e. of coefficient ...	06	—	07	16	06	15	14	09	18	19	20	19	16
IMPUTED INTELLIGENCE	Observed coefficient	84	80	—	54	78	75	43	56	37	17	−19	−06	29
	Theoretical value ...	80	78	—	70	64	60	60	55	44	35	13	−06	−12
	Deviation	04	02	—	16	14	15	17	01	07	18	32	00	41
	P. e. of coefficient ...	06	07	—	14	08	09	16	14	17	20	19	20	18
MIRROR	Observed coefficient	71	48	54	—	43	38	75	34	57	54	44	31	−44
	Theoretical value ...	76	74	70	—	61	58	57	52	42	34	12	−06	−11
	Deviation	05	26	16	—	18	20	18	18	15	20	32	37	33
	P. e. of coefficient ...	10	16	14	—	16	17	09	18	14	14	16	18	16
MEMORY	Observed coefficient	69	84	78	43	—	74	54	64	17	28	−05	−35	03
	Theoretical value ...	70	68	64	61	—	53	53	48	39	31	11	−06	−10
	Deviation	01	16	14	18	—	21	01	16	22	03	16	29	13
	P. e. of coefficient ...	11	16	18	16	—	09	14	11	20	19	20	18	20
SPOT PATTERN	Observed coefficient	62	67	75	38	74	—	38	51	25	34	07	−44	19
	Theoretical value ...	66	64	60	58	53	—	50	45	36	29	11	−05	−10
	Deviation	04	03	15	20	21	—	12	06	11	05	04	39	29
	P. e. of coefficient ..	12	15	09	17	09	—	17	15	19	18	20	16	19
TAPPING	Observed coefficient	48	57	43	75	54	38	—	48	28	44	34	07	−31
	Theoretical value ...	66	64	60	57	53	50	—	45	36	29	11	−05	−09
	Deviation	16	07	17	18	01	12	—	03	08	15	23	12	22
	P. e. of coefficient ...	16	14	16	09	14	17	—	16	19	17	18	20	19
SORTING	Observed coefficient	73	76	56	34	64	51	48	—	38	00	−22	−14	02
	Theoretical value ...	60	58	55	52	48	45	45	—	33	27	10	−05	−09
	Deviation	13	18	01	18	16	06	03	—	05	27	32	09	11
	P. e. of coefficient ...	09	08	14	18	11	15	16	—	17	20	19	16	20
SOUND	Observed coefficient	48	34	37	57	17	25	28	38	—	07	34	17	−17
	Theoretical value ...	48	51	44	42	39	36	36	33	—	21	08	−04	−07
	Deviation	00	17	07	15	22	11	08	05	—	14	26	21	10
	P. e. of coefficient ...	16	18	17	14	20	19	19	17	—	20	19	20	20
LINES	Observed coefficient	25	22	17	54	28	34	44	00	07	—	35	19	−13
	Theoretical value ...	39	37	35	34	31	29	29	27	21	—	06	−03	−06
	Deviation	14	15	18	20	03	05	15	27	14	—	29	22	07
	P. e. of coefficient ...	19	19	20	14	19	18	17	20	20	—	18	19	20
WEIGHT	Observed coefficient	07	−14	−10	44	−05	07	34	−22	34	35	—	38	−35
	Theoretical value ...	14	14	13	12	11	11	11	10	08	06	—	−01	−02
	Deviation	07	28	32	32	16	04	23	32	26	29	—	39	33
	P. e. of coefficient ...	20	20	19	16	20	20	18	19	19	18	—	17	18
TOUCH	Observed coefficient	03	−28	−06	31	−35	−44	07	−14	17	19	38	—	−48
	Theoretical value ...	−07	−07	−06	−06	−06	−05	−05	−05	−04	−03	−01	—	01
	Deviation	10	21	00	37	29	39	12	09	21	22	39	—	49
	P. e. of coefficient ...	20	19	20	18	18	16	20	16	20	19	17	—	15
DEALING	Observed coefficient	−03	45	29	−44	03	19	−31	02	−17	−13	−35	−48	—
	Theoretical value ...	−13	−12	−12	−11	−10	−10	−09	−09	−07	−06	02	01	—
	Deviation	10	57	41	33	13	29	22	11	10	07	33	49	—
	P. e. of coefficient ...	20	16	18	16	20	19	19	20	20	20	18	15	—

Average deviation = ·165. Average p. e. = ·162.

How far, then, do these observed correlations form an ideal hierarchy? They clearly do not fit into the proposed scheme with perfect precision. Nor indeed can we expect them to. Like all empirical observations they are subject to error. What we have to demand is the following.

Firstly, their deviations from the ideal hierarchy should, on the whole, be neither more nor less than the probable erroneousness of the observations. The theoretical values for the ideal hierarchy may be obtained by various mathematical formulae[1]. These are given in the tables, together with the deviations of the observed coefficients from them, and the probable errors for the coefficients given in two decimal places only. For the Elementary School group the average deviation works out at ·100, while the average probable error comes to ·101. For the Preparatory School group the average deviation works out at ·165, while the average probable error comes to ·162. So far, then, a neater agreement between observation and theory could scarcely be desired[2].

Hardly less reassuring is the accordance disclosed on turning from the average deviation to the extreme deviations. In a 'normal' chance distribution, we should expect a deviation three times greater than the probable error to occur about once in 24 times. Here we have 78 coefficients for each group. Here, then, we should expect such a deviation to occur about three times in each. Actually it occurs four times in the Elementary School, and twice in the other. Again, a deviation

[1] The following simple formula has been supplied for this purpose by Dr Spearman (to whom I am here particularly indebted for several improvements on my own demonstration of a hierarchy):

Let $r(s, t)$ denote the required theoretical value, satisfying the condition

$$\frac{r(A, P)}{r(B, P)} = \frac{r(A, Q)}{r(B, Q)},$$

and at the same time according as well as possible with the correlations actually observed. Then

$$r(s, t) = m_s \, . \, m_t,$$

where

$$m_s = \frac{a_s}{\sqrt{2\Sigma - a_s}} \, . \, \sqrt{\frac{n-2}{n-1-\frac{n \, . \, a_s}{2\Sigma}}},$$

a_s = the sum of all the correlations with the performance s,

Σ = the sum of all the different correlations altogether,

n = the number of performances,

and m_t has a value analogous to m_s.

[2] For perfect mathematical strictness in comparing the probable errors and the average deviations, certain corrections should first be made. These, however, are complicated, and leave the results virtually the same as before; they have consequently here been omitted.

four times as great as the probable error may be expected to occur by mere chance about once in 124 times. Here it occurs twice in the Elementary School, and not at all in the other. Some of these deviations are themselves suggestive. At the Elementary School, three of the four large deviations occur with Imputed Intelligence, namely in its correlations with the Spot Pattern, Mirror, and Memory tests; such irregularities are here quite natural, since the method of estimating intelligence was not homologous with the methods of estimating the other capacities. The other correlations of intelligence may in consequence very likely have been reduced. The largest augmentation occurs with Memory; and towards Memory the schoolmaster's estimate of Intelligence is, as we have already seen, specially liable to be biassed. The deviation of the Alphabet-Touch correlation appears to be merely 'accidental.' At the Preparatory School, the deviations of Dealing are probably due to the exaggeration in its negative coefficients. The deviation of the Alphabet in its correlation with Memory might be explained by the fact that these two tests both involved rapid recognition of printed letters.

The tendency for subordinate groups of allied tests to correlate together is discernible, but small. At the Preparatory School there are distinct signs that the sensory tests are specially connected; the correlations of Weight with Sound, with Lines and with Touch are above ·30 instead of under ·10, and those of Touch with Sound and with Lines are over ·15 instead of being negative. At the Elementary School the correlations of Weight with Lines and with Touch, and of Touch with Lines, are again somewhat high; but the deviations are never more than twice the probable error. Among motor tests, at the Elementary School Dealing correlates with Tapping and with Sorting a little more highly than is demanded by the hierarchy, but at the Preparatory School the two corresponding coefficients are respectively negative and approximately zero. As to the sensori-motor tests, Sorting at both schools correlates with Alphabet a little highly for its place, though only at the Preparatory School is the difference greater than the probable error. All these special deviations are under three times the probable error. In this respect, the results of a wider application of the Test-methods confirm and extend the observations of Dr Spearman, who found in his own experiments "the range of the central function... so universal, and that of the specific functions so vanishingly minute."

The main significance of this hierarchy of experimental performances, is, as it appears to me, that we are led to infer that all the functions of the human mind, the simplest and most complicated alike, are probably processes within a single system. A process typical of higher psychophysical 'levels' may be connected with a process typical of lower psychophysical 'levels' far less intimately than either is with a process of intermediate 'levels.' Yet this relatively small correlation is not a disproof, but a consequence of, their inclusive organisation within a single integrative system of psychical dispositions or of neural arcs. The contrary assumption of a radical dichotomy between "the general mammalian foundation of the central nervous system" and the "specifically human capacity" of General Intelligence,—towards which Dr Archdall Reid[1], and even Professor Thorndike[2] seem to incline,—proves a serious barrier to the advance of the biological standpoint in individual psychology.

The nature of the general or fundamental function cannot here be discussed with any hope of finality. The methods employed must first be extended to yet higher levels of mental process. The process of Abstraction was indirectly involved in one of the memory tests. But there results were negative. With this exception, the highest mental levels—the conceptual and relational—have been left by our experiments absolutely untouched.

It is clear, however, from our extension of the tests into regions representing a stage of mental development higher than sense-perception, that the absolute identification of General Intelligence and General Sensory Discrimination (if it has ever been suggested by any but its opponents[3]) cannot be maintained. The sensory tests fall together in the hierarchy, and the principle common to them, which determines both their falling together, and the order into which they fall, appears to be in the main but a specific mode or determination of a wider principle which determines the order of the whole series: for, as we have just seen, the generic or hierarchical tendency has far more

[1] See quotations *inf.* p. 170.

[2] See his discussion of Dr Spearman's hypothesis, *Amer. J. Psychol.*, July 1909, "Relation of Accuracy in Sensory Discrimination to General Intelligence," from which the phrases cited in the text are borrowed.

[3] With reference to my criticism of the passage cited above (p. 159) formulating his view of the relation of General Sensory Discrimination and General Intelligence, Dr Spearman has written to me: "This conclusion of mine was badly worded. I did not mean (as others have naturally taken it) that general intelligence was based on sensory discrimination; if anything, *vice versa*. I take both the sensory discrimination and the manifestations leading a teacher to impute general intelligence to be based on some deeper fundamental cause, as sketched in the *Zeitschrift für Psychologie*, Vol. XLI. p. 110, para. 5."

influence upon the size of the sensory coefficients than any specific or extra-hierarchical tendencies for sensory tests to correlate particularly highly together. Thus the correlations between pairs of sensory processes, like those between all other pairs, "appear" mainly, if not "wholly due to their being all variously saturated with some common fundamental Function," which also saturates the rest of the processes tested, and determines the correlations between all other pairs. This, however, does not prove that the highest common factor of all processes correlated with Intelligence is identical with the *highest* common factor in sensory discriminations: but only that the former is a necessary factor, though not necessarily the sole, perhaps even a vanishingly minute factor in the latter.

One important universal condition of all experiments upon school-children is Goodwill, that is, Interest as determining Zeal. Some such factor seems often to have contributed to influence differentially the psychological measurements obtained from untrained subjects in previous investigations, and it might plausibly be supposed that Goodwill was the main condition permeating the hierarchy of coefficients set forth above, and determining its order. If so, the order of tests in the hierarchy should correspond to the degree in which Interest or Zeal was displayed in the subjects. Lists have been obtained from representative boys, arranging the tests, as far as possible, in the order in which they 'liked' or 'disliked' them. This order in the average also tallies with the degree of Zeal which the tests seemed to the experimenter to evoke in the children. It is,—beginning with the test in which interest was keenest: Mirror, Card-dealing, Card-sorting, Spot Pattern, Sound, Tapping, Alphabet, Weight, Dotting machine, Touch, Lines, Memory. There is little difference between the orders given by the boys of both schools. None of the lists throw light on the hierarchies. We have, therefore, to look for some other common factor besides Goodwill in the sense of Zeal determined by Interest.

The test which correlates most with all the other tests, and consequently heads the hierarchy, is the Dotting test. The Dotting test was specially devised to measure power of sustained effort of maximal concentration, in short to test Voluntary Attention. The inference is that the power of Voluntary Attention is the capacity, common to all the functions tested, which enters most into the processes involved. The hypothesis that Attention is the essential factor in Intelligence is already a well-known one. In view of it, before the hierarchies were drawn up, the tests were arranged in order, according to the degree in which Attention might be expected to be required in the successful performance of the tasks. Such arrangements were obtained from interrogations of the boys, and independently from two or three psychologists. The average arrangement is as follows: Dotting, Spot Pattern, Memory, Mirror, Alphabet, Sorting, Sound, Lines, Touch, Weight, Dealing, Tapping. This corresponds fairly closely with the order of the various correlations with Intelligence, and nearly as closely with the orders given by the hierarchies. The most interesting discrepancies are those of the Alphabet test and the Memory test. In both hierarchies the Alphabet test stands second to the Dotting test; it may be that the high correlations of Alphabet-finding with Imputed Intelligence and tests of Intelligence, point to the connection between quickness to recognise letters and readiness to think in terms of written words, which in civilised societies forms an important element in rational intelligence. The fact that Imputed Intelligence does not head the list, but follows Dotting and Alphabet, need not, on the present hypothesis, cause surprise; for, if Intelligence essentially involve some central factor such as Attention, the schoolmaster's opinion may be an impure and indirect method of estimating what is estimated far more directly and scientifically by these experimental tests. The displacement of Memory is very suggestive. In the hierarchy for the Preparatory School, it takes its place along with the 'association' tests. In the hierarchy for the Elementary School, it falls in among the sensory tests. An observation of the manner of the boys when performing this test, and an inspection of their papers, suggest that the Elementary boys relied for the most part upon a primary memory image of the words, while the Preparatory boys relied rather upon associations formed between them. Thus, instead of reproducing the words in the correct order, the Elementary boys often wrote down the last two or three as they were ringing in their ears, or reproduced the column in inverted order, and, in endeavouring to complete the number of words, commonly waited for the missing word to "recur spontaneously"; whereas the Preparatory boys seldom wrote the words in reversed or inaccurate sequence, and, on forgetting, muttered the words they had retained in their proper order, in the hope of the chain of associations suggesting the missing link. Thus, so far as memory implies mere retentiveness of sensory images, it seems to bear little relation to intelligence; so far as memory implies organisation of new associations, it seems to bear a high relation to intelligence.

This point, taken in conjunction with several stray inferences noticed under the conclusions as to the several groups of tests[1], strongly suggests that it is one feature or function of attentive consciousness in particular, which forms the basis of Intelligence—namely, the power of re-adjustment to relatively novel situations by organising new psycho-physical co-ordinations. A comparison of the successive correlations with Intelligence yielded by the same test, where it was repeated with the same reagents on different occasions, seems to lend support to this suggestion. The differences are most marked in the simple correlation of each of the two or three series with the Headmaster's provisional estimate. These are given in Table VII. In the average correlations with *all* the provisional estimates the differences are still present, and nearly always similarly but less strikingly shown. (It is the average of all these correlations, not merely of the coefficients given below, that furnishes the 'average raw correlation.')

TABLE VII. *Intelligence coefficients for successive series with the same test.*

	Elementary School						Preparatory School		
No. of series	1st	2nd	3rd	4th	5th	6th	1st	2nd	3rd
Dotting apparatus...	·67	·62					·84		
Spot pattern[2]	·19	·57	·58	·69	·64	·61	·27	·64	·47
Mirror..................	·51	·22					·54		
Memory	·57	·38	·46				·81	·71	
Alphabet...............	·53	·45	·38				·83	·65	·45
Sorting	·44	·29	·56				·43	·71	
Dealing	·51	·42	·46				·13	– ·03	
Tapping	·23	·44					·43		
Sound..................	·50	·43					·37		
Lines	·28	·17					– ·04	·54	
Touch..................	·25	– ·05	·17				·03	– ·25	
Weight	·06	– ·14	·03				·17	– ·08	

It will be noticed that (with one or two exceptions, of which the Spot Pattern is the most prominent) the correlation is highest on the first occasion, that is to say, when the task is newest. This is not due to a difference of operator, since the first set of experiments was not in every case undertaken by the same operator; and further where the operator taking the first set took also the third, the correlation for his series is (with the above exceptions) always higher than that for his later series. The suggestion is that this reduction of correlation is due to reduction in Attention. Whether this in turn is due to decrease of

[1] See pp. 136, 137, 144.

[2] See p. 152.

interest and zeal, or to mechanization of the subject's general procedure; and whether the subsequent partial recovery is due to the greater interval elapsing before the third series, or to the personal equation of the operator, it would be premature here to speculate.

We have seen throughout that the greater the change, and the greater the complexity, and the greater the novelty involved in the task performed, the greater also (*ceteris paribus*) is the Imputed Intelligence of the performer. To relative novelty all the other attributes are probably secondary. Thus high intelligence seems to mean high capacity for continually systematising mental behaviour by forming new psycho-physical co-ordinations, older co-ordinations being retained, so that newer co-ordinations bring with them increased complexity and incessant change. In such progressively integrative actions of the mind the efficient and directive agent is attentive consciousness. And in this sense we may agree that so-called 'Voluntary' Attention is, of all recognised psychological processes, the essential factor in General Intelligence. This interpretation may help to bring the general use of the term Intelligence into relation with the special significance it has acquired in animal psychology. It further suggests that we may eventually seek the psycho-physical basis, underlying this capacity, in a particular characteristic of general neural constitution; the accentuation of such a neural characteristic would then produce the type of mind known as intelligent, while its biological inheritance would form the condition of the transmissibility of the mental trait.

THE INHERITANCE OF INTELLIGENCE.

The third and last phase of our problem is in many ways the most important of all. The gathering interest in "the possible improvement of the human breed," the growing belief that the innate characters of the family are more potent in evolution than the acquired characters of the individual, the gradual apprehension that unsupplemented humanitarianism and philanthropy may be suspending the natural elimination of the unfit stocks—these features of contemporary sociology make the question whether ability is inherited one of fundamental moment.

Hitherto, writers have dogmatically assumed and asserted, some—the existence of class differences in native intelligence, others—the absence of such differences, all without any sure basis for their opinion.

Supporting their existence is the authority of the first and foremost exponent of Eugenics, Sir Francis Galton. His investigations of '*Hereditary Genius*' have, however, dealt rather with types of ability peculiar in nature, or exceptional in degree. The best known statistical enquiry into the transmission of ordinary mental faculties is that of Professor Karl Pearson. The essence of his method was roughly as follows: a large number of schoolmasters and schoolmistresses were consulted about pairs of brothers and sisters in their schools, and were asked to fill up a statement of their opinion of the ability, temper, popularity, etc., of these various pairs. The evidence thus accumulated on collateral transmission was strongly affirmative, and Prof. Pearson states as his main conclusion "that the mental characters in man are inherited in precisely the same way as the physical." Now, to discover that, in social, moral, and intellectual nature, children of the same family resemble one another, suggests that they owe it to a common parent, but does not prove that its reappearance is due to biological inheritance, since mental characteristics—especially as they appear to the casual observer—may with equal probability have been handed down by training and tradition, and re-acquired by each succeeding generation through imitation and habit. Prof. Pearson's results are, therefore, inconclusive.

As an authority for the opposite view I may quote Dr Archdall Reid. His argument is, "supposing a child of refined and educated English parents were reared from birth by African cannibals. Then, in body, when grown, the child would resemble his progenitors more than his captors, but does anyone believe the same of his mind?...The common sense of mankind has universally recognised this radical difference between mind and body," namely, that the capacities of the mind can be, and should be, trained, the capacities of the body may be left to develop of themselves. From such general considerations he infers that "the evidence is overwhelming that the mental and moral qualities are not inherited in the same sense as the physical qualities." He adds as the explanation that, in addition to instincts like those of the lower animals, man "has an enormous memory, and enormous power of utilising its contents." ("...The two combined make up what we term intelligence....") "It is this educability that confers on man all his morality, all his intelligence, all his intellectuality, all his reasoning power, all his adaptability....The instincts of men have everywhere been the same, for instincts are inherited in the same sense as physical characters are inherited. But man's knowledge, aspirations, ideals, and all that flows from them, belong to a different and to a higher category. They are acquirements, and as such are not inherited by offspring." And, simply "because no man's experiences are quite the same as those of any other man, individual men of the same family or class differ widely amongst themselves; men of different classes differ yet more, and men of different nations even yet more."

This explanation, however, really concedes all that Eugenics could desire. For this "educability" is obviously a mental character; it is also admittedly inherited. It is consequently quite possible that in different families such educability may itself be inherited in different amounts. To determine whether such inherited differences actually exist, and, if so, what is their relation and their proportion to acquired mental differences,—these are questions which special experiment alone can finally decide. For, if there are important innate differences, then (in Dr Archdall Reid's own words) "these are so completely masked and overshadowed by immensely more important acquired differences that they cannot be recognised without much closer scientific investigation than has yet been attempted[1]."

There is here, then, a legitimate, definite and urgent problem. Inductive generalisation and deductive inference are alike inconclusive. Experimental evidence is imperative. Meagre though they be, therefore, the indications afforded by the foregoing results may justifiably be exploited.

The investigation was concerned with two representative groups. Each of these was fairly homogeneous; each represented a distinct social type. The boys of the Elementary School were drawn from the lower middle classes. Their parents were chiefly small tradesmen and artisans, and, in most cases, presumably neither pre-eminent nor defective in intelligence. The parents of the Preparatory boys were, as we have seen, mostly persons occupying high positions in the ecclesiastical, civil, or academic world—positions implying eminence in ability and culture. The class represented by them is a product of a highly differential social selection, perhaps the most efficient form of selection now operating in our society. Whether the difference of intelligence in the parents was itself inherited, or acquired, we have no need to ask. We have only to observe whether the difference reappears in the children, and whether the reappearance is to be attributed to direct inheritance. The experimental data furnished by the application of the 12 tests to

[1] *Sociological Papers*, Vol. III. pp. 93, 94.

TABLE VIII.

	Elementary School								Preparatory School			
	Av.	M.V.	Best	Worst	Clever	Ordinary	Dull	Imbecile	Av.	M.V.	Best	Worst
DOTTING APPARATUS (1 unit=about 7 dots per min.)	12	1½	17	10¼	14½	13	11¼	8½	15	2	21	10
SPOT PATTERN (number of exposures per pattern)	45·3	21·1	10	157	19·5	41	71·6	157	38·6	12·5	11	86·6
MIRROR (number of secs. per 6 tracings)	388·9	90·1	181	747	347	383·7	429·8	2464	257	54	156	526
MEMORY (4 marks = 1 correct word)	134·2	27·7	223·3	68·6	167	135	134·2	—	216	29	265·5	153·5
ALPHABET-FINDING (number of secs. per 26 letters)	91	15·4	50	138	75·6	90·5	103	306	74	13	48·5	119
CARD-SORTING (number of secs. per 50 cards)	48·1	4·1	34	61·6	41	49·3	51·5	87	41·3	4·1	36·5	49·5
CARD-DEALING (number of secs. per 50 cards)	31·6	5·9	20·3	46	26	31·6	36·6	46	27·3	1·8	20	34
TAPPING (number of taps per 15 secs.)	80·5	8	97	59	86·5	80·2	76·3	—	95·5	11·9	119	56
SOUND (threshold in vibration differences)	6	1·9	1	11·5	4·6	5·6	7·1	10	3·5	2·2	·3	8
LINES (A. C. E. in cms.)	·41	·107	·20	1·05	·36	·36	·50	—	·39	·179	·07	1·30
TOUCH (threshold in mms.)	36·2	9	19	58·3	31·3	37·3	38·3	20	38·9	11	12·5	63·7
WEIGHT (threshold in gms.)	8·75	1·5	6	16	8·5	9·5	8·1	4	9·3	1·6	5	11·5

the two respective groups have been brought together in Table VIII. The first column of figures gives the average measurement for each test at the Elementary School group. The individuals' Mean Variation about the average follows. Next are given the best and the worst individual performances in the same group, and then the averages for the three sub-groups—namely the clever, the ordinary, and the dull or infra-normal boys respectively. For convenience of reference the achievements of the weak-minded boy are also added. The last four columns give the group-average, with its Mean Variation, and the optimum and pessimum, for each test at the Preparatory.

With two exceptions the average performances of the boys of the Preparatory School are all superior to those of the boys of the Elementary School. The two tests in which they are equal to, or weaker than, the latter are those for Discrimination of Weight and Discrimination of Two Points upon the Skin, and these two are the only two tests which yielded negative correlations with Intelligence. Hence, wherever there are correlations with Intelligence, there (so far as we can discover) boys of superior parentage are themselves superior. Moreover, at the Sound, Tapping, Memory, Mirror, Alphabet, and Dotting tests, the Preparatory boys are superior even to the cleverest section of the Elementary boys.

Now in the case of the lowest social classes, general inferiority at mental tests might be attributable to unfortunate environmental and post-natal influences, such as improper nourishment, improper air, and, generally, to neglected conditions of physical health. But (with the possible exception of the dullest boy of the 30) such conditions could not be suspected with the boys who, at a fee of 9*d.* a week, attended the Central Elementary School. Again, unlike superiority in scholastic examinations, superiority in the experimental tests could not have depended, wholly or in part, upon any previous acquisition of material. Nor yet could it in every case have depended upon previous special practice. In the Mirror and Memory tests no doubt special practice played an important part in raising the proficiency of the Preparatory boys. In Pitch-discrimination, Musical training, and in Tapping (perhaps also in Alphabet-finding and Dotting) motor training might be suggested as the determining factors; but we have seen that the opportunities for musical training were probably greater with the Elementary boys; and the special facilities for manual training at the Elementary School probably developed motor rapidity and motor coordination as much as the special facilities for games of muscular skill at the Preparatory School; moreover, the superiority is not uniformly conspicuous in motor tests as such: it is nearly as marked in Sorting, but much less marked in Dealing. Nor can the Preparatory boys' achievements be attributed entirely to superior training generally. School education is immediately concerned with certain special departments of learning; and operates primarily by developing certain special interests and aptitudes. So far as such education may also be conceived as indirectly exercising the mind generally for *all* activities into which intelligence may subsequently enter, the training in general intelligence (in the sense in which the term has here been used) would probably be considered by most authorities to be as good as the best Elementary School of an academic city, as at a private school preparing its scholars for examinations of a specific kind. As implied above, the former type of school from its very nature tends rather to train its scholars to use their minds, while the latter tends rather to train its scholars to store their minds. Hence, so far as scholastic education is concerned, the boys of the Elementary School seem on the whole to have been better equipped to perform the tests than the others, if there was any appreciable difference of equipment in this respect at all. There remains the educative influence of home and social life,—of the wider experience and more intellectual atmosphere enjoyed by the Preparatory boys as compared with the Elementary boys. Important for mental development as this influence undoubtedly is, I yet believe (as my remarks on the personal attitude of the reagents, and upon the effect of zeal and interest must have suggested) the part played by this factor in the tests was small. Here, however, one must confess, such speculative arguments can convey little conviction to those who have not witnessed the actual manner of the respective boys; perhaps the following experimental data may, therefore, carry more weight.

Firstly, we seem to have, for one important instance, a direct estimate of the influence of such irrelevant factors. Among a group of some 60 boys from the Elementary School, 10 to 16 years old, proficiency in the Alphabet test was found to correlate with Age to an extent of ·29. Now Age, at any rate during this particular period, in a group otherwise fairly homogeneous, may be taken as a simple and direct means of estimating the extent to which post-natal, or environmental, factors, likely to influence intelligence generally, have operated in the several boys. The older the boy, the greater the amount of training his mind must have acquired from individual experience. At the same time, his intelligence may also have undergone a greater spontaneous development. So that the coefficient of correlation between Alphabet and Age indicates that the connexion between Alphabet and training through general experience is no more, and probably less, than ·29. Hence, compared with the connexion between Alphabet and Intelligence, this connexion is negligible.

A second piece of evidence is afforded by a series of experiments carried out upon the Elementary boys 18 months after the several series already described. In September, 1909, Mr Flügel and myself revisited the school, and repeated the more important tests upon as many of our original subjects as were still at the school, or could still be traced[1]. The results were unexpected. It had been anticipated that the new measurements might show suggestive relations to the new occupations or the new schools of the boys who had left the Elementary school, or to the progress in class of the boys who remained. On the contrary, the new series show nearly as high correlations with earlier series, as the earlier series did with each other; and the measurements for the boys tested remained in most cases much as before. The data may be summarised most briefly by expressing the improvement or deterioration for the set of boys re-tested as a percentage of the average calculated for the same set of boys from the original experiments. Touch shows a deterioration of 3 $^{\circ}/_{\circ}$; Comparing Lines an improvement of 8 $^{\circ}/_{\circ}$; Dealing an improvement of 15 $^{\circ}/_{\circ}$; Card-sorting an improvement of 6 $^{\circ}/_{\circ}$; Alphabet-sorting an improvement of 4 $^{\circ}/_{\circ}$; Memory a deterioration of 9 $^{\circ}/_{\circ}$; the Spot Pattern a deterioration of 7 $^{\circ}/_{\circ}$; Dotting a deterioration of 3 $^{\circ}/_{\circ}$; the Mirror alone shows a marked improvement, viz. 31 $^{\circ}/_{\circ}$, and this is doubtless attributable to the persistence of the practice gained during the earlier experiments. With this exception, the capacities re-tested seem during the interval to have remained all but stationary. Yet the boys' mental equipment has not. A somewhat 'dull' boy, for instance, who was 25th in the amalgamated list for Six Tests in 1908, has in 1909 risen to a place in the school occupied in 1908 by a 'clever' boy, who was then 4th on the amalgamated list; yet his new measurements, instead of concomitantly rising to equal those of the 'clever' boy, are now equivalent to those of the boy who was 24th. Similarly with most of the other boys. Thus, though the period between the ages of 13 and 15 is for boys one of rapid progress in knowledge,

[1] I am much indebted to the employers of some of the boys for their readiness to allow them to be tested during hours of employment, and to furnish interesting information as to the intelligence of the boys concerned.

interests, and acquired aptitudes, yet in the capacities measured by the tests no corresponding alteration is made. Hence, these capacities appear to constitute a relatively permanent endowment; and consequently it seems legitimate to assume that they depend upon innate differences in the individuals concerned.

For all these various reasons we may conclude that the superior proficiency at Intelligence tests on the part of the boys of superior parentage, was inborn. And thus we seem to have proved marked inheritability in the case of a mental character of the highest "civic worth."

Parental intelligence, therefore, may be inherited, individual intelligence measured, and general intelligence analysed; and they can be analysed, measured, and inherited to a degree which few psychologists have hitherto legitimately ventured to maintain.

TABLE IX. *Complete List of Correlations.*

	Reliability coefficient		Coefficient of amalgamated series		Av. raw coefficient		Corrected coefficient		
	Elem. Sch.	Prep. Sch.	Elem. Sch.	Prep. Sch.	Elem. Sch.	Prep. Sch.	Elem. Sch.	Prep. Sch.	Av.
Imputed Intelligence...	·88	·91	—	—	—	—	—	—	—
and Dotting apparatus...	·86	—	·60	·84	·65	·84	·75	·96	·85
Spot pattern	·55	·50	·76	·75	·61	·44	1·00	·66	·83
Mirror	·52	—	·67	·54	·50	·47	·74	·68	·71
Memory (Total)	·70	·93	·57	·78	·48	·76	·60	·82	·71
,, Concrete ...	—	—	·58	·84	—	—	—	—	—
,, Abstract ...	—	—	·48	·78	—	—	—	—	—
,, Nonsense...	—	—	·43	·75	—	—	—	—	—
Alphabet	·60	·48	·61	·80	·50	·61	·68	·91	·79
Sorting	·84	·38	·52	·56	·45	·60	·53	1·00	·76
Dealing	·88	·80	·44	·29	·48	·05	·54	·06	·30
Tapping	·51	—	·47	·43	·44	·28	·65	·41	·53
Sound	·67	—	·40	·37	·40	·35	·52	·41	·46
Lines	·50	·54	·29	·17	·34	·31	·51	·44	·47
Touch	·73	·75	·13	– ·06	·14	– ·14	·17	– ·17	·00
Weight	·86	·51	– ·13	– ·19	– ·01	– ·14	– ·01	– ·20	– ·10
Dotting Apparatus and Spot pattern ...			·57	·62	·52	·40	·80	·62	·71
Mirror............			·50	·71	·55	·71	·84	1·00	·92
Memory			·20	·69	·17	·73	·22	·84	·53
Alphabet			·77	·84	·61	·54	·85	·84	·84
Sorting			·67	·73	·64	·57	·75	1·00	·87
Dealing			·69	– ·03	·72	– ·14	·83	– ·17	·33
Tapping			·57	·48	·55	·48	·83	·73	·78
Sound............			·52	·48	·52	·48	·68	·63	·65
Lines			·48	·25	·40	·30	·60	·45	·52
Touch............			·38	·03	·22	·25	·30	·31	·30
Weight			·16	·07	·06	·16	·07	·25	·16

	Coefficient of amalgamated series		Av. raw coefficient		Corrected coefficient		
	Elem. Sch.	Prep. Sch.	Elem. Sch.	Prep. Sch.	Elem. Sch.	Prep. Sch.	Av.
SPOT PATTERN and Mirror	·41	·38	·40	·37	·75	·75	·75
Memory	·26	·74	·25	·55	·41	·84	·62
Alphabet.........	·59	·67	·56	·41	·96	·83	·89
Sorting	·45	·51	·47	·20	·68	·47	·57
Dealing	·51	·19	·43	·25	·66	·40	·53
Tapping	·41	·38	·34	·25	·64	·50	·57
Sound............	·47	·25	·34	·23	·55	·40	·47
Lines	·25	·34	·16	·44	·50	·85	·67
Touch............	·03	− ·44	·13	− ·17	·21	− ·29	− ·04
Weight	·11	·07	·19	·05	·27	·10	·18
MIRROR and Memory	·05	·43	·08	·44	·13	·64	·38
Alphabet.........	·29	·48	·39	·28	·71	·56	·63
Sorting	·34	·34	·37	·32	·64	·71	·67
Dealing	·40	− ·44	·48	− ·40	·72	− ·61	·05
Tapping.........	·41	·75	·25	·74	·48	1·00	·74
Sound............	·34	·57	·24	·55	·40	·93	·66
Lines	·16	·54	·08	·50	·16	·94	·55
Touch............	·08	·31	·17	·25	·27	·40	·38
Weight	− ·05	·44	− ·06	·37	− ·10	·71	·30
MEMORY and Alphabet	·31	·84	·28	·69	·47	1·00	·73
Sorting	·19	·64	·19	·52	·27	·90	·58
Dealing	·19	·03	·14	·05	·18	·06	12
Tapping.........	− ·05	·54	·01	·52	·01	·80	·40
Sound............	·01	·17	·13	·20	·19	·27	·23
Lines	·06	·28	·03	·19	·05	·27	·16
Touch............	·16	− ·35	·11	− ·23	·15	− ·27	− ·06
Weight	·05	− ·05	·05	·15	·07	·22	·14
ALPHABET and Sorting	·74	·76	·59	·60	·83	1·00	·91
Dealing	·66	·45	·61	·23	·83	·37	·60
Tapping.........	·54	·57	·37	·47	·67	·94	·80
Sound............	·52	·34	·43	·20	·68	·35	·51
Lines	·16	·22	·20	·17	·37	·32	·35
Touch............	·62	− ·28	·44	− ·29	·66	− ·48	·09
Weight	·07	− ·14	·19	− ·03	·26	− ·06	·10
SORTING and Dealing	·72	·02	·66	·19	·77	·32	·54
Tapping.........	·61	·48	·52	·43	·78	1·00	·89
Sound............	·52	·38	·19	− ·32	·25	− ·62	− ·18
Lines	·14	·00	·21	·03	·32	·07	·19
Touch............	·22	− ·14	·17	− ·17	·20	− ·32	− ·06
Weight	·23	− ·22	·20	− ·19	·23	− ·33	− ·05
DEALING and Tapping.........	·65	− ·31	·52	·23	·79	·36	·57
Sound............	·32	− ·17	·32	− ·06	·41	− ·06	·17
Lines	·47	− ·13	·38	− ·05	·59	− ·07	·26
Touch............	·23	− ·48	·26	− ·37	·32	− ·51	− ·08
Weight	·01	− ·35	·05	− ·26	·05	− ·40	− ·17
TAPPING and Sound............	·47	·28	·31	·26	·53	·44	·48
Lines	·08	·44	·08	·30	·16	·57	·36
Touch............	·26	·07	·07	·06	·12	·09	·10
Weight	·22	·34	·13	·38	·11	·74	·42
SOUND and Lines	− ·07	·07	− ·03	·14	− ·05	·23	·09
Touch............	− ·01	·17	− ·01	·19	− ·01	·26	·12
Weight	− ·13	·34	·05	·31	·06	·53	·29
LINES and Touch............	·26	·19	·16	·13	·26	·21	·23
Weight	·19	·35	− ·11	·08	− ·16	·15	·00
TOUCH and Weight	·29	·38	·29	·38	·37	·61	·49

FURTHER OBSERVATIONS ON THE VARIATION OF THE INTENSITY OF VISUAL SENSATION WITH THE DURATION OF THE STIMULUS.

By J. C. FLÜGEL and W. McDOUGALL.

The problems investigated.—The experimental conditions of our research.—Further experiments on the 'action-time' of lights of different intensities.—The former observations confirmed.—The decline of intensity of sensation when stimulus is prolonged beyond action-time.—The difference-threshold for brightness (with lights exposed for less than their 'action-time') measured in terms of the duration of the stimulus.—Weber's law verified by the same method. —Comparison of the difference-thresholds obtained with successive and with simultaneous presentations.—The latter method has two defects, which however tend to neutralise one another.—Comparison of our results with those of Stigler.—Further observations on the action-time of just perceptible light.—Comparison of the visibility of double and single just perceptible flashes.—The Talbot-Plateau law holds for lights affecting the rods only.

THE observations here recorded form a continuation of the work on this subject published by one of us in the first volume of this *Journal*[1]. They were undertaken with the object of confirming and extending that work by repeating some of the principal observations there described and by applying the method there used to the investigation of several new problems that had arisen during the course of the former research[2].

[1] W. McDougall, "The Variation of the Intensity of Visual Sensation with the Duration of the Stimulus," This *Journal*, Vol. I. p. 151.

[2] Confirmation of the original determinations of action-time by this method seemed very desirable, since they differed so widely from the earlier determinations made by Exner and by Götz Martius and since they were made by a single observer only, working single-handed and under very great difficulties arising from imperfections of home-made apparatus.

The record of our observations may most conveniently be given in five sections according as the results bear chiefly upon:

(i) The confirmation of the general results of the former experiments on the 'action-time' of light, *i.e.* the time during which a light of a given intensity must act upon the retina in order to produce the most intense sensation that it is capable of producing.

(ii) The investigation of the rate of decline in intensity of sensation when a light acts upon the retina for a period longer than its action-time.

(iii) The investigation of the difference-threshold for brightness (expressed in terms of duration) with lights exposed for less than their action-time.

(iv) The comparison of the difference-thresholds obtained with simultaneously and successively given momentary visual impressions.

(v) The investigation of certain points connected with momentary impressions affecting the rods of the retina only.

With the exception of one set of observations referred to in pp. 181, 188 all the experiments recorded were performed in the Psycho-Physical Laboratory at Oxford in the spring and summer of 1909. The principal piece of apparatus used throughout our research was a metal disc similar in design to the millboard disc used in the observations recorded in the former article and described on p. 160 of Vol. I of this *Journal*, except that the two adjustable open sectors at the edge of the disc had an extreme range of 0·45° instead of 0·40° as in the older disc, and that inside and adjoining one of these peripheral openings was another open sector with the same range of adjustment, but which could be adjusted independently of the outer one. By means of this arrangement it was possible to present two simultaneous stimuli, the duration of each of which might be varied independently by making the necessary adjustments of the two adjoining open sectors. During the majority of the experiments, however, the inner sector was kept completely closed, so that the disc was, for practical purposes, exactly similar to the one previously used except that the two open sectors could, when desired, be made slightly larger than was the case in the old one. The disc was turned by a large water-motor, which, having its own special source of supply, was found to give a very constant rate of speed. Rough adjustment to the required rate of revolution of the disc was made by varying the gearing that connected the disc to the pulley of the motor, while fine adjustment could be effected by regulating the supply of water to the motor. The rate of rotation of the disc was timed before and during each series of observations and was seldom found to vary appreciably. The source of light used (except in the arc-lamp series recorded on p. 186) was a projection lantern with single-wick oil-lamp carefully trimmed at the beginning of each day's work and burning at its maximum power. By this means a light of very constant intensity was obtained, this intensity, however, being considerably less than that of the light ($\frac{1}{4}$ L) obtained with the single acetylene gas burner in the earlier experiments. This lantern was brought close up to the rotating disc and so placed that the beam of light was projected upon the peripheral part of the disc and through the open sectors during the time of their passage. The disc in turn was brought as near as possible to an opening in the wall of the dark room in which the subject sat. Over this opening on both sides were fixed stout pieces of millboard with corresponding circular apertures 2 cm. in diameter, through which alone the light could enter the dark room. The aperture on the inner (dark room) side was further narrowed by placing over it a piece of thick black paper with a circular hole 13 mm. in diameter, which, during the passage of the open sectors of the disc, constituted the bright field at which the subject looked. When the light from the lantern was interrupted by the surface of the disc, a very little diffused light penetrated through the small double aperture, sufficient to enable the subject to fixate the disc in anticipating each momentary flash, but insufficient to produce any considerable degree of retinal fatigue. The subject sat in the dark room, his head supported by a chin-rest and his eye on a level with the aperture. In order to facilitate communication with the operator, when the door of the dark room was closed, he was provided with a speaking tube. When the operator had made the required adjustments of the open sectors and had set the disc in rotation, he gave the attention-signal to the subject, who then fixated the aperture and observed the flashes occurring at the successive passages of the two open sectors, until he had decided that there was a difference in brightness between the two flashes or that they were indistinguishable, whereupon he recorded his judgment through the speaking tube, saying 'Brighter' and 'Darker' at the appearance of the brighter and duller flashes respectively, or 'Same' if he could not distinguish between them. The operator then stopped the disc and noted to which of the open sectors the judgments 'brighter' and 'darker' corresponded, and readjusted them when necessary. Having recorded his judgment the subject looked away from the aperture, or closed his eyes, until he again received the signal for attention. As the work is distinctly fatiguing, it was found inadvisable to continue a series of observations with one subject for more than 10 or 15 minutes at a time; and subject and operator usually changed places several times at each sitting.

I. The first attempt to repeat McDougall's observations on action-time was undertaken in 1907 by two observers, Mr C. L. Burt and one of the writers (Flügel). The procedure adopted was the combination of the method of minimal changes with that of right and wrong cases employed by McDougall in the earlier observations and described in the former article (p. 160). After a little practice on the part of the new observers, one flash generally appeared to them distinctly brighter than the other when the width of the open sectors was in the proportion of 3 to 4, the brighter flash corresponding to the wider sector when the exposure was below action-time and to the narrower sector when it was prolonged above action-time. The source of illumination was the lantern already referred to and the action-time was estimated to be between 50_σ and 60_σ[1]. This is considerably below that at which we arrived in our later experiments, and it would seem either that, owing to want of practice at the work, the difference in brightness between the two flashes became indistinguishable at an earlier point in the series of increasing intensities than was the case in the later investigations undertaken after longer practice; or that some other circumstance prevented a sufficiently accurate estimate of the action-time. This is the more probable as the investigation was carried out under less favourable conditions than was our later work, and with less satisfactory apparatus, the new Oxford Psycho-Physical Laboratory not being in existence at that time. Owing to these difficulties, the work was discontinued at the time and not taken up again until the spring of 1909, when it was continued in the new laboratory with the apparatus and arrangements already described.

In the first series undertaken under these new conditions the 13 mm. aperture through which the light entered the dark room was left uncovered so that the ray from the lantern was projected directly into the eye of the observer during the passage of the open sectors of the disc. In the following table, *W* represents the width in degrees of the wider open sector, *N* that of the narrower sector. The figures in the

[1] According to an unpublished paper written at the time by Mr Burt, who has kindly placed his records of these experiments at our disposal.

next column represent the number of times which the flashes corresponding to N and W respectively were judged the brighter. For convenience of comparison, they have been reduced to percentages, the actual number of judgments made at each stage varying from 6 (where the difference of brightness was easily perceptible) to 45 (where it could only be distinguished with difficulty).

Observer: Flügel. 1 *rev. of disc in* 2 *secs.*

N	7°	0 °/₀	N	12°	54·5 °/₀
W	10°	100	W	16°	45·5
N	8°	20	N	12°	69
W	11°	80	W	17°	31
N	9°	0	N	13°	73·1
W	12°	100	W	17½°	26·9
N	10°	27·3	N	14°	58·3
W	13½°	72·7	W	19°	41·7
N	11°	14·3	N	15°	78·6
W	15°	85·7	W	20°	21·4

In interpreting the figures of this and the following series, we have been guided mainly by the following assumptions:

(1) That so long as there is a large predominance of the number of times W is judged the brighter, W gives an exposure that is below action-time.

(2) That when this predominance in favour of W is considerably reduced or altogether abolished, W gives an exposure that is already longer than action-time, since in this case the decline in the intensity of the sensation (that sets in as soon as the stimulus is prolonged beyond action-time) will already have begun, and, by reducing the intensity of the sensation corresponding to W, will tend to render it indistinguishable as regards brightness from that corresponding to N, which latter gives an exposure that is still slightly less than action-time. In these cases action-time is therefore considered to fall between the duration of N and that of W, being slightly longer than the former and shorter than the latter.

(3) That, when there begins to be a distinct predominance of the number of times N is judged the brighter, the duration of N corresponds approximately to action-time; since, if this is the case, the duration of W will be considerably longer than action-time, and the sensation given by W will therefore be considerably less intense than that given by N which is at its maximal intensity. In this series there takes place a sudden reversal of judgment when N is at 12° and W at 16° W which had hitherto appeared brighter now appearing duller,

which shows that 12° is nearer action-time than 16°. The beginning of uncertainty at 10°—13½° would seem to indicate that 13½° gave an exposure already slightly above action-time. This is confirmed by the judgments at 13°—17° where *N* appears distinctly brighter for the first time (so long as *N* and *W* are kept approximately in the proportion of 3 to 4) from which we might conclude that 13° corresponds to action-time. But the results obtained at 11°—15° where *W* is still distinctly the brighter and at 14°—19° where the preponderance of *N* judgments (if we may so call the judgments that *N* is brighter than *W*) is not great, seem to point to a somewhat higher action-time. Taking the series as a whole, we seem justified in considering the action-time of light of this intensity as somewhere between 13° and 14½°, *i.e.* about 71—80$_\sigma$[1].

[1] At the same time as the above series, a set of observations under precisely similar conditions was made by another subject. The results of this series are totally unlike those of any other we have obtained, inasmuch as the flash of longer duration was almost invariably judged the brighter, whatever the actual duration of the flash or the relation of *N* to *W*. It would seem that there are two ways of explaining this; either this subject has an action-time for light of this intensity that is very much longer than that of our other observers or his judgments were not determined in the same way as theirs, *i.e.* we may suppose that the difference between the results obtained with this subject and those with other subjects depends upon sensational or perceptual factors. The latter view seems more likely to be correct. It is *a priori* improbable that there exist under normal conditions very large individual differences in such a fundamental matter as the action-time of light, and our work together with that of other investigators has (so far as we are aware) elsewhere failed to shew any such considerable differences. We seem justified then in provisionally accepting the second explanation, though it is difficult to say just in what the perceptual difference consists. At first it was thought that the subject might be judging by the relative duration rather than by the relative brightness of the two flashes, but the results of some experiments made to test this view, seem to shew that there was no confusion between brightness and duration as long as the attention was specially directed to the detection of differences of duration between the two flashes. The few observations we were able to make on this point do not prove, however, that these differences of duration exerted no influence on the estimation of brightness when the attention was directed to the latter, and indeed it seems to us probable that it is in some influence of this kind that the explanation of the anomalous results obtained with this subject must be sought. We are led to this opinion by comparing these results with observations made by ourselves in the course of these and subsequent experiments. During the first few observations made after a long interval of rest, there is often a strong tendency to judge the longer flash as such to be the brighter also, and this tendency often reasserts itself as soon as the subject has become fatigued by an unduly prolonged sitting. One cause of this phenomenon is not far to seek. The full appreciation of the brightness of the shorter flash requires a more delicate adjustment of attention and accommodation than that needed for the appreciation of the longer flash, and consequently is apt to be improved as the subject becomes used to the general conditions of the experiment and more particularly to the exact length of the interval between the flashes, and to fall off

In our next series the source of illumination and general conditions were the same as before except that a piece of white paper of moderate thickness was fixed over the inner of the two apertures through which the light passed into the dark room. This considerably reduces the intensity of the stimulus (probably by about 40°/₀) and has the advantage that the apparent brightness of the circle of light is not subject to be affected by slight changes of the position of the eye. At the same time the interval between the two flashes was increased by causing the disc to make 1 revolution in 3 seconds, instead of 1 in 2. The following table gives the number of times per cent. which

again as the subject becomes tired and his attention relaxes. But this is perhaps not the only factor at work. It seems probable that there is a real tendency to confuse brightness with duration under the conditions of our experiments. Duration and brightness tend to fuse into a kind of massiveness, which is greater in the case of the longer flash and which by an unpractised or tired subject is easily mistaken for brightness alone. These considerations incline us to believe that inability to distinguish adequately between the brightness and duration of the two flashes was at the bottom of the results obtained with this last observer, although there is no means of shewing definitely that this was the case.

It is perhaps worth while, however, to raise one other consideration as to the influences which may possibly have been at work here. In the case of visual stimuli with duration over action-time, the sensation aroused is of varying intensity at different moments. None of the subjects was aware of any period of rising intensity of sensation, *i.e.* the sensation seems to attain its full intensity instantaneously. It may be that this is actually the case or that there is a very brief period during which the sensation-intensity rises from zero to its maximum, but that the whole process is too rapid to be apprehended. Be this as it may, the experiments recorded in the next section show beyond doubt that the fall in intensity when the stimulus is prolonged beyond action-time, takes place more gradually; the actual intensity of the sensation at different moments of its existence may, then, be represented by a curve which rises quite or almost perpendicularly, so that its highest point is attained immediately, and which soon afterwards begins to fall and continues to do so, though at a comparatively slow rate, for a considerable period. When a sensation caused by a light exposed during its action-time only is compared with a sensation caused by a light of the same intensity but exposed longer than its action-time, it is as if the curve in the first case came to an end at its highest point, while in the second case it ends only after it has declined appreciably from this point. It is obvious that if the sensation in the first case is to seem brighter than that in the second, our judgment of the brightness of the latter must be based upon its intensity in the last moments of its existence rather than in the first, for in its early stages, the intensity of the sensation corresponding to the longer stimulus is presumably the same as that corresponding to the shorter. Here, then, in the influence of the different moments during which a sensation lasts upon the judgment of the brightness of the sensation, we have a factor which may possibly account for certain individual peculiarities such as that before us. In the case of this observer, it is conceivable that the early stages of the sensation exerted a greater influence on the resulting total impression than was the case with other subjects. It was, however, not possible to obtain any introspective data bearing upon this point, which must therefore remain a pure conjecture.

N and W respectively corresponded to the apparently brighter sensation.

Observer: McDougall. 1 *rev. in* 3 *secs.*

N	9°	27·3 %	N	12°	31·6 %
W	12°	72·7	W	16°	68·4
N	10°	27·7	N	13°	70·8
W	13½°	72·3	W	17½°	29·2
N	11°	8·3	N	14°	58·3
W	15°	91·7	W	19°	41·7
			N	15°	100
			W	20°	0·0

Interpreting these results in the same way as those of the earlier series, the action-time of light of this intensity would seem to correspond to the passage of an open sector of 15°, *i.e.* would be 125_σ.

At the same time another series was made under precisely similar conditions which gave the following results.

Observer: Flügel. 1 *rev. in* 3 *secs.*

N	6°	0 %	N	9¾°	33·3 %	N	13¼°	33·3 %
W	8°	100	W	13°	66·7	W	18°	66·7
N	6¾°	0	N	10½°	43	N	14¼°	66·7
W	9°	100	W	14°	57	W	19°	33·3
N	7½°	31·2	N	11¼°	41·2	N	15°	66·7
W	10°	68·8	W	15°	58·8	W	20°	33·3
N	8¼°	22·2	N	12°	81·8	N	15¾°	88·9
W	11°	77·8	W	16°	18·2	W	21°	11·1
N	9°	30	N	12¾°	40			
W	12°	70	W	17°	60			

This series is less satisfactory than the preceding one, this being perhaps largely due to the fact that the subject had before been working with the disc revolving once in 2 secs., and had not yet become thoroughly accustomed to the longer interval between the flashes, but, such as it is, it is fairly consistent with the preceding series, showing if anything a somewhat shorter action-time.

After these observations had been made, experiments on the difference-threshold for brightness as measured in terms of the duration of the stimulus showed that it was possible to distinguish a difference of intensity of sensation when the difference of duration of the stimuli was considerably smaller than that we had so far used in our observations on action-time; therefore, as the determination of action-time must be the more exact the less is the difference of duration of N and W, we made a new series of observations, keeping N and W more nearly equal in width than we had hitherto done. The source of illumination and general arrangements were the same as in the two preceding series; the intensity of the stimulus, however, was slightly increased by removing the piece of millboard that covered the aperture in the wall of the dark room on its outer side. In these series 18 judgments were given at each position of W and the figures are therefore given in full. In the first of the two following series N and W were kept in the proportion of 9 to 10, in the second in the proportion of 5 to 6.

Observer: McDougall. 1 *rev. in* 3 *secs.*

N	9·9°	2 %	*N*	12·6°	3 %	*N*	15·3°	13 %
W	11°	16	*W*	14°	15	*W*	17°	5
N	10·8°	3	*N*	13·5°	11	*N*	16·2°	15
W	12°	15	*W*	15°	7	*W*	18°	3
N	11·7°	2	*N*	14·4°	8			
W	13°	16	*W*	16°	10			

Since the greater brightness of W was distinct when the duration of W was 14°, and was no longer perceptible when duration of W was 15°, we may infer with some confidence that the action-time of the light for this subject lay between the durations 14° and 15°, *i.e.* between 117_σ and 125_σ. This series must be regarded as yielding the most accurate determination of the action-time of all that we have made.

Observer: Flügel. 1 *rev. in* 3 *secs.*

N	10°	4 %	*N*	10½°	3 %	*N*	11⅔°	12 %	*N*	12½°	13 %
W	12°	14	*W*	13°	15	*W*	14°	6	*W*	15°	1

This series again shows a somewhat shorter action-time for Flügel than for McDougall, the action-time in this case apparently being about 104_σ (12½°).

Later, after we had completed the experiments described in the next section, each observer made another series to determine the action-time of light of high intensity. The source of illumination was an arc-lamp, the conditions being otherwise unchanged.

The following table shows the results obtained.

Observer: McDougall. Arc Lamp.

N	3°	0 %	*N*	6°	40 %	*N*	9°	100 %
W	4°	100	*W*	8°	60	*W*	12°	0
N	4°	0	*N*	7°	55	*N*	10°	100
W	5½°	100	*W*	9½°	45	*W*	13½°	0
N	5°	35	*N*	8°	66·7			
W	7°	65	*W*	11°	33·3			

Observer: Flügel.

N	3°	16·7 °/₀	*N*	4½°	33·4 °/₀	*N*	6°	83·3 °/₀
W	4°	83·3	*W*	6°	66·6	*W*	8°	16·7
N	3¾°	30·8	*N*	5¼°	50	*N*	7½°	100
W	5°	69·2	*W*	7°	50	*W*	10°	0

The first of these two tables seems to indicate an action-time corresponding to 7° or 58_{σ}. The second table seems to show for Flügel again a somewhat shorter action-time than for McDougall, perhaps about 6° or 50_{σ}.

Taking the observations on action-time as a whole, it will be seen that they are in very fair agreement with each other and with those reported in the earlier paper, the action-time being shorter the more intense the stimulus. An interesting individual difference is brought out by comparing the results obtained with the two observers with stimuli of the same intensity. In all cases the action-time for Flügel seems to be slightly shorter than that for McDougall. It would be interesting to know whether such individual differences in the time required for a visual stimulus to develop its full effect in consciousness indicate real differences in sensation or are of more central origin. Various possible explanations of these differences have been considered in the note on p. 183 in connection with the results obtained with another observer.

II. *The Decline in Intensity of Sensation when the Duration of the Stimulus exceeds Action-time.*

That a visual sensation declines appreciably in intensity soon after it has reached its maximum was assumed by Helmholtz and Exner, though subsequently denied by Martius. Charpentier, however, seems to have demonstrated the existence of such decline by the simple experiment of rotating a dark disc with an open sector at the rate of about 30 revolutions a minute before an evenly illuminated surface. The band of light then seen (if the eye is held motionless) is not of the same intensity throughout its length, its leading part being appreciably brighter than the succeeding parts. The existence of this decline is of course also shown in the preceding observations, as well as in all those recorded in the earlier paper, by the fact that, when the duration of the stimulus exceeds action-time, the shorter stimulus (*i.e.* that corresponding to the passage of the narrower open sector) gave a sensation that was judged to be more intense than that evoked by the longer stimulus. It was also shown by some observations in the former article (p. 180) that by means of the apparatus used for the investigation of action-time, it is possible to measure the rate of this decline with some degree of accuracy. It was therefore determined to devote a number of experiments to this problem.

The first attempt was made in 1907 by Mr Burt and one of the writers at the same time as the observations recorded on p. 181 of this article. The procedure adopted in this case[1] was to fix W at widths successively greater by one degree than the width representing the estimated action-time, and at each step to determine what width of N yielded a sensation indistinguishable from that yielded by W; the psycho-physical method employed being the combination of the methods of minimal changes and of right and wrong cases used in the determination of action-time.

The following table copied from Mr Burt's paper gives the results obtained, the figures on the right indicating the widths of N which gave sensations judged to be of the same intensity as those given by the opposite widths of W.

Observers: Burt and Flügel. 1 rev. in 2 secs.

W N	W N	W N	W N	W N
$10° = 10°$	$12° = 9°$	$15° = 7°$	$25° = 5°$	$45° = 4°$
$11° = 10°$	$13° = 8°$	$18° = 6°$	$30° = 4{\cdot}5°$	

Estimation of the results of this and of our subsequent experiments on this point is made easier if we assume, as we seem justified in doing, both from actual observations reported in the former article (pp. 173 ff.) and because it seems a necessary consequence of the Talbot-Plateau law, that a stimulus exposed for a certain fraction of its action-time and the same stimulus reduced by the same fraction of its *intensity*, but exposed for its full action-time, will give sensations of equal brightness, *e.g.* that a stimulus L exposed for $\frac{1}{2}$ its action-time will give a sensation of the same brightness as $\frac{1}{2}L$ exposed for its full action-time. If this is so, when we have found the width of open sector that corresponds to the action-time of the light used, by reducing this width by $\frac{1}{2}$ we at the same time reduce the intensity of the sensation to that of the sensation evoked by a stimulus of $\frac{1}{2}$ the intensity exposed for its full action-time. Thus in our procedure, when we have found a width of W that evokes a sensation judged to be of the same brightness as that evoked by N, when N gives an exposure of (say) $\frac{1}{2}$ the action-time of the light used, we may infer that the

[1] For the account of these experiments, we are indebted to the above mentioned paper by Mr Burt.

sensation evoked by N, and therefore also that evoked by W is equal in intensity to that which would be obtained if we reduced our stimulus to $\frac{1}{2}$ its intensity and then exposed it for its full action-time. By this means we obtain a measure of the rate of decline in intensity of the sensation, when the stimulus is prolonged beyond its action-time. Thus in the above series we see that W at 25° corresponds to N at 5° and (assuming action-time to be equal to the time of passage of an open sector of 10°) we can therefore infer that in the difference between the time of passage of 10° and that of 25° (*i.e.* about 83_σ) the intensity of the sensation has declined to that of the sensation which would be evoked by a stimulus of $\frac{1}{2}$ the intensity acting for its full action-time, or, expressing the intensity of the sensation in terms of stimulus intensity, we may say that it has declined by 50 °/₀.

Further experiments on this point were begun in the new laboratory at the same time as the observations on action-time reported on p. 182. The source of light and arrangements were the same as in these experiments, the light from the projection lantern falling directly on the eye of the subject and the disc revolving once in 2 secs. After some preliminary experiments it was decided to abandon the method employed in the earlier series in favour of the method of right and wrong cases. In the experiments on action-time the differences in duration between the passage of N and that of W, although as a rule discernible when the attention was specially directed to them, were not such as to interfere seriously with the judgment as to the relative brightness of the two flashes (except perhaps in the case of the one subject referred to in the note above). In the present experiments when increasingly wider *Ws* had to be compared with increasingly narrower *Ns*, the difference of duration soon became quite unmistakable, and were indeed much more striking than the differences of brightness. The subject being moreover acquainted with the results of the earlier experiments on the decline of intensity beyond action-time, it was found impossible to obtain unbiassed judgments, the estimation of the brightness of W being largely based on the amount of the perceived difference of duration between W and N. It was found possible to avoid this difficulty, however, by keeping W constant and altering the width of N after each judgment, but only within narrow limits around the point which previous rough experiments had shown to be likely to give a sensation equal to that given by W. When this procedure was adopted, the subject, although he could of course easily distinguish the difference of duration between W and N, could not distinguish such differences between the individual *Ns*, which varied every time; he had consequently to base his judgment on brightness alone, since he had no longer any means of knowing whether any particular *N* was just above or just below the duration which he expected to give a sensation of the same intensity as that given by *W*.

To show what degree of accuracy is possible in this kind of comparison after a small amount of practice, we give the figures of this first series in full. In the following tables the first column gives the width (in degrees) of *N*, and the figures in the columns marked *B*, *D* and *S* refer to the number of times that the corresponding *N* was judged brighter, darker, or of the same brightness respectively as the *W* with which it was compared.

Observer: Flügel. 1 *rev. in* 2 *secs.*

W =	17°			18°			19°			22°		
	B	*D*	*S*	*B*	*D*	*S*	*B*	*D*	*S*	*B*	*D*	*S*
N = 12°	5	2	3									
11°	5	2	3									
10°	6	3	1	6	2	2						
9°	3	4	3	8	0	2	8	0	2			
8°	3	5	2	5	2	3	4	1	5	9	0	1
7°				3	3	4	4	2	4	5	1	4
6°				2	6	2	3	5	2	8	1	1
5°							1	7	2	2	4	4
4°										0	8	2

It will be seen that *W* at 17° corresponds (as regards brightness of sensation evoked by it) to a width of *N* of between 9° and 10°, that 18° corresponds to 7°, 19° falls between 6° and 7°, and 22° between 5° and 6°. Taking the action-time of light of this intensity (p. 183) as somewhere between 13° and $14\frac{1}{2}$°, or probably about 75_σ, we see that in 19_σ (*i.e.* the duration of the passage of 17° or 94_σ *less* 75_σ) it has fallen about 29 °/ₒ in 25_σ (18°) about 48 °/ₒ, in 30_σ (19°) about 52 °/ₒ, and in 47_σ (22°) about 59 °/ₒ.

In these figures we are of course expressing sensation-intensity in terms of stimulus intensity, *i.e.* when we say that the sensation has declined in intensity by a certain amount, we mean that it is judged equal in intensity to the sensation evoked when the intensity of the stimulus is diminished by this amount. Further, since it is impossible to discover the *exact* action-time of the light used, the times given are of course only approximately correct.

A further series of observations with both of us as subjects, was afterwards made under the same conditions as the experiments on action-time reported on p. 185, the intensity of the light being reduced by placing a sheet of white paper over the aperture through which the light passed into the dark room, and the rate of rotation of the disc being 1 revolution in 3 secs. The method employed was the same as that of the preceding series. The following are the results obtained, a fairly large number of judgments at different widths of *N* being recorded at each position of *W*.

Observer: McDougall.

Width of *W*		Width of *N*
18°	=	9°
20°	=	10—11°
25°	=	8—7°
30°	=	8—7°
35°	=	8—7°
45°	=	8—7°

Observer: Flügel.

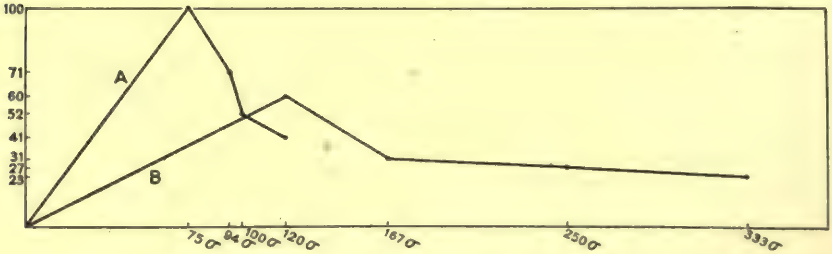
Width of *W*		Width of *N*
20°	=	8—7°
25°	=	8—7°
30°	=	7—6°
40°	=	6—5°

It will be noticed that the results obtained with one subject (McDougall) at the first two widths of *W* are not consistent with each other, 20° being apparently brighter than 18°. It is probable, however, that 9° degrees given as corresponding to 18° is really too low an estimate, since although the number of 'brighter' judgments were equally divided between *W* and *N* when the latter was at 9°, the predominance of judgments in favour of *N* at 10° and 11° is very small, whereas *W* appeared very distinctly brighter than *N* when the latter was at 8°. The two series made with *W* at 18° and 20° respectively, are on the whole less satisfactory than those made with a greater width of *W*, the exact turning-point at which *W*, from being brighter, becomes darker, being less well-marked. This of course makes the estimation of the exact width of *N* that corresponds to these widths of *W* more difficult. It is clear, however, that this series is in the main in agreement with the earlier series, in so far as it shows that very soon after a sensation has attained its maximum, there takes place an astonishingly rapid decline in its intensity which soon, however, becomes very much slower or ceases altogether.

If we accept the action-time of this light for the first observer (McDougall) as equal to the passage of an open sector of 15° or 125_σ (p. 185), we see that in 83_σ (25°, or 208_σ *less* action-time or 125_σ) the sensation has declined in intensity by about 50 °/₀, and thereafter ceases to decline appreciably.

The determination of the action-time for the second observer (Flügel) was not very satisfactory. It seemed, however, to be, if anything, a little shorter than that for McDougall. If we take it to be about 120_σ, then in 47_σ (*W* at 20°) the sensation appears to have declined by about 48 °/₀, though the fact that in 88_σ (25°) no further decline is recorded, seems to show that this figure is rather too high. In 130_σ (30°) it has declined 55 °/₀ and in 213_σ (40°) about 62 °/₀.

There is considerable difference between the results obtained from the two observers, the decline in the case of Flügel being considerably greater than that in the case of McDougall. The apparently more rapid fall at the beginning may possibly be due to an over-estimation of the action-time of the former observer, but this cannot account for the fact that in the one case (Flügel) the intensity of the sensation seems to decline continuously with increasing widths of *W* up to 40°, while in the other (McDougall) there is no indication of an appreciable decrease after 25°. Whether real individual differences in the decline of the sensation are at the bottom of these results, or whether the differences are due to some varying influences of the different intensity of the sensation at different moments upon the total impression of brightness, such as was suggested in the note above, or whether they are due to mere accidental errors of observation, is an interesting question, which may perhaps be decided by further experiment. The data before us are not, however, sufficient to enable us to decide this point at present.



In the accompanying diagram we have tried to represent the varying intensity of the sensations resulting from different durations of the stimulus for two lights of different intensity. The figures along the abscissae represent the durations of the stimulus in thousandths of a second, and the height of the curves the corresponding intensities of the sensations evoked. The curve *A* represents the results obtained from the series recorded on pp. 182, 190, the curve *B* those for the series on pp. 185, 191 (the intensity of the stimulus in the latter case being less than in the former by about 40 °/₀). It must be noted, however, (1) that these curves do not (at least in their rising portions) represent the actual course of the sensation, but only the intensities of the sensations resulting from particular durations of the stimulus; (2) that the intensity of the sensation is here measured in terms of the intensity of the stimulus in the way described in p. 188. The rising portions of the curves are straight lines, since when the duration of the light is less than its action-time, the intensity of the sensation measured in this way is directly proportional to the duration of the stimulus. It will be seen that the curve corresponding to the sensation evoked by the more intense stimulus both rises and falls more rapidly than that corresponding to the sensation evoked by the less intense stimulus, the difference between the two being very striking[1].

III. In this section we report a number of experiments devoted to the investigation of the difference-threshold for brightness with lights exposed for less than their action-time, the threshold being measured in terms of the duration of the stimulus. This investigation promised to be of interest, because (1) by means of the apparatus used by us it was possible to determine the threshold of discrimination in the case of stimuli successively applied, whereas all former investigators of the difference-threshold for brightness had, so far as we knew, worked with simultaneous stimuli; (2) since our apparatus rendered possible the presentation of simultaneous as well as successive stimuli, it seemed that we should be able to compare with each other thresholds for simultaneous and successive presentations obtained with the same observer and under conditions otherwise similar. As regards the first consideration, it seemed to us that the objections raised in the earlier paper against Exner's procedure in his experiments on action-time (p. 155) (viz. that to present simultaneously and side by side the two fields to be compared is unsatisfactory, (1) physiologically, as it gives rise to contrast effects; (2) psychologically, because it is easier to compare two successive than two simultaneous impressions), hold good also against methods of investigating the brightness difference-threshold by means of simultaneous presentations. By comparing thresholds obtained with simultaneous and with successive presentations, it seemed likely that we should be able to form some estimate as to how far these objections were valid.

[1] It will be noticed that the first section of the decline of curve *A* is less steep than the second section. It seems probable that in this respect our curve fails to give a true picture of the facts and that the summit of the curve should be rounded rather than sharply peaked.

For our first observations on the difference-threshold for successive presentations, the conditions were similar to those of our other experiments, the rate of rotation of the disc being one revolution in three seconds. In the first series, in which McDougall was observer, W was fixed at 12° and N at 8°. N was then increased in steps of $\frac{1}{2}^\circ$ while W remained at 12°, ten judgments being recorded at each step. It was found that the difference in brightness between the flashes was almost invariably recorded correctly (*i.e.* the flashes corresponding to W were judged the brighter) when N stood at $11\frac{1}{2}^\circ$. When N was increased to $11\frac{3}{4}^\circ$, however, it could no longer be distinguished from W. Thus with W at 12° a difference of $\frac{1}{2}^\circ$ (or a little over 4_σ) still gave a just perceptible difference of brightness, *i.e.* the difference-threshold on this point (100_σ) corresponded to a deviation from the standard of about $\frac{1}{25}$ of its duration.

In our next series, in which McDougall was again observer, the width of W was increased to 15°, giving an exposure of 125_σ, this being about the action-time of the light used (it is probable, however, that the action-time was a little longer than that recorded on p. 185, as the lamp was a little further from the disc, and the intensity of the stimulus therefore slightly reduced). On gradually increasing the width of N as in the former series, it was found that W still gave a perceptibly brighter flash than N when the latter stood at $14\frac{1}{4}^\circ$, beyond which W and N could not be distinguished. This gives a threshold of about $\frac{1}{20}$.

On Flügel repeating these observations with W at 15°, however, a considerably lower threshold was found, W and N still giving impressions that were just perceptibly different when N was at $14\frac{3}{4}^\circ$, thus indicating a threshold of about $\frac{1}{60}$. Flügel was, however, of opinion that he was aided in this series by perceiving not only differences of brightness, but also differences of duration between the two flashes, whereas McDougall had professed inability to distinguish any differences of duration when N and W were so nearly equal in width as in these observations. In order to investigate the power of discriminating short differences of duration under the conditions of our experiments, a short series was made with each of us with this object specially in view, the subject directing his attention to the duration of the flashes and neglecting as far as possible any differences of brightness. It was found easy to set the attention for differences of duration or of brightness as desired. The results to some extent bore out the introspections made during the previous series, Flügel displaying a slightly more delicate discrimination of small differences of duration than McDougall. The former was able to discriminate a time difference of ½° without difficulty, and gave 70 °/₀ of correct answers with a difference of ¼°, whereas the latter gave 69 °/₀ of correct answers with a difference of ½°, but was apparently unable to distinguish at all a difference of ¼°. Thus it is possible that this apparently lower threshold for differences of brightness on the part of Flügel was at least partly due to a confusion of brightness with duration, since the power of discriminating differences of duration between the flashes is at least as great as that of discriminating differences of brightness[1]. From the point of view of our experiments it is of course unfortunate that this should be the case, since the possibility of mistaking longer duration for greater brightness must render the interpretation of our results less certain than it would otherwise be; and we saw above that some such mistake was probably the cause of our failure to get any useful results at all from one of our subjects. From the result of our other experiments, however, it would seem that there is as a rule but little liability to this confusion on the part of well practised and unfatigued subjects, except perhaps (as in the case of Flügel above) when the differences in brightness are so small as to be only just perceptible.

The following observations were made after an interval of about three months, the conditions of the experiments being unchanged except that the subject was placed with his eye 1½ m. from the illuminated surface, and that he worked binocularly instead of monocularly, as in all the earlier experiments. W being fixed at 10° and N at 5°, N was widened by steps of 1° (a number of observations with N at 9½° however being made when necessary) until it could no longer be distinguished from W, from 15 to 20 judgments being recorded at each step. Under these conditions, the first subject (Flügel) gave 87 °/₀ of correct judgments when N was at 8° and 68 °/₀ with N at 9°, breaking down completely with N at 9½°. The other subject (McDougall) gave 70 °/₀ of correct judgments with N at 9° and 65 °/₀ at 9½°. In order to verify the operation of Weber's law by our method, an episkotister with an open sector of 180° was then placed in the path of the ray from the lantern and rotated rapidly by means of an electric motor. The intensity of the stimulus being thus reduced by half, the series of observations was repeated with both subjects under precisely similar

[1] This is rendered the more probable by the fact that in the later series recorded below Flügel exhibited a much higher threshold and that in these series he was unaware of being helped in any way by the perception of any difference of duration.

conditions. In this new series the threshold for McDougall was about the same as that of the former series, there being 80 °/₀ of correct judgments with N at 9° and 60 °/₀ at 9½°. Flügel, however, contrary to expectation, exhibited a threshold considerably lower than that obtained with the brighter light, there being no mistakes at all with N at 9° and 75 °/₀ of correct judgments at 9½°. During the next series the episkotister was removed and W reduced from 10° to 5°. Assuming as we did in the preceding section and as we are justified in doing by the observations reported in the former article that sensation of the same intensity may be evoked by exposing the full stimulus for half its action-time and by exposing a stimulus of half the intensity for its full action-time, the intensity of the sensation in the present series ought to be very nearly the same as in the last series (in reality probably a little greater, since, as the action-time increases with diminishing intensity of the light, the brighter light would have attained a greater proportion of its maximal intensity than the duller light), and we should therefore expect the same increment of the total duration to give a just perceptible difference as in the experiments with W at 10°. This expectation was justified by the results obtained. It was found that a difference of ¼° was now distinguished with about the same ease as ½° in the earlier experiments. With N at 4¾° McDougall gave 85 °/₀ of correct answers and Flügel 65 °/₀, the rather low figure in the latter case being probably due to the fact that the subject was somewhat fatigued during the latter part of the experiment. These experiments seem thus to afford further evidence of our assumption that, as regards the intensity of the sensation, it is the same whether we diminish the action-time or the intensity of the stimulus in a given proportion; since the results obtained are just what we should expect from the operation of Weber's law if our assumption were correct.

One further series of observations bearing on Weber's law was undertaken with each subject. For this series W was fixed at 10° as in the earlier experiments, and the episkotister was replaced in position and adjusted so as to have an open sector of 90°, the stimulus thus being reduced to ¼ its original intensity. Under these conditions, McDougall gave 70 °/₀ and Flügel 75 °/₀ of correct judgments with N at 9½°, this again being in accordance with Weber's law.

To facilitate examination of the results of the last four series, we have tabulated them as follows, the figures representing the percentage of correct judgments made by each subject at the corresponding width of N.

Width of W	Width of N	McDougall	Flügel	
10°	9°	70 %	68 %	Full intensity
	9½°	65	43	
5°	4½°	100	86	Full intensity
	4¾°	85	65	
10°	9°	80	100	½ intensity
	9½°	60	75	
10°	9°	100	100	¼ intensity
	9½°	70	75	

It will be noticed that the figures obtained with Flügel at the lower intensities are all distinctly superior to those obtained at the full intensity, this of course seeming to indicate a departure from Weber's law. Thinking that this might perhaps be due to some effect of practice or to some temporary disability on the occasions of the observations made with the light of full intensity, we made a similar series of observations with the full intensity soon after the last series with ¼ intensity. The results, however, agreed with those already obtained, there being only 60 % of correct judgments with N at 9°. We were thus driven to the conclusion that the intensity of the light used by us when exposed for 83_σ (*i.e.* W at 10°) is, at any rate, for this subject, above the limits within which Weber's law holds strictly; and that for the other subject the upper limit of the range of intensity of stimulus within which Weber's law holds good is distinctly higher.

IV. Having thus roughly determined the difference-threshold for brightness with successive presentations, we proceeded to investigate the same threshold with simultaneous presentations. For this purpose we made use for the first time of the inner open sector of our disc, by means of which, together with the adjoining outer sector, two simultaneous flashes of independently adjustable duration could be given. The other outer sector was closed, so that we had now a double flash once every 3 secs. instead of a single flash once every 1½ secs. In order that the light passing through the two adjustable open sectors might be confined to separate areas, the single circle 13 mm. in diameter, through which the light entered the dark room, was replaced by two circular openings of the same size, separated at their nearest point by a distance of 5 mm. At the same time the subject retired to a distance of 1½ m., in order that the images of both areas might fall upon the fovea. The procedure was the same as that adopted for the investigation of the threshold with successive exposures, both open sectors, however, being frequently readjusted so that sometimes the inner, sometimes the outer open sector was the wider of the two, in order that the subject might not know on which side to expect the longer flash. The open sectors of the disc were so adjusted that both flashes began at the same moment.

A very short series of observations sufficed to show that with this procedure the threshold was much higher than when the two flashes were presented successively. Keeping one of the two sectors at 15° in each observation, it was found that the judgments became uncertain when there was a difference of 3° (25_σ), and that the flashes practically ceased to be distinguishable when the difference was reduced to 2° (16_σ), the results obtained from both of us agreeing very closely. Thus, if we may conclude from the results of the first two series recorded in the last section that, when the two flashes to be compared were given successively, a difference of brightness was still just perceptible with W at 15° and N just over 14°, it would seem that the difference-threshold for brightness with simultaneous presentations was, under these conditions, about three times as high as that with successive presentations.

That the threshold determined by the latter method is very considerably lower than that obtained by the former, is borne out by two further series undertaken with each of us, in which W was adjusted at 10°. In the first of these two series McDougall appeared to be just able to perceive a difference of brightness when N was at 7°, Flügel when N was at 8°. In the second series the results obtained from both observers agreed very closely in showing that there was a just perceptible difference of brightness with N at 8°. With light of the same intensity, but with successive instead of simultaneous flashes, 8° had been shown to be very easily distinguishable from 10°, the threshold in this case appearing to be reached with one subject (Flügel) when N was at 9°, with the other subject (McDougall) when N was at 9½°. These further observations seem to show that the difference-threshold with simultaneous flashes is at least twice as high as that with successive flashes. It thus seems fairly clear that the power of discrimination of differences of brightness, according to the procedure adopted by us in this case, is much greater when the lights to be compared are presented successively than when they are presented simultaneously. We have here for the first time a rough measure of the superiority of delicacy of comparison of successively given impressions, as compared with the delicacy of comparison of simultaneously given impressions. And we venture to think that this measure is of some interest, if only because, so far as we can see, such quantitative comparison of the delicacy of simultaneous and successive comparison can be made by no other method than the one we have used.

It is interesting to compare our results with those recently obtained by another method by Dr R. Stigler[1]. The principal object of Stigler's research was the same as that of our own observations recorded in this and in the preceding section, viz. the difference-threshold with stimuli acting on the retina for less than their action-time. But before devoting himself to this problem, Stigler conducted a number of experiments with the aim of discovering the action-time of the light he used. His procedure in both sets of experiments, those on action-time and those on the difference-threshold, was the same, viz. to present to the observer two small semicircular areas of light, separated by a narrow dark line, the duration of the bright semicircles being independently variable by means of an arrangement of discs with open sectors through which the light passed. The longer stimulus was made to begin before the shorter, both stimuli always ceasing at the same moment. The source of illumination was an incandescent gas burner, but, since before reaching the discs the light passed through a slit ·214 mm. in width (being then focussed at the required distance by means of a lens), the absolute intensity of the stimulus used was very small, no doubt considerably below that used by ourselves.

It is therefore to be expected that the action-time of this light would be considerably longer than that of our own. Stigler found, however, when using the smallest practicable differences between the duration of the two bright fields ($16—20_\sigma$), that with exposures up to 680_σ, and with one subject even up to 1333_σ, the longer flash, when at all distinguishable from the shorter, was nearly always judged the brighter of the two. Even when we take into consideration the low intensity of the light used, it is of course, as Stigler himself points out, impossible to suppose that action-time had not yet been reached, and we must therefore assume that some error of judgment incidental to the procedure adopted is at the bottom of these results. Stigler supposes that since the sensation corresponding to the longer stimulus attains its maximum intensity before that corresponding to the shorter (since in his procedure the longer stimulus begins before the shorter), the former sensation in some way exerts a preponderating influence on

[1] R. Stigler, "Über die Unterschiedsschwelle im aufsteigenden Teile einer Lichtempfindung." *Pflüger's Archiv für Physiologie*, June, 1908.

the judgment of comparison as long as the difference of duration between the two stimuli is small. When this difference of duration is not too small to produce any perceptible difference of brightness at all, "so beherrscht der erste Eindruck des seine Maximalhelligkeit früher erreichenden Lichtreizes das Vergleichsurteil des Beobachters so dass dieser nicht mehr imstande ist die Umkehr der Reizzustände beim gleichzeitigen Verschwinden der beiden Lichtreize wahrzunehmen."

In a later series, in which greater differences of duration were employed, the longer flash continued always to appear the brighter until the difference between the durations of the two flashes was 0·05 sec. When the difference was increased to 0·2 sec., however, the difference in brightness ceased to be easily perceptible, and, when still further increased to 0·4 sec., the expected reversal of intensity was at length observable, an exposure of 0·6 sec. appearing darker than one of 0·2 sec. The smallest difference in duration with which the shorter flash could be perceived as the brighter was 0·2 sec., and Stigler therefore concludes that this is the minimum difference necessary to overcome the effect of the apparent greater brightness of the stimulus corresponding to the sensation which first attains its maximum intensity, though the difference must be considerably increased before this effect can be invariably overcome.

This of course makes Stigler's method quite unsuitable for an accurate investigation of action-time. If his own explanation of his results is correct, the chief cause of the failure of his method is due to the fact that the longer stimulus was made to begin before the shorter, and might be avoided by arranging for both stimuli to begin at the same moment, but even then the objections against any simultaneous presentation of the stimuli (confirmed as they are by our own observations just recorded) would still hold good. Stigler's procedure, regarded as a method for the determination of action-time, suffers from the most serious defects of Exner's, besides introducing still greater difficulties of its own.

Turning to the results of Stigler's experiments on the difference-threshold with lights exposed for less than their action-time, it was found, as was to be expected, that the difference of duration necessary to produce a just perceptible difference of intensity is not an absolutely constant amount, but increases together with the absolute duration of the stimuli. This increase, however, so far as is shown by Stigler's observations, is not in exact conformity with Weber's law, the increment of duration necessary to produce a just perceptible difference of brightness varying irregularly from about 1/10th to about 1/35th of the actual duration.

This result does not agree exactly with the few observations we ourselves made on Weber's law, which produced results much more strictly in accordance with the law. It must be remembered, however, that Stigler worked with a light of much lower intensity than that used by us. Further, since Stigler's observations on this point were more numerous than our own, it is possible that a longer series on our own part would have brought results more in accordance with those of Stigler.

The most striking apparent disagreement between Stigler's results and our own is the lowness of the threshold obtained by Stigler compared with that obtained by ourselves with simultaneous stimuli, which apparently corresponded to an increment of between 1/5th and 1/8th of the actual duration (p. 198). An inspection of Stigler's thresholds reveals the fact that they bear a much closer resemblance to those of our own obtained with successive stimuli (about 1/20th) than to those obtained with simultaneous stimuli. Thinking that this disagreement must be due largely to the difference in the procedure adopted by Stigler and ourselves, we determined to make a short series of observations with conditions more nearly approaching those of the former observer. It was unfortunately impossible, with the apparatus at our disposal, to present two adjoining semicircles to be compared; it was, however, easy to arrange for the two stimuli to end instead of beginning together as hitherto, the longer stimulus thus always beginning before the shorter, just as in Stigler's procedure. The conditions were otherwise similar to those of our other observations with simultaneous presentations, W being fixed at 10°.

Under these circumstances a distinctly lower threshold was found than when both stimuli began together, McDougall apparently being able to perceive a difference of brightness when N was at $8\frac{1}{2}°$; for Flügel the difference being reduced to $\frac{1}{2}°$ ($N = 9\frac{1}{2}°$) before the just perceptible difference was reached. This series of observations, though not prolonged sufficiently to give very accurate results, shows unmistakably that the power of discrimination is increased when the longer stimulus is made to begin before the shorter. This result, together with Stigler's experiments on action-time, in which, according to Stigler's own conclusion, the earlier-beginning sensation in some way inhibited the later-beginning one and prevented the latter from attaining the intensity it would have done if it had been given alone, seems to show that there is a very strong contrast effect under these conditions. This contrast effect would no doubt be greater when the two stimuli were presented in the form of two closely adjoining semicircles as in Stigler's experiment, than when there were two distinct circles separated by a small space as in our own; and it would of course make itself felt by producing a lower threshold of discrimination, since it actually increases the difference between the sensations which are to be compared. There can, therefore, we think, be little doubt that to the existence of this strong contrast effect in Stigler's experiments is due the low difference-threshold obtained by Stigler and the apparent discrepancy between his results and our own.

Stigler's experiments, taken in conjunction with our own, seem to show that both the objections raised in the former paper and in the last section (p. 193) against the attempt to determine the action-time of light or the brightness difference-threshold by simultaneously presenting two adjoining areas to be compared are well founded. Our own experiments show also that perception of differences of brightness between successive impressions is very much easier than in the case of simultaneous impressions. This greater ease of comparison when the stimuli are presented successively must presumably give a threshold that is more closely determined by purely sensational factors and less complicated by small fluctuations of attention, etc.—at any rate, one that is more comparable in these respects to the difference-thresholds obtained for other senses, than is the threshold obtained with simultaneous presentations. As regards the other objection, it has been shown that under certain conditions, *e.g.* when the longer stimulus begins before the shorter, there exists a very strong contrast effect. How far this contrast effect is present when both stimuli begin together, our observations do not enable us to say, but the possibility of its presence must necessarily introduce into the interpretation of the results obtained by the simultaneous presentation method an uncertainty that is not present in the case of the method of successive presentation.

Lastly, it may be noted that these two defects, working as they do in different directions when present together, tend in some degree to cancel one another; for, when the two adjoining stimuli are presented together physiological contrast tends to increase the actual difference between the corresponding sensations, while in consequence of the greater difficulty of comparison, the power of discriminating between these sensations is diminished. This is brought out by a comparison of

Stigler's results with our own, Stigler's thresholds for simultaneous stimuli (obtained by a method which allowed full play to both disturbing factors) being, as we noted above, not unlike those obtained by ourselves for successive stimuli. Owing to this tendency of the two defects of the method of simultaneous presentation to neutralise one another, it might be possible to obtain by this method observations on brightness difference-threshold, action-time, decline of intensity when the stimulus is prolonged beyond action-time, etc., which would be in very fair agreement with our own.

We venture to think that the foregoing considerations justify the statement that the only method for determining the true threshold of discrimination of brightness is the one we have applied, and that therefore our results described above (p. 197) constitute the first true measure of this threshold. For it is clear that, when it is attempted to measure this threshold by presenting for discrimination two areas simultaneously (as in the case of Masson's disc), the difference of brightness is accentuated to an unknown degree by simultaneous contrast. And it seems clear that, since the brightness of visual sensation varies so widely with the duration of the stimulus (declining so rapidly from its maximum when the stimulus is prolonged beyond its action-time), the two stimuli to be discriminated must be allowed to act on the retina for a minimal time only. For the determination of the true threshold of discrimination, it is therefore necessary either to allow each of the two stimuli to act during a period just equal to its action-time, or to make them of the same intensity and, keeping the duration of action of both below action-time, to determine the threshold of discrimination in terms of difference of duration of their action (as we have done in the experiments reported above).

V. After the completion of the foregoing research, having a few days at our disposal for further investigation, we determined to make a few observations with lights of very low intensity affecting the rods of the retina only, in continuation of the observations reported on p. 181 of the former article. For this purpose the intensity of the light was cut down by means of an iris diaphragm, until it was only just visible to the dark-adapted eye of the subject stationed at the further end of the dark room. The small circle, 13 mm. in diameter, which had hitherto served as the bright field at which the subject looked, was replaced by a square, the side of which was 30 mm., and, in order to ensure the stimulus falling upon the peripheral part of the retina, the subject was provided with a small point of light to serve as a fixation point, situated at an angle of about 20° from the 30 mm. square.

Our first series of these observations with light of low intensity was undertaken with the object of repeating the observations on the action-time of light affecting the rods only recorded in the earlier paper (p. 183 ff.). The procedure was the same as that of these earlier experiments. *N* being completely closed, series of 20 flashes with widths of *W* of 45°, 35°, 30°, 25°, 20° and 15° respectively were presented, the width of *W* varying irregularly from series to series, but remaining constant within each series. The disc revolved continually during each series, but the beam of light was interrupted from time to time by the experimenter, so that the flashes were presented to the subject at irregular intervals. The subject was instructed to say 'Now!' whenever he was aware of a flash, and the number of times that the flash was seen in each series was noted by the experimenter. Working by this method, we may presume that the action-time has been reached as soon as an increase of the width of *W* fails to produce an increase in the proportion of flashes perceived to the total number given. The following table gives the results obtained with each of us, the figures representing the percentage of observed flashes at each width of *W*, the absolute number of flashes presented at each width varying from 40 to 100.

	Flügel	McDougall
$W = 10°$	40	13
20°	58	47
25°	67	51
30°	80	62
35°	80	75
45°	80	80

It will be noticed that with Flügel there is no increase in the visibility of the flash when *W* is increased beyond 30°. This gives an action-time in fair agreement with the previous determination (200_σ or a little more), being probably a little below 30° or 250_σ. McDougall, however, seems to show a distinctly higher action-time, possibly about 35° or 292_σ. Taken together, the results from the two observers indicate that the action-time of just perceptible light is well above 200_σ or $\frac{1}{5}$ sec. The possible bearing of these experiments on the principle of lighthouse flashes was referred to in the former article. It will be seen that the present experiments confirm the conclusion there arrived at, that the time usually accepted by engineers as sufficient for a light of very low intensity to develop its maximum sensation, viz. $\frac{1}{10}$ sec., is very much too short.

While considering the problem of lighthouse flashes, it occurred to one of us that the question whether two flashes given in rapid succession were in any way more easily and certainly perceptible than a single flash (of the same duration as each of the two other flashes) under the conditions of an observer on board ship striving to 'pick up' a distant light, would be an interesting subject of investigation. For this purpose we reduced the speed of the disc to one revolution in 9 secs., opened N to 10° and W to 45°. We then fastened over W a strip of mill-board, which completely covered the open sector for a distance corresponding to 25° in the middle, leaving an open space of 10° on either side. There were therefore produced in every 9 secs. a single flash and a double flash, each of the individual three flashes lasting for 250_σ (10° at 1 rev. in 9 secs.), the two flashes composing the double flash being separated by an interval of 650_σ (25°). At the same time the side of the square opening through which the light passed was reduced to 5 mm. We then proceeded as before, presenting 10 series of 20 flashes each, each series containing 10 single and 10 double flashes, thus giving 100 single and 100 double flashes in all. The experimenter from time to time interrupted the beam of light in such a manner that the single and double flashes were presented at irregular intervals and in irregular order. The fixation point was removed before the beginning of this series, and the subject changed his position from time to time (without however appreciably altering the distance between himself and the illuminated surface) so as to increase the difficulty of seeing the flash and render the conditions as much as possible like those of picking up a distant light from a moving ship.

The results of this experiment show that for Flügel the double flash is distinctly more easy to pick up than the single one, 58 double flashes and only 28 single flashes being seen, while for McDougall there is very little advantage on the side of the double flash, the number of flashes seen being 51 and 47 respectively. In view of the contradictory result obtained with the two observers, it is scarcely possible to say whether or not a double flash is in most cases distinctly more visible than a single flash. It may be remarked, however, that with neither observer was the double flash always recognised as such or in any way clearly distinguished from the single flash, although of course the subject's attention was not particularly directed to this point.

A further short set of observations was undertaken with N increased from 10° to 20°, the total duration of the single flash and of the double flash thus being equal. This resulted, as might be expected, in an increase of the number of single flashes seen, the advantage being now actually in favour of the single flash, the figures obtained with McDougall being 23 and 26 respectively, the total number of flashes being 50 in each case. Through lack of time it was unfortunately impossible to repeat this series with the other observer.

The last point to be investigated by us was the question as to whether the rule that, for stimuli exposed for less than their action-time, reduction of the duration of the stimulus by a certain amount is equivalent (as far as the intensity of the resulting sensation is concerned) to reduction of the intensity of the stimulus by the same amount holds good for lights of such low intensity as to affect the rods only. For this purpose the intensity of the light was slightly increased by widening the aperture of the iris diaphragm, N was completely closed and W made equal to $8\frac{1}{2}°$, the disc rotating as before at the rate of one revolution in 9 secs. The episkotister, with an open sector of 180°, was then rotated in the path of the ray of light, the intensity of which was thus reduced by half. After 100 observations had been made, the episkotister was removed and the series repeated with N at $4\frac{1}{4}°$, half duration thus being substituted for half intensity. These observations were made by both of us, the absolute intensity of the light used being somewhat greater in the series in which McDougall was observer. The number of flashes seen by the latter was 64 in the first series (with episkotister) and 70 in the second, the corresponding figures for Flügel being 38 and 34. From these results we seem justified in assuming that the rule we have referred to, and therefore also the Talbot-Plateau law on which it is based, holds good for light affecting the rods of the retina only.

Summary of Principal Results.

The applicability of the method of determining action-time described in the former paper and the accuracy of the results obtained by it was verified by new experiments made by three observers under more satisfactory conditions of experiment.

When the action of light upon the retina is prolonged beyond its action-time the intensity of the sensation declines very rapidly, and more rapidly the more intense the sensation, the more intense sensation declining to 50 % of its initial value in about $\frac{1}{40}$ second.

The difference-threshold for intensity of visual sensation was measured by a method which is claimed as the only method by which the falsification by physiological 'contrast' can be avoided, and the measure of it was found to be about $\frac{1}{20}$ of the value of the stimulus. Weber's law was verified by this method.

It is shown that when two stimuli are simultaneously applied the difference-threshold is much higher than when they are successively applied, *i.e.* comparison of successive sense-impressions is much more accurate than comparison of simultaneous impressions.

The criticisms of the Helmholtz-Exner method of determining action-time made in the former article are justified by experiments which demonstrate the disturbing effects of physiological 'contrast.'

The action-time of light of intensity so low as to affect only the rods of the dark-adapted retina is shown to exceed $\frac{1}{5}$ second.

The Talbot-Plateau law is shown to hold good for sensations excited through the cerebro-retinal apparatus for vision in dim light of which the rods are the retinal terminals.

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	A Working Hypothesis for Æsthetic Experiments with Colour-combinations, by E. Bullough.
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	Observations on 'Contrast' in Smoothly-graded Discs, by C. H. Nicholl and C. S. Myers.
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I. INSTINCT AND INTELLIGENCE[1].

BY CHARLES S. MYERS.

The writer's standpoint stated.—Criticism of the accepted differences between instinct and intelligence.—Rudiments of conation and meaning in instincts.—The plasticity of instincts.—Examination of the difficulties of enumerating human instincts.—Human and animal intelligence.—Instinct and intelligence from the aspect of evolution.—General conclusion.

INSTINCT and intelligence are generally regarded as two distinct modes of mental activity. In the following paper I hope to give adequate reasons for abandoning this view. I shall endeavour to show that instinct and intelligence are everywhere inseparable, and that in every so-called instinctive or intelligent act, a concomitant aspect of intelligence or instinct may be obtained. I regard the separation of instinct and intelligence as a purely artificial act of abstraction—convenient, no doubt, for the purposes of psychological science, but resulting merely from regarding mental behaviour from two different points of view. I conceive the relation of instinct and intelligence to be essentially similar to that of object to subject. So far as instinctive behaviour can be regarded from the standpoint of the individual experience of the organism, it appears, however imperfectly, as "intelligent,"—characterised by finalism. So far as intelligent behaviour can be regarded from the standpoint of observing the conduct of other organisms, it appears, however imperfectly, as "instinctive,"—characterised by mechanism. Thus intelligence and instinct, choice and tropism, finalism and mechanism, are equally true and valid; they are our necessarily "anthropo-psychic" interpretations of one and the same problem regarded from different standpoints.

[1] This paper formed part of a symposium on the subject held at a joint meeting of the Aristotelian and British Psychological Societies and of the *Mind* Association in London in July, 1910.

It is universally admitted that intelligence and instinct are distinguished from each other by two principal characters. One of these consists in consciousness or unconsciousness of end, the other in plasticity or fixity of reaction. The common assumptions are (1) that in typically instinctive behaviour the organism is wholly unaware of the end thereby to be attained, and (2) that such behaviour is unalterable and from the very outset perfect. It appears to me that neither of these criteria is altogether satisfactory.

I shall begin by considering the instincts of animals, treating them first from the psychical or subjective aspect. The old view that instincts are merely "complex reflexes" dies hard. Even Professor Lloyd Morgan, if I understand him correctly, hesitates to relinquish it. He has described the consciousness that is involved in a chick's *first* peck at food as consequent on the act, not as simultaneous with it. "On this one occasion the accompanying consciousness arises wholly by backstroke[1]." And quite recently he has expressed his belief that all a moorhen chick experiences when swimming for the first time is "a specific presentation, a specific response, a specific emotional tone, all coalescent into one felt situation[2]." Now surely, even on the very first occasion of the functioning of an instinct, there is something more than this, something which distinguishes an instinct from a reflex. As Mr McDougall rightly insists, every instinct has its "conative aspect[3]"; in other words, it is accompanied by a feeling of activity. We cannot, I think, doubt the existence of this "aspect" or "feeling," nor can we derive it, as I understand Professor Lloyd Morgan to do, from afferent impulses of visceral origin[4]. (In this place it is obviously impossible to criticise the modern efforts to reduce the conative and affective elements of consciousness to the sensational element. These attempts end logically in some such position as Professor Titchener's, that conation is a "pretender" as a conscious element, and that affection is merely sensation at a lower stage of mental development[5]. I cannot expect those who adopt such an attitude to follow me further.)

But there is even more than this "feeling of activity" at the very first performance of an instinct. There is another element, which, so far as I am aware, has hitherto been completely ignored. To my mind

[1] *Habit and Instinct*, London, 1896, p. 135.

[2] *Brit. Journ. Psych.* 1909, Vol. III. p. 13.

[3] *An Introduction to Social Psychology*, London, 1908, p. 26.

[4] *Ibid.* pp. 137, 139, 140.

[5] *A Text-book of Psychology*, New York, 1909, Part I. pp. 49, 261.

it is certain that, on the occasion of the chick's first peck or the duckling's first swim, the bird is dimly, of course very dimly, conscious of the way in which it is about to act. I believe this, because no organism can ever execute a new movement which does not involve other movements that have been performed previously. A completely new movement is as impossible as a completely new thought. When a chick first attempts to peck, many of the muscles then called into action must have been contracted before. Thus the feeling of activity arising on the occasion of a chick's first peck is not altogether a new one. It is related, as each of our own experiences is related, to past experiences. And the very vague awareness of results which is associated with those previous feelings of activity gives the chick a vague awareness of the result of its first peck, *before* it has actually performed the action. Such awareness is, of course, rudimentary in the extreme. The chick or duckling cannot then—or indeed ever—be aware of the aims of its instincts, as we are aware of them. But it is important to note what rudimentary consciousness of this kind exists, and to realize that it is the embryonic representative of meaning.

The question arises—are instincts of all kinds and in all circumstances characterized by these rudiments of conation and meaning? I think that they are absent under two conditions only, first, if the instinct has been repeated sufficiently often; secondly, if the instinct is from the first unalterable by later experience. But I would suggest that the same word instinct cannot be suitably employed to embrace, in addition, either of these conditions. For from the standpoint of individual experience, the first few times of performance of an instinct must be very different from the thousandth time of performance; the instinct has become a "habit." And an instinct which is from the first unalterable is, as I shall immediately urge, nothing but a reflex.

Having attempted to show that the subjective aspects of so-called instinctive and intelligent behaviour differ in degree and not in kind, I turn now to consider the alleged difference in their objective aspects, —the fixity of instinct, the plasticity of intelligence.

An instinct has been defined as "a complicated reaction that is perfect the very first time[1]." I question whether this is ever literally the case, if only the reaction could be submitted to close enough examination. Young birds usually learn to fly and to sing by imitating their elders. Even the young of ants, where "instinct" is considered

[1] H. Driesch, *The Science and Philosophy of the Organism.* Gifford Lectures (Aberdeen), 1908, p. 110.

to reach its highest development, have been observed to learn by imitation from older ants[1]. Instincts are almost always modifiable and perfected by later experience. Indeed, a "perfect reaction" is apt not to need subsequent modification. It can adequately be worked by mechanism. Consciousness, especially those elements of conation and meaning which we have just been considering, will become unnecessary, nay, must even prove disadvantageous. A reflex is the nearest example of such a condition. (But even a reflex proves to be not "absolutely" fixed, and may prove to be not "absolutely" unconscious. All that can be said is that its central consciousness, if present, is always a *terra incognita*, never communicable to the Ego of the organism.) From the point of view of definition, it would be better to call the flight of moths towards a lamp a reflex, not an instinct; no amount of experience alters the reaction.

Nor do different individuals of the same species or the same individuals on different occasions show that uniformity of action, which has been often regarded as characteristic of instincts. Take, for instance, the following observations by Mr and Mrs Peckham on the habits of solitary wasps, which, with some of his remarks, I quote from Professor Hobhouse[2].

"'When the provisioning is completed the time arrives for the final closing of the nest, and in this, as in all the processes of Ammophila, the character of the work differs with the individual. For example, of two wasps that we saw close their nests on the same day, one wedged two or three pellets into the top of the hole, kicked in a little dust and then smoothed the surface over, finishing it all within five minutes. This one seemed possessed by a spirit of hurry and bustle, and did not believe in spending time on non-essentials. The other, on the contrary, was an artist, an idealist. She worked for an hour, first filling the neck of the burrow with fine earth which was jammed down with much energy, this part of the work being accompanied by a loud and cheerful humming, and next arranging the surface of the ground with scrupulous care and sweeping every particle of dust to a distance. Even then she was not satisfied, but went scampering around hunting for some fitting object to crown the whole. First she tried to drag a withered leaf to the spot, but the long stem

[1] E. Wasmann, ***Comparative Studies in the Psychology of Ants and of Higher Animals*** (Eng. trans.). St Louis, 1905, p. 68. Cf. also Lloyd Morgan, *op. cit.* p. 131.

[2] *Mind in Evolution*, London, 1901, pp. 68, 70. Cf. also G. W. and E. G. Peckham, *Wasps Social and Solitary*, Boston, 1905.

stuck in the ground and embarrassed her. Relinquishing this, she ran along a branch of the plant under which she was working, and, leaning over, picked up, from the ground below a good sized stone, but the effort was too much for her, and she turned a somersault on to the ground. She then started to bring a large lump of earth, but this evidently did not come up to her ideal, for she dropped it after a moment, and, seizing another dry leaf, carried it successfully to the spot and placed it directly over the seat.' . . .

"'Presently she [in this instance a specimen of another species, *Pompilus scelestus*] went to look at her nest and seemed to be struck with a thought that had already occurred to us—that it was decidedly too small to hold the spider. Back she went for another survey of her bulky victim, measured it with her eye, without touching it, drew her conclusions, and at once returned to the nest and began to make it larger. We have several times seen wasps enlarge their holes when a trial had demonstrated that the spider would not go in, but this seemed a remarkably intelligent use of the comparative faculty.'

"Whatever the correct interpretation of this last observation, enough has been said to show that these wasps adapt means to ends in a way suited to the individual occasion. They are by no means confined to a series of reactions evoked with mechanical uniformity by a uniform stimulus. On the contrary, they are able to deal within limits with each emergency presented by the individual differences of the prey they have captured."

We have called attention (p. 211) to the fact that even ants are capable of learning from their elders. But this power of learning, or at all events of learning by experience, is by most psychologists considered a sign of intelligence[1]. If so, the very humblest forms of animal life appear to be intelligent. The protozoon Stentor, for example, first reacts to a fall of powder by turning aside. Should this action not bring it beyond reach of the powder, it reverses the direction of its ciliary movement. If it still fails to be successful, it withdraws into its tube. Finally, if the fall of powder continues, the organism detaches itself from its support, and swims away to another. When, after a short interval, the fall of powder is repeated, the organism starts at once with the fourth reaction, instead of proceeding through the three previous stages which have proved ineffective[2].

It may be urged, however, that the essential objective features of

[1] Cf. M. F. Washburn, *The Animal Mind*, New York, 1908, p. 19.

[2] H. Jennings, *The Behaviour of Lower Organisms*, New York, 1906.

intelligence are "the novelty of the adjustment and the individuality displayed in these adjustments[1]." But novel adjustments are observed where the influence of intelligence, as generally understood, would doubtless be disputed. Professor Forel brought back to Europe a number of Algerian ants, which build their nests with a wide entrance in their native country. In their European home these ants found that their quarters were continually infested with the common ant. So they set to work to close the wide entrance of their nest[2]. Or again, a dung-beetle, rolling its dung-ball along the sand, finds itself in a hollow, the sides of which are too steep for the ball to be pushed up from below. So the beetle buts down the sand at one side "so as to produce an inclined plane of much less angle[3]." We are of course free to believe that in such cases there is no true "novelty of adjustment," that the mode of reaction was already innate in the organism, only waiting for the rare situation which might evoke it. But my contention is that such a belief, if adequately elastic, is as applicable to intelligent as to instinctive behaviour.

I will now indicate the position so far reached. Instead of defining instincts as "complex reactions which are perfect the very first time," I have endeavoured to show that they are all within very variable limits improvable by practice or by imitation, or are modifiable by changed conditions of environment. Further, in place of the usual definition of instincts as merely "fixed innate activities," I have maintained that the essential mental concomitants of instincts are the feeling of activity and a vague consciousness of the behaviour to be achieved. I would urge that the existence of these central factors proclaims the presence of intelligence throughout instinct, and that, as the organism becomes endowed with an increasingly larger number of mutually incompatible modes of reaction, the intelligent aspect apparently comes more and more to the fore while the instinctive aspect apparently recedes *pari passu* into the background.

It is proverbial that the ordinary person, if asked whether *man* has instincts, replies "No. The behaviour of animals is regulated by instinct. Man is moved by intelligence, by reason." With one accord, however, psychologists insist that such early human acts as suckling, crying, crawling and walking, are instincts, and that even later acts are of like nature, e.g. the "sexual instinct." Others add to this list enor-

[1] Lloyd Morgan, *Animal Life and Intelligence*, London, 1891, p. 458.

[2] *L'Année psychologique*. 2e Année, 1895, p. 41. Cf. also Wasmann, *op. cit.* p. 142.

[3] Lloyd Morgan, *ibid.* p. 368.

mously. In one book[1] I find enumerated the instincts of imitation, curiosity and play; the expressive, aesthetic, moral and religious instincts; the parental and social instincts; the collecting, constructive, destructive and fighting instincts. May we not complete the list by adding the instincts of thought, reason, intelligence?

This difficulty in delimiting the human instincts arises from the criteria employed. Evidently these are: (1) What instances of human behaviour are analogous to the recognized instincts in animal life, and (2) what lines of conduct are common to all, or to large numbers of, mankind? The criteria are hence objective. But in man, at least, there should be no difficulty in substituting a subjective criterion, thus avoiding the notorious errors of interpretation arising from the former method. It should be easy for man to be able to describe the difference which he himself experiences when acting instinctively and when acting intelligently.

Herein, I believe, is the root of the difficulty. Man is never aware that he is acting instinctively; and on this account he naturally denies instincts to himself and his fellows, while ascribing them to animals. When a mother sacrifices her life to save her child, does she recognize that she is acting instinctively or unintelligently? At the dawn of the sexual instinct,—or even earlier, say at the first exercise of the walking instinct,—can we be said to have any cue which informs us that we are not acting intelligently but instinctively? From our own introspection we can only answer negatively.

It may be urged, on the one hand, that the human organism, when acting instinctively, achieves "its end under the driving power of the instinctive impulse awakened within it[2]," bringing his intelligence to bear as best he may, so as to satisfy that end. But is this impulse *always* felt as such, and does it when present appreciably differ from other forms of impulse which would not generally be classed as instinctive? Stress, on the other hand, may be laid on the fact that "each of the principal instincts conditions . . . some one kind of emotional excitement, whose quality is specific or peculiar to it[3]." But instinct is not to be identified with emotion; the former is not the necessary or universal condition of the latter. Thus neither instinctive emotion nor instinctive impulse appears to help us in differentiating instinct from intelligence. And we reach the same conclusion in the

[1] E. A. Kirkpatrick, *The Fundamentals of Child Study*, New York, 1903.

[2] McDougall, *op. cit.* p. 40 (footnote).

[3] *Ibid.* p. 47.

case of man as we have already reached in the case of animals, that instinct and intelligence are inseparable.

The difference between animal and human intelligence is at first sight, and by many, considered fundamental. One may argue as extremely as Father Wasmann, for example, that "intelligence . . . exclusively signifies the power to act with deliberation and self-consciousness," but that "animals have no intelligence at all. If they were gifted with a spiritual power of abstraction, it would necessarily be manifested in their outward actions, especially by the formation of an arbitrary phonetic or graphic language. Animals, however, have no language; hence they have no intelligence[1]." According to this view, we separate human from animal intelligence and identify the latter with instinct. Now to discuss the relation of human and animal intelligence is obviously beyond the scope of this paper. At all events, in the present state of comparative psychology, a decision on the subject is impossible. My belief is that the difference between human and animal intelligence, great as it is, is one of degree rather than of kind. I believe that we may recognize in animal life occasional dim flashes of those higher "spiritual powers" which are in full flame in the human mind.

Lastly, there remains the consideration of instinct and intelligence from the broader standpoints of evolution and philosophy. Three different views of psychic evolution have been advanced, corresponding to the better known ones of somatic evolution. The first ascribes reflexes, and in the usual sense instincts, to the degradation of behaviour which has been intelligently, purposefully acquired in the ancestry of the organism. The second view also accepts the heredity of acquired mental "dispositions," but attributes their acquisition to the environment instead of to an all-wise intelligence. The third attributes psychic evolution to variations in the germ plasm which are preserved by natural selection. There is little or no evidence in favour of the first of these views. The second (a Lamarckian) view is still hotly disputed. Only the third (or Darwinian) view meets with definite acceptance among psychologists as among biologists. By its acceptance, however, we may appear to be giving ourselves up to a wholly mechanical interpretation of the evolution of mind. I have therefore attempted in conclusion to show that there is scope, as well as need, for the finalistic interpretation also.

[1] *Comparative Studies in the Psychology of Ants and of Higher Organisms* (Eng. trans.), St Louis, 1905, pp. iii, 198.

For each of these two interpretations is traceable to our experience of activity, finalism to our experience of subject-activity, mechanism to our experience of object-activity. Each of them, too, comes to be extended beyond its sphere of origin. We extend the mechanistic interpretation to ourselves when we recognize that if all the conditions determining our behaviour were but given, one result and no other could issue therefrom, and that if only we could know all those conditions and had already observed their result, we could confidently predict the resulting behaviour. Such admissions do not conflict with our recognition that very often our actions cannot thus be predicted, that they are devised to attain ends, and that those ends are of our own making. This two-fold interpretation of his behaviour each of us recognizes within himself. He extends it also to his fellow-men. The question arises whether he is justified in extending it also to the behaviour and the evolution of living and lifeless objects generally.

From one point of view, certainly, we cannot avoid applying the finalistic interpretation to these objects, inasmuch as without it nature would be meaningless. We have just insisted that mechanism can only predict the result of given conditions, provided that a like result of like conditions has been observed already. Without previous experience, mechanism could never foretell that hydrogen and oxygen would yield water. It can never foretell the apparent discontinuities in evolution or the paths of history. Further, mechanism has no concern with ends, yet our mind finds evidence of finalism everywhere. Each piece of behaviour appears adapted for an end. Ends appear already framed in organisms which have no apparent power of framing ends for themselves.

We find that the non-nervous tissues of living objects are often possessed of a variety of methods, any one of which will serve to reach one and the same end, in cases, for example, of injury after which regeneration starts in one of several possible methods to reach one and the same result[1]. It is indeed in the degree of adaptability to all possible disturbances that the psychical is distinguished from the non-psychical, the physiological from the physical, and, we may add, the entire Universe from that pure abstraction,—purposeless mechanism. For ends exist not only in Life but throughout the Universe, if only we view the Universe as a huge organism; the difference lying only in the size of the system and in the breadth of the subjective outlook.

[1] Cf. the striking example given by H. Driesch in *The Science and Philosophy of the Organism*. Gifford Lectures (Aberdeen), 1907, pp. 159–161.

With the dawn of life, ends begin to form within individual living organisms. With the dawn of instinct and intelligence, awareness of these ends within individual experience develops, as I have attempted to show earlier in this paper; and ultimately, with increasing mental complexity, there is not merely this awareness of ends, but finally also distinct awareness that they are ends, and an increasing power to modify and frame fresh ends. This is the subjective, finalistic, intelligent factor which is inseparable from its objective mechanistic analogue, instinct, and develops with it.

I conclude, then, that instincts are not, as has been generally supposed, identifiable with reflexes; nor are they, as others have urged, a *tertium quid* beside reflexes and intelligence. According to my view and my use of the words, instinct regarded from within becomes intelligence; intelligence regarded from without becomes instinct.

II. INSTINCT AND INTELLIGENCE[1].

By C. LLOYD MORGAN.

Instinct and intelligence inseparable.—Dr Myers' double aspect interpretation.—Factors of re-instatement as determinants of intelligent behaviour.—The instinctive situation as affording data of experience.—The beginnings of experience dependent on hereditary responses together with their initiating presentations and visceral accompaniments.—The biological criterion of instinctive behaviour.—Modes of serviceable response.—A wider connotation of the term "instinct."—Instinct as part of, or the outcome of, the innate constitution of the psycho-physical organism.

I TAKE it that one of the aims of this symposium is to render clear by discussion from different points of view the genetic relationships of those factors in the mental life which we group under the terms instinct and intelligence. It is to this genetic problem that I shall in the main restrict my contribution.

In his opening paper, Dr Myers contends that instinct and intelligence are everywhere inseparable. I am prepared to agree that both factors are present in the most intimate relationship throughout very nearly the whole range of animal behaviour as exhibited by those organisms in which the central nervous system has reached a sufficiently high level of development and differentiation to justify the use of the words "instinctive" and "intelligent." But, if I understand him rightly, what in Dr Myers' view are inseparable are two aspects of one mental process. "In every so-called instinctive or intelligent act a concomitant aspect of intelligence or instinct may be obtained." The difference of aspect is correlated or identified with—is "essentially similar to"—that of object and subject. It is true that "as the organism becomes

[1] This paper formed part of a symposium on the subject held at a joint meeting of the Aristotelian and British Psychological Societies and of the *Mind* Association in London in July, 1910.

endowed with a greater number of mutually incompatible modes of reaction, the intelligent aspect comes more and more to the fore while the instinctive aspect recedes *pari passu* into the background." If I correctly apprehend this double-aspect interpretation it follows that, *quâ* different aspects of one something, every step and stage of progressive advance in the one aspect or manifestation, intelligence, has as its concomitant a like advance in the other aspect, instinct. This seems to accord with Dr Myers' concluding words: "Instinct regarded from within becomes intelligence; intelligence regarded from without becomes instinct." Translating this from the general into the concrete I presume that I may interpret a particular case as follows: Yesterday I was playing golf; and, having carefully scanned a finely rolling green, having taken into consideration the varying slopes over which my ball must pass and the direction of a strongish wind, and having estimated the requisite strength of my stroke, I succeeded in holing a long and curly putt. Regarding the procedure from within I rather plumed myself on my intelligence. Dr Myers had he been present and regarding the putt from without (not the mere co-ordinated skill but the accompanying mental procedure) would have termed it instinct. My companion, a man of plain common sense, said it was a fluke.

As a matter of fact before I played the ball I pictured the course it would take. Previous experience on this and other such greens had given rise to certain definite anticipations, and these were present as determining conditions of the mental procedure which accompanied my skilful stroke. I regard the presence of some implicit expectation (in the lower forms) or explicit anticipation (in the higher forms) as distinguishing marks or criteria of intelligence. In other words for the intelligent organism the present experience at any given moment comprises more or less "meaning" in terms of previously-gotten experience. In a recent paper[1], from which Dr Myers does me the honour to quote, I used the phrase "factors of re-instatement" for the "meaning" constituent of the total intelligent disposition. I applied the term "instinctive" to those factors in behaviour, the determining conditions of which are prior to individual experience, and the term "intelligent" to that behaviour which involves re-instatement dependent upon previous experience, the net results of which are revived. The instinctive factors, I said, depend entirely on how the nervous system has been built up through heredity under that mode of racial preparation which we call evolution; intelligent behaviour depends

[1] *Brit. Journ. Psych.* Vol. III. p. 11 (Dec. 1907).

also on how the nervous system has been modified and moulded in the course of that individual preparation which we call the acquisition of experience.

I may perhaps be allowed to recapitulate the interpretation of intelligent and instinctive behaviour which I suggested in the paper to which Dr Myers has referred. I took first the case of a moorhen about two months old which dived for the first time,—and dived in a manner quite true to moorhen type,—when it was scared by a rough-haired puppy. I endeavoured to analyse the situation—perhaps too briefly and succinctly since my aim was to bring out only the salient features. There was the moorhen swimming in the stream. Sensory presentations through eye, ear and skin, from the organs concerned in behaviour, from the internal viscera, from the whole organic "make up"—these together with a supplement of "factors of re-instatement," gained during two months of active vigorous life, constituted what I conceived to be the actually existent experience of the moment. Here was a body of experience, then and there present, functioning as experiencer and ready to assimilate the newly introduced instinctive factors. Then comes along that blundering puppy; and the moorhen dives. I submit that one may feel justified in saying that there was something about the total puppy-presentation which was so far new as to elicit an instinctive response which in itself afforded new data to experience in the course of its performance; I submit also that there was, giving colour to the felt situation, a specific quality of emotional tone which had not hitherto been felt in just this particular way.

I suggested, then, that the instinctive situation afforded new factors which were assimilated to the existing experience of the moorhen. It may be said, however, that there are no new factors; there are only old factors grouped in a new way; the new experience of diving, it may be urged, is only a modification of the old experience of swimming. This I am quite ready to admit; nay more, I have myself stated my belief that this is so[1]. What is new is the whole net result coalescent into one felt situation. Shall we say that while much of the matter may have been previously presented under other "forms," what is here new is the "form" of the situation as a scare-begotten dive? But why apply to this form the term "instinctive"? My reply to this question is simple and direct, and goes to the very heart of the problem; because this particular form of behaviour exhibited by the moorhen on the

[1] *Habit and Instinct*, pp. 136, 137; *Animal Behaviour*, p. 106.

occasion of its first dive is dependent as such on how the nervous mechanism has been built up through heredity under that mode of racial preparation which we call biological evolution; because this particular form is not determined as such by previous experience. If in further criticism of the point of view I wish to make clear, it be urged that though perhaps the "form" of the scare-begotten dive-situation is mainly due to the hereditary make-up of the nerve-centres it is partly dependent (e.g. in its relation to swimming) on how the nerve-centres have been moulded under experience, I venture to remind my critic that we are endeavouring to disentangle the factors of behaviour; that all I urge is that the instinctive factor is here predominant; and that just in so far as the "form" of the situation is dependent on previous experience we have the presence of the intelligent factor. Here I am in agreement with Dr Myers. In a moorhen two months old, instinct and intelligence are inseparable. None the less the instinctive and intelligent factors are distinguishable in psychological analysis.

Let us now go back to nearly the beginning of our little moorhen's life, to a time when he was not two long months' old, an experiencer of some standing as moorhens go, but when he had seen but a few days of life beyond the confines of the egg-shell. I quote from the paper already referred to because Dr Myers has, in taking a few words from the context in which they stand, missed an important point. We started with our birdling as experiencer swimming about in the stream. How did he reach this level of conscious organisation? I cannot trace in detail the genesis of his body of experience; but I remember the day when I first placed him gently in a tepid bath. Even then he was an experiencer, though his store of factors of revival was exceedingly limited. Of swimming experience he had none. Racial preparation had however fitted the tissues contained within his black fluffy skin to respond in a quite definite manner. And in the first act of swimming there was afforded to his experience analogous factors to those I have given above in considering his later dive—"a specific presentation, a specific response, a specific emotional tone, all coalescent into one felt situation." Now Dr Myers states that I here express the belief that all a moorhen chick experiences when swimming for the first time is "a specific presentation"—quoting the words I have placed in inverted commas. But I said that even then he was already an experiencer; I said that the factors Dr Myers quotes were analogous to those which accompanied his first dive; I then spoke of them as "*new factors*"—part of "a new situation which the experiencer can assimilate."

Although I did not say so, I thought that my readers might give me credit for believing that the moor-chick in the bath, *quâ* experiencer, had already gained the experience necessarily involved in using the same limbs and the same muscles in walking. Dr Myers however urges that "there is another element, which, so far as I am aware, has hitherto been completely ignored. To my mind it is certain that, on the occasion of the chick's first peck or the duckling's first swim, the bird is dimly, of course very dimly, conscious of the way in which it is about to act. I believe this because no organism can ever execute a new movement which does not involve other movements that have been performed previously. A completely new movement is as impossible as a completely new thought. When a chick first attempts to peck, many of the muscles then called into action must have contracted before. Thus the feeling of activity arising on the occasion of the chick's first peck is not altogether a new one. It is related, as each of our own experiences is related, to past experiences. And the very vague awareness of results, associated with those previous feelings of activity, gives the chick a vague awareness of the result of its first peck, before it has actually performed the action." I really thought that I had said much the same sort of thing myself[1]. But after all this is a very minor matter. I quote Dr Myers here because I am in substantial agreement with much at any rate of what he says. It is true that I have not used the phrase "feeling of activity," nor was I aware that I had derived it from afferent impulses of visceral origin. But I have again and again spoken of "behaviour-experience"; and I am quite prepared to admit, nay to contend, that all experience involves a consciousness of process as transitional and nowise static. But if I can fully agree with Dr Myers that the first dive is assimilated to previous experience of swimming and the first swim to previous experience of walking, it appears to me (1) that we are bound to admit that such assimilation involves factors of re-instatement and so far depends on how the nervous system has been modified by previous experience, (2) that the newly introduced instinctive situation involves a fresh context—and a context which derives its specific "form" from the inherited make up of the organism and its central nervous system. If the first peck has some dim and vague "meaning" (and for me such

[1] See references given to *Habit and Instinct* and to *Animal Behaviour*.

"meaning" involves dim and vague intelligence—involves some element of pre-perception) the accomplished peck supplies the data for new meaning—not merely meaning in terms of previous other-use of the same muscles, but meaning in terms of this specific pecking-use biologically established through the natural selection of variations (or mutations?) of germinal origin. In the first peck or the first swim therefore, according to my interpretation, we have *as* peck and *as* swim the instinctive factor relatively, but only relatively pure—relatively impure in so far as it is accompanied by such dim pre-perception as may be due to other previously-gotten experience. Some intelligence is here inseparable from instinct, because we have not got to the very beginning of experience, and because some factors of re-instatement are present.

But I want to get, if possible, at the very beginning of experience, say in the moorhen chick. I have suggested therefore that if we go yet one stage back to the time when the little bird was struggling out of the cramping egg-shell, there came what we may fairly regard as the initial presentations, generating the initial responsive behaviour, in the earliest instinctive acts, accompanied, we may presume, by the initial emotional tone, coalescent to form what I have ventured to term the primary tissue of experience. Now it appears to me that the only factors of revival which can here be present are those which might conceivably be derived from experience previously gained within the unbroken egg. I am ready to yield this much for what it is worth, merely remarking that for practical interpretation it is not worth much. If I may be allowed to neglect it as a vanishing quality, then I conceive we reach the stage at which the experiencer as such has its primary genesis. It is called into existence by the earliest instinctive behaviour (whenever and however that earliest stage occurs) and here, for strictly empirical interpretation, I find the very first beginnings of the individual experience. From that stage onwards fresh primary tissue is added as new instinctive behaviour is evoked by the appropriate presentations; but from that stage onwards the primary tissue is intimately blended with the secondary tissue due to the revival of what has so far been incorporated in the growing body of experience.

It appears to me then that for purposes of psychological interpretation, in so far as this is concerned with the early stages of the genesis of experience, we should so far broaden the connotation of the term "instinct" as to include all those primary and inherited modes of behaviour, including reflex acts, which contribute to what I have termed the primary tissue of experience. If there be reflexes or modes of instinctive behaviour which have no concomitants in consciousness with them the psychologist has no concern. He may cheerfully hand them over to the biologist.

I have elsewhere[1] endeavoured to give a biological definition of instinct. I will not attempt to recapituate here, but will only make some comments on points which arise out of Dr Myers' paper in so far as they bear upon this discussion. He quotes Dr Driesch's definition of an instinct as "a complicated reaction that is perfect the very first time," and doubts whether this is ever literally the case, if only the reaction could be submitted to close enough examination. Now if we qualify Dr Driesch's statement and say that what the biologist regards as a typical case of instinctive behaviour is *relatively* perfect on the first occasion, we shall, no doubt, be charged with quibbling, and shall be told that the phrase "relatively perfect" has no exact meaning. It will be said that a given reaction must be either perfect or imperfect, and we are logically bound to accept one or the other. I myself so far agree with the criticism that I suggest the following modification of the brief definition: instinctive behaviour is practically serviceable on the occasion of its first performance. Take the flight of the swallow as an example which may illustrate a vast number of instinctive acts. Is there a biologist who has adequate acquaintance with the facts, who would dream of asserting that the instinctive performance at the outset has anything approaching in delicacy and effectiveness the perfected skill of the mature bird—a skill shot through and through with meaning for perceptual life on the wing[2]? None the less I am convinced from personal observation[3] that the relatively imperfect instinctive flight of the young swallow taken from the nest is practically serviceable and has survival value. It is good enough to preserve the little bird from falling to the ground and running the risk of destruction the very first time it leaves the nest. The outcome of natural selection is not to produce either behaviour or organic structure which is so perfect that no trace of imperfection can be discovered by the closest examination. One of the least imperfect organs is the normally developed human eye; and yet, as we all know, Helmholtz found in the organ of vision many defects[4]. The products of natural

[1] *Habit and Instinct*, p. 27. *Animal Behaviour*, p. 71.

[2] Cf. *Animal Behaviour*, p. 88.

[3] *Habit and Instinct*, p. 71.

[4] *Popular Lectures on Scientific Subjects*. "The eye as optical instrument," pp. 197 ff.

selection are practically serviceable, not theoretically perfect. Furthermore, as we have already seen, there is, for individual development, another way in which instinctive behaviour is, in all intelligent animals, practically serviceable. It affords the rude outline sketch of that far less imperfect behaviour, the finishing touches of which are supplied by practice under the guidance of intelligence. The net result (what is for popular speech the perfected instinct) is a joint product of instinct and intelligence, in which the co-operating factors are inseparable, but none the less genetically distinguishable.

We really know so little of the psychology of the invertebrates that our interpretation of their behaviour is in large degree conjectural. As a matter of conjecture I am of opinion that the examples of behaviour in the solitary wasps observed by Dr and Mrs Peckham and quoted by Dr Myers are likewise joint-products of similarly co-operating factors. It must be remembered however that among insects there are found cases of instinctive behaviour which much more closely approach conformity with Dr Driesch's definition. A complicated series of acts, showing wonderful nicety and accuracy of adoption, is performed once and once only in the lifetime of the individual without any opportunity for imitation, so called. Of this the behaviour of the Yucca moth[1] and the stinging of prey by the solitary wasps are examples. With regard to the latter case, by the way, the admirable observations of the Peckhams[2] should be carefully studied by those who like Prof. Bergson still quote the solitary wasps as examples of the surgical knowledge with which instinct has provided these arthropod physiologists.

I have suggested above that, for the biologist, a criterion of instinctive behaviour is that it is serviceable on the first occasion. But the biologist, for the purposes of his interpretation of animal life, will ask: Serviceable for what end? Most broadly and generally serviceable for survival to which sundry bodily activities contribute. In further detail serviceable for avoiding danger by shrinking, quiescence, or flight; serviceable for warding off the attacks of enemies; serviceable for obtaining food, capturing prey, and so forth; serviceable for winning and securing a mate, for protecting and rearing offspring; in social animals, serviceable for co-operating with others and so behaving that not only the individual but the social group shall survive. Under each head diverse modes of behaviour may be grouped—modes of behaviour

[1] *Habit and Instinct*, p. 14.

[2] *On the Habits and Instincts of the Solitary Wasps*, pp. 12 ff. Cf. *Animal Behaviour*, p. 74.

which have this in common that they severally subserve what *we* regard as one end. Each group may be regarded as comprising sundry examples of "an instinct." In this way we reach such a classification of "instincts" as Mr McDougall has suggested[1]. Among birds however there are a great number of modes of instinctive response which are difficult thus to classify[2]. Taking those which Mr McDougall regards as characteristic of man, he is, I think, probably right in urging that each such instinct (or group of instincts) has its characteristic emotional tone such as fear, anger, the emotions connected with mating, or with the rearing of offspring. I so far follow Professor William James as to believe that *quâ* emotional there are involved visceral factors and those due to general organic tone. Any given presentation gives rise to (1) activity behaviour, (2) visceral disturbance, and (3) modification of general sensibility, and the instinct-emotion experience is the net result of all these three factors. I think it probable that these arise "by backstroke" through the incoming of afferent impulses from the parts of the organism concerned. But I do not deny that the initial presentation may stimulate other parts of the sensorium, or other brain-centres, and thus evoke factors in consciousness which are not of "backstroke" origin[3]. There seems to me nothing unreasonable in this view. But I insist on the fact that even if this be the case, we still have a bit of the primary tissue of experience.

In the whole of this difficult subject so much turns on the definition of terms! I have endeavoured to make reasonably clear the sense in which I use the word "instinct" (chiefly in its adjectival form as qualifying the word behaviour) in the universe of discourse of genetic psychology and in that of biology. But the term is also used with a different and much wider connotation. I cannot adequately discuss the views implied in this broader use; but I will, in conclusion, say a few words on the matter.

I take it that the moor-chick comes into the world with an innate potentiality to respond to environmental influence, presentations and so forth, in certain specific ways. I am aware that potentialities are kittle cattle. Let me therefore say that what I mean by potentiality is an actually existent structure in virtue of the possession of which the organism does functionally respond in specific ways under the

[1] *Social Psychology*, chap. 3.

[2] Many of those for example that are described at length in *Habit and Instinct* and in ch. III. section iii. p. 84 of *Animal Behaviour*.

[3] Cf. *Darwin and Modern Science*, Essay XXI. "Mental Factors in Evolution," p. 435.

appropriate circumstances. There is an innate potentiality, then, a so-called instinctive faculty, dependent upon the inherited make-up of the organism, to respond with the behaviour which I have defined as instinctive. But presently the originally instinctive behaviour is modified by the introduction of "meaning" through factors of revival, and thus becomes in my terminology so far intelligent. This, however, is every whit as much the outcome of the innate potentiality of the moorhen as the originally instinctive performance. If you know your bird, including its past history, you can bet on its intelligent procedure with as much confidence as on its instinctive behaviour. If therefore we identify instinct and innate potentiality regarding the actual performance as its manifestation, all intelligent behaviour has an instinctive element. It is that element which cannot be explained by the grouping of the factors of experience since it is the innate ability so to group them. Hence it has been said that genius is akin to—nay is the supreme human exemplar of—instinct. Since a man's every act and thought may be regarded as the outcome of his innate potentiality so to act and think, it is clear that at the heart and core of his mental and bodily activity is the underlying inherent instinct in this sense of the word—the unexplained remainder when empirical psychology has said its last word in terms of the antecedence and sequence of the factors in consciousness.

I am well aware that in speaking of an "unexplained remainder" I am approaching if I am not overstepping the boundary line which separates the empirical from the metempirical universe of discourse. It appears to me[1] that we are bound to accept—or if it be preferred to postulate—as inherent in the constitution of experience and of that objective aspect of experience which we term nature, certain "forms of synthesis"—for example, crystallisation in the inorganic world, life in the organic world, perception in the mental world. We may trace their changes and relationships—the conditions under which they survive and persist or are eliminated. But there they are. Empirical science just accepts them as constitutive of the universe under investigation. Metempirical philosophy endeavours to account for their existence. If the term "instinct" be applied to a basal principle innate in the human mind—well and good. I for one am ready to agree that "there it is."

Am I right in surmising that here I am in touch with part at least of Dr Myers' thought? Am I right in surmising that the "feeling

[1] Cf. the middle paragraph on p. 16 *British Journ. Psych.* Vol. III.

of activity" of which he speaks, accompanies the actualising, in the moment of experience, of the innate potentiality? Am I right in surmising that it is this actualisation empirically given which may be regarded subjectively as intelligent, and may also be regarded objectively as instinctive? I know not. Perhaps I am altogether off the track of his thought. In any case when I analyse, to the best of my ability, much that is written by masters of philosophy and others in elucidation of the instinctive element in human thought, I find the essential feature to be this: that instinct is part of, or is the outcome of, the innate constitution of the mind. This, I repeat, implies a wholly different connotation of the term from that which as mere biologist and comparative psychologist I have elected to accept.

Note. It may serve to make my interpretation of instinctive and intelligent behaviour clearer if I here re-state my view that "a control-system, which is the physiological embodiment of what for the student of mental science is experience, has been differentiated from the centres concerned in instinctive behaviour, and thus, in a sense, stands apart from the organic happenings over which its guidance is exercised"; that "the performance of an instinctive act so stimulates the centres of intelligent control as to afford the primary data of experience"; and that "it is therefore essential to distinguish between two orders of heredity: first, that which obtains within the automatic system, and which thus determines the nature of the hereditary responses; secondly, that which obtains within the system of intelligent control, and which thus determines the hereditary likes and dislikes." (*Interpretation of Nature*, Section x.) I regard the cerebral hemispheres as the differentiated control system and conceive that they play no functional part in the automatism of instinctive behaviour. Carrying a stage upward Professor Sherrington's doctrine of the spinal animal, I suggest that the instinctive vertebrate as such is a thalamencephalon-downwards animal.

III. INSTINCT AND INTELLIGENCE[1].

By H. WILDON CARR.

Defends the view of Bergson that instinct and intelligence are two modes of psychical activity radically heterogeneous and at the same time complementary, that have been progressively developed along different main lines of evolution.

THE theory of the nature of instinct and its relation to intelligence that I propose to put forward in this paper is one that I owe to a study of the philosophy of Bergson. I have no claim to write on this subject on account of any direct experimental work or special study. My interest is not in the biological or psychological aspects of the question but is simply philosophical, and the arguments I shall use belong to metaphysics.

I take up the problem as it is presented in Dr Myers' paper. He holds that instinct and intelligence are concomitant aspects of one and the same psychical activity, and that the relation between them is essentially similar to that of object to subject. I think this view would offer no difficulty if the only instincts we had to consider were natural dispositions, or tendencies, or inclinations such as I may discover by looking into my own consciousness, or by comparison of the conduct of other organisms that I imagine to be moved by the same motives as myself. Dispositions such as pugnacity, curiosity, acquisitiveness, secretiveness, and the like are named instincts, and they are distinguished from intelligence as a part of our mental nature that is independent of any reflection on experience, and they may be represented as a kind of material of human character, born with each individual, which intelligence moulds, and trains and educates. But it

[1] This paper formed part of a symposium on the subject held at a joint meeting of the Aristotelian and British Psychological Societies and of the *Mind* Association in London in July, 1910.

seems to me that this is as far as the theory will go, it breaks down entirely if called upon to explain or account for those highly specialised and complicated actions that we meet with only in what we call lower forms of life. I cannot understand what is meant if it be really asserted that the instincts of ants and bees have a concomitant aspect of intelligence. There is nothing in our own intellectual life at all analogous to these activities, and they are completely different to any aspect that our own intelligence can be made to present.

Instinct and intelligence are not observable facts, but interpretations. The observable facts are a certain class of actions of living creatures—not all their actions, for vital actions, such as respiration, digestion, and the like are not considered, but only those actions which constitute their behaviour in the outer world. The broad distinction between those actions which we call intelligent and those which we call instinctive is familiar to everyone. In a general way, we call instinctive, those actions which appear to us to be due to natural disposition, and intelligent, those which imply reflection on experience. In ordinary discourse, however, the words are hardly ever employed with scientific accuracy, and we ought to be careful, therefore, in our illustrations to use only examples that admit of no ambiguity, examples of pure instinct on the one hand, and of pure intelligence on the other. There are abundant examples of instinct, established by undoubtedly accurate observation, more especially in observations of insect life, that reveal highly complex and purposive actions, which cannot by any possibility have been learnt by individual experience, or imitated from the actions of other individuals. And also there are instances of intelligence that involve nothing whatever that can be called instinctive. Take as an example, Nelson's design in the battle of Trafalgar. Everyone would, I think, agree that this is an instance of pure intelligence. If anyone were to describe it as an instinct, it could only be by using the word metaphorically to express the perfection of the design as interpreted by the action. When we consider two such outwardly dissimilar instances of behaviour as, let us say, that of a paralysing wasp, and that of an admiral directing a naval engagement, have we before us only two different aspects of a mental behaviour that is essentially identical, but appears to us in the one case characterised by mechanism, in the other case by finalism? Or, are they two modes of psychical activity, fundamentally distinct, different in the nature of their mentality, in the direction of their activity, in the kind of knowledge that each is fitted to receive and use?

When we look at this question from the standpoint of evolution, we see that there are two main lines of the evolution of animal life along which we may observe a progressive psychic evolution. Along one of these, the vertebrata, the evolution of mentality has been clearly an evolution of intelligence, of the form or mode of mentality that has reached its highest perfection in man. Along the other line, that of the arthropoda, the evolution has been equally clearly an evolution of instinct, reaching its highest perfection at the end of that line in the ants and the bees. And it seems to me that nothing can be more evident than that along this latter line, at no stage of it has instinct evolved toward intelligence, but always, where the evolution has been progressive, toward more perfect instinct. And also along the other line, at the head of which we place ourselves, evolution has been at the sacrifice of instinct.

These two modes of activity, intelligence as we observe it in man, and instinct as we observe it in the ants and the bees, present a wide contrast. They differ very markedly in respect of two principal characters, which, as Dr Myers says, are universally admitted as a distinction between them, viz. consciousness or unconsciousness of end, and plasticity or fixity of reaction. But neither of these characters, singly or together, constitutes the essential difference between them. They are only a difference of degree. The fundamental difference is one of kind, and lies in the mode of apprehension of reality, and the kind of knowledge that serves the activity of each. It is this essential difference that accounts for the degree of consciousness or unconsciousness, plasticity or fixity that characterises each, and not *vice versa*. It is a difference that does not lie in any outwardly observable character, it is not a scientific but a metaphysical distinction, which rests on a criticism of the nature and limitations of intellectual and instinctive knowledge. Intelligence is the power of using categories, it is knowledge of the relations of things. It is a knowledge that gives us the representation of a world of objects externally related to one another, a world of objects in space, of measurable actions and reactions. In physical science we have the perfect expression of this knowledge, and also the revelation of its limitation. The categories of science are quantitative, it embraces everything that is measurable; that which cannot be measured, life and consciousness, escapes it. And in this is revealed its limitation, it gives us no direct knowledge of reality. To be intelligible is to be explicable in terms of something else. Intelligence is an outward view of things, never reaching the actual reality it seeks to know. If instinct be, as it appears to be, the very opposite of intelligence, an inward looking, a knowledge of things seen from within, of a reality which physical science can only know externally, it is clear that if we as intelligent creatures possess intellectual knowledge alone, we can never know what this instinctive knowledge is. Its nature could at the best be a mere speculation. But if our own consciousness reveals to us that we are not confined to the understanding for all knowledge, but that we have a power of intuition, a direct vision of reality that is not clothed, so to speak, with the categories of the understanding, we shall then possess these two modes of apprehension in our own experience. That this is so is the main contention of Bergson's philosophy, and the ground on which this theory of the nature of instinct rests. I must not attempt to discuss this larger theory. I recognize, of course, that it is not generally accepted.

When we regard instinct and intelligence as endowments of living creatures, the purpose of which is to serve their activity, and direct their behaviour, we can deduce the nature of these faculties from their function. Each appears to carry a natural limitation, the one a limitation of comprehension, the other of extension, in the logical meaning of the terms. The best illustration of this is in the use of tools. The manufacture of artificial objects as tools, particularly the making of tools for making tools, and the power of varying the manufacture indefinitely is a certain mark of pure intelligence. The tool that instinct uses is always organic, generally a part of the bodily structure, but always in organic connection with it. It is incomparably more perfect, judged by its power to achieve its purpose, but it confines its possessor to a very narrow range of activity. The artificial tool, on the contrary, is a very imperfect instrument, but it opens to its possessor a practically unlimited range of action. Composed of inorganic material, it can take any form, serve any purpose, be modified in face of any difficulty. This detachability of the tool is peculiarly a character of intelligence. It is illustrated in language, which is a means of communication by signs which are not adherent to the thing signified. Intellectual knowledge, by enabling its possessor to range countless phenomena under few and definite categories, to represent things in the greatest possible number of points of view, gives a complete command over the material world. Instinct on the other hand, judged by its accomplished actions, appears to possess a knowledge which is perfect and direct, but limited to a particular object, often to a small part of an object. The difficulty in describing instinctive knowledge lies in the fact that we can only communicate by using intellectual categories, and we have to describe that which, if it exists, is distinguished by the fact that it does not take the external form which the category imposes. Bergson has proposed the word "sympathy" in its original, or at least its technical meaning, to express the essentially internal nature of instinctive apprehension. It corresponds to the aesthetic faculty, the feeling-in of a state of one's own into an aesthetic object, that exists in us side by side with our faculty of normal perception.

It is these limitations in the two kinds of mentality that explain the characters of consciousness and unconsciousness, plasticity or fixity, that qualify each. Instinct is normally unconscious and fixed because its range is narrow and its power of choice almost negligible. An instinct is, to quote the definition of Dr Driesch, criticised by Dr Myers and Professor Lloyd Morgan, "a complicated reaction that is perfect the very first time." I do not think that the substantial accuracy of this definition is affected by observations that would seem to show that there are cases where instinctive actions are improved by practice or by imitation. These only prove that in some cases of instinct there is a certain accompanying intelligence. It is consistent with my theory that intelligence should always be present in some degree. The point is that the two things, even when they exist together in the same experience, are fundamentally different, neither derived from nor dependent the one on the other. Instinct does not require or pre-suppose deliberation or choice, these, on the other hand, are the necessary accompaniment of intelligence, and hence intelligent action is ordinarily conscious and plastic. Clearly also, if this view be true, the difference between instinct and intelligence is not the difference between human and animal intelligence, for this is not a difference of kind but of proportion and degree of perfection. Intelligence in human beings has attained a development which has practically obliterated instinct. The instincts of human beings are seen in the first movements of infancy,—such as sucking, crying and crawling,—or in later actions in what are described as tendencies or inclinations. The difference in the mentality is manifest even in these few and feeble remains of instincts, few and feeble when compared with the developed instincts of the higher insects. The movement of the new-born infant to the mother's breast is instinct, because it reveals an innate knowledge of a material thing, apprehended not as an object related to other objects, but as part of the vital activity itself, a knowledge of life by life.

Intelligence, on the other hand, is the innate ability, not manifest till a later growth, to understand the relation of an attribute to a subject.

The observations which Dr Myers has described and which may be held to prove that instinct is often, and may perhaps always be, converted into intelligence do not, it seems to me, lend any support to the view that instinct and intelligence are two aspects of one activity. They do, however, I think, conclusively support the main contention on which the theory I am advocating rests, that instinct is cognition, but cognition of a different kind to that of intelligence. Cognition is the character which distinguishes instinct from activities such as tropism, from all the activities of plant life, and also from the reflex actions that constitute the vital functions of animals. Instinct can become conscious, and, to the extent that it is able to be so, it becomes intelligent. If instinct were not cognition this would be inexplicable. Instinct is able at times to overcome, though to a very slight extent, the limitations of its knowledge, by adding to it the intelligent kind of knowledge. Intelligence seems to surround instinct like a fringe, instinct is the nucleus or the focus of activity, but when circumstances require it, intelligence can come to its aid.

The difficulty of any explanation of instinct and intelligence that regards them as one mode of activity, whether derived from one another or concomitant aspects, is seen in the conflicting theories of their origin. On the one hand we have the neo-Darwinian theory denying the hereditability of acquired characteristics, and declaring psychic evolution to be the result of a natural selection of purely accidental variations in the germ-plasm. On the other hand, we have the neo-Lamarckian theory placing intelligence at the origin of instinct, and at every stage of its development. The former reduces instinct to a pure mechanism, and yet recognizes that it is something more than a mere reflex, the latter makes instinct an intelligence fallen or degraded into automatic and unconscious action. Dr Myers accepts the neo-Darwinian theory and endeavours to reconcile it with a finalistic interpretation. To do so he falls back on his analogy of the relation of object and subject, and represents mechanism and finalism as the objective and subjective characters of instinct and intelligence as concomitant aspects of one psychism. In my view mechanism and finalism are mutual contradictions, both of which are inherent in the limitations which intellectual knowledge imposes on us. They can only be reconciled when we recognize that the true reality is the living activity itself. We must find the ground of instinct and intelligence in the original impulse of the life movement, all that we see is their psychic evolution along two divergent paths, following two main lines of somatic evolution.

Briefly then, the metaphysical theory that finds confirmation in these speculations which it offers as an interpretation of the phenomena of instinct and intelligence is this. Our intellect considered as the endowment of a living creature is not a faculty of pure speculation, but a practical mode of directing its activity, and giving it command over the material world. We may discover its purpose and its limitations, and we may deduce from them the mode of its apprehension of reality, and the nature of the reality which it apprehends, or the form which the reality must assume for it. But beside the intellect, and implied in our knowledge of its limitations, is a power of intuition, that is, of apprehending reality not limited by the intellectual categories, and this reality is the living activity itself apprehended as a real duration. If it be true that there are in our own consciousness these two kinds of knowledge, it is at least highly probable that the modes of psychical activity corresponding to them are to be discovered in other forms of animal consciousness.

IV. INSTINCT AND INTELLIGENCE[1].

BY G. F. STOUT.

Instinctive behaviour determined by congenital endowment with the cooperation of plastic intelligence dependent on interest in the pursuit of ends; distinguished from intelligent, non-instinctive behaviour by the nature of the congenital equipment.—Congenitally determined interest or emotion not as such instinct; only instinct in so far as specific ways of expressing or satisfying the emotion or interest are also congenitally determined.—The instincts of human beings less instinctive than those of animals.

THREE main questions emerge in the papers of Mr Myers, Mr Carr, and Mr Morgan. (1) Is instinct a special way of knowing or is it a merely biological fact? Mr Carr, following Bergson, holds it to be a peculiar mode of cognition. Mr Morgan and Mr Myers concur in regarding it as essentially a form of biological adaptation and as being, in this respect, on a level with reflex action. On this point, I agree with them as against Mr Carr. (2) Is every intelligent action also instinctively determined? Mr Myers says yes; Mr Morgan says no. I agree with Mr Morgan. (3) Is every instinctive action as such also determined by intelligence? Mr Morgan says no; Mr Myers says yes. I agree with Myers, and my position is here much more radical and uncompromising than his.

I propose to deal with each of these problems in turn, and I shall commence with the third,—with the question whether every instinctive action is also intelligent. The crucial issue, here, concerns the nature of the mental process in the first performance of an admittedly instinctive action. If the first performance involves intelligence, it will not be disputed that the same holds true of subsequent performances.

[1] This paper formed part of a symposium on the subject held at a joint meeting of the Aristotelian and British Psychological Societies and of the *Mind* Association in London in July, 1910.

(1) Mr Morgan holds that instinctive behaviour cannot at the outset be determined by intelligent consciousness. His reason appears to be as follows. The only criterion of intelligence is learning by experience: but in the first execution of a train of instinctive movements, there can be no learning by experience, because the required experiences are themselves absent; they only come into being in dependence on the instinctive movements as these run their course. Now I am not disposed to concede that the only criterion of intelligence is learning by experience. But waiving this point, for the present, I would call attention to a curious ambiguity which seems to affect Mr Morgan's argument. An animal in consequence of a train of previous experience, intelligently modifies its behaviour from the outset, when it is again confronted with a similar situation. This implies what we call learning by experience. But when does the animal learn its lesson? Does the actual process of learning take place on the second occasion or on the first? Plainly it takes place on the first and not on the second. On the second occasion the lesson is utilised: but in order to be utilised it must already have been learned. Thus if the actual process of learning involves intelligent consciousness, intelligence must accompany every instinctive act which leads to intelligent modification of behaviour on its repetition in a similar situation. But Mr Morgan's position is that an instinctive action which leads to intelligent modification of behaviour on its repetition may none the less be itself wholly unintelligent. What he regards as implying intelligence is not the actual process of learning by experience, but only its product, the state of having already learnt by experience. Such a view seems to run counter to all that we otherwise know concerning the development of knowledge. Setting aside the special question concerning instinct there is nothing to show that learning by experience is ever an unintelligent process involving merely a sequence of blind sensations and feelings without discrimination and identification and without any apprehension of successive and simultaneous parts as related to the whole and to each other within the whole.

When it is thus exactly defined, Mr Morgan's view becomes, I think, much less plausible. But its plausibility does not wholly depend on the failure to distinguish between the actual process of learning, and its result as expressed in subsequent behaviour. It rests also on the supposed impossibility of mentally referring to the future except in the way of looking forward to an experience which has already occurred on a past similar occasion, and is now recalled by association. The assumption involved in this position seems to be that apart from the associative revival of past experience, the mind can be aware only of its own feelings and sensations as they actually exist at the moment. But all intelligent activity, including that of animals, is directed to the attainment of future results. Hence, where there is no mental reference beyond the immediate present, there can be no intelligence: consequently there can be no intelligence in the first performance of an instinctive action as such. There can be none because there is no prevision of the result to be achieved. This view is plausible and may even appear self-evident. But it will not, I think, bear rigorous scrutiny. In the first place, I would point out that if the mind of the animal is initially aware only of the actual sensations and feelings belonging to the present moment of experience, mere revival by association cannot, of itself, make any difference in this respect. If a past process contains no reference to the future, the mere revival of it will not contain any such reference. Reproduction, abstractly considered, accounts for the renewed apprehension of what has been apprehended before; but it does not of itself account for the emergence of new knowledge. Thus, on the view we are considering, an animal at the commencement of its second performance of an instinctive act, might have its actual present experience in the way of feeling and sensation enriched by elements of the same kind reproduced by association. But this would not, of itself, suffice to explain the birth of the new power of transcending its blind and ignorant present so as to anticipate a future event which, as such, cannot actually be experienced when it is only anticipated. Such a power can in the last resource only be accounted for as involved in the fundamental nature of that relation between mind and reality, or between reality and mind which we call knowledge.

It will of course be said that though the faculty of mentally anticipating the future does not itself depend on the revival of the content of past experience through association, yet such revival may be a necessary condition of its being called into actual exercise, and this position, it may be conceded, has a certain *primâ facie* show of self-evidence. For how, it may be asked, can the mind anticipate when there is nothing to determine what it is that is to be expected by it. How can it look forward to a future which is utterly indefinite? And how can the direction of expectant thought be defined except by previous experience on similar occasions? Such questions seem to me to admit of a simple answer. It is conceded by everybody, and by Mr Morgan in particular, that in the first performance of an instinctive act, an animal is cognisant of a perfectly specific object, which is a complex whole of distinguishable constituents "all coalescent into one felt situation." Further, as Mr Morgan himself admits and maintains, "all experience involves a consciousness of process as transitional and in no wise static." These points being presupposed, I see no intrinsic absurdity in the assumption that even in the commencement of the first performance of an instinctive action, the given situation may be apprehended as about to have a further development. Such anticipation, if it exists, is not wholly indefinite; for the mental reference is to a coming change and development in a certain specific situation, and is therefore, to that extent, itself a specific anticipation of the future. Of course it is relatively indeterminate; for the animal has no clue to the particular character of the changes which are to take place. The particular character of the changes only becomes specified as they actually occur in consequence of the instinctive movements which are specially provided for in the inherited constitution of the animal. The really vital point is, that when they do occur, they occur as the further specification of something already vaguely anticipated, so that each successive stage of the advancing experience involves not only the apprehension of an actual present, but of a future which has become present.

The significance of this can only be appreciated when we consider the process in its conative as well as its cognitive aspect. Given that a certain actual situation is apprehended as alterable, it becomes possible to want it altered. This accounts not only for the mental reference to a further development of the initial situation, but also for the thought of a development required for satisfying a felt want. Thus, under the conditions I am assuming, there will not be merely blind restlessness but conation in the proper sense as active tendency directed to an end, which is not merely an end for an external observer, but for the animal itself. It is true indeed that the animal will initially have no anticipation of the special means by which the end is attainable, or the special form which it will assume. It is precisely this deficiency which is supplied by the inherited constitution of its nervous system as pre-adjusted for a certain mode of behaviour in certain circumstances. But this instinctive equipment will not, in my view, be sufficient to account for the animal's actual behaviour. In the first place it is a mechanism which does not work automatically, but requires to be set in operation and maintained in operation by the continuous urgency of the conative impulse. Further, the active tendency towards an end must, in accordance with all that we otherwise know of mental life, find expression in the attitude of attention. In sharp contrast to what takes place in merely reflex action, the animal will be on the alert to mark whatever new phases the developing situation brings with it. This will be so because it feels interested in the situation, and specially in the situation as having a future. It will, accordingly, show more or less initiative in watching or searching for coming experiences. It will, so to speak, go to meet them. In merely reflex action, on the contrary, there is no such initiative; the organism remains passive till the appropriate stimulus finds it, and then it explodes like a loaded pistol when the trigger is pulled.

The presence of conation will also manifest itself in yet another way; it will make possible more or less appreciation of relative success or failure in the course of the instinctive action, so that steps felt as successful will be persisted in and further developed, whereas steps felt as unsatisfying will be discontinued and sometimes repeated with variations until success is attained. And the strength and persistence of the controlling conation will be measured by the degree of perseverance exhibited in these repeated trials.

It will be seen that I have confined myself to considering what kind and degree of intelligence is abstractly possible in the performance of an instinctive act, independently of what has been learned by experience on prior occasions of a similar nature. But even in a first performance, there is often evidence of intelligence more definite and developed than any I have provided for. The explanation is doubtless to be found in the fact, rightly emphasised by Mr Myers, that there are virtually no first movements, "that no organism can ever execute a new movement which does not involve other movements that have been performed previously." But this consideration, however important it may be in other respects, seems to me of secondary significance in reference to the fundamental problem. It is secondary because it presupposes that the animal has already learned by experience, and does not therefore touch the question as to the conditions under which learning by experience, in the first instance, takes place, the way in which the associations which subsequently operate, are first wrought by the work of the mind.

Let us now turn from the possibilities suggested by our general knowledge of psychical process and consider the observed facts. Here I may safely be very brief. For the observed facts are not, I presume, open to dispute. Animals in their instinctive actions do actually behave exactly as if they were continuously interested in what is for them one and the same situation: they actually behave as if they were continuously attentive "looking forward beyond the ignorant present to meet what is coming." They apparently watch, wait, are on the alert. They also behave exactly as if they appreciated a difference between relative success and failure; trying again when they do not succeed at first, and varying their procedure so far as it is felt as unsuccessful.

I abstain from dwelling further on these points which are abundantly illustrated in my *Manual of Psychology*, Bk. III. ch. I. and are also illustrated by the examples given in the papers read by Mr Myers and Mr Morgan. Here it will be more advantageous to insist on my original question: How can the actual process of learning by experience which is supposed to generate intelligence, be itself entirely unintelligent? How can a series of experiences in the way of blind sensation and feeling result, on a subsequent occasion, in the open-eyed pursuit of an end? So far as I can discover, this is supposed to take place merely through the revival of past experiences by association. But the bare revival of an experience cannot be or contain more than the original experience itself. If this consist of blind sensation and feeling, so will its reproduction. No intelligent alteration of behaviour such as animals actually display could be accounted for in this way. The intelligence is shown in a more or less systematic modification of the whole conduct of the animal when a new situation arises resembling the old one. A simple illustration is afforded by the behaviour of the chickens described in Mr Morgan's *Habit and Instinct*. "A young chick," says Mr Morgan, "had learned to pick out pieces of yolk from others of white egg. I cut little bits of orange peel...and one of these was soon seized, but at once relinquished; seizing another, he held it for a moment in the bill, but then dropped it....That was enough; he could not again be induced to seize a piece of orange peel. The obnoxious material was now removed, and pieces of yolk of egg substituted, but they were left untouched...subsequently he looked at the yolk with hesitation, but presently pecked doubtfully, not seizing but merely touching. Then he pecked again, seized and swallowed." How can such adaptive variation in the whole method of procedure be explained by the mere reproduction of meaningless sensations and feelings? On this view, when present sensations are combined with revivals of past sensations, both the present and the revived experiences will give occasion to their appropriate reactions. This, of itself, will only account for resultant movements, in which the different reactions will be combined in so far as they are compatible, and will neutralise each other so far as they are incompatible. Thus the chick might be expected to perform some movement like that of ejection in consequence of the revived distaste. But there seems to be no good reason why this movement should not be combined simultaneously with the original instinctive action of aiming its beak at the object. What actually happened in the case of the pieces of orange peel was that the chick, after learning its lesson, definitely refused from the outset to have anything to do with them. And when he is again presented with the piece of yolk his whole conduct is modified in a still more systematic way. He looks hesitatingly at the yolk; he then makes a tentative peck, only touching it, not seizing it. When this preliminary trial proves satisfactory, he pecks again, seizes and swallows. The original process in which the animal learned to behave in this manner, cannot, I think, have been wholly unintelligent.

(2) We may now turn to the question especially raised by Mr Carr, the question whether Instinct is a peculiar way of knowing, distinct from what is ordinarily called Intelligence. Like Mr Myers and Mr Morgan, I find myself compelled to reject this view. My reason is, that, so far as the evidence extends, I find nothing in the instinctive behaviour of animals which cannot be accounted for by the combination of certain purely biological adaptations with psychical processes marked by intelligence fundamentally akin in nature to all other intelligence. The prospective attitude of attention, the striving after ends, unity and continuity of attention, the try-try-again procedure, the learning by experience which leads to subsequent modification of behaviour, on similar occasions; all these are characteristic marks of intelligent mental process in general. It is altogether false to suppose that they are found only in connexion with instinctive actions. This being so, there is no special form of psychical activity which requires to be distinguished by the technical term "Instinct." If the term is to have a distinctive and useful meaning it must refer directly, not to a form of psychical process, but to purely biological adaptation comparable to the prearrangements of structure and function which in human beings subserve the digestion of food. Consider, for example, the procedure of the spider in spinning its web. This involves psychical initiative and adaptation on the part of the spider: but if we consider only this aspect of the process there is nothing to mark it as instinctive. But besides this the action is also conditioned by a congenital nervous organisation specially preadapted for the execution, under appropriate conditions, of the movements required

for spinning the web distinctive of the species. This inherited neural prearrangement is in itself no more dependent on psychical process than the possession of the glands secreting glutinous matter. The spider has not made or moulded the nervous mechanism but finds it as a ready made instrument of its activity in the pursuit of ends. Now, it is just so far as the spider's procedure is conditioned by this special nervous prearrangement that its action is instinctively determined. What then is the differentia of Instinct as contrasted with other purely biological adaptations? This question naturally resolves itself into two others. (*a*) How is Instinct distinguished from lower, and (*b*) from higher forms of congenital equipment? As regards the first inquiry, it will be plainly sufficient to differentiate Instinct from the congenital provision which is most nearly akin to it, that for Reflex Action.

The distinction between Instinct and Reflex Action has been already virtually indicated. The marks by which we recognize an action as instinctive rather than reflex are precisely the same marks which show the presence of intelligent consciousness,—conative impulse, unity and continuity of attention, perseverance with adaptive variation of behaviour corresponding to felt success or failure, and, in many cases, the evidence of having learned by experience. The differentia of Instinct, then, as contrasted with a series of reflex actions, however complex, is that in Instinct congenital prearrangements of the neuro-muscular mechanism for special modes of behaviour do not of themselves suffice to explain the animal's conduct. Their biological utility depends from the outset on their operation being sustained, controlled and guided by intelligent interest in the pursuit of ends. A series of purely reflex movements may suffice where a corresponding train of external stimuli comes to the animal, without being sought, in a fixed and appropriate order, so that each in turn separately elicits the required reaction. But where external conditions do not thus adapt themselves to the organism, the organism must adjust itself to them by intelligent initiative and control. So far as this is the case, there is no longer a mere train of psychologically detached reflexes, each determined only by its own separate stimulus. Instead of a sequence of psychologically isolated reactions, we find the unity of a single activity developing itself progressively through its partial phases towards its end. Of course, the degree of intelligence required is more or less, according as the instinctive equipment is more or less complex and specialised. Thus, in such creatures as ants and bees, where instinctive endowment reaches the highest grade of specialisation and complexity, the part played by cooperating intelligence is correspondingly small, though by no means absent. In dogs and apes, on the contrary, the reverse holds good. Though intelligence is throughout necessary to instinct as contrasted with reflex action, yet from another point of view, it is the rival of instinct constantly tending to supersede it. Vital adaptation, in the first instance, calls in psychical adaptation to help it; but, in the sequel of biological development, it is gradually ousted from its place and function by its servant and ally. The climax of this process is reached in man, where instinctive endowment dwindles to the minimum of complexity and specialisation, so that careful scrutiny is required to detect its presence at all.

This brings us to our third main problem :—Is all intelligent activity also instinctively determined? Mr Myers answers this question in the affirmative, apparently on the ground that all intelligent activity depends ultimately on congenital predisposition. The fact may be conceded; but the conclusion drawn by Mr Myers does not follow, unless he can show that the inherited nervous organisation which conditions the development of intelligence, is throughout of essentially the same nature, so that we do not need the word Instinct to mark off a distinct kind of connate endowment. It is on this point that Mr Morgan disagrees with Mr Myers. He insists, with good reason, that we need the term instinct to distinguish congenitally definite modes of behaviour. But what kind and degree of congenital definiteness is required to constitute an action instinctive? Mr Morgan answers that the action as determined by inherited organisation must, from the outset, be "definite enough to be serviceable." The suggestion is valuable and instructive, and I presume that the formula does in the main cover the field of animal behaviour generally admitted to be instinctive. But the suggested criterion is too purely biological to meet psychological requirements. I would, therefore, supplement it by another which seems more satisfactory from a psychological point of view. The kind of congenital definiteness distinguishing instinctive behaviour from behaviour determined by intelligence to the exclusion of Instinct is a definiteness such as would require to be explained as the result of learning by experience or conscious contrivance, if it were not directly provided for by inherited constitution of the nervous system, as determined by the course of biological development. I may illustrate by reference to Thorndike's well-known experiments. A cat placed in a cage where the only means of exit is by pulling a certain loop, gradually learns how to get out, by a prolonged series of tentative efforts; its originally diffused and fluctuating mobility becomes thus restricted within a definite channel, so that whenever it is again confronted with a similar situation, it straightway proceeds to pull the loop. Its action has then, for an external observer, the same sort of definiteness which characterises Instinct. None the less its behaviour is not instinctive; it is not so because it is not directly predetermined by inherited organisation, but becomes gradually determined as the result of learning by experience. On the other hand, if, on being first placed in such a cage, instead of bursting into a series of varying and more or less random efforts, it had at once attempted to pull the loop, then, apart from casual coincidence, we should say that the action of pulling the loop was instinctively, as well as intelligently, determined. It is instinctively determined inasmuch as, without being learned by experience or ideal contrivance, it imitates through biological adaptation the definiteness due to learning by experience or ideal contrivance. We must however carefully distinguish two modes of biological adaptation, both of which are found in instinctive actions. In one class of instances, the power of executing the movements involved in instinctive action has not been adequately provided for by previous practice, so that it has to be referred to congenital endowment. This is so, for example, in the case of the chicken's first peck, or the first dive of Mr Morgan's moorhen, or the first flight of a swallow. But it may also happen and frequently does happen that the kind of movements required for the action and the results following these movements are already familiar through previous experience. The animal already knows how to achieve a certain perceptible result by certain lines of behaviour. It may even be that past experience has made it acquainted with various alternative lines of behaviour leading to the same perceptible result; and it may be that different individuals have learned to prefer different modes of procedure. Under such conditions it might seem that there is no longer any place for Instinct. But a little consideration will show that the animal's behaviour may still have a definiteness like that due to learning by experience or to ideal contrivance, which none the less cannot be accounted for as due to such learning or contrivance. The animal has already learned how to do something; but there may be nothing in its previous experience which prompts it to perform this act, rather than any other, in a certain special situation. It may have had no previous experience of felt satisfaction due to the performance of the act on previous occasions of like nature. If then it uniformly performs this definite act in preference to all others when confronted by the situation, and if in a similar situation the same act is always performed by members of the same species, we are bound to recognize the presence of Instinct as a determining factor. The cats, on being first put into the cage, already knew how to pull a loop down. What they had not learned was that this was the way to satisfy their felt need in the given circumstances. If then, they had uniformly adopted this course without previous trials and failures their behaviour would have been, in this respect, instinctive. Instincts of this kind not only depend for their utility on the concomitant exercise of intelligence, but also in a very great measure on previous exercise of intelligence in the past history of the animal. The conditions under which the solitary wasps finally close up the entrance of their nests seem to be very largely of this nature. They have already had experience of the sort of procedure required to stop a hole. What has to be accounted for by a special neural prearrangement is that they regularly stop this particular hole under certain special conditions. The variation in the behaviour of different individuals may be partly accounted for by differences in their previous experiences. But it may also be due to congenital variations in the instinctive predispositions. For we must not forget that such congenital variations are no less essential to biological development than the principle of heredity.

In conclusion, I shall briefly touch on the nature of congenital dispositions subserving intelligent activity which is not also instinctively determined. Dispositions of this kind do not give rise to actions externally resembling those which would otherwise be due to previous learning by experience. On the contrary, what they provide for is rather a special capacity for acquiring skill and knowledge. And this again may, at least in part, be resolved into special readiness to become more or less intensely and persistently interested in activities and objects of a certain kind, and a special retentiveness for the connected experiences. The result is correspondingly rapid, extensive, varied, and accurate acquisition of knowledge and skill. Even such actions as walking or the utterance of articulate sounds, in the case of the human being, though they may involve, at the outset, something of the nature of a rudimentary instinct, yet in the main depend on such congenital capacity for learning. The child, in the main, learns to walk or to speak by a prolonged course of experimental activity, involving a strong and persistent interest in this special direction, and also a special power of retaining and utilising the results of previous experience. This sort of congenital endowment is well illustrated by the difference between men of genius and ordinary persons. Though most of us have more or less of a gift for music, yet there are few comparable with Mozart in this respect. Mozart's interest in music was, almost from infancy, intense and absorbing. His retentiveness in this direction was correspondingly great. Practically, he forgot nothing which interested him. When fourteen years old, he wrote down from memory an elaborate composition which he had only heard once in the Sistine Chapel. But Mozart had not an Instinct for music in the strict technical sense in which we speak of the Instincts of animals. He had not a congenital aptitude specialised for the production of certain pieces of music, as, for instance, some singing birds sing the song characteristic of their species, without requiring to learn it by experience or imitation. In principle, Mozart acquired his musical skill and knowledge in the same way as ordinary people. His exceptional natural gift did not make any essential difference in this respect. The difference lay rather in his greater capacity for learning by experience and by free experiment, and the greater capacity was conditioned by intense and absorbing interest, and by extraordinary retentiveness. Hence his originality. All of us are, in a sense, more or less original. But our original productions are apt to appear commonplace to other men. What is genuinely new, so far as we ourselves are concerned, turns out to be insipid repetition when compared with past achievements in the same direction. But Mozart started from a point in advance of the general progress of the race; and consequently what was a new development for him was also conspicuously novel for mankind in general.

Instinct is no such capacity for learning by experience as I have attempted to analyse. It is mainly confined to animal life, and in the life of animals it has a twofold function. On the one hand, it is a substitute for learning by experience. On the other, it has an educative value as a condition of learning by experience; it has this value inasmuch as it provides an animal with the experiences which are useful to it, and thus enables it to learn just what it requires to learn. In the case of human beings, this function of instinct is, in the main, superseded by Instruction. All that either Instinct or Instruction can do is to supply appropriate experiences. How this material will be utilised depends on other factors. You may bring a horse to the water or the water to the horse, but you cannot make him drink.

The importance of Instinct as a substitute for the lessons of experience, becomes greater as we descend the scale of animal intelligence.

But the more instinct serves as a substitute for experience, the more fixed and specialised must the instinctive equipment be, in order to provide in advance for the special exigencies which arise in the life-history of the animal. On the contrary, in proportion as the educational function of instinct becomes more pronounced, instinctive endowment becomes less fixed and specialised. As we pass up the scale of animal life from insects and fishes to man, we find an increasing difference of this kind. Almost every special phase of the life-history of ant or bee is provided for by instincts of a highly specialised kind relatively incapable of modification by experience. The dog or the cat is, in this respect, strongly contrasted with the ant, and in the case of human beings there are hardly any well marked instincts as distinguished from special capacities for learning by experience in certain directions.

V. INSTINCT AND INTELLIGENCE[1].

By WILLIAM McDOUGALL.

Instinctive actions imply innate perceptual dispositions.—Mental process a wider class than intelligent process.—Protest against usage of "instinct" to denote particular actions.—Some reasons for rejecting the Bergsonian view of instinct.—Instinct cannot be adequately described in terms of mechanism.—Criticism of Prof. Lloyd Morgan's definition of instinct.—Examination of Prof. Stout's conception of instinct and his consequent denial of human instincts.—Inadequate recognition of the human instincts a grave defect of present-day psychology.

In regard to the questions to which my colleagues in this symposium have chiefly directed our attention, I agree so closely with Dr Myers and Professor Stout that I may content myself with a few very brief remarks upon them.

Myers tells us that he regards the conceptions of instinct and intelligence as but two different ways of conceiving the same thing, namely mental process in general; the two conceptions being arrived at by considering mental process from the outer and the inner standpoints respectively. If this means that he regards instinctive processes and intelligent processes as of essentially similar nature, as involving the same fundamental modes of mental activity, then, like Stout, I agree with him entirely. But, if he means that we cannot properly and usefully distinguish between mental processes that are conditioned wholly or mainly by innate dispositions on the one hand, and on the other hand such as are conditioned by dispositions that have been largely built up through the experience of the individual, then I do

[1] This paper formed part of a symposium on the subject held at a joint meeting of the Aristotelian and British Psychological Societies and of the *Mind* Association in London in July, 1910.

not agree with him. For this distinction seems to me important, and I think the words instinctive and intelligent may properly be used to mark this distinction.

I agree with Myers in believing that, on the first occasion of the exercise by an animal of any instinct, the sense-impressions by which the reaction is evoked and guided are not to be regarded as giving rise to mere sensations or sensation-complexes without meaning. But I do not agree with his statement that this element of meaning in the purely instinctive reaction "has hitherto been completely ignored." For I myself have written of such processes that the sense-impression "must be supposed to excite, not merely detailed changes in the animal's field of sensation, but a sensation or complex of sensations that has significance or meaning for the animal; hence we must regard the instinctive process in its cognitive aspect as distinctly of the nature of perception, however rudimentary[1]."

And I am prepared to go further in this direction than Myers. I am inclined to believe that some of the instinctive activities of animals are guided by anticipatory representations of the ends to be achieved, representations that owe little or nothing to previous individual experience. This, I say, is to go beyond Myers; for, like Professor Lloyd Morgan, Myers seems to hold that meaning is necessarily given by "factors of revival." But I hold that "the factors of revival" are conditioned by dispositions built up by perceptual experience; that just such dispositions may be, and are, provided in the innate constitution of the animal; and that these play in instinctive activities the same role that similar acquired dispositions play in the activities generally called intelligent. I hold further that there is no such fundamental difference between the dispositions that condition perception and representation respectively, as to warrant us in drawing a rigid line between them, and in saying that, while dispositions subserving perception may be inherited, those subserving representation are not, or cannot be, inherited[2]. If I were asked what forms of instinctive activity seem to me to imply such innate representations, I would point especially to the nest-building of birds and, perhaps, to the spinning of spiders[3].

[1] *An Introduction to Social Psychology*, p. 28. London, 1908.

[2] It must not be supposed that I am here assuming the inheritance of acquired characters. I leave untouched the question of the origin of these hereditary dispositions.

[3] If anyone is inclined to scoff at this suggestion, I would ask him to contemplate and reflect upon the nests of the weaver-birds of southern India. The construction of such nests seems to me to imply on the part of the birds either an understanding of

In this connexion I would note a point in which I cannot agree with Stout. Stout will not agree to restrict the designation intelligent to processes that involve modification of innately determined modes of behaviour; he maintains that the process that is capable of resulting in such modification is *ipso facto* intelligent, whether or no such modification of innate dispositions be effected by it. It seems to me that Stout is here rejecting a very useful definition of intelligence which, thanks largely to the work of Lloyd Morgan, has become widely accepted. Will it not suffice to say that the activities of a nature modifiable by experience are *ipso facto* mental or psychical; but that intelligence is not operative, is not manifested, if no modification of the innate tendencies is effected? My point may be illustrated by reference to those insects which perform a series of perceptual reactions upon certain objects on a single occasion only. The Yucca moth[1] is, I believe, such an insect. The laying of its eggs involves the perception of the form and parts of the Yucca flower; i.e. it involves an act of psychical synthesis by which the complex of sense-impressions received from the flower by the sense-organs of the insect are combined in a psychical unity; and it involves the successive discrimination of parts within this psychical unity; and this psychical unity is, I submit, an essential condition of the motor reactions by which the biological end is secured; it is an essential link between the multiplicity of physical impressions and the multiplicity of physical movements which constitute the train of behaviour. It is this psychical unity which evokes the conative tendency by which the whole train of perceptual activities is maintained; for, in Stout's words, the animal does not "remain passive till the appropriate stimulus finds it"; rather, it goes out to meet it, "showing more or less initiative in watching or searching for coming experiences." The behaviour thus differs fundamentally from a mere sequence of mechanical reflexes; it implies a distinctly mental process; yet if the act is normally performed only once, or if on repetition it is performed without adaptive modification, it may, I think, properly be called purely instinctive and not at all intelligent. I would add that, though such purely instinctive activity seems to be theoretically

mechanical principles and intelligent foresight of the needs of themselves and their young, or innately conditioned representations of the form of the nest. And of the two alternatives I prefer the latter. I hold that such innate powers are exceptional. I do not, like Prof. Camillo Schneider (*Vorlesungen über Thierpsychologie*, 1909), see evidence of innate representations wherever there is apparent teleological adaptation of behaviour among the lower animals.

[1] See Lloyd Morgan's description of its behaviour in *Habit and Instinct*, p. 14.

possible, I recognize that it may perhaps never actually occur; and that instinctive activity usually involves some modification of the dispositions concerned which makes itself felt in the course of subsequent activities. The distinction I am defending is therefore one of purely theoretical and minor importance.

Against one minor feature of Myers' paper I wish to protest, namely against his usage of the word instinct to denote an instinctive action. It is true that in this he has the sanction of general usage; but to describe any particular action as an instinct is, I submit, a loose and confusing usage against which we ought to set our faces. We ought rather to use the term an instinct to denote that feature of the innate constitution of any organism, that inherited disposition, in virtue of the possession of which the organism acts instinctively; just as we ought to distinguish between a habit and the habitual actions of which the habit is the enduring condition, or between a memory trace or disposition and the act of remembering, or between character and volition. In each case the former member of the pair of terms denotes some enduring condition of the mode of activity denoted by the second. The distinction may seem trivial and hardly worthy of strict observance; but it has, I think, considerable importance. Much confusion would have been spared to psychology if writers had always observed the similar distinction between an idea, a thought, or an act of representation and that enduring something which renders possible its reproduction.

I am inclined to believe that to the non-observance of this distinction is largely due the common tendency to confine the connotation of the words instinct and instinctive to the motor side only of the instinctive process. Against this tendency I shall find occasion to protest before concluding this paper.

I am wholly at one with Myers, Lloyd Morgan, and Stout in rejecting Prof. Bergson's doctrine of instinct and intelligence, so ably expounded and defended by Mr Carr. Like Stout, I cannot see any good reason for believing that there is any "special form of psychical activity which requires to be distinguished by the technical term instinct." For I hold that instinctive and intelligent activities are only to be distinguished by reference to the origin of the dispositions by which they are conditioned. When these dispositions have been wholly or mainly given in the innate constitution, the activity is properly and conveniently called instinctive; when they have been formed or much modified by the earlier experience of the individual (i.e. when present behaviour is in large measure regulated in accordance with past experience), we properly speak of it as intelligently controlled; there is thus no sharp line to be drawn between the two classes of behaviour; the distinction is gradual, and, with the few possible exceptions of actions such as those of the Yucca moth referred to above, all mental activities are both instinctive and intelligent in the sense that they are conditioned by dispositions which have been given in the innate constitution, but which have been modified and developed more or less extensively by individual experience.

I can discover no force in the argumentation by which the Bergsonian doctrine of instinct is supported. Whatever plausibility it may seem to have is achieved by confining attention to the most purely instinctive activities on the one hand, and to the most purely intellectual on the other hand; and by falsifying the description of both. Following Bergson, Mr Carr draws his examples of instinct only from the insects. Taking as his principal example, the securing of her prey by Ammophila, a solitary wasp; and accepting a description of the process which has been shown to exaggerate greatly the precision and regularity of the movements of the wasp and of their effects, as also the strictness of the conditions that have to be satisfied by her behaviour, if it is to achieve its biological end; Carr refers to her movements as unerring and invariable, and as revealing "a knowledge which is perfect and direct." Dr and Mrs Peckham have lately observed and described this behaviour without the theological bias that seems to have dominated and partly vitiated the work of Fabre. They have shown that the whole train of activity is very variable and by no means unerring in securing the most favourable result; that, in short, it is just of the type of perceptual activity conditioned by innate dispositions unmodified, or but little modified, in their nature and operation by individual experience. The wasp merely stands over her prey and, passing the end of her abdomen along the under side of its body, thrusts in her sting several times at the spots where it encounters the intersegmental rings. It can hardly fail to happen that the sting sometimes reaches a nerve ganglion or its immediate neighbourhood; but there seems to be no reason to believe that it generally does so. And the effect of the operation is frequently the immediate death of the caterpillar, an event which does not render it unpalatable to Ammophila's grub.

Carr asserts that in the *Arthropoda* evolution has been clearly an evolution of instinct reaching its highest perfection in the *Hymenoptera*. "Nothing," he says "can be more evident than that along this latter line, at no stage of it has instinct evolved towards intelligence, but always where the evolution has been progressive, toward more perfect instinct." But it is just among the solitary wasps, from which he has chosen his example of instinct, that we find evidence of a degree of intelligence which (with the doubtful exception of that of the higher mammals) approaches most nearly to the human. Let anyone read the second chapter of the Peckham's book[1], where they describe the way in which Ammophila returns on foot to her nest, dragging her prey over scores of yards of irregular ground and between the stems of a field of tall corn; let him consider the wealth of facts which show that this finding of the way back to the hole prepared for the reception of her prey depends upon the power of visual recognition of the features of the spot, and upon a detailed knowledge of the locality, of the objects and their spatial relations, gradually built up by weeks of flying and walking to and fro; let him reflect on these facts with an open mind, and he will be brought to recognize that Ammophila acquires and intelligently guides her movements in the light of a knowledge of her chosen locality; a knowledge which, allowing for the peculiarities of the compound eye, is very similar to the knowledge in virtue of which we ourselves are able to find our way about a familiar neighbourhood. He may indeed be led to conclude that, in respect to this special form of intelligence, some of the solitary wasps actually surpass some adult human beings; and I, for one, should hesitate to disagree with him[2].

This capacity of building up a knowledge of a locality by many successive visual perceptions, so highly developed in the solitary wasps, seems to me especially instructive as regards the relation of instinct to intelligence; it illustrates beautifully the cooperation and reciprocal dependence of instinct and intelligence. For this intelligent mental activity, so similar to our own, is absolutely essential to the success of Ammophila in the task which absorbs so much of her energy, the task of providing food and shelter for the offspring which she will never see

[1] *Wasps Social and Solitary*, London, 1905.

[2] As examples of the class of facts I refer to I may mention the following;—the habit of many solitary wasps of making a locality-study before leaving a newly made nest; the difficulty in finding the nest occasioned the wasp by any rearrangement of the surrounding objects during her absence; the occasional failures to find the nest, rectified sometimes by prolonged and eager exploration; the experiments of Forel and of von Buttel Reepen which show the dependence of bees and wasps upon visual recognition of form and colour (A. Forel, *The Senses of Insects*, and H. von Buttel Reepen, *Sind die Bienen Reflex-Maschinen*?); the observations of the same authors and of Father Wasmann which show the large part played in the life of the ants and bees by familiarity with a variety of odours acquired by individual experience.

and of which she presumably has no knowledge of any kind. Her instincts prescribe the ends and the general form of her activities, the digging of the hole, the prolonged search for the caterpillar, the stinging of it, the carrying of it to the hole, the laying of her egg upon it, the sealing up of the hole; the whole train of behaviour being a conative process of the most pronounced type, no doubt mostly on the perceptual level, a series of perceptual reactions maintained and led on from stage to stage by strong conations. But, even if we admit that many of the details of this train of behaviour are narrowly prescribed by innate dispositions, we have to recognize that all this instinctive activity would be futile without that cooperation of the products of previous individual experience which enables the wasp to find her way back to the hole she has prepared for the reception of her prey.

Again, in the final act of this combined operation of instinct and intelligence, Ammophila, chosen by Mr Carr and Professor Bergson as the type of the purely instinctive creature, sometimes displays just that mode of activity which Bergson has declared to be the most characteristic of intelligence, namely she uses a tool; she takes up a peeble and uses it as a ram to pound down and smooth off the earth with which she seals her nest[1].

These are but a few of the most striking of the facts which show that among the solitary wasps there has been a very considerable evolution of intelligence of the same order as our own; and the degree of this evolution will appear all the greater, when we reflect that these wasps are deprived, save in a most meagre degree, of all the advantages for intellectual development of a period of youth. For the very considerable difference between the mental life of the higher insects and that of the higher mammals consists not in that the former is purely instinctive and the latter largely intelligent; both are richly endowed with instincts and also with a considerable capacity for profiting by experience. It is determined rather by the fact that in the

[1] Since Prof. Bergson and Mr Carr have chosen to rely on Fabre's description of the behaviour of wasps, ignoring the more recent observations, they should not ignore the passages in which Fabre himself has described intelligent control of instinctive tendencies by the wasps; e.g. of Ammophila Fabre writes—"Alors la méthode de la compression cérébrale est une ressource que l'hyménoptère emploie à sa guise, lorsque les circonstances le réclament, lorsque la proie, par exemple, parait devoir opposer quelque résistance pendant le trajet. Le mâchonnement des ganglions cervicaux est facultatif; l'avenir de la larve n'y est pas intéressé; l'hyménoptère le pratique, lorsque besoin en est, pour se faciliter le travail de transport." *La Théorie de l'instinct.* Nouveaux Souvenirs Entomologiques, par J. H. Fabre, Paris, 1883.

insects the development of this capacity is severely limited by a fundamental circumstance of their life history; namely, the possession of an external chitinous skeleton by the adult creature necessitates the process of metamorphosis by which the imago is suddenly launched fullgrown upon the world to follow a mode of life radically different from that of the grub; hence the experience of his grubby youth can be of little or no service to him, and he is called upon to shift for himself in a life that is altogether new to him; he therefore needs to have most of his instincts in full working order from the first. The insect is thus deprived of that period of youth under the fostering care of parents which among all the higher mammals is a principal condition of the development of such intelligence as they display. In the young mammal the instincts ripen slowly and successively at considerable intervals; many of them come into operation only when the young creature has acquired considerable control over its bodily organs; and they are exercised in playful activities which lead to the accumulation of experience and considerable modifications of the innate dispositions, before they are needed for the carrying out of the serious tasks of life. This period of youth, characterised by the accumulation of experience in preparation for the serious tasks of life, reaches its maximum in the most developed forms of human life, and its long duration is one of the principal conditions of all the higher developments of the human mind; the vast accumulation of experience thus rendered possible modifies so greatly the innate dispositions and their modes of operation as to obscure completely for some of us the fact of their existence; just as in the higher mammals a lesser degree of accumulation of experience obscures in a lesser degree the nature of their innate dispositions.

If we put aside all the evidence of intelligence similar to our own in insects and other animals and consider for a moment the nature of some of their admittedly instinctive activities, it remains very difficult to understand how instinct as conceived by Bergson should achieve them. Instinct, we are told, is an unconscious knowledge, a sympathetic intuition of life, of true process and duration; whereas intelligence is a knowledge of the static relations of things, which falsifies all true process by representing it as a succession of positions in space. This view of the nature of instinct may seem plausible when we keep in view only the behaviour of the social animals towards their fellows; but, if it is accepted, it remains difficult to see how such sympathetic intuition of the inner life of the caterpillar should guide Ammophila to plant her sting in its ganglia; it is much as though we were asked to believe that a sympathetic understanding of the effect of a "knock-out blow" on the jaw will guide one's fist in planting such a blow. And it is even more difficult to understand how such a sympathetic intuition of life-processes should guide a bird in the building of its nest with sticks and hair, or a honey-bee in the construction of its comb.

I turn to venture a critical remark on Professor Lloyd Morgan's conception of instinct. Lloyd Morgan holds the view that the strictly mechanical interpretation of natural processes is the only one permissible to science. He is thus committed to the doctrine that instinctive action is merely compound reflex action (reflex action being regarded as a purely mechanical sequence of events); and in this way he is led to ignore that aspect of instinctive process which to me seems most fundamental, namely its conative character. In so far as he recognizes conation as a mode of mental process, it is merely as a sum of muscular or kinaesthetic sensations arising by "backstroke" when the mechanically determined action is effected. I suggest that it is just because his conception of instinctive process neglects the conative factor that he cannot see in human behaviour the operation of instincts, in the sense in which I have used the word. For what I have called instincts are for him nothing but certain groups of capacities for movement rooted partly in innate nervous structure, the members of each group having nothing in common but their subservience to some one biological end, such as reproduction or the rearing of offspring. If the view I take be true, the acceptance of Lloyd Morgan's conception of instinct would cut us off from the understanding of the deeper relations between the human and the animal mind and of the continuity of mental evolution. For I hold that the instincts are essentially differentiations of the will to live that animates all organisms and whose operation in them makes the essential difference between their psycho-physical activities and the physical processes of inorganic nature. Or, to adopt one of Bergson's striking phrases, I hold that the instincts are differentiations of "l'élan vital" by means of which it pushes on along diverging paths, creating by their agency the various great families of the animal kingdom; each animated by the great instincts common to all, the tendencies to seek food and to reproduce its kind; each animated also by special instincts characteristic of the group; each instinct creating for its own service the bodily organs and the nervous structures best suited to serve it as the instruments by which it may secure the satisfaction of its conative impulse[1].

[1] This view is in no way inconsistent with Darwinian principles. I insist merely upon the fact that conation, though too often neglected by biologists, is one of the fundamental factors necessarily presupposed by the Darwinian scheme.

I cannot accept Lloyd Morgan's proposal to define instinctive behaviour as that which "is practically serviceable on the occasion of its first performance." For this definition fails to distinguish it from purely reflex action on the one hand, and from the behaviour governed by the most developed reason on the other. Further, while I agree with Lloyd Morgan in holding that the imperfections of many instinctive actions on their first performance render unacceptable the definition proposed by Driesch, I think these imperfections are so great in many cases as to render his own definition untrue of much instinctive behaviour. As I pointed out above, it is a peculiarity of the higher mammals, which enjoy a period of youth, that the motor expressions of most of their instincts are very imperfect at the period of the creature's life in which the conative tendency of the instinct first comes into operation; for this is a necessary condition of all acquirement of skill. When the young kitten attentively watches the dangled button or the rolling ball, and makes its first futile effort to seize it, its behaviour is instinctive, but can hardly be called "practically serviceable."

I cannot conclude my paper without touching on a question which has been almost ignored by my colleagues, but which, to my mind, gives to the topic of this symposium most of its interest and all of its importance; namely, the question of the place of instinct in the human mind.

When in a recent work [1] I set out to describe the human instincts and to exhibit them as the foundation of all our mental life, I believed that I was but attempting to give greater precision to a generally accepted view. I was therefore as much surprised as disappointed to find that all of my colleagues assign to instinct a vanishingly small part in the development and operations of the human mind. They seem to be agreed in recognizing the sucking, wailing, and crawling of the infant as having some instinctive basis; but beyond this none of them is prepared to go. From this it follows that they must regard the whole of my book on Social Psychology as purely fanciful, as an utterly baseless and worthless fabrication. I ought, perhaps, to be grateful to them for having so tactfully avoided any painfully plain statement of this opinion. Nevertheless it was, it seems to me, their plain duty to make this plain statement, in order to put a stop, if possible, to the propagation of so much false doctrine among the rising generation.

This rejection of the human instincts I find most difficult to understand on the part of Professor Stout; for in respect to the nature

[1] *An Introduction to Social Psychology*, 1908.

of instinctive activity I regard myself as but a humble follower of him. I venture to think that his negative attitude towards the human instincts involves some inconsistency, in terminology at least, if not in principle. Like Lloyd Morgan, Stout holds that all that is distinctive of instinctive process is that it vents itself in bodily movement through innately coordinated motor channels; he writes—"it is just so far as the spider's procedure is conditioned by this special nervous pre-arrangement (i.e. of purely motor channels) that its action is instinctively determined"; and further—"the differentia of Instinct, then, as contrasted with a series of reflex actions, however complex, is that in Instinct congenital prearrangements of the neuro-muscular mechanism for special modes of behaviour do not of themselves suffice to explain the animal's conduct. Their biological utility depends from the outset on their operation being sustained, controlled, and guided by intelligent interest in pursuit of ends[1]." Stout, then, agrees with Lloyd Morgan in holding that instinct consists wholly in neuro-muscular mechanisms the operations of which are in themselves purely mechanical; but, whereas Lloyd Morgan holds that this mechanism suffices in itself to carry out the instinctive action, Stout holds that such action would be a mere train of reflexes, and that instinctive action is essentially the operation of such a mechanism under the extrinsic control and support of intelligent conation.

Stout's reasons for denying all human instincts, save the few which manifest themselves in early infancy, may then be stated as follows:—The cognitive, conative, and affective aspects of instinctive process are common to all forms of mental activity; the only feature peculiar to instinctive activity is the expression of mental process by means of innately coordinated motor mechanisms; the only processes of the human mind that express themselves through such mechanisms are certain processes of early infancy; therefore these are the only instinctive processes of the human mind.

I submit that to deny on these grounds, as Stout does, all the principal human instincts is a logically unjustifiable and very inconvenient and confusing procedure. For Stout, as we have seen, only those mental processes are instinctive which manifest themselves through innately coordinated motor channels. Now all our mental processes manifest themselves through the agency of preformed motor coordinations, innate or acquired. For Stout, then, as for me, instinc-

[1] How Stout can reconcile this position with some form of the doctrine of psycho-physical parallelism to which, I believe, he subscribes, I cannot understand.

tive process can be marked off from other modes of behaviour only by reference to the origin of some part of its conditioning factors in the innate constitution of the organism. For Stout the innate factors by which it is marked off are the motor mechanisms only by which the mental process manifests itself in bodily movement; for me they are also and chiefly the innate disposition by which the whole instinctive mental process is conditioned.

Stout can hardly deny that a purely instinctive mental process is conditioned throughout by innate dispositions; and he will hardly deny that the processes of the adult human mind are conditioned by dispositions that are largely the products of foregoing mental activity, of the previous experience of the individual. I would, then, put this question to Stout—Why should he refuse to accept as the mark of instinctive process the innateness of the dispositions by which it is conditioned in all its stages, accepting instead the innateness of the motor mechanisms through which the instinctive process merely manifests itself in bodily movements?

I can think of no satisfactory answer to this question; and I can see many grave objections to the acceptance of Stout's position. First, it seems to imply an extreme form of the dualistic theory of soul and body; or, at least, it implies an artificial and unwarranted distinction between the innate motor mechanisms and the rest of the innate psycho-physical dispositions which are concerned in the instinctive process; for it seems to imply that the mental process runs its course in a sphere wholly apart from the motor mechanisms, and that it plays upon these mechanisms from above, as, according to the old doctrine, the soul was conceived playing upon the motor nerves as upon the keys of a piano.

Secondly, it arbitrarily selects one part of the total psycho-physical system by which the instinctive process is conditioned, and, separating it from the rest of the system by an unwarrantable abstraction and neglecting the rest of the total disposition, it makes the operation of this one part alone the mark of instinctive process.

Thirdly, the part of the total instinctive disposition thus arbitrarily selected is the part of least importance. I submit that the specific conative tendency revealed in each instinctive process is a far more important and characteristic feature of the process than the operation of innate motor coordinations. Why then does Stout, in seeking for the differentia of instinctive process, select the less important and reject its more essential feature? Merely, I take it, because the more essential feature, the specific conative tendency, continues to reveal itself at all levels of mental development and throughout the life of the human mind, while the innate motor factor comes clearly into view only in instinctive processes that are relatively pure. Now, if there were good reason to believe that the innate motor coordinations were wholly lacking to the human instincts, or that they become wholly obscured and inoperative in the developed human mind, there would be some slight and partial justification for Stout's position. But neither alternative is true. The innate motor coordinations are not wholly lacking or wholly superseded by acquired coordinations in the human organism, not even in the most highly civilised of mankind. I will ask first—are they wholly lacking or superseded or suppressed in such an animal as a dog? The dog, coming upon the trail of a rabbit, or approached by another dog while enjoying his bone, makes use in either case of a system of innate motor coordinations which operate in a similar way in all members of the species; through them a specifically directed conative tendency works towards its biological end; and their operation under the guidance and control of this specific conative tendency produces a train of bodily attitudes, movements, and visceral changes, constituting the characteristic expression of the total innate disposition which is the instinct. An unsophisticated and hungry man also, when threatened with the forcible abstraction of his savoury mess of potage, reacts in an equally specific manner; in him a similar specific conative tendency is aroused and finds expression through an innately coordinated system of motor channels which is alike in all members of the species. And the fact that some members of civilised societies acquire by long training and practice the power of suppressing well nigh all these characteristic expressions, save those that are effected by organs of the visceral system, merely obscures for us, but does not essentially affect, the similarity of the process in man and dog.

In a similar way each of the specific conative tendencies of the human mind has at its service a system of innately coordinated motor channels, which continues to be operative in some degree whenever the conative tendency is excited. When under the spur of a natural impulse I fold my arms about one of my children and, with a special facial expression and an emotion which are equally involuntary and unpremeditated, press my face to his, I am persuaded that I am making use (or a specific conative tendency which is an innate part of my constitution is making use) of innately coordinated motor channels, a system of channels which, no doubt, has been modified in some degree by previous mental activities.

I cannot doubt that the same is true also when I start back instantaneously from a sudden loud noise with a specific complex of visceral changes; or when a sharp word of rebuke causes a young child or a dog to shrink and to remain motionless with lowered head and averted face.

Nor have we any sufficient ground for believing that the motor coordinations provided in the innate constitution of each organism for the service of its specific conative tendencies are any less complete and definite in ourselves than in the dog, the ant, or the wasp, or even the Yucca moth. For, with the exception of those that subserve the life of the first months of infancy, they are called into operation within us only when considerable powers of voluntary motor control and a variety of motor habits have been acquired, the presence of which inevitably obscures the nature and degree of the innate coordinations.

To sum up my objections to Stout's position stated in the three foregoing paragraphs, I would say that each innate specific conative tendency has at its service an innately coordinated system of motor or efferent nerve-channels; that these belong together functionally and phylogenetically as one feature, one psycho-physical disposition, of the inherited constitution of the organism; that each implies the other; that instinctive activity always involves their cooperation; and that, of the two, the conative tendency is the more essential and fundamental feature of the total innate disposition which is the instinct.

Fourthly, there remains another and, perhaps, even more serious objection to Stout's position. The unmodified operation of innately coordinated motor channels is, as Stout asserts, one of the marks of purely instinctive activity; but that it is the only one, as he implies, is not, I submit, true. Purely instinctive activity is no less clearly marked off from all other mental process by the fact that the cognition, the perception, which evokes the instinctive movement, is also conditioned by the operation of an unmodified innate perceptual disposition. The fact that each species of solitary wasp seizes and stings and carries off animals of one class only, one species preying only on caterpillars, another only on spiders, a third only on grasshoppers, and so on, this fact implies that the conditions of this instinctive activity comprise in each species an innate perceptual disposition no less highly specialised than the motor mechanisms that subserve the ends of the instinct. And the cooperation of the unmodified innate perceptual disposition is as peculiar to, and as distinctive of, purely instinctive activity as is the cooperation of unmodified motor coordinations. For, in all other mental processes, the innate perceptual dispositions cooperate, when they do so, only after more or less profound modification through foregoing mental activities in which they have played their parts.

The same truth may be illustrated by the behaviour of the chick which, among all the sounds of a summer day, responds in a specific manner, with a specific conative tendency and a system of innately coordinated movements, and without any guidance from previous experience, to a particular call uttered by his mother. This can only be because his innate constitution provides some special disposition that selects this sound from among all the rest and makes of this sense-impression an effective excitant of the specific conative tendency.

Stout will not, I presume, contend that the possession of the innately coordinated motor mechanisms will suffice to account for such facts of selective perception effected independently of previous individual experience; nor will he, I presume, contend that the possession of the innate conative tendency to fly to the shelter of the mother's wing's suffices to account for the fact that this one sound excites this tendency, while most other sounds fail to do so. And, if he is not prepared to sustain either of these positions, I cannot see how he can escape the point of my argument.

These innate perceptual dispositions continue to be present and operative in the developed human organism, though their constitutions are subject to much modification and differentiation through experience. Is not this truth illustrated when (as truly depicted in Mr Galsworthy's play, "The Silver Box,") a woman in whom the maternal instinct is strong finds her attention irresistibly drawn to, and fixed upon, the wail of a child that comes up to her drawing room mingled with all the noises of a London street; and when this sound evokes in her one of the specific conative tendencies that are the foundation of all her mental life? Is it not equally well illustrated when the youth's gaze and attention are irresistibly attracted by the female form?

A typical example, then, of a purely instinctive action, implies the existence in the creature's innate constitution of, first, a specialised perceptual disposition; secondly, a specific conative tendency that is excited when this perceptual disposition is played upon by the appropriate sense-impression; and, thirdly, some coordinated system of motor channels through which the conative tendency works towards its satisfaction. The three things belong together; each implies the other two; each can subserve the life of the organism or of the species only in conjunction with the other two; all three together constitute a functional unit which is transmitted as such from generation to generation; and to such a functional unit of the innate constitution only, and to no part of it alone, and to no other fact or feature of the organic world, can, I submit, the name instinct be properly applied.

But now a doubt seizes me. Perhaps Stout does not accept the view that our inherited constitution comprises specific conative tendencies, tendencies directed towards particular biological ends and only to be satisfied by the attainment of those ends. For I am not aware that he has explicitly avowed his acceptance of this view in any of his writings. Perhaps he holds that we inherit merely certain capacities for acquiring certain kinds of knowledge and skill, such as he illustrates by reference to the musical faculty of Mozart. I admit that such special faculties as the musical and the mathematical present a very obscure problem, towards the solution of which I am not able to offer any suggestion; but I submit that the conative tendencies which I have described as the most fundamental and essential features of the human instincts are of a different order from those highly special faculties. Is the maternal tendency nothing but a power of acquiring knowledge about children? Or is Stout prepared to follow Bain in deriving maternal love wholly from the pleasures of contact with the soft skin of the infant? Is the sex-tendency nothing more than a special facility for acquisition of knowledge about the other sex?

If Stout does not recognize the presence of specific conative tendencies in the innate constitution of the human mind, how does he propose to account for the facts of sex, or the facts of the maternal relation in human life? If he does recognize them, why has he not attempted to define them, and how can he justify his position that they are not integral parts of the instincts? Surely, among the mammals the relations of the sexes are regulated by instinct, the motor activities of sex are maintained and guided by a specific innate conative tendency. Surely, the same is true of the behaviour of the animal mother towards her young. And, if these are instinctive modes of behaviour, surely, the corresponding modes of behaviour in human beings are also the expression of similar instincts. To deny this would be, it seems to me, to fly in the face of old established usage of language, and to obscure and deny the most fundamental similarities between the human and the animal mind.

Stout has written many true things of conation, and no one can admire more than myself his treatment of the topic and his fine constructive work in general; but his psychology has always seemed to me to hang in the air, to be a superstructure without foundation, just because he has never described, defined, or even enumerated the specific conative tendencies that are implied by it. I believe that, if he will undertake to supply this missing foundation, he will be led to modify his doctrine of instinct in a way that will leave little scope for disagreement between us.

Stout is not alone in building his psychological superstructure without having dug out foundations for it; such neglect is the most common and most striking defect of much of the most interesting psychological works of recent years. To illustrate this statement I need only to refer to the works of Professor Freud of Vienna and of Professor Jung of Zürich and their followers. These workers base all their theories upon the assumption of specific conative tendencies in the human mind. But of all these tendencies they explicitly recognize and define one only, namely, the sex tendency; the rest they have been content to leave completely undefined; it inevitably results from this neglect that their doctrines attach an exaggerated and almost exclusive importance to the one conative tendency recognized by them.

It is this lacking foundation which I have attempted to supply in my description of the human instincts[1]; and in the same work I have given what I hold to be the only intelligible account of the relation of human volition to raw conation. However defective my work may be, I am convinced that it is only by recognizing the composite character of instincts, the conjunction in each one of perceptual, motor, and, above all, conative dispositions, and by recognizing the essential similarity of human instincts to those of the animals, whether of the ape, the dog, or the wasp, that this crying defect of so much modern psychology can be remedied, that the continuity of the human with the animal mind can be displayed, and that a science of human character and will can be built up.

[1] *Op. cit.*

INSTINCT AND INTELLIGENCE.

A REPLY.

By CHARLES S. MYERS.

The writer's views restated. Examination of the views of Mr Carr, Professor Lloyd Morgan, Mr McDougall and Professor Stout.

In the following paper I propose to examine the views which my colleagues in the recent symposium have brought forward in relation to my own conception of the connexion between instinct and intelligence.

At the outset I may conveniently restate the position I adopted in my contribution to the symposium. Our notions of instinct are derived from the wonderful effectiveness of the neural prearrangements with which all organisms are to a greater or less extent congenitally endowed, and from the ease of applying a mechanistic explanation to such reactions. But neural prearrangements are likewise present in instances of so-called intelligent activity; for the limits of intelligence are variously fixed in each individual and in each species. The only differences between so-called instinct and so-called intelligence consist objectively in the amount of plasticity and subjectively in the complexity of consciousness. Now save where subsequent reflexion is possible, an organism is never *aware* that it is acting instinctively; intelligent consciousness is always present. There is at least a rudiment of conation and meaning in the exercise of every instinct, and there is a varying amount of plasticity. In every intelligent process, on the other hand, there is a very high degree of congenital plasticity,—a vast number of alternative reactions; but could we know at any moment all the influences at work and fore-know the resultant of those influences, we could with certainty foretell the outcome of intelligent activity subjectively and objectively.

Thus the psychology and physiology of instinct are inseparable from the psychology and physiology of intelligence. There is not one nervous apparatus for instinct and another for intelligence. We ought to speak not of instinct and intelligence but of instinct-intelligence, treating the two as one indivisible mental function which in the course of evolution has approached now nearer to so-called instinct, now nearer to so-called intelligence. These two terms we must recognize as pure abstractions, relating to different *aspects* of the same mental process, not to different mental *processes*. There is always the mechanistic, and there is always the finalistic, aspect in instinct-intelligence, were we only capable of beholding it. Regarded from the objective standpoint, instinct-intelligence appears as instinct; regarded from the subjective standpoint, it appears as intelligence. The obviousness and complexity of each of these appearances depend on the mental development of the organism which regards and of the organism which is regarded.

Mr Carr takes a diametrically opposite view. He holds, with M. Bergson, that instinct makes use of a consciousness which is totally different from the consciousness of intelligence. He believes that consciousness has attained its highest development in one direction in insects, in another direction in man, and that in man intelligence has reached such a height as practically to obliterate instinct. Evidently then, according to Mr Carr, we can have no conception of the consciousness involved in instinctive behaviour. We are endowing insects with a mental possession, of the nature of which we have not, and never can have, the faintest glimmering. I fail to find in insect life any behaviour which is inexplicable on the hypothesis of a series of reactions, which, although to a large extent fixed, are also plastic and modifiable, involving in their function all the signs of conation and attention.

It has been contended both by M. Bergson and by Mr Carr that intuition is much more nearly allied to what they term the instinctive consciousness than to the intelligent consciousness. Indeed at times they appear to regard intuition as analogous in man to the instincts of lower animals. But surely intuition is only the intelligent consciousness at work without involving the consciousness of the Ego. The product of its activity suddenly comes to the surface, carrying with it, however, no clue as to whence it was derived. It is accompanied by a characteristic experience, an experience perhaps describable as one of uncanny helpfulness from within, but there is nothing either in this concomitant, or in the product, of intuitive activity to distinguish it from intelligent activity. We are apt to underestimate the extent to which we make use of higher unconscious processes in everyday life. Perhaps we are trying to recall something. After vainly employing every kind of mnemonic help, by a curious determination we relegate the recall to the unconscious, and often with an immediate success. Indeed, apart from volitional effort at recollection, all revival is essentially of this "intuitive" nature. So too are many of our judgments and decisions. We hesitate which of several available courses to adopt, and finally we leave the decision in the hands of the unconscious; we intuitively settle our choice of action. But there is nothing in all this to warrant the belief that a distinct form of consciousness is involved in intuition. As well suppose a distinct form of consciousness to explain the phenomena of post-hypnotic suggestion, multiple personality and the like! It is surely far easier to believe that intelligent activity need not involve the consciousness of the Ego.

If I understand Mr McDougall's present opinions rightly, they differ only slightly from my own. For he admits that intelligent and instinctive processes involve the same fundamental modes of activity, and he accompanies me (nay, in mode of endowment, he goes even further) in endowing instincts with perceptual and conative dispositions. The one and only distinction he draws between instincts and intelligence has reference to the mode and date of origin of such conditioning dispositions. In instinctive activity the dispositions are "wholly or mainly" innate; in intelligent activity they have been formed or have been much modified by previous individual experience. The words "wholly *or mainly*" make me suspect that Mr McDougall would agree with me that the dispositions conditioning an instinct are never entirely "innate." Further Mr McDougall would almost certainly share my view that the dispositions conditioning intelligent activity are to an enormous extent innate. Consequently he appears to agree with me that in reality instinctive and intelligent behaviour are psychologically indistinguishable.

I turn now to Professor Lloyd Morgan. He believes that instinct and intelligence differ functionally as well as genetically. He holds that in the original exercise of an instinct no meaning is involved, and that instincts only become later modified by the introduction of meaning. Throughout his paper he is vainly endeavouring to get at the beginning, the "primary tissue," as he terms it, of instinctive experience; vainly, because according to my contention there never can be a beginning of experience, —a beginning which has no relation to previous experience. Pure instincts deprived of meaning are like pure sensations deprived of meaning; they are psychological figments. I have sufficiently indicated in my contribution to the symposium how any one instinct must always be associated with some previously active instinct, which has already acquired meaning by association with some previously active instinct, and so on. I may add that I am disposed to accept Mr McDougall's conjecture that, independent of such origin, meaning may come as a congenital endowment to the instinctive process.

Professor Stout agrees with me that an instinct involves no special form of psychical activity, but he reserves the term for those congenital neuro-muscular prearrangements which are accompanied by intelligent consciousness. According to him, it is by virtue of this concomitant intelligence that instinctive activity is sharply divided from reflex. Instinct is thus the resultant of adding to a fixed neural mechanism an intelligent interest in the pursuit of ends. Intelligent consciousness, in other words, involves an independent plastic activity separable from the mechanical system which it controls.

Thus, according to Professor Stout, what is ordinarily called instinct is composed of mechanically prearranged and of intelligent elements; and what is ordinarily called intelligence, he goes on to say, is characterised by the capacity to learn by experience. The essential difference between instinct and intelligence (ordinarily so called) lies, then, in the possession by the former of a prearranged mechanism, by the latter of "instructibility." But "instructibility," the capacity of learning by experience, is now recognized to be an essential feature not only of intelligent but also of instinctive activity, so called. The difference, then, between instinct and intelligence, according to Professor Stout, is that while the former possesses, the latter is devoid of, an innate prearranged mechanism on which intelligence can act. I, on the other hand, prefer to see an innate mechanism and a certain plasticity in both kinds of activity. In what is ordinarily called instinctive behaviour, the innate mechanism is relatively fixed and given; in what is ordinarily called intelligent behaviour, the mechanism is relatively plastic and acquired. But I maintain that such differences are only relative and that no mental state (or process) can be spoken of as solely instinctive or as solely intelligent. It is instinctive *or* intelligent according to the standpoint. Some superhuman being would as surely find our human intelligence determined by mechanism, as we commonly believe the mental activity of animals to be determined by instinct.

On these grounds I protest against erecting one physiological apparatus for instinct and another for intelligence. Indeed how on these lines one could ever account satisfactorily for the first appearance of intelligence passes my comprehension. Nor do I see any reason to adopt Professor Stout's view that instinct once called to its aid intelligence, which thereupon gradually ousted instinct from its place. Throughout the psychical world there is but one physiological mechanism; there is but one psychological function—instinct-intelligence.

CORRELATION CALCULATED FROM FAULTY DATA.

By C. SPEARMAN.

I. *A correlational formula eliminating "accidental" errors of measurement.*

A FEW years ago I called attention to the fact that the apparent degree of correspondence between any two series of measurements is largely affected by the size of the "accidental" errors in the process of measurement[1]. It was pointed out that this disturbance is not in the least bettered by making the series longer. As a remedy, a correction formula was proposed, based on the idea that the size of these "accidental" errors can be measured by the size of the discrepancies between successive measurements of the same things[2].

Now, all experimenters seem to be unanimous in finding that such discrepancies are liable to be startlingly large. The importance of the point is therefore established. For an estimate of the correlation between two things is generally of little scientific value, if it does not depend unequivocally on the nature of the things, but just as much on the mere efficiency with which they happen to have been measured.

But nevertheless the formula has met with much opposition. When first published, some eminent authorities at once declared it

[1] This illusion is, of course, just as bad when the correspondence is judged by general impression instead of by coefficients; in fact, worse, as then no correction is possible.

[2] *Am. J. Psych.* Vol. xv. 1904.

to be mathematically incorrect. This attitude appears now to have been abandoned in favour of a more moderate line of resistance. The formula is allowed to be true for really "accidental" errors; but it is urged that, in psychology at all events, the discrepancies between successive measurements often cannot properly be termed "accidental," but may arise from the fact that the second later measurement does not deal with the same function as the earlier one, owing to the modifications introduced by practice, fatigue, etc. Hence, the correction formula becomes invalid; indeed, as it throws together two different functions, it is even meaningless.

Some idea of this sort will be found already in my original paper[1]; and it was quickly and clearly emphasized by Wissler[2]. But the crucial point was reached when Udny Yule gave a new and much simpler proof of the formula, putting its validity beyond further cavil, but showing in the plainest light the assumptions on which it is based[3]. These are, that the errors of measurement are not correlated with one another or with either of the series measured. Clearly, such assumptions are far from carrying conviction *a priori*. And, finally, Dr W. Brown has furnished some actual experimental instances of their invalidity, as well as some interesting theoretical discussion[4].

One remedy that has been suggested is to make the measurements so efficient, that the correction will not be needed. But how are we to tell whether our measurements really are efficient enough, except by trying with the correction formula? The suggestion is like telling a man to brush his coat until it is clean but never look whether it is so. Also, the half measure has been advised, of making the successive measurements prescribed by the formula, noting whether there is much

[1] *Ibidem*, pp. 254, 255.

[2] *Science*, Vol. XXII. pp. 309 ff.

[3] In a private letter sent to me in October, 1908; his proof is attached in appendix *e*. My own far less elegant proof makes, I think, a little less extensive assumptions; but the difference is unimportant. He has also had the great kindness to look through this paper and to suggest numerous criticisms, to which it is very much indebted. I have, further, the pleasure of acknowledging helpful criticisms and remarks from Dr Nunn, Mr Sheppard, Mr Burt, and Dr Betz.

[4] The experimental evidence brought by him at the Geneva Congress cannot be admitted. Let x denote the true measurement, x_1 and x_2 the first and second measurements actually obtained, d_1 and d_2 their errors; for x he substituted x_1, which $=x+d_1$, thus illicitly bringing in d_1, the very quantity in question. He, further, advanced the view, that the formula ceases to hold good whenever ability (measured by x) is correlated with "variability" (measured by $x_1 \sim x_2$); but he appears to have based this on the mistaken notion, that correlation with $x_1 \sim x_2 = d_1 \sim d_2$ proves correlation with either d_1 or d_2. In a later paper, however, he brings more satisfactory experimental evidence; also, he seems to give up the view about variability (*Biometrika*, Vol. VII. p. 352).

discrepancy, but not proceeding to use the formula for evaluating the effect of the discrepancy on the coefficient. This is equally futile; for the seriousness of the discrepancy can only be gauged by its effect on the coefficient.

The difficulty must be met more drastically. To begin with, we may note that in a large number of cases, the questioned assumptions are legitimate enough, for instance, in the measurement of physical objects. Unfortunately, this is not very helpful; for in most physical measurements the errors are exceedingly small; and the correction formula has proved that such very small errors affect the coefficient too little to demand, for most purposes, elimination. This elimination is needed rather in such sciences as psychology and sociology; in testing, for instance, a person's power of bisecting a line, we find that almost every successive trial yields a distinctly different result. But it is just in such sciences that the assumptions become most dangerous.

Here, however, we can fall back on the universal and invaluable device of analysing, in thought, such variations into components of two kinds. Firstly, there are the variations of a *regular*, generally a continuously progressive character. These demand and admit of investigation, explanation, and, in large degree, control. In our above example, we should find that the accuracy of bisection was being increased by practice, diminished by fatigue, etc. It is with regard to this kind of component that Wissler, Yule and Brown are unquestionably right in calling attention to the dubious validity of the old formula's assumptions; it is certainly more than hazardous to assume, for instance, that fatiguability in one performance is uncorrelated with ability in that performance, or with fatiguability in another performance. At the same time, recent research seems to indicate that such correlations are far smaller than popularly supposed; I am not myself aware of any conclusions arrived at by means of the old formula which would probably be upset on taking such correlations into account.

Secondly, superposed on the above regular variations, we find others of such a discontinuously shifting sort, that investigation, explanation, and control are almost baffled. Hence, we call these by some such name as "accident." Of course, exceptional cases may be conceived where the line demarcating the accidental from the regular components becomes obscure, but in the immense majority of cases it is perfectly clear and usable.

Now, it is the superposed accident that the present paper attempts to eliminate, herein following the custom of all sciences, one that appears to be an indispensable preliminary to getting at nature's laws. This elimination of the accidents is quite analogous to, and serves just the same purpose as, the ordinary processes of "taking means" or "smoothing curves." The underlying regular variations, on the other hand, do not in general require elimination, but only determination. Every mental performance, for instance, must necessarily be at some stage or other of practice or fatigue; every stage is equally "true," and forms an equally legitimate subject of investigation. All that can reasonably be demanded of a formula is to produce the coefficient for some definite stage, and we will here choose the average stage during the period of measurement.

But, as regards this second or "accidental" component to be eliminated, the assumptions as made by the old formula seem to possess an exceedingly wide validity. This paper proposes, then, to suggest a new formula, or rather to raise the old one to a higher generality, such as to involve these assumptions only as regards the "accidental" components, where they are legitimate; not as regards the regular components, where they may be called in question. We will take, however, the precaution of discussing the circumstances under which the assumptions may conceivably become invalid even as regards the "accidents."

The method is as follows. Let each individual be measured several times with regard to any characteristic to be compared with another. And let his measurements be divided into several—usually two—groups. Then take the average of each group; this we will term the "group average." *The division into groups is to be made in such a way, that any differences between the different group averages* (*for the same individual*) *may be regarded as quite "accidental."* It is further desirable, that the sum total of the accidental variations of all the individuals should be not very unequal in the different groups; ordinarily, this will occur without further trouble, but in any case it can be arranged[1].

Such a division seems feasible in most psychological and sociological work. A test of verbal memory, for instance, might well consist of memorizing twenty series of words (exclusive of some preliminary series

[1] It must be noted that we can rarely assume the "accidents" in the measurements of a single individual eventually to cancel one another on taking an average. This would postulate a much larger number of measurements than usually attainable. And as the errors thus introduced for each individual are squared in calculating the coefficient, they would not tend mutually to cancel one another when added together, but would exercise a definite and often large bias.

for "warming up"). Then series 1, 3, 5, ... 19 would suitably furnish one group, while the even numbers gave the other. Any discrepancy between the averages of the two groups might, as a rule, be regarded as practically all due to the "accidents."

Quite a small number of measurements will suffice when they extend over a brief period or when the variation may be assumed to proceed uniformly. If the variation is sensibly uniform—an assumption always valid if the period is sufficiently brief,—three measurements will be enough; one group can consist of 1 and 3; the other can be represented by 2. If four groups are available, one can consist of 1 and 4, the other of 2 and 3. When there is no lapse of time between the measurements, each one of them may replace a group; a common instance is that of measurements of children which consist in classifications or orders of merit derived from the general impressions of their teachers.

The result of this division into group averages (or classifications, etc.) is that each of these has two components, the average underlying "regular" measurement, and the average superposed "accidental" disturbance. And as regards the latter component, there appears no reasonable objection to assuming it to be uncorrelated both with the accidental components of the other group averages and with the underlying "regular" measurements. On making this assumption, we obtain the following equation (for proof, see app. c):

$$\mathbf{r}_{xy} = \mathbf{r}_{x[p],y[q]} \cdot \sqrt{\frac{1+(\mathbf{p}-1)\mathbf{r}_{x[1],x[1]}}{\mathbf{p}\cdot\mathbf{r}_{x[1],x[1]}} \cdot \frac{1+(\mathbf{q}-1)\mathbf{r}_{y[1],y[1]}}{\mathbf{q}\cdot\mathbf{r}_{y[1],y[1]}}} \quad \ldots \text{(I)},$$

where $r_{x[1],x[1]}$ denotes the average correlation between the single groups averages for x; $r_{y[1],y[1]}$ does the same for y; $r_{x[p],y[q]}$ denotes the correlation of the average of p group averages for x with the average of q for y; and r_{xy} is the desired correct correlation, i.e., that between the average values of the underlying regular measurements of x and y respectively.

Here, p and q may have any chosen values, but it is best to make them = the total number of groups formed of x and y respectively; for thus the greatest possible approximation to the correct value of the correlation is obtained directly, and the least possible influence is left to the factor expressed in (I) as a square root. The number of groups should generally be *two* only, since thus concentrating the measurements into few groups facilitates the complete elimination of all the "non-accidental" discrepancies between the group averages. The formula then becomes:

$$\mathbf{r}_{xy} = \mathbf{r}_{x[2], y[2]} \cdot \sqrt{\frac{1 + \mathbf{r}_{x[1], x[1]}}{2\mathbf{r}_{x[1], x[1]}} \cdot \frac{1 + \mathbf{r}_{y[1], y[1]}}{2\mathbf{r}_{y[1], y[1]}}}.$$

It may be noted that, by putting $p = q = 1$, we return to my original formula[1], the only difference being the present improved method of constituting the groups[2].

If $r_{x[1], x[1]}$ or $r_{y[1], y[1]}$ is unknown, there is no resource but putting it equal to 1, as is tacitly done in the Bravais formula as ordinarily calculated. The result, however, as in that formula, is not the correct coefficient, but merely the minimum which the correct coefficient cannot fall short of.

The "probable error" of sampling is, approximately,

$$= \cdot 6745 \frac{1 - \mathbf{r}^r{}_{x[p], y[q]}}{\sqrt{n}} \cdot K \quad \text{......................(II)},$$

here p and q denote the total number of groups of x and y respectively; K denotes the square root in (1), and n is the number of individuals. Thus, the p. e. of the correct coefficient = the p. e. of the Bravais coefficient calculated in the ordinary manner divided by the ratio of the latter to the correct coefficient[3].

Let us now consider the possible exceptions even to the above curtailed assumptions. For simplicity, we will take the case of x and y each furnishing two group averages, which we will term x_a, x_b, y_a and y_b. The assumptions made are, then, that the "accidental" components in these four terms respectively are uncorrelated with one another and with x and y.

Take, first, the possibility of correlation of the accidents of x_a with those of x_b. This could only mean that the accidents had a general bias in favour of some individuals as compared with others. Then, clearly, the formula will give the correlation, not between the true values of x and y, but, in general, between these values as biassed. And it could hardly be expected to do more. Such a bias can only be eliminated by improving the methods of obtaining the data.

Take next any correlation of the accidents in x_a with x or y. The above again holds good, except that in this special case statistics do

[1] *Am. J. Psych.* Vol. XVIII. p. 168.

[2] It is often useful to choose two *different* values for p and also for q, and to see whether they lead to two very discrepant values for r_{xy}. A large discrepancy indicates something wrong in the assumptions or elsewhere.

[3] It thus coincides with the approximation suggested by me to Mr Burt and published by him in the *British Journal of Psychology*, Vol. III. p. 111, 1909.

furnish a possible means of remedying the faultiness of the data, namely, Yule's formula for eliminating irrelevant factors. This will be discussed in the next section.

It remains to consider the possibility of correlation of the accidents in some of the x measurements with those in some of the y measurements. In experimental psychology, for instance, it is not uncommon for each individual to be tested separately, and for each test in x to be accompanied by a test in y. Suppose, now, any individual to be accidentally indisposed: his results for both x and y will be accidentally depressed; the same will occur, more or less, for the other individuals; hence arises a spurious correlation between x and y. It may, however, easily be avoided; let the accidents in the pth tests of x and y be called d_p and e_p; we need only arrange so as to omit $d_p \cdot e_p$ from $s(d_{ab} \cdot e_{ab})$, see appendix a. Of course, it is advisable, where possible, to get better data to start with; in the above case, it might be practicable to test x and y on separate occasions; or means might be devised of ascertaining when the individuals are indisposed, etc.

A point to be noticed about this formula is that, like the former one proposed by me, it will occasionally produce coefficients greater than unity. Some authors have strongly objected to this[1]. But the objection would only be justifiable if the coefficient pretended to be perfectly accurate. At most, it is only the true coefficient plus the error due to testing a limited sample instead of the whole class; the general magnitude of such an error is indicated by the so-called "probable error." And though a true coefficient cannot exceed unity, there is no reason why a coefficient plus an error should not do so. In such case, of course, the coefficient must be taken as $= 1$, this being its most probable value.

In view of the easy statistical elimination of the accidental errors, it might be thought no longer necessary to make long careful measurements. But this would be a grave mistake; for as seen from equation (II), such accidents swell the correct coefficients probable error. Hence the function of the formula here proposed is by no means to replace accurateness of original data, but on the contrary to emphasize the necessity of such accurateness, to estimate the degree of its realization, and only in the last instance to supplement its defectiveness.

It should be noted that the above proposed elimination of accidental variations bases itself on the original Bravais coefficient, this

[1] Cf. Pearson, *Biometrika*, Vol. III. p. 160. His other principal criticisms will be discussed in Section VI.

appearing to me the most generally satisfactory one hitherto invented. But at the same time, it must be admitted that this coefficient has many weaknesses and that other coefficients have many advantages[1]. It must, however, be demanded of these other coefficients, no less than of the Bravais one, that they be analogously modified so as to eliminate the effect of the accidents.

II. *Yule's Correction for Irrelevant Factors.*

Difficulties are not yet over. So far, we have only eliminated from our coefficient the effect of "accidents" in our measurements. But our data are no less liable to be affected by non-accidental or general tendencies. To avoid such disturbing tendencies is primarily, of course, the concern of the method of obtaining the data. There is, however, one important case where faultiness of the data in this respect admits of subsequent correction statistically; it is the case where the undue tendency consists in the character under estimation being influenced by some *irrelevant factor*. These arise from the fact that the scientific conquest of nature is essentially achieved by artificially simplifying her processes; the factor that we happen to be investigating is allowed to vary, while the remaining factors are kept as constant as possible, their effect being regarded as irrelevant for the purpose in hand. Suppose, for instance, we wanted to ascertain the correlation between ability for composition and for mathematics. It would not be legitimate to pool together schools devoting different amounts of time to instruction. Otherwise, the fact that the children high in the one subject tended to be high in the other also might merely mean that these children came from schools giving more instruction in both subjects than the other schools. Such difference of amount of instruction constitutes a factor irrelevant and disturbing to our purpose; it requires elimination.

Unfortunately, these irrelevant factors are innumerable and ubiquitous. The shallow, or careless worker, or one trying to make statistical calculations replace special knowledge of the subject, is at their mercy. Even the most thorough and competent investigator, after completing his experiments, working out his correlations, and eliminating the influence of accidents, will still have many a misgiving about the irrelevancies; some factor may now occur to him of which he did not

[1] See the very interesting paper of Thorndike on "Empirical Studies in the Theory of Measurement," *Archives of Psychology*, Vol. III. 1907. Also, H. Bruns, *Wahrscheinlichkeitsrechnung und Kollektivmasslehre*, 1906; and G. F. Lipps, *Die Theorie der Kollektivgegenstände*, 1902.

think before; or he may now see reason to take a more serious view of some factor previously tolerated under the belief of its harmlessness; or he may be facing some factor whose gravity he has all along realized well enough, but whose presence he has seen no way of escaping.

At this point, therefore, we have urgent need of some further statistical process, to enable us to estimate and eliminate such disturbing elements. And such has actually been discovered for us by Udny Yule. The fundamental significance of this event for the development of correlational research appears—both for the above reasons and for others of even greater importance—scarcely to admit of overestimation. The nature and usage of his corrective process have been fully explained elsewhere[1]. I need only mention here that it must be applied after eliminating the effect of the "accidents," not *vice versa.*

III. *Errors still persisting in the coefficient.*

Unfortunately, Yule's corrective process can rarely be carried out very completely; for though the calculations are simple enough, we almost always have great difficulty in rightly conceiving the nature of the chief irrelevant factors and also in obtaining sufficient information about them[2]. Hence it is useful to consider in what manner the coefficient will be affected by any errors still adhering to it.

$$\text{Let}\quad \bar{r}_{xy} = \frac{S(\overline{xy})}{\sqrt{S(\overline{x}^2)\,.\,S(\overline{y}^2)}}\;\frac{S(xy + xt + ys + st)}{\sqrt{S(x^2 + 2xs + s^2)\,S(y^2 + 2yt + t^2)}},$$

where $\bar{r}_{xy}$ denotes the coefficient obtained finally; s and t represent the errors in x and y that have still escaped elimination. Then $S(xt)$, $S(ys)$, $S(st)$, $S(xs)$, $S(s^2)$, $S(yt)$ and $S(t^2)$ are the values still possibly disturbing the coefficient. The above expression serves to show whether the disturbance tends to make the coefficient too large or too small.

We will take some cases that might occur in psychological experiments. $S(xt)$ would retain an appreciable value when there had been imperfect elimination of any correlation between x and the bias in the

[1] See Yule, *Proc. R. S. London*, Vol. LX. Also my own paper, *Am. Journ. Psychology*, Vol. XVIII. p. 161.

[2] This raises a vital question. At what point can the corrective process be considered as perfectly complete? This point is often not reached, I believe myself, until the corrected coefficient becomes = complete unity, or else zero. Here, and here only, the law under investigation has been completely disentangled from all other interfering factors. This consideration should, I think, dominate correlational research. In it is revealed the extreme significance of Yule's formula.

measurement of y. This would occur, for example, if the individuals who had a weak ability for the performance x were allowed to notice their inferiority, and thereby gained an extra stimulus to do well in y: the effect would be to make the final coefficient too large. If the individuals were not stimulated but only depressed by their insuccess in x, $S(xt)$ would take a negative value, and the final coefficient would be too small. On the whole, with moderately good experimentation, this source of error should generally be negligible. $S(yt)$ is, of course, similar.

$S(xs)$, or $S(yt)$, becomes appreciable when there is an uneliminated correlation between an ability and the bias in its measurement. This might occur, for instance, when the method of marking tended to exaggerate the differences between the good performances as compared with the bad ones. Its effect is to make the final coefficient too small. It can be avoided by basing the calculation on ranks instead of on measurements. In any case it does not seem formidable.

More serious is the liability to appreciable values of $S(st)$. This occurs when the bias in the measurement of x is correlated with that as regards y. For instance, if two performances are tested always in the morning, the measurements of those individuals who cannot do their best until the evening will be unfavourably biassed in both performances. Such a correlation, it may be noticed, will almost always be positive and there tend to make the final correlation too large.

But by far the greatest danger lies in $S(s^2)$, or $S(t^2)$. For the errors entering into these sums, being squared, become positive and thereby *lose all tendency to neutralize one another on being added up.* To reduce this danger is, in fact, the purpose of the correlational formula proposed in this paper. And this formula will be quite effectual in so far as two measurements can be obtained of x, such that the errors in the one are really independent of those in the other. But often it will be found impossible to avoid the same bias pervading more or less both measurements. A notable instance is when the "intelligence" of a class of children is estimated by two different teachers. Under ordinary circumstances, it is found that the two estimates show high correspondence, about ·80 or more. But when it can be arranged that the two teachers really form their estimates independently of one another, do not discuss the children together, nor hear of the same examination results, etc., then this correspondence shows a surprising shrinkage, thus revealing the previous high coefficient to have been spurious. In this way, however, the coefficient given by the proposed formula can only become too small; that is to say, the correction introduced by the formula is quite right as far as it goes: its only fault is in not going far enough, owing to the defectiveness of the data.

On the whole, it is clear that to obtain an approximation to the true correlational coefficient is by no means a simple matter. My own experience leads me to think that the sources of error considered above are more insidious even than the error arising from taking a small sample. The very large and imposing series of cases, which have been obtained at the expense of all the other moments of accuracy, are but as "whited sepulchres."

IV. *Amount of increased reliability to be obtained by increasing the number of measurements.*

A very convenient conception is that of the "reliability coefficient" of any system of measurements for any character. By this is meant the coefficient between one half and the other half of several measurements of the same thing, the division of the measurements into two halves or groups being done as described on p. 274.

It is often very useful to be able to estimate how much this reliability coefficient will probably be increased by any given additional number of measurements, or how much it will probably be reduced by any given diminution in the number of measurements. It can be shown that the following relation holds good:

$$\mathbf{r}_{x[p],x[p]} = \frac{\mathbf{p} \cdot \mathbf{r}_{x[q],x[q]}}{\mathbf{q} + (\mathbf{p} - \mathbf{q})\mathbf{r}_{x[q],x[q]}} \quad \text{............... (III)}^1,$$

where $r_{x[q],x[q]}$ is the known reliability coefficient of x when the latter has been measured $2q \times i$ times, i being any number, and $r_{x[p],x[p]}$ is the required most probable reliability coefficient if x be measured $2p \times i$ times.

Here, and also in the following section, all the measurements of x (or of y) are supposed to have been of equal general accuracy. But the case of unequal accuracy also admits of solution, see appendix *b*.

V. *Increase of correlation between two different characters to be obtained by increasing the number of measurements.*

The above principle can be usefully applied to the correlation between two different characters.

[1] For proof, see appendix *b*.

It will be found that[1]

$$r_{xy} = \frac{2(\mathrm{p}-1)(\mathrm{q}-1)\,\mathrm{r}_{x[1],y[1]} \times \mathrm{r}_{x[p],y[q]}}{2\sqrt{\mathrm{pq}}\,(\mathrm{p}-1)(\mathrm{q}-1)\,\mathrm{r}_{x[1],y[1]} - (\mathrm{p}+\mathrm{q}-2)\,\mathrm{r}_{x[p],y[q]}} \quad \text{..(IV)},$$

where $r_{x[1],y[1]}$ denotes the correlation of the average of any number, say i, measurements of x with that of any number, say u, of y; $r_{x[p],y[q]}$ denotes the correlation of the average of $p \times i$ measurements of x with that of $q \times u$ of y, and r_{xy} is, as before, the correct correlation between x and y.

This formula may especially be of service in supplementing the previous method, equation (I). For any inadequate fulfilment of the assumptions of both methods will, in general, affect their respective results differently. Hence, concordance of the results by both methods will greatly strengthen the evidence for the assumptions being valid.

An empirical formula was previously given by me for the same purpose (*Am. Jour. Psych.*, Vol. XV. pp. 88—91). Though of a different form from the above, it gives very similar results under the usual values of the terms entering into it. Still more accurately corresponding with the above theoretical formula are the empirical results given by Thorndike in his above-mentioned paper[2].

VI. *Discussion of some criticisms.*

Some time ago I had to take exception to the work of Professor Karl Pearson, on the ground that it is vitiated by observational errors and irrelevant factors[3]. To this he has made two replies[4]. But I am sorry to find that he has very seriously misrepresented my views, and even misquoted my figures. I can only regret all obscurities on my part that doubtlessly have contributed to this confusion, and hope that the following may elucidate matters.

My chief weakness he finds in what he takes to be the treatment of the "probable errors," about which he makes many strong comments. But here it is necessary to distinguish clearly between errors of two different sorts, those of sampling and those of observation. Suppose, for instance, that we wanted to investigate the head-length of skulls of prehistoric Thebes. We should only be able to obtain a limited number of them, and would have to assume that these were an adequate sample of the

[1] For proof, see appendix *c*. The formula simplifies greatly in the usual case that $p=q$.

[2] *Archives of Psychology*, Vol. III. p. 41, 1907.

[3] *Am. J. Psych.* Vol. XV. pp. 96—8.

[4] *Biometrika*, Vol. III. p. 160. *Drapers' Company Research Memoirs*, Biometric Series, IV. 1907.

whole; hereby an error is involved, whose general magnitude is measured by the "probable error" of sampling. But a totally new danger comes on the scene in the process of actually observing the head-lengths; if our instrument or method of observation is sufficiently crude, we may make here additional errors, even far exceeding those of sampling. Now, it is to these accidental errors of observation that my attention has been chiefly drawn. Pearson, on the other hand, while he has made widely and justly appreciated contributions to the theory of sampling errors, has scarcely touched on the errors of observation. And he seems to have remained "eingestellt" for sampling errors when reading my papers; for he takes in this sense everything that I really wrote about the errors of observation. Hence, of course, nothing but cross-purposes.

Take, to begin with, the main point at issue, correlation by ranks as compared with that by measurements. Suppose that there has been an examination in Latin and in Mathematics; we want to see how far a boy's success in the one subject agrees with that in the other. Evidently, there are two ways possible. We can note that he has got, say, 98 marks in Latin, but only 49 in Mathematics; this is called the method of measurements. Or we can remark that, out of 100 boys, he is first in Latin, but only 60th in Mathematics; this is the method of ranks. Under certain circumstances the method of ranks had seemed to me to be the less affected by the observational errors. This is especially the case where these errors increase in size towards one or both extremities of the range under consideration. In physical measurements, indeed, this will be rare; a large head is measured as easily as a small one, and in neither case should the error be excessive. But in psychology this is otherwise; the following, for instance, are the different thresholds for pitch found for 24 children, the unit being 1/3 v.d.: 5, 8, 10, 10, 13, 14, 14, 15, 17, 17, 18, 18, 20, 24, 25, 28, 33, 40, 45, 60, 70, 70, 70, 90. Any experienced psychologist will know that the error of determination at the bottom of this scale is at least some twenty times as great as at the top; to ignore this large inequality is to distribute "weight" very wrongfully, and therefore to do much injury to the reliability of the calculated coefficient. Translation from measurements into ranks is in such cases equivalent to a readjustment of the "weights" so as to equalize the upper and lower halves of the scale and reduce the importance of the extremities; no doubt, such device admits of much improvement. Now, Pearson takes all this about the observational error to refer to the error of sampling, expresses his disapproval most emphatically, and thinks to overthrow it by demonstrating that ranks and measurements produce, on the assumption of Gaussian distribution, *sampling* errors of quite equal magnitude. This, of course, has nothing to do with the point[1].

Let us take the next most important matter, the question of "squaring the differences." In our above example, the most natural way of estimating the degree of correspondence between the two examinations would be to notice the differences between the results of the one and those of the other (in doing so, we might either regard the differences of marks or those of rank). But the Bravais method of "product moments" introduces a refinement; it bases itself, not on these differences simply, but on their squares. I suggested in this Journal (Vol. II. Part 3, 1906) that under certain circumstances the omission of the squaring might reduce the accidental error (using this expression as equivalent to the observational error plus sampling error). I gave a formula for that purpose, which we will here term the R formula. In answer to this Pearson demonstrated that the sampling error by the R method is not less, but greater than that by the Bravais or r method, which he uses. But, as before, his demonstration does not touch the error of observation. Further, it is based on the assumption of a Gaussian distribution; and this assumption is a most precarious one, especially as regards the latter kind of error. It could easily be shown that there are other distributions where, on the contrary, the squaring is disadvantageous, just as, under similar circumstances, the average becomes less reliable than the median. Müller, Kraepelin, and others have shown such distributions to occur largely in psychological work, and my experience has often led me to suspect their influence in correlations also. It must be remembered that squaring lays stress on the extreme discrepancies between the series compared (not, as some people have said, on the extreme values in the series); and the reliability of these is often gravely in question. To take an example, suppose that all the individuals with one single large exception have shown a close relation between their performances in one experiment and in another; is there no ground for fearing that the one exception may be due to some accident, such as misunderstanding the procedure required, etc.? Only in this way can I explain that

[1] As regards the general question of ranks and measurements, it is pleasing to find that Udny Yule totally disagrees with Pearson's adverse comments and, on the contrary, finds my proposal of ranks "a very important step in the simplification of methods dealing with non-measurable character" (*Stat. Soc. Journ.* Vol. LXX. 1907, p. 656).

I have often found successive samples from the same class of events to fluctuate less when calculated by R than by r. The fact is that the Gaussian assumption is only a mathematical make-shift; we may often conveniently enough reckon formulae from it; but in actual application, we should constantly bear in mind its real limitations.

Seeing that, at any rate on the Gaussian assumption, the R method has a slightly larger probable error, Pearson severely criticizes the fact that I had found this to be only about $\cdot 43\sqrt{n}$, whereas that of r shows a much larger figure, $\cdot 67\sqrt{n-1}$. To explain this "paradox" he points out that, while the R method is only applicable to positive correlations, its probable error is taken from both positive and negative ones; and that the latter have a smaller range than the former. But there is no real paradox at all. We can no more argue that a probable error of ·43 by one method is smaller than a probable error of ·67 by another method, than we can say that 5 pounds are less than 10 francs. Two different methods, as a rule, are expressed in terms of different value; before making any such comparisons we must reduce them to common terms. And on doing so, the apparent superiority of R, of course, disappears. As regards the discrepancy emphasized by Pearson between the extreme positive and the extreme negative values, this seems to be of minor importance; he overlooks my empirical evidence that the *mean* positive and negative variations are nearly equal; and this is now corroborated by his own tables, as these show that the asymmetry between the positive and negative values only becomes marked in the extreme ranges, where the frequency—and therefore the effect on the probable error—is very small.

More serious is the charge against the R method, that it cannot deal directly with large negative correlations, but has first to convert these into positive ones by inverting one of the orders compared; and sometimes this is impossible, for even after the inversion the correlation may still remain negative. On looking into this more closely, however, its formidable appearance greatly diminishes; for it occurs solely when the correlation is so small as practically to be equivalent to zero, and therefore has no need of inversion at all; even in the extreme case selected by Pearson, the negative correlation is less than its own probable error.

Still such an anomaly, however harmless in actual practice, does indeed, I must admit, disqualify the R method from setting up to be a perfectly independent method ranking equally with the r method, still less claiming a large superiority over it. But how Pearson ever came to conceive that I made such a claim I have failed to discover. Far from doing so, and despite the above-mentioned occasional superiority of the R, I expressly entitled it a "footrule," as lying half-way between the r method with its complications (which I likened to an "elaborate micrometer") and judgment without mathematical method at all (which I compared to a "mere glance of the eye"). It is hard to understand how such strong expressions as these should ever have been taken to mean just the contrary. R's chief mission is merely to gain quickly an approximate valuation of r. As an example of the kind of work for which it was intended, I had occasion to put some 50 persons through a number of tests, principally as a demonstration; the work was rough, but still not so bad as to prevent all interest in the results. As there were 270 correlations to calculate, I could not possibly have attempted the task but for the extreme facility of R. Further, the method seemed well adapted for the schoolmaster who wants to know how far this year's examination tallies with that of last year, how far success in one subject has gone with that in another, how closely two teachers agree with one another in their estimates of children, and many more such problems. Seeing that R is meant to be subsidiary to r, the only real question is whether or not it actually produces values sufficiently approximating to the latter. Pearson selects a number of cases to prove discrepancy between the two; but he overlooks the fact that the discrepancy is never more than double the probable error, and that he himself declares any result less than 2—3 times the probable error to be devoid of significance[1]. If we take a general impartial review of the evidence hitherto adduced (for instance, that of Burt, Wimms, and Brown[2]), the correspondence of R with r appears to be amply good enough for the purpose in view; in fact R seems quite usable, not merely for assay purposes as originally contemplated, but even sometimes for research.

And even such discrepancies as do occur between the two coefficients are by no means wholly chargeable to the fault of R, as Pearson assumes[3]. For the differences between the two are only that R uses ranks and omits squaring; and both these differences, as we have seen, are often advantageous, so that then the discrepancies are more the fault of r. It may seem contradictory that R should under any circumstances

[1] For instance, *Drapers' Company Research Memoirs*, Biom. Series, IV. p. 15.

[2] Burt, *B. J. P.*, Vol. IV. p. 107. Wimms communicated his data to the Brit. Psych. Soc., Jan. 1910. For Brown, who is much the fullest on this point, see his paper quoted, p. 357.

[3] *Ibidem*, p. 38.

claim to be more accurate than its own ideal, r. But we must remember that its ideal is the true r, not the actually calculated one; the latter is the true one plus various errors (sampling, observation, irrelevant factors, etc.).

Pearson remarks repeatedly, and even italicizes, that I state the probable error of R to be $\cdot 4266/\sqrt{n}$, instead of $\cdot 4266/\sqrt{n-1}$. It is gratifying that criticism should have to turn so much to such a trivial matter. As it happened, however, I really made neither statement, but the quite accurate one, that the probable error is "$\cdot 43/\sqrt{n}$ with two correct decimals, when n is not less than 10[1]."

In spite of his attack on ranks, he has made an interesting contribution towards their use. He has worked out the relation between the coefficients of ranks (or rather "grades") and those of measurements, assuming Gaussian distribution. It is $r = 2\sin\left(\frac{\pi}{6}\rho\right)$, where ρ denotes the coefficient for ranks. On calculation, r and ρ turn out to be almost identical, thus corroborating my empirical observation to the same effect.

He has also done us the service of demonstrating, on the Gaussian assumption, the relation between squaring and non-squaring. It is

$$\sin\frac{\pi}{6}\rho = \cos\frac{\pi}{3}(1-R) - 1.$$

I had found by actual observation the empirical formula

$$\rho = \sin\left(\frac{\pi}{2}R\right).$$

The theoretical values, it will be found, fit the observational ones with admirable closeness, the mean discrepancy being under ·01 and the maximum only about ·02, amounts that are negligible, at any rate in psychology. Hence, it is no small surprise that Pearson several times reproaches the empirical formula with being "erroneous." Even had there been any significant discrepancy between the two, it would not have affected the validity of the empirical value expressing actual observations, but only of the theoretical one based on such a weak assumption. And as, on the contrary, the discrepancy is so completely insignificant, there appears no great advantage, even on the Gaussian assumption, in abandoning the use of the older and simpler formula for ordinary rough purposes.

[1] *Brit. J. Psych.* Vol. II. p. 108.

On the whole, if we eliminate all these misapprehensions and oversights, there seems to be no serious difference of opinion on all these points between Pearson and myself. And to judge from the continually rising importance attributed by his school to observational errors and to irrelevant factors, even here the gap between us would appear to be rapidly closing.

Appendix.

a. Coefficient in the case of 2 *groups of measurements.*

Let $x(f_k)$ denote the kth measurement of the fth individual.

Let the superposed "accidental" disturbance be denoted by $d(f_k)$; the underlying "regular" measurement by $x'(f_k)$; the average value of $x'(f_k)$ by $x(f)$; and the kth "regular" deviation from this average by $u(f_k)$.

Then

$$x(f_k) = x'(f_k) + d(f_k) = x(f) + u(f_k) + d(f_k).$$

Let the measurements of each individual be divided into two groups, say a and b, in such a manner that any discrepancies between the averages of the two groups may be regarded as quite "accidental" (see p. 274).

Let the average of the values of $x(f_k)$, $x'(f_k)$, $d(f_k)$ that occur in group a be denoted by $x_a(f)$, $x_a'(f)$, $d_a(f)$. And let the average of *all* the values of $x(f_k)$, $x'(f_k)$, $u(f_k)$ be denoted by $x_{ab}(f)$, $x_{ab}'(f)$, $u_{ab}(f)$.

Then $\quad x_a(f) = x_a'(f) + d_a(f) = x_{ab}'(f) + d_a(f),$

since by assumption $x_a'(f) = x_b'(f) = x_{ab}'(f)$,

$$= x(f) + u_{ab}(f) + d_a(f)$$

$$= x(f) + d_a(f), \text{ since } u_{ab}(f) = 0.$$

And $\quad x_{ab}(f) = x(f) + u_{ab}(f) + d_{ab}(f) = x(f) + d_{ab}(f).$

Analogously

$$x_b(f) = x(f) + d_b(f),$$

$$y_a(f) = y(f) + e_a(f),$$

$$y_b(f) = y(f) + e_b(f),$$

$$y_{ab}(f) = y(f) + e_{ab}(f).$$

Hence, summing for all individuals,

$$\frac{S(x_{ab}y_{ab})}{\sqrt{S(x_a x_b)\,.\,S(y_a y_b)}} = \frac{S(x+d_{ab})(y+e_{ab})}{\sqrt{S(x+d_a)(x+d_b)\,.\,S(y+e_a)(y+e_b)}}$$

$$= \frac{S(xy + xe_{ab} + yd_{ab} + d_{ab}e_{ab})}{\sqrt{S(x^2 + xd_a + xd_b + d_a d_b)\,.\,S(y^2 + ye_a + ye_b + e_a e_b)}}$$

$$= \frac{S(xy)}{\sqrt{S(x^2)\,.\,S(y^2)}} = r_{xy} \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots(1),$$

since each of the sums $S(xe)$, $S(yd)$, $S(de)$, etc. $= 0$, as we will assume, in accordance with pp. 273–4, that the d's and the e's are uncorrelated with one another and with x and y.

Further, $S(d_a^2)$ and $S(d_b^2)$ will be assumed to have not very dissimilar magnitudes (see p. 274). Hence *a fortiori* $S(x_a^2)$ and $S(x_b^2)$ will not be very unequal, so that approximately

$$\sqrt{S(x_a^2)\,.\,S(x_b^2)} = \tfrac{1}{2}\left[S(x_a^2) + S(x_b^2)\right] \quad\ldots\ldots\ldots\ldots\ldots(2).$$

Hence

$$1 + \frac{1}{r_{x_a x_b}} = \frac{S(x_a x_b) + \sqrt{S(x_a^2)\,.\,S(x_b^2)}}{S(x_a x_b)}$$

$$= \frac{2S(x_a x_b) + S(x_a^2) + S(x_b^2)}{2S(x_a x_b)}$$

$$= \frac{2S(x_{ab}^2)}{S(x_a x_b)} \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(3).$$

And, similarly,

$$1 + \frac{1}{r_{x_a x_b}} = \frac{2S(y_{ab}^2)}{S(y_a y_b)} \quad\ldots\ldots\ldots\ldots\ldots\ldots(4).$$

Then, as

$$\frac{S(x_{ab}y_{ab})}{\sqrt{S(x_a x_b)\,.\,S(y_a y_b)}} = \frac{S(x_{ab}y_{ab})}{\sqrt{S(x_{ab}^2)\,.\,S(y_{ab}^2)}}\,.\,\sqrt{\frac{S(x_{ab}^2)\,.\,S(y_{ab}^2)}{S(x_a x_b)\,.\,S(y_a y_b)}},$$

we get by (1), (3) and (4)

$$r_{xy} = r_{x_{ab}y_{ab}}\,.\,\frac{1}{2}\sqrt{\left(1 + \frac{1}{r_{x_a x_b}}\right)\left(1 + \frac{1}{r_{y_a y_b}}\right)} \quad\ldots\ldots\ldots(5).$$

I

b. Proof of formula III.

Take now the more general case of p, instead of 2, groups of measurements for x, denoted by

$$x_1,\; x_2, \ldots x_k \ldots x_h \ldots x_p = x + d_1,\; x + d_2, \ldots x + d_p,$$

where x is the underlying regular measurement, while the d's are the superposed accidental components.

Let $\frac{x_1 + x_2 + \ldots + x_p}{p}$ be denoted by $x[p]$, $\frac{d_1 + d_2 + \ldots + d_p}{p}$ by $d[p]$.

Let $y_1, y_2, \ldots y_u \ldots y_v \ldots y_s$, $e_1, e_2, \ldots e_s$, $y[s]$, $e[s]$ have similar meanings with regard to y.

Since, as we have seen, such sums as $S(xe)$, $S(xd)$, $S(d_k d_h)$ each $= 0$, we get, summing for all individuals,

$$S(x[p] \,.\, y[s]) = S(x + d[p])(y + e[s]) = S(xy),$$

$$S(x_k x_h) = S(x + d_k)(x + d_h) = S(x^2),$$

and

$$S(y_u y_v) = S(y + e_u)(y + e_v) = S(y^2).$$

So that

$$r_{xy} = \frac{S(xy)}{\sqrt{S(x^2) \,.\, S(y^2)}} = \frac{S(x[p] \,.\, y[s])}{\sqrt{S(x^2[p]) \,.\, S(y^2[s])}} \,.\, \sqrt{\frac{S(x^2[p]) \,.\, S(y^2[s])}{S(x^2) \,.\, S(y^2)}}$$

$$= r_{x[p],\, y[s]} \,.\, \sqrt{\frac{\frac{S(x_1 + x_2 + \ldots + x_p)^2}{p^2}}{S(x_k x_h)}} \,.\, \sqrt{\frac{\frac{S(y_1 + y_2 + \ldots + y_s)^2}{s^2}}{S(y_u y_v)}}$$

$$= r_{x[p],\, y[s]} \,.\, \sqrt{\frac{S_k S(x_k^2) + p(p-1) S(x_k x_h)}{p^2 S(x_k x_h)}}$$

$$\times \sqrt{\frac{S_u S(y_u^2) + s(s-1) S(y_u y_v)}{s^2 S(y_u y_v)}} \quad \ldots\ldots(6),$$

the additional S denoting summation for all groups.

But, from (2),

$$\frac{pS(x_k x_h)}{S_k S(x_k^2)} = \frac{2}{p(p-1)} S_{kh} \left[\frac{S(x_k x_h)}{\sqrt{S(x_k^2) \,.\, S(x_h^2)}}\right] = \frac{2}{p(p-1)} S_{kh}(r_{x_k x_h})$$

$=$ average correlation between x_k and $x_h =$ say, $r_{x[1],\, x[1]} \ldots(7)$.

From (6) and (7), putting $y = x$, we get

$$1 = r_{x[p],\, x[s]} \,.\, \sqrt{\frac{1}{p r_{x[1],\, x[1]}} + \frac{p-1}{p}} \,.\, \sqrt{\frac{1}{s r_{x[1],\, x[1]}} + \frac{s-1}{s}},$$

from which

$$r_{x[p],\, x[s]} = \sqrt{\frac{p r_{x[1],\, x[1]}}{1 + (p-1) r_{x[1],\, x[1]}}} \,.\, \sqrt{\frac{s r_{x[1],\, x[1]}}{1 + (s-1) r_{x[1],\, x[1]}}} \ldots(8).$$

In the usual case that $s = p$, this becomes

$$r_{x[p],\, x[p]} = \frac{p r_{x[1],\, x[1]}}{1 + (p-1) r_{x[1],\, x[1]}} \quad \ldots\ldots\ldots\ldots\ldots\ldots(9),$$

or, writing q for p,

$$r_{x[q],\, x[q]} = \frac{q r_{x[1],\, x[1]}}{1 + (q-1) r_{x[1],\, x[1]}}.$$

And the two last equations give, on reduction,

$$r_{x[p],\,x[p]} = \frac{p r_{x[q],\,x[q]}}{q + (p-q)\, r_{x[q],\,x[q]}} \quad\ldots\ldots\ldots\ldots\ldots\ldots(10).$$

Although this formula applies immediately to groups of approximately equal liability to accidental disturbances, it can easily be extended to cases of unequal liability. For an actual measurement of any degree of accuracy is, clearly, equivalent to the average of a number of measurements of an inferior degree of accuracy. So that two actual measurements (or groups of such) of unequal accuracy may be conceived as the averages of two unequal numbers of measurements all of equal (inferior) accuracy. Thus p could represent $S_k(m_k g_k)$, where m indicates the number of groups, and g their respective precisions.

c. Proof of formulae I and IV.

In (5) let each of the groups, a and b, be composed of $\frac{p}{2}$ sub-groups each satisfying all the assumptions we made about a and b.

Then (5) may be written as

$$r_{xy} = r_{x[p],\,y[q]}\,\frac{1}{2}\sqrt{\left(1+\frac{1}{r_{x\left[\frac{p}{2}\right],\,x\left[\frac{p}{2}\right]}}\right)\left(1+\frac{1}{r_{y\left[\frac{q}{2}\right],\,y\left[\frac{q}{2}\right]}}\right)},$$

where the indices in brackets have the same signification as in appendix b.

This, owing to (8), becomes

$$= r_{x[p],\,y[q]}\cdot\sqrt{\frac{1+(p-1)\,r_{x[1],\,x[1]}}{p r_{x[1],\,x[1]}}\cdot\frac{1+(q-1)\,r_{y[1],\,y[1]}}{q r_{y[1],\,y[1]}}}\ldots(11)$$

$$= r_{x[p],\,y[q]}\cdot\sqrt{\frac{(p-1)(q-1)}{pq\,r_{x[1],\,x[1]}\cdot r_{y[1],\,y[1]}}}\cdot\sqrt{\frac{1}{p-1}+r_{x[1],\,x[1]}}$$

$$\times\sqrt{\frac{1}{q-1}+r_{y[1],\,y[1]}}\,.$$

The two factors on the right

$$=\frac{1}{2}\left[r_{x[1],\,x[1]}+r_{y[1],\,y[1]}+\frac{1}{p-1}+\frac{1}{q-1}\right.$$

$$\left.-\left(\sqrt{r_{x[1],\,x[1]}+\frac{1}{p-1}}-\sqrt{r_{y[1],\,y[1]}+\frac{1}{q-1}}\right)^2\right]$$

$$=\sqrt{r_{x[1],\,x[1]}\cdot r_{y[1],\,y[1]}}+\frac{p+q+2}{2(p-1)(q-1)}$$

$$+\tfrac{1}{2}\left(\sqrt{r_{x[1],\,x[1]}}-\sqrt{r_{y[1],\,y[1]}}\right)^2 - R,\text{ say}\ldots\ldots(12).$$

Hence, from (11) (putting there $p=q=1$) and (12),

$$r_{xy}=r_{x[p],\,y[q]}\left(\sqrt{\frac{(p-1)(q-1)}{pq}}+\frac{p+q-2}{2\sqrt{pq(p-1)(q-1)}}\cdot\frac{r_{xy}}{r_{x[1],\,y[1]}}\right)+R', \text{ say}\ldots\ldots(13).$$

We will neglect R', which vanishes when the two coefficients ($r_{x[1],\,x[1]}$ and $r_{y[1],\,y[1]}$) and also the two numbers of groups (p and q) tend to equality; it becomes largest, and then positive, when the coefficient and also the number of groups for one character compared are both much greater than those for the other character.

We get then, finally, on reduction,

$$r_{xy}=\frac{2(p-1)(q-1)\,r_{x[1],\,y[1]}\cdot r_{x[p],\,y[q]}}{2\sqrt{pq(p-1)(q-1)}\,r_{x[1],\,y[1]}-(p+q-2)\,r_{x[p],\,y[q]}}\ldots(14).$$

d. Proof of formula II.

Let z denote

$$r_{12}\sqrt{\frac{1+(p-1)r_{34}}{pr_{34}}\cdot\frac{1+(q-1)r_{56}}{qr_{56}}},$$

where 1, 2, 3, 4, 5, 6 indicate any values.

Taking logs,

$$\log z=\log r_{12}+\tfrac{1}{2}\log[1+(p-1)r_{34}]-\tfrac{1}{2}\log pr_{34}+\tfrac{1}{2}\log[1+(q-1)r_{56}]-\tfrac{1}{2}\log r_{56},$$

and differentiating

$$\frac{dz}{z}=\frac{dr_{12}}{r_{12}}-\frac{1}{2}\frac{dr_{34}}{r_{34}[1+(p-1)r_{34}]}-\frac{1}{2}\frac{dr_{56}}{r_{56}[1+(q-1)r_{56}]}.$$

Square all such equations, add, and divide by the number of equations, then

$$\begin{aligned}\frac{\sigma_z^2}{z}=\frac{\sigma_{r_{12}}}{r_{12}^2}&+\frac{\sigma_{r_{34}}^{\;2}}{4r_{34}^2[1+(p-1)r_{34}]^2}\\&+\frac{\sigma_{r_{56}}^{\;2}}{4r_{56}^2[1+(q-1)r_{56}]^2}-\frac{\sigma_{r_{12}}\sigma_{r_{34}}R_{r_{12}r_{34}}}{r_{12}r_{34}[1+(p-1)r_{34}]}\\&-\frac{\sigma_{r_{12}}\sigma_{r_{56}}R_{r_{12}r_{56}}}{r_{12}r_{56}[1+(q-1)r_{56}]}+\frac{\sigma_{r_{34}}\sigma_{r_{56}}R_{r_{34}r_{56}}}{2r_{34}r_{56}[1+(p-1)r_{34}][1+(q-1)r_{56}]},\end{aligned}$$

where[1]

$$R_{r_{12}r_{34}}=\begin{bmatrix}(r_{13}-r_{12}r_{23})(r_{24}-r_{23}r_{34})\\+(r_{14}-r_{34}r_{13})(r_{23}-r_{12}r_{13})\\+(r_{13}-r_{14}r_{34})(r_{24}-r_{12}r_{14})\\+(r_{14}-r_{24}r_{12})(r_{23}-r_{24}r_{34})\end{bmatrix}\div 2(1-r_{12}^2)(1-r_{34}^2),$$

with similar expressions for $R_{r_{34}r_{56}}$ and $R_{r_{12}r_{56}}$.

[1] See Pearson and Filon, *Phil. Trans.* A, Vol. CXCI. p. 262. I am greatly obliged to Professor Filon, not only for his valuable paper, but also for being kind enough to send me equation (15) deduced by its means for the case that $p=q=2$. Above I give his deduction, but generalized to include all values of p and q.

Also, from the same paper, $\sigma_{r_{12}} = \dfrac{1 - r_{12}^2}{\sqrt{n}}$.

Thus we have

$$\frac{n\sigma_z^2}{z^2} = \frac{(1 - r_{12}^2)^2}{r_{12}^2} + \frac{(1 - r_{34}^2)^2}{4r_{34}^2\,[1 + (p-1)\,r_{34}]^2} + \frac{(1 - r_{56}^2)^2}{4r_{56}^2\,[1 + (q-1)\,r_{56}]^2}$$

$$+ \frac{1}{2r_{12}r_{34}\,[1 + (p-1)\,r_{34}]} \times \begin{bmatrix} (r_{13} - r_{12}r_{23})\,(r_{24} - r_{23}r_{34}) \\ + (r_{14} - r_{34}r_{13})\,(r_{23} - r_{12}r_{13}) \\ + (r_{13} - r_{14}r_{34})\,(r_{24} - r_{12}r_{14}) \\ + (r_{14} - r_{24}r_{12})\,(r_{23} - r_{24}r_{34}) \end{bmatrix}$$

$$+ \frac{1}{2r_{12}r_{56}\,[1 + (q-1)\,r_{56}]} \times \begin{bmatrix} (r_{15} - r_{12}r_{52})\,(r_{26} - r_{25}r_{65}) \\ + (r_{15} - r_{16}r_{56})\,(r_{26} - r_{21}r_{61}) \\ + (r_{16} - r_{15}r_{65})\,(r_{25} - r_{21}r_{51}) \\ + (r_{16} - r_{12}r_{62})\,(r_{25} - r_{26}r_{56}) \end{bmatrix}$$

$$+ \frac{1}{4r_{34}r_{56}\,[1 + (p-1)\,r_{34}]\,[1 + (q-1)\,r_{56}]} \times \begin{bmatrix} (r_{35} - r_{34}r_{54})\,(r_{46} - r_{43}r_{65}) \\ + (r_{35} - r_{36}r_{56})\,(r_{46} - r_{43}r_{63}) \\ + (r_{36} - r_{34}r_{64})\,(r_{45} - r_{46}r_{56}) \\ + (r_{36} - r_{35}r_{65})\,(r_{45} - r_{43}r_{53}) \end{bmatrix} \quad \text{......(15).}$$

This equation (15) holds good for all values of the indices 1, 2, 3, 4, 5, 6. Let them now be replaced by $x[p]$, $y[q]$, x_k, x_h, y_u, y_v, where these terms have the same meaning as in appendix *b*.

Then, as the indices k and h indicate groups of measurements differing from one another only in the distribution of the accidental disturbances among the individuals, k and h may legitimately be conceived to have such values that, in general, the correlations produced both by x_k and by x_h are equal to the average of the correlations produced by all the groups for x. Analogously, as regards the indices u and v.

We get, then,

$$r_{12} = r_{x[p],\,y[q]} = \text{say, } f \quad \text{........................(16),}$$

$$r_{34} = r_{x[1],\,x[1]} = \text{say, } g \quad \text{........................(17),}$$

$$r_{56} = r_{y[1],\,y[1]} = \text{say, } h \quad \text{........................(18).}$$

Also from (8) $r_{13} = r_{14} = r_{x[p],\,x[1]} = \dfrac{\sqrt{p}\,.\,g}{\sqrt{1 + (p-1)\,g}}$ ....(19),

and $r_{25} = r_{26} = r_{y[q],\,y[1]} = \dfrac{\sqrt{q}\,.\,h}{\sqrt{1 + (q-1)\,h}}$ ....(20).

And $r_{35} = r_{45} = r_{36} = r_{46} = r_{x[1],\, y[1]}$, which, utilising (11),

$$= f\frac{\sqrt{[1+(p-1)g][1+(q-1)h]}}{\sqrt{pq}} \quad\text{...............(21)},$$

$$r_{23} = r_{24} = r_{x[1],\, y[q]} = f\frac{\sqrt{1+(p-1)g}}{\sqrt{p}} \quad\text{............(22)},$$

$$r_{15} = r_{16} = r_{x[p],\, y[1]} = f\frac{\sqrt{1+(q-1)h}}{\sqrt{q}} \quad\text{............(23)}.$$

Then, as $$z = f\sqrt{\frac{1+(p-1)g}{pg}\cdot\frac{1+(q-1)h}{qh}},$$

substituting from (16)—(23) in equation (15), we get finally

$$\sigma_z^2 = \frac{1}{n}\frac{1+(p-1)g}{pg}\cdot\frac{1+(q-1)h}{qh}\left[(1-f^2)^2 + f^2\frac{(1-g)[1+g-9g^2-(4p-9)g^3]}{4g^2[1+(p-1)g]^2}\right.$$
$$\left.+ f^2\frac{(1-h)[1+h-9h^2-(4q-9)h^3]}{4h^2[1+(q-1)h]^2} + f^4\frac{qh(1-g)+pg(1-h)+(1-g)(1-h)}{pqgh}\right] \quad\text{......(24)}.$$

The three terms on the right are usually small. Neglecting them, we get with sufficient approximation for most psychological purposes,

$$\sigma_z = \frac{1-f^2}{\sqrt{n}}\sqrt{\frac{1+(p-1)g}{pg}\cdot\frac{1+(q-1)h}{qh}},$$

or the probable error of the correct coefficient r_{xy}

$$= \cdot 6745\,\frac{1-r^2_{x[p],\, y[q]}}{\sqrt{n}}\cdot\frac{r_{xy}}{r_{x[p],\, y[q]}},$$

where $r_{x[p],\, y[q]}$, as in (II), denotes the correlation between the average of all the p group averages for x and the average of all the g ones for y.

e. Yule's proof of the correction formula.

x_1 and y_1 are measures of x and y at a certain series of measurements.

x_2 „ y_2 „ „ „ another „ „

Let $x_1 = x + \delta_1,\ x_2 = x + \delta_2,\ y_1 = y + \epsilon_1,\ y_2 = y + \epsilon_2,$

all terms denoting deviations from means.

Then, if it is assumed that δ, ϵ, the errors of measurement, are uncorrelated with each other or with x or y,

$$\Sigma(x\delta) \text{ etc.} = 0, \quad \Sigma(x_1 y_1) = \Sigma(xy).$$

Hence $$r_{x_1y_1}\sigma_{x_1}\sigma_{y_1} = r_{xy}\,\sigma_x\sigma_y,$$

and similarly
$$r_{x_2y_2}\sigma_{x_2}\sigma_{y_2} = r_{xy}\,\sigma_x\sigma_y,$$
$$r_{x_1y_2}\sigma_{x_1}\sigma_{y_2} = r_{xy}\,\sigma_x\sigma_y,$$
$$r_{x_2y_1}\sigma_{x_2}\sigma_{y_1} = r_{xy}\,\sigma_x\sigma_y,$$

or
$$r_{xy}^4 = r_{x_1y_1}\cdot r_{x_2y_2}\cdot r_{x_1y_2}\cdot r_{x_2y_1}\,\frac{\sigma_{x_1}^2\sigma_{x_2}^2\sigma_{y_1}^2\sigma_{y_2}^2}{\sigma_x^4\sigma_y^4}\quad\ldots\ldots\ldots(1).$$

But also, since $\Sigma(x\delta)=0$, $\Sigma x_1x_2 = \Sigma x^2$,

and
$$r_{x_1x_2}\sigma_{x_1}\sigma_{x_2} = \sigma_x^2,$$

or
$$\sigma_{x_1}\sigma_{x_2} = \frac{\sigma_x^2}{r_{x_1x_2}} \text{ and } \sigma_{y_1}\sigma_{y_2} = \frac{\sigma_y^2}{r_{y_1y_2}}\ldots\ldots\ldots\ldots\ldots\ldots(2).$$

From (1) and (2)
$$r_{xy}^4 = r_{x_1y_1}r_{x_2y_2}r_{x_1y_2}r_{x_2y_1}.$$

SOME EXPERIMENTAL RESULTS IN THE CORRELATION OF MENTAL ABILITIES[1]

By WILLIAM BROWN.

1. *General purpose of the investigation.*
2. *Nature of the groups of individuals measured.*
3. *Enumeration and detailed description of the tests employed.*
4. *General remarks upon the tests.*
5. *Correlation results.*
6. *Use of the method of 'partial' correlation.*
7. *Conclusion.*

THE following research was devised for the purpose of determining to what extent correlation exists between certain very simple mental abilities in cases where the individuals experimented upon are, as near as may be, identically situated with respect to previous practice, general training, and environment; and how closely, if at all, these elementary abilities are related to general intellectual ability as measured by teachers' judgments, school marks, etc. Every effort was made to keep the groups of individuals tested as *homogeneous* as possible; and instead of measuring irrelevant factors and 'correcting' for them in the later stages of the research, the influence of such irrelevant factors was excluded right from the beginning by a rigorous segregation of the material, and in other ways.

The groups of individuals to which the tests were applied, were as follows:

Group I. 66 boys of a London elementary school, all between the ages 11 and 12.

Group II. 39 girls of a London elementary school, all between the ages 11 and 12.

Group III. 40 boys of a London higher grade school, all between the ages 11 and 12.

[1] The present article forms the third part of the writer's thesis on "The Use of the Theory of Correlation in Psychology," approved for the degree of Doctor of Science in the University of London to be published by the Cambridge University Press. The reader is referred to this publication for fuller information as to the mathematical methods employed.

Group IV. 56 training college students (women), of the same year and of approximately the same age.

Group V *a*. 35 university students (men).

[Group V *b*. 23 university students (women).]

Little need be said as to the nature of the groups. Group III was as homogeneous as could possibly be expected or desired. The individuals were not only of the same age but also belonged to the same form and had all worked for months past under exactly the same environment (same teacher etc.). They were however a rigorously *selected* class, as might be expected from the character of the school.

Group IV was also thoroughly homogeneous. During an entire year previous to the application of the tests they had lived under exactly the same environment.

In Group II there was a slight mixing of 'standards' which introduced some degree of heterogeneity, but the effect of this on the results must have been very small.

Group I was also slightly heterogeneous owing to mixture of standards, and the results show that the effect of this was somewhat greater than in the preceding case.

Group V *a* was fairly homogeneous, but was of course a 'selected' group. The same remarks apply to Group V *b*, but, in this case, owing to the smallness of the numbers (23) tested, the results were worked out by the method of ranks (ρ), which was considered good enough under such circumstances, and they are recorded avowedly as mere approximations.

Other groups of school children were also tested, but as the marking of the results is not yet complete, no further reference will be made to them here.

As regards the *tests* employed, they were chosen not so much for their novelty (though a few of them are new and the method of applying the tests was determined in every case entirely by the requirements of the circumstances) nor so much for their *a priori* likelihood of showing inter-correlation, as for their convenience in admitting of application to an entire group of subjects simultaneously and *unobtrusively*. The following is a list of them:

1. Crossing through letters *e* and *r* in a page of print.
2. Crossing through letters *a*, *n*, *o*, and *s* in a page of print.
3. Crossing through every letter in a page of print.
4. Adding up single digits in groups of ten. Measurement of (*a*) speed, (*b*) accuracy.

5. Bisecting ten printed lines (80 mm. long), and putting in one of the points of trisection in each of ten other lines (90 mm. long).

6. Müller-Lyer Illusion. Measurement of (*a*) size, (*b*) mean variation.

7. Vertical-Horizontal Illusion. Measurement of (*a*) size, (*b*) mean variation.

8. Mechanical Memory (permanent), tested by means of nonsense-syllables.

9. Memory for poetry.

10. Combination test (Ebbinghaus).

In the case of Groups II and III, recourse was also had to

11. Marks for Drawing.

12. Total School Marks.

13. Grading for General Intelligence (two independent measures).

Finally, with Groups V (*a*) and V (*b*), the following test was also employed.

14. Association-time (uncontrolled). Measurement of rate of sequence of ideas called up by a stimulus-word.

The performances of the several groups in these tests admit of comparison in terms of the mean, standard deviation, and coefficient of variation, provided that the probable errors of these constants are also evaluated.

With the exception of test (9), and, in some cases, of test (8), every test was applied twice, the second test being given about a fortnight after the first, and at the same hour of the day. In the case of the school-children, I myself applied both tests in the presence of the form-master or mistress. The adults whose measurements are recorded and employed in the present research were also tested, with hardly an exception, by myself. It should be added that the research commenced with a very much larger number of adults (university students and others), mounting to over 100, but the smaller numbers recorded as Groups V (*a*) and V (*b*) were alone used for the evaluation of coefficients, since they alone displayed sufficient reliability and homogeneity for the purpose.

1. *E R Test.*

Pages of French words, arranged in irregular order so that they did not 'make sense,' and so chosen that the number of *e*'s and *r*'s was approximately constant from line to line, were employed. The page was given out face downwards, and at a given signal the subject turned it over and proceeded to cross through every *e* and every *r* that he came to, beginning with the first line and moving down line by line, until he received the signal to stop. The time allowed for the test was 5 minutes in the case of the children and 3 minutes in the case of the adults. The subject was urged to avoid passing over any of the stated letters but otherwise to work as quickly as possible. Before the commencement of the test, a full explanation of it was given to the group, illustrated by examples on the blackboard. This was done in the case of every test. A different set of words was employed in the second test.

System of marking: 1 mark for each letter crossed through correctly;

− 1 mark for each letter passed over or crossed through incorrectly.

Results of E R Test.

Group	Mean	σ	Coefficient of variation	Reliability coefficient (r_1) for each test	Rel. coefficient (r_2) for amalgamated pair of tests $r_2=\frac{2r_1}{1+r_1}$ *
I	377 ± 6	68 ± 4	18 ± 1·1	·60	·75
II	362 ± 8	71 ± 5	19·5 ± 1·5	·65	·79
III	417 ± 6	57 ± 4	13·7 ± 1·1	·75	·86
V (*a*) †	204 ± 4·5	41 ± 3·3	20 ± 1·7	·97	—
V (*b*) †	—	—	—	·58	—

* r_2 measures the extent to which the amalgamated results of the two tests would correlate with a similar amalgamated series of two other applications of the same test. If x_1, x_2, x_1', x_2' be two pairs of results (x denoting, as usual, deviation from the mean value), we may assume that

$$\sigma_{x_1}=\sigma_{x_2}=\sigma_{x_1'}=\sigma_{x_2'}=\sigma_x \text{ (say)},$$

and that $$S(x_1x_1')=S(x_1x_2')=S(x_2x_1')=S(x_2x_2')=n\sigma_x^2 r_1.$$

Hence we get

$$r_2=\frac{S(x_1+x_2)(x_1'+x_2')}{n\sigma_{x_1+x_2}\sigma_{x_1'+x_2'}}$$

$$=\frac{4n\sigma_x^2 r_1}{n(2\sigma_x^2+2r_1\sigma_x^2)}$$

$$=\frac{2r_1}{1+r_1}. \quad \text{Q. E. D.}$$

It is easily seen that the amalgamation of 4 tests gives a reliability coefficient $=\frac{4r_1}{1+3r_1}$; and, in general, for n tests we have

$$r_n=\frac{nr_1}{1+(n-1)r_1}.$$

This last formula furnishes a ready means of determining from the reliability coefficient of a single test, the number of applications of the test which would be necessary to give an amalgamated result of any desired degree of reliability.

† One test only.

2. *A N O S Test.*

The method of procedure was identical with that described above for test 1, except that the letters to be crossed through were of four kinds instead of two, and that the time allowed was 5 minutes for children and also for adults.

Results of A N O S Test.

Group	Mean	σ	Coefficient of variation	Rel. coefficient, r_1	Rel. coefficient, r_2
I	161 ± 5	$58 \pm 3{\cdot}5$	$36 \pm 2{\cdot}4$	·77	·87
II	$191 \pm 5{\cdot}6$	$51 \pm 3{\cdot}8$	$26{\cdot}5 \pm 2{\cdot}2$	·84	·91
III	228 ± 6	$56 \pm 4{\cdot}1$	$24{\cdot}4 \pm 2{\cdot}0$	·81	·89

3. *Motor Test.*

In this test the subjects were asked to cross through *every* letter in a page of printed French words. Time allowed in all cases 3 minutes. Method of procedure otherwise identical with that for 1 and 2.

Results of Motor Test.

Group	Mean	σ	Coefficient of variation	Rel. coefficient, r_1	Rel. coefficient, r_2
I	718 ± 13	148 ± 9	$20{\cdot}6 \pm 1{\cdot}3$	·91	·95
II	720 ± 16	148 ± 12	$20{\cdot}5 \pm 1{\cdot}7$	·85	·92
III	813 ± 11	$103 \pm 7{\cdot}7$	$12{\cdot}7 \pm 1{\cdot}0$	·76	·86

4. *Addition Test.*

Duplicates of pages from one of Kraepelin's *Rechenhefte* were used, adapted to the purpose by the printing of short horizontal lines below each tenth figure, and by the omission of all figures below the thirtieth in each column. Time allowed for each test, 5 minutes. The speed of addition [4 (*a*)] was measured by the number of sums (groups of 10 digits) worked in the given time, the accuracy of addition [4 (*b*)] by the percentage of correct answers. In the case of the children 4 (*a*) was measured by the number of *digits* added, marks being allowed for the part of a sum with which they usually ended.

Results of Addition Test.

Group	Mean		σ		Coefficient of variation		Rel. coefficient, r_1		Rel. coefficient, r_2	
	Sp.	Acc.	Sp.	Acc.	Sp.	Acc.	Sp.	Acc.	Sp.	Acc.
I	235 ± 6	163 ± 3	67 ± 4	31·3 ± 1·8	28 ± 1·8	19 ± 1·2	·82	·33	·90	·50
II	210 ± 6	149 ± 4	52 ± 4	33·4 ± 2·5	24·6 ± 2·0	22·4 ± 1·8	·69	·46	·82	·63
III	287 ± 9	171 ± 1·4	79 ± 6	13·3 ± 1	27 ± 2·2	8 ± ·6	·68	[0]	·81	—
IV *	55 ± 1·5	173 ± 1·5	16 ± 1·0	16·8 ± 1·1	30 ± 2·9	9·6 ± ·86	·93	·29	·96	·45
V (*a*) *	61 ± 2·1	184 ± 1·2	19 ± 1·5	11·3 ± ·9	32 ± 2·8	6 ± ·5	·95	·22	·97	·36
V (*b*)	—	—	—	—	—	—	·98	·59	·99	·74

* 1 sum of 10 digits taken as unit; in other cases, the number of digits added was taken as the measure.

5. *Bisection and Trisection of Lines.*

Each test paper contained ten printed lines, each 8 cm. long, for bisection; and ten printed lines, each 9 cms. long, for trisection. The lines were printed three in a row, and those situated immediately under others were shifted a little to one side. It was very certain, however, that the bisection or trisection of any one of the lines was influenced by the positions of the neighbouring lines and of the edges of the paper. This fact diminishes the value of the test, but does not of itself deprive the test of all use as a measure of one form of sensory discrimination. A more serious drawback was found to be the very great individual variability displayed, which made the reliability coefficients very low. In order to get a more reliable measure for Group V the results for bisection and trisection in both tests (i.e. of the division of 40 lines in all by each individual) were thrown together and the total taken as a measure of sensory discrimination. *Only one* point of trisection was asked for, this being put in alternately towards the left and the right ends of the successive lines. Trisection was done very unsatisfactory by the school-children, but, in the case of adults, gave a higher measure of reliability than did bisection.

The *average crude error* was taken as the measure of inaccuracy, since it was found to give more concordant results than the other possible ways of measuring the inaccuracy.

Results of Bisection and Trisection.

Group	Mean	σ	Coefficient of variation	Rel. coefficient, r_1	Rel. coefficient, r_2
Bisection only I	311 ± 11	129 ± 8	41 ± 2·8	·35	·52
Bisection + Trisection					
V (*a*)	480 ± 14	129 ± 10	26 ± 2·2	B. ·36 T. ·35	B. ·53 T. ·52
V (*b*)	—	—	—	B. ·44 T. ·87	B. ·93 T. ·82

6. *Müller-Lyer Illusion.*

The adjustable apparatus, designed by Dr W. H. R. Rivers, was used to measure the size of this illusion. The length of the standard line was 75 mm., and results recorded below are also in mm. Each child of Groups II and III was tested individually by myself, being asked to make 10 adjustments of the apparatus, alternately lengthening and shortening the variable line, and having the standard line alternately to the right and to the left. To obtain the reliability coefficient, the results were divided into two halves and correlated. The average deviation was taken as a size of the illusion, and the mean variation (M.V.) was also determined. Ten subjects of Group IV were tested, 10 times each, by one of the mistresses of the college. Groups V(*a*) and V(*b*) were tested by myself, but four times only.

The present test was the only one employed which involved the use of apparatus or the testing of the subjects separately.

Results of Müller-Lyer Illusion Test.

Group	Mean		σ		Coefficient of variation		Rel. coefficient, r_1	For *size* of illusion only, r_2
	Size	M.V.	Size	M.V.	Size	M.V.		
II	17·3 ± ·31	3·3 ± ·14	2·8 ± ·22	1·3 ± ·10	16·2 ± 1·3	39 ± 3·4	·65	·79
III	16·8 ± ·4	3·1 ± ·12	3·8 ± ·3	1·1 ± ·08	22·5 ± 1·8	36 ± 3·1	·86	·92
IV	13·7 ± 1·1	2·3 ± ·18	5·3 ± ·8	·8 ± ·13	38 ± 6·5	36 ± 6·1	·76	·86
V (*a*)*	16 ± ·67	—	3·7 ± ·48	—	23 ± 3·1	—	·57	—
V (*b*)*	—	—	—	—	—	—	·68	—

* One test only.

7. *Vertical-Horizontal Illusion.*

The material for this test consisted of a set of 10 large L-shaped figures clearly printed on a very large sheet of paper, which could be folded in two. Each L had unequal arms, the shorter being 10 cms. in length, the longer 14 cms., and the vertical was alternately the shorter and the longer of the two. These papers having been distributed, it was explained by means of the blackboard that the task to be performed was to mark off a part along the longer arm (estimated from the angle), such that it seemed equal to the shorter arm, the subject limiting his attention strictly to each figure in turn and estimating *by eye only.* When all the ten figures had been marked in this way the subjects were asked *to go over them once more,* altering those which seemed too long or too short, the object of this being to make sure of the full effect of the illusion (of the existence of which, by the way, not one of the subjects tested—Groups I, II, III, and IV—was aware). A fortnight later the test was repeated with other papers. The average size and the M.V. of the illusion were evaluated as in (6) above. Two objections may be made to this method of applying the test: (1) the presence of surrounding L's influenced the judgment; this was partly obviated by the way the figures were arranged on the page, and I believe the influence was actually very small, each L being large enough to exclusively rivet the attention of the subject upon itself in its turn; (2) the *eyesight* of the subjects of the experiment was not previously tested. This objection is much more serious. Even if the illusion is not to be entirely explained as the effect of *astigmatism,* the latter must play an important part in determining the result. All we can say, then, is that the test measures the *balance of effect* of the various factors contributing towards the falsifying of judgments comparing horizontal and vertical distances. A somewhat remarkable result, which I do not remember to have heard or seen reported before, is that with as many as 20 measurements of each subject, quite a large proportion of the subjects show a *negative* illusion, i.e. they *under*estimate the vertical instead of overestimating it. One might retort that this is simply a case of *over-correction,* were it not for the still more remarkable fact that in the case of all the children measured, the proportion is exactly $\frac{1}{3}$, in Group I 22 out of 66, in Group II, 13 out of 39, in Group III 13 out of 40. In Group IV the second test has unfortunately not yet been marked; for the first test alone the proportion is $\frac{12}{68}$.

Results of Vertical-Horizontal Illusion Test.

Group	Mean		σ		Rel. coefficient, r_1		Rel. coefficient, r_2	
	Size	M.V.	Size	M.V.	Size	M.V.	Size	M.V.
I	25 ± 4·6	3·2 ± ·11	53 ± 3	1·3 ± ·08	·69	·43	·82	·60
II	29 ± 7·1	—	65 ± 5	—	·59	—	·74	—
III	31 ± 8·2	3·3 ± ·11	76 ± 6	1·0 ± ·08	·75	[0]	·86	—

8. *Mechanical Memory Test.*

In this test a printed list of 10 nonsense syllables was placed face downwards before each of the subjects, and at a given signal the subjects turned the papers over and applied themselves to the learning of the syllables as intensely as possible. On a second signal, 2—3 minutes later, the papers were once more turned face downwards, and collected by the experimenter. The subjects were then asked to think no more about the syllables for the present. On the following day, at the same hour, blank slips of paper were distributed and the subjects were asked to write down the syllables they had learnt the previous day, so far as possible *in the right order*. As a system of marking which was found to be sufficiently satisfactory for the purpose, 2 marks were given for each syllable right and in the right order, and 1 mark for each right but in the wrong order. The time allowed for learning was 3 minutes in the case of Groups I and II, but this was found to be too long in the case of Groups III and IV, who were eventually given 2 minutes and 2½ minutes respectively. On account of the difficulty thus raised (and, in the case of Group II, for another reason) two series of results could unfortunately be obtained from Groups I and IV only.

Results of Mechanical Memory Test.

Group	Mean	σ	Coefficient of variation	Rel. coefficient, r_1	Rel. coefficient, r_2
I	14·5 ± ·8	9·7 ± ·57	67 ± 5	·51	·68
II*	9·6 ± ·6	5·7 ± ·43	59 ± 6	—	—
III*	11·8 ± ·8	6·8 ± ·53	57 ± 6	—	—
IV	31·5 ± ·7	7·7 ± ·5	24 ± 2	·50	·67

* Results of one test only.

9. *Memory for Poetry.*

This test was applied but once, and to Groups I and III only. Three verses of Hood's 'Queen Mab,' which it appears that neither of the groups had seen or heard of previously, were set to be learnt for 5 minutes, and the subjects were asked to attempt to reproduce them 24 hours later. The frequency constants were found to be as follows:

Group	Mean	σ	Coefficient of variation
I	$20 \pm \cdot 85$	$9 \cdot 7 \pm \cdot 6$	$49 \pm 3 \cdot 7$
III	$28 \cdot 6 \pm 1 \cdot 00$	$9 \cdot 0 \pm \cdot 75$	$31 \cdot 6 \pm 2 \cdot 9$

10. *Combination Test.*

This was the well-known *Combinations-Methode* of Ebbinghaus, in which the subject is shown a passage of continuous prose with from one-third to one-quarter of the words replaced by blanks, and is asked to supply the missing words or words of similar significance.

In applying this test, a thorough explanation, including blackboard demonstrations and examples, was first given to the class and the papers were then distributed face-downwards. At a given signal the class turned the papers over and proceeded to read the passage through carefully (*writing nothing*) with a view to grasping the general sense of the entire passage. On a second signal, 3 minutes later, they proceeded to fill in the blanks *in order* from the beginning, endeavouring to find in each case a word which would suit the sense both of the particular sentence in which it occurred and also of the entire passage. This second period lasted 5 minutes, at the end of which time the signal was given to stop. Such was the method of procedure in both applications of the test in the case of the school-children, and both results were found to be quite satisfactory. In the case of the adults the times allowed were different, being $1' + 10'$ for the first passage, and $1' + 3'$ for the second, and the reliability coefficient for Group V was found to be abnormally *low*. As this seemed to be due mainly to the unsatisfactory way in which the first test was performed (I had chosen the passage badly), the results of the second test were alone used for the purposes of correlation. In Group IV the reliability coefficient was higher, though still not very high, and the two series of results were therefore amalgamated in the usual way.

In marking the papers, words supplied by the subject were counted right if they made sense in their sentence and tolerable sense in the entire passage, and Ebbinghaus' system of values was adopted; viz. each blank filled in correctly = 1 mark.

Each blank filled in incorrectly = − 1 mark.

Each blank passed over = − ½ mark.

Results of Combination Test.

Group	Mean	σ	Coefficient of variation	Rel. coefficient, r_1	Rel. coefficient, r_2
I	22 ± 1·3	16·3 ± ·95	74 ± 6·3	·74	·85
II	19·7 ± 1·2	11·4 ± ·87	58 ± 6	·56	·72
III	41·6 ± 1·8	17 ± 1·3	41 ± 3·5	·73	·84
IV	71·3 ± 2·1	24 ± 1·5	33 ± 3·3	·46	·63
V (*a*)*	18·5 ± ·78	7·14 ± ·45	38 ± 3·4	[·22]	—
V (*b*)	—	—	—	·69	·82

* One test only (the 2nd).

Measurements 11, 12, *and* 13.

Measurements 11 and 12 (marks for Drawing and Total School Marks) need no further explanation. The grading for General Intelligence was obtained from two of the schools—Groups II and III—from the former of which two separate and independent gradings, by different teachers, were provided. These independent gradings correlated with one another to the extent of ·90, which gave a reliability coefficient r_2 for the amalgamated grading = ·95.

14. *Association-Time.*

This test was applied to certain individuals of Group V (*a* and *b*) only, and was of the following nature. A word of ordinary significance (a noun) was read out to the subjects and they were expected to write down as rapidly as possible during the two minutes which followed words representing the various 'ideas' which passed through their mind in the time. After a short pause, another quite different word was called out and the writing repeated. Finally a third word was called out. The total number of words or phrases written down was taken as a measure of the rate of sequence of associated ideas in the subject's mind. The test was repeated a fortnight later.

This method gives fairly reliable results—for Group V (a) $r_1 = \cdot67$ and for Group V (b) $r_1 = \cdot87$—but is vitiated by the mechanical process of writing. The impurity could be eliminated by applying a simple writing test (speed) also, and then employing the formula for the 'partial' correlation of 3 variables; it was not, however, done in the present research.

General Remarks upon the Tests.

The results tabulated in the last few pages, when tested by means of the formula for the P.E. of a difference [$\sigma_{x-y} = \sqrt{\sigma_x^2 + \sigma_y^2}$ and therefore $\text{P.E.}_{x-y} = \sqrt{\text{P.E.}^2_x + \text{P.E.}^2_y}$], show certain differences between group and group in respect of average ability, variability and reliability for correlation which justify our plan of working correlation coefficients separately for the several groups, but do not seem otherwise to give many positive results of general significance and importance, such as e.g. evidence as to the relative variability of the two sexes. A more careful and thorough examination of the tables may give cause for some qualification of the preceding statement. At any rate the individual figures are of considerable interest. The reliability coefficients, even for the single tests, are in most cases sufficiently high,—in fact much higher than I had dared to expect considering the circumstance that in all but one test the subjects were examined collectively. The less satisfactory tests, as applied in this research, seem to be those for accuracy of addition, bisection and trisection of lines, M.V. of vertical-horizontal illusion, and, in a slighter degree, mechanical memory. The combination test in the case of the adults, was also rather unsatisfactory, but in the case of the school children it gave fairly high results. The tests in which the applications give very reliable results are the motor test, the *a n o s* test, speed of addition, the combination test (with children) and the *e r* test. The coefficients of variation are rather high, clustering about the values 20—30; in a few cases they are considerably higher.

The tests were applied during the course of the summer of 1909, and my sincere gratitude and thanks are due to the headmasters, headmistresses and others, through whose kindness I was enabled to bring the research to a successful conclusion. In collecting some of the material, I benefited greatly from the invaluable cooperation of Mr A. A. Cock, Assistant Master of Method, King's College, London, to whom I owe a very special debt of gratitude. His expert advice on several points in the research was extremely helpful.

As a fact of considerable importance, it should be added that the tests were so applied as to disturb the ordinary routine of the schools *as little as possible.*

Correlation Results.

The values of the frequency constants for the various groups of subjects show very clearly that any plan of throwing them together (as they stand) and working out coefficients from the combined series would produce a considerable amount of 'spurious' correlation and make the results almost valueless. One exception, indeed, to this state of affairs was found in the case of Speed and Accuracy of Additions [4 (*a*) and 4 (*b*)] in Group I and in a group of 20 boys not otherwise included in the present research. The means and S.D.'s in these two sets of boys for these two characteristics were found to be the same, within the limits of probable error. These 86 boys were therefore taken together for this particular correlation and a correlation table was drawn up, whereby the value of η could be calculated as well as that of r, and the nature of the regression curve and regression line determined. To make the investigation into this particular problem of speed and accuracy in adding complete, small correlation tables were drawn up for all the other groups, separately from one another, and the value of η was calculated in each of these cases also[1]. Apart from these cases the plan of grouping in correlation tables seemed quite unsuitable for such small numbers. The values were therefore taken as they stood, but the full product-moment formula, $\frac{S(xy)}{N\sigma_1\sigma_2}$, was employed throughout, with the single exception of Group V (*b*) where the numbers were so very small (23) that the method of ranks $\left[\rho = 1 - \frac{6S(\nu_1 - \nu_2)^2}{N(N^2-1)},\ r = 2\sin\left(\frac{\pi}{6}\rho\right)\right]$ was considered sufficiently accurate; here nothing but a general impression of the nature of the correlation could be expected.

The following Tables give the values of the correlation coefficients between series formed by the amalgamation of the two measurements made in each test. The numbers immediately below the coefficients are the probable errors, and those in thick type are the reliability coefficients (r_2) for the amalgamated series, showing to what extent each amalgamated series would correlate with another quite similar series.

[1] **More complete details will be found in my Thesis, to be published shortly.**

GROUP I. 66 *boys* (*elementary school*) *ages* 11—12.

	e r	a n o s	Combination	Mech. memory	Memory for poetry	Addition (speed)	Addition (acc.)	Motor (all letters)	M. V. of V.-H. Ill.	Bisection	V.-H. Ill.
e r	**·75**	·78 *·03*	·45 *·07*	·40 *·07*	·27 *·08*	·59 *·05*	·30 *·08*	·53 *·06*	– ·19 *·08*	0	0
a n o s	·78 *·03*	**·87**	·48 *·07*	·29 *·08*	·28 *·08*	·51 *·06*	·24 *·08*	·21 *·08*	– ·31 *·08*	0	·11 *·08*
Combination	·45 *·07*	·48 *·07*	**·85**	·52 *·06*	·52 *·06*	·40 *·07*	·38 *·07*	·13 *·09*	0	·15 *·08*	0
Mech. memory	·40 *·07*	·29 *·08*	·52 *·06*	**·68**	·49 *·07*	·27 *·08*	·31 *·08*	·14 *·08*	0	·10 *·09*	·24 *·08*
Memory for poetry	·27 *·08*	·28 *·08*	·52 *·06*	·49 *·07*		·41 *·07*	·38 *·07*	·12 *·08*	0	·13 *·09*	·10 *·09*
Addition (speed)	·59 *·05*	·51 *·06*	·40 *·07*	·27 *·08*	·41 *·07*	**·90**	·13 *·08*	·25 *·08*	0	0	·12 *·09*
Addition (acc.)	·30 *·08*	·24 *·08*	·38 *·07*	·31 *·08*	·38 *·07*	·13 *·08*	**·50**	0	– ·17 *·08*	·41 *·07*	·18 *·08*
Motor (all letters)	·53 *·06*	·21 *·08*	·13 *·09*	·14 *·08*	·12 *·08*	·25 *·08*	0	**·95**	·09 *·09*	0	0
M. V. of V.-H. Ill.	– ·19 *·08*	– ·31 *·08*	0	0	0	0	– ·17 *·08*	·09 *·09*	**·60**	– ·22 *·08*	·21 *·08*
Bisection	0	0	·15 *·08*	·10 *·09*	·13 *·09*	0	·41 *·07*	0	– ·22 *·08*	**·52**	·12 *·09*
V.-H. Ill.	0	·11 *·08*	0	·24 *·08*	·10 *·09*	·12 *·09*	·18 *·08*	0	·21 *·08*	·12 *·09*	**·82**

31 coefficients > 2 × P.E.

25 coefficients > 3 × P.E.

GROUP II. 39 *girls* (*elementary school*) *ages* 11—12.

	School marks	Combination	Mech. memory	Letters a n o s	General intelligence	Letters e r	M.-L. Ill.	Motor (all letters)	Addition (speed)	Drawing	V.-H. Ill.	Addition (acc.)	M. V. of M.-L. Ill.
School marks		·54 *·08*	·59 *·07*	·27 *·10*	·64 *·06*	·00	·16 *·11*	·00	·00	·00	– ·15 *·11*	·00	·00
Combination	·54 *·08*	·72	·37 *·09*	·00	·43 *·09*	– ·15 *·11*	·00	·00	– ·13 *·11*	·22 *·10*	·22 *·10*	– ·25 *·10*	·00
Mech. memory	·59 *·07*	·37 *·09*		·20 *·10*	·55 *·08*	·00	·00	·00	– ·13 *·11*	·00	·00	– ·23 *·10*	– ·16 *·11*
Letters a n o s	·27 *·10*	·00	·20 *·10*	·91	·13 *·11*	·80 *·04*	– ·21 *·10*	·21 *·10*	·00	·13 *·11*	– ·11 *·11*	·00	·15 *·11*
General intelligence	·64 *·06*	·43 *·09*	·55 *·08*	·13 *·11*	·95	·00	·00	·13 *·11*	·10 *·11*	·00	·00	00	·00
Letters e r	·00	– ·15 *·11*	·00	·80 *·04*	·00	·79	– ·20 *·10*	·49 *·08*	·13 *·11*	·00	– ·18 *·10*	·00	·00
M.-L. Ill.	·16 *·11*	·00	·00	– ·21 *·10*	·00	– ·20 *·10*	·79	– ·21 *·10*	·12 *·11*	– ·44 *·09*	– ·21 *·10*	·00	– ·21 *·10*
Motor (all letters)	·00	·00	·00	·21 *·10*	·13 *·11*	·49 *·08*	– ·21 *·10*	·92	·33 *·10*	·00	·00	·30 *·10*	·00
Addition (speed)	·00	– ·13 *·11*	– ·13 *·11*	·00	·10 *11*	·13 *·11*	·12 *·11*	·33 *·10*	·82	– ·40 *·09*	·00	·24 *·10*	·00
Drawing	·00	·22 *·10*	·00	·13 *·11*	·00	·00	– ·44 *·09*	·00	– ·40 *·09*		·27 *·10*	·11 *·11*	·00
V.-H. Ill.	– ·15 *·11*	·22 *·10*	·00	– ·11 *·11*	·00	– ·18 *·11*	– ·21 *·10*	·00	·00	·27 *·10*	·74	·00	·00
Addition acc.	·00	– ·25 *·10*	– ·23 *·10*	·00	·00	·00	·00	·30 *·10*	·24 *·10*	·11 *·11*	·00	·63	·00
M.V. of M.-L. Ill.	·00	·00	– ·16 *·11*	·15 *·11*	·00	·00	– ·21 *·10*	·00	·00	·00	·00	·00	

26 coefficients > 2 × P.E.
12 coefficients > 3 × P.E.

GROUP III. *Higher Grade School, 40 boys between the ages of* 11 *and* 12.

	School marks	Gen. intell.	Memory for poetry	Combination	Drawing	Letters e r	Add. (sp.)	M.V. of M.-L. Ill.	Letters a n o s	M.V. of V.-H. Ill.	Mech. memory	M.-L. Ill.	V.-H. Ill.	Letters (all) motor	Add. (acc.)
School marks		·78 *·04*	·60 *·07*	·60 *·07*	·51 *·08*	·30 *·10*	·28 *·10*	·28 *·10*	·17 *·10*	– ·20 *·10*	·40 *·09*	– ·20 *·10*	– ·30 *·10*	·23 *·10*	·11 *·11*
General intell.	·78 *·04*		·57 *·07*	·69 *·06*	·42 *·09*	·28 *·10*	·24 *·10*	·22 *·10*	·10 *·11*	– ·29 *·10*	·19 *·08*	0	– ·30 *·10*	·32 *·10*	0
Memory for poetry	·60 *·07*	·57 *·07*		·44 *·09*	·44 *·09*	·23 *·10*	0	·52 *·08*	·14 *·10*	0	·38 *·09*	0	0	·19 *·10*	– ·11 *·11*
Combination	·60 *·07*	·69 *·06*	·44 *·09*	**·84**	·46 *·09*	0	·32 *·10*	0	·10 *·11*	– ·20 *·10*	·28 *·10*	0	– ·11 *·11*	·2[illegible] *·10*	0
Drawing	·51 *·08*	·42 *·09*	·44 *·09*	·46 *·09*		·11 *·11*	·14 *·10*	0	·19 *·10*	– ·24 *·10*	·39 *·09*	– ·19 *·10*	0	0	0
Letters e r	·30 *·10*	·28 *·10*	·23 *·10*	0	·11 *·11*	**·86**	·35 *·10*	·26 *·10*	·74 *·05*	·26 *·10*	0	– ·31 *·10*	0	·25 *·10*	0
Add. (sp.)	·28 *·10*	·24 *·10*	0	·32 *·10*	·14 *·10*	·35 *·10*	**·81**	0	·20 *·10*	– ·11 *·11*	0	– ·32 *·10*	– ·10 *·11*	·20 *·10*	·33 *·10*
M.V. of M.-L. Ill.	·28 *·10*	·22 *·10*	·52 *·08*	0	0	·26 *·10*	0		·24 *·10*	·26 *·10*	·29 *·10*	0	– ·14 *·10*	– ·14 *·10*	0
Letters a n o s	·17 *·10*	·10 *·11*	·14 *·10*	·10 *·11*	·19 *·10*	·74 *·05*	·20 *·10*	·24 *·10*	**·89**	– ·16 *·10*	0	– ·35 *·10*	0	0	– ·11 *·11*
M.V. of V.-H. Ill.	– ·20 *·10*	– ·29 *·10*	0	– ·20 *·10*	– ·24 *·10*	·26 *·10*	– ·11 *·11*	·26 *·10*	– ·16 *·10*	$r_1=0$	0	·32 *·10*	·33 *·10*	0	·13 *·10*
Mech. memory	·40 *·09*	·49 *·08*	·38 *·09*	·28 *·10*	·39 *·09*	0	0	·29 *·10*	0	0		0	– ·16 *·10*	0	0
M.-L. Ill.	– ·20 *·10*	0	0	0	– ·19 *·10*	– ·31 *·10*	– ·32 *·10*	0	– ·35 *·10*	·32 *·10*	0	**·92**	·29 *·10*	0	– ·11 *·11*
V.-H. Ill.	– ·30 *·10*	– ·30 *·10*	0	– ·11 *·11*	0	0	– ·10 *·11*	– ·14 *·10*	0	·33 *·10*	– ·16 *·10*	·29 *·10*	**·86**	0	– ·14 *·10*
Letters (all) motor	·23 *·10*	·32 *·10*	·19 *·10*	·28 *·10*	0	·25 *·10*	·20 *·10*	– ·14 *·10*	0	0	0	0	0	**·86**	0
Add. (acc.)	·11 *·11*	0	– ·11 *·11*	0	0	0	·33 *·10*	0	– ·11 *·11*	·13 *·11*	0	– ·11 *·11*	– ·14 *·10*	0	$r_1=0$

51 coefficients > 2 × P.E. 28 coefficients > 3 × P.E.

GROUP IV. ($n = 56$.) *Provisional and incomplete table of coefficients.*

	Combination	Addition (acc.)	Addition (sp.)	Mech. memory
Combination	**·63**	·53 *·06*	·34 *·08*	·31 *·08*
Addition (acc.)	·53 *·06*	**·45**	·43 *·07*	·20 *·09*
Addition (sp.)	·34 *·08*	·43 *·07*	**·96**	·18 *·09*
Mech. memory	·31 *·08*	·20 *·09*	·18 *·09*	**·67**

Variability coefficient for speed of addition, $r\,\frac{\text{add. (sp.)}_1 + \text{add. (sp.)}_2}{\text{add. (sp.)}_1 \sim \text{add. (sp.)}_2} = 0{\cdot}33 \pm {\cdot}08$.

,, ,, accuracy ,, $r\,\frac{\text{add. (acc.)}_1 + \text{add. (acc.)}_2}{\text{add. (acc.)}_1 \sim \text{add. (acc.)}_2} = -0{\cdot}66 \pm {\cdot}05$.

GROUP V (a). ($n = 35$.)

	e r	Assoc. time	Addition (acc.)	Combination	Addition (sp.)	M.-L. Ill.	Bisection + trisection
e r	**·97**	− ·18 *·11*	− ·26 *·10*	·19 *·11*	0	·42 *·09*	− ·24 *·10*
Assoc. time	− ·18 *·11*	**·87**	·39 *·09*	·33 *·10*	·37 *·09*	0	0
Addition (acc.)	− ·26 *·10*	·39 *·09*	**·36**	− ·16 *·11*	·38 *·09*	0	0
Combination	·19 *·11*	·33 *·10*	− ·16 *·11*	**·22***	·19 *·11*	− ·24 *·10*	0
Addition (sp.)	0	·37 *·09*	·38 *·09*	·19 *·11*	**·97**	0	·13 *·11*
M.-L. Ill.	·42 *·09*	0	0	− ·24 *·10*	0	**·57**	− ·29 *·10*
Bisection + trisection	− ·24 *·10*	0	0	0	·13 *·11*	·29 *·10*	**B = ·53** **T = ·52**

9 coefficients > 2 × P.E. 5 coefficients > 3 × P.E.

* The second test only was used in this case for correlation with other tests, since the low correlation between the two was almost certainly due to the unsatisfactory nature of the first. The value ·22 is, then, r_1.

[GROUP V (*b*). ($n = 23$.)*

Method of ranks used:

$$\rho = 1 - \frac{6S(d^2)}{N(N^2-1)}. \qquad r = 2\sin\left(\frac{\pi}{6}\rho\right).$$

	M.-L. Ill.	Assoc. time	Combination	Bisection + trisection	Addition (acc.)	Addition (sp.)	e r
M.-L. Ill.	·68	– ·78 *·05*	– ·90 *·03*	– ·84 *·04*	– ·53 *·10*	– ·66 *·08*	– ·42 *·11*
Assoc. time	– ·78 *·05*	·67	·58 *·09*	·53 *·10*	·58 *·09*	·35 *·12*	·43 *·11*
Combination	– ·90 *·03*	·58 *·09*	·82	0	·28 *·13*	·13 *·14*	·52 *·10*
Bisection + trisection	– ·84 *·04*	·53 *·10*	0	B = ·61 T = ·93	·29 *·13*	·40 *·12*	0
Addition (acc.)	– ·53 *·10*	·58 *·09*	·28 *·13*	·29 *·13*	·74	·35 *·12*	0
Addition (sp.)	– ·66 *·08*	·35 *·12*	·13 *·14*	·40 *·12*	·35 *·12*	·99	0
e r	– ·42 *·11*	·43 *·11*	·52 *·10*	0	0	0	·58]

16 coefficients > 2 × P.E. 12 coefficients > 3 × P.E.

* These results are recorded as avowedly rough approximations only, owing to the smallness of the sample.

In these tables the tests are arranged according to order of magnitude of the *average* correlation of each with all the rest (within any particular group), and all coefficients smaller than their probable errors are put down as 0. Of the coefficients recorded, the total number of those > 2P.E. is 139, and *of those* > 3P.E. *the total is* 86. The first thing to be noticed in the groups of coefficients arranged in this way is that *not one of them shows the 'hierarchical arrangement,'* and it is a very significant fact that the group which approaches it most nearly (Group I) is the group where 'spurious correlation' due to heterogeneity of material was to be suspected (see p. 297). Now it will be apparent, on the slightest reflection, that any extraneous source of correlation (such as e.g. difference of the state of *discipline* to which different numbers of the group had been accustomed immediately antecedent to the occasion of applying the tests) the influence of which is in a *constant* direction but varies in amount from test to test according to the varying degrees to which the individual tests are susceptible to its influence, *must* tend to produce the hierarchical arrangement, and unless counteracted by other more potent tendencies, *would* do so. In fact, it would be the 'central factor' supposed to be indicated by such a form of arrangement of coefficients. Spurious correlation of this nature might arise from the use of unfamiliar apparatus in the tests, or from the novelty of the tests, or in many other ways. The form of procedure adopted in the present research was specially devised to reduce such extraneous sources of correlation to a minimum, being assimilated as far as possible to the ordinary class-work of the school.

A definite solution of the question of the existence or non-existence of one central mental ability is yet to be sought. It can only be obtained by the use of much larger random samples than those hitherto employed, since the probable errors must be small compared with the coefficients if precise inferences are to be drawn from the latter, and in the case of small samples this condition is satisfied only for *large* correlation coefficients, which when obtained are often merely the result of selecting tests which measure closely similar mental abilities. In all results hitherto quoted in support of ultimate identity of general intelligence and general sensory discrimination the correlations contributed by the latter are so small compared with their P.E.'s that nothing definite can be inferred from them. On the other hand, in such cases it is easy to propound hypotheses, since the bounds of possibility are nowhere limited in any unambiguous way.

As results in fairly definite contradiction of the hypothesis of one single 'central factor,' I quote the following coefficients from the tables:

Group I.

e r: correlation with addition (sp.) = ·59 ± ·05,
" " motor (all letters) = ·53 ± ·06,
both occur *later* in the table than that with mech. memory (·40 ± ·07) and memory for poetry (·27 ± ·08).

a n o s: correlation with addition (sp.) = ·51 ± ·06; *later* than mech. memory, ·29 ± ·08.

mech. memory: correlation with combination = ·52 ± ·06; *later* than a n o s, ·29 ± ·08.

Group II.

School marks: correlates with Gen. Intell. ·64 ± ·06; *later* than a n o s, ·27 ± ·10.

a n o s: correlates with e r ·80 ± ·04; *later* than Gen. Intell. ·13 ± ·11, and mech. memory, ·20 ± ·10.

e r: correlates with a n o s ·80 ± ·04; *later* than combination, − ·15 ± ·11.

motor (all letters): correlates with e r ·49 ± ·08; *later* than combination, etc. = 0.

addition (sp.): correlates with Drawing − ·40 ± ·09; *later* than seven coefficients, all < ·14.

addition (acc.): correlates with motor test, ·30 ± ·10; *later* than five zero coefficients.

GROUP III.

General Intelligence: correlates with V.-H. Ill. $-\cdot30\pm\cdot10$; *later* than M.-L. Ill. $=0$, a n o s $=\cdot10\pm\cdot11$.

Combination: correlates with addition (sp.) $\cdot32\pm\cdot10$; *later* than e r $=0$.

Drawing: correlates with mech. memory $\cdot39\pm\cdot09$; *later* than M.V. of M.-L. Ill. $=0$, e r $=\cdot11\pm\cdot11$.

a n o s: correlates with M.-L. Ill. $-\cdot35\pm\cdot10$; *later* than mech. memory $=0$.

,, ,, ,, e r $\cdot74\pm\cdot05$; *later* than combination $=\cdot10\pm\cdot11$, etc.

GROUP V (*a*).

Addition (sp.): correlates with add. (acc.) $\cdot38\pm\cdot09$ and assoc. time $\cdot37\pm\cdot09$; both *later* than e r $=0$.

e r: correlates with M.-L. Ill. $\cdot42\pm\cdot09$; *later* than addition (sp.) $=0$.

There are also many other anomalies, though perhaps not so striking, in the tables.

Certain sub-groups can be chosen from the tables so as to show a hierarchical arrangement, e.g. *Group III.* School Marks, General Intelligence, Mechanical Memory, and Combination. In fact the general law,—so far as the results allow of the confident formulation of any law at all,—would seem to be that the tests fall into a number of such sub-groups, correlating highly among themselves, but not at all highly with members of other sub-groups, though an individual member of one sub-group may, exceptionally, correlate highly with an individual of another sub-group. In order to bring out these relations more clearly and also to show the relations of the main groups with one another, the table on the next page (p. 316) was drawn up. The results there to some extent explain themselves. Differences in correlation between the two sexes, though well marked, do not seem to follow any general law. On the whole the correlations are lower in the girls than in the boys, higher in the women than in the men. A striking feature is the fairly large number of instances of *negative* correlation in the girls corresponding to positive correlations in the boys.

Comparing relative *order* of tests in the tables for Groups I, II and III, I get:

I	*Rank*	*II*	*Rank*	*III*	*Rank*
e r	1	Combination	3	Combination	3
a n o s	2	Mech. memory	4	e r	1
Combination	3	a n o s	2	Add. (sp.)	5
Mech. memory	4	e r	1	a n o s	2
Addition (sp.)	5	Motor	7	Mech. memory	4
,, (acc.)	6	Add. (sp.)	5	V.-H. Ill.	8
Motor	7	V.-H. Ill.	8	Motor	7
V.-H. Ill.	8	Add. (acc.)	6	Add. (acc.)	6

The rank-correlations here are $\rho_{\mathrm{I,II}}=\cdot66$, $\rho_{\mathrm{I,III}}=\cdot74$, $\rho_{\mathrm{II,III}}=\cdot55$[1]. As

[1] I have compared the relative order of tests in the two groups of subjects (30 elementary school boys, and 13 high grade preparatory school boys, both groups between the

Coefficients < 3 P.E. put in square brackets.

Tests	Group I Boys (elementary)	Group II Girls (elementary)	Group III Boys (higher grade)	Group IV Women students	Group V (*a*) Men students	Group V (*b*) Women students
Combination test and						
School marks	—	·54 ± ·08	·60 ± ·07	—	—	—
General intelligence ...	—	·43 ± ·09	·69 ± ·06	—	—	—
Drawing	—	[·22 ± ·10]	·46 ± ·09	—	—	—
Mech. memory	·52 ± ·06	·37 ± ·09	[·28 ± ·10]	·31 ± ·08	—	—
Memory for poetry......	·52 ± ·06	—	·44 ± ·09	—	—	—
Letters a n o s	·48 ± ·07	[0]	[·10 ± ·11]	—	—	—
,, e r	·45 ± ·07	[– ·15 ± ·11]	[0]	—	[·19 ± ·11]	·52 ± ·10
Addition (speed).........	·40 ± ·07	[– ·13 ± ·11]	·32 ± ·10	·34 ± ·08	[·19 ± ·11]	[·13 ± ·14]
,, (acc.)	·38 ± ·07	[– ·25 ± ·10]	[0]	·53 ± ·06	[– ·16 ± ·11]	[·28 ± ·13]
Association time.........	—	—	—	—	·33 ± ·10	·58 ± ·09
Mechanical Memory and						
School marks	—	·59 ± ·07	·40 ± ·09	—	—	—
General intelligence ...	—	·55 ± ·08	·49 ± ·08	—	—	—
Memory for poetry......	·49 ± ·07	—	·38 ± ·09	—	—	—
Letters a n o s	·29 ± ·08	[·20 ± ·10]	[0]	—	—	—
,, e r	·40 ± ·07	[0]	[0]	—	—	—
Addition (speed).........	·27 ± ·08	[– ·13 ± ·11]	[0]	[·18 ± ·09	—	—
,, (acc.)	·31 ± ·08	[– ·23 ± ·10]	[0]	[·20 ± ·09]	—	—
Letters a n o s and						
Letters e r	·78 ± ·03	·80 ± ·04	·74 ± ·05	—	(·57)	(·56)
Addition (speed).........	·51 ± ·06	[0]	[·20 ± ·10]	—	—	—
,, (acc.)	·24 ± ·08	[0]	[– ·11 ± ·11]	—	—	—
Letters e r and						
Addition (speed).........	·59 ± ·05	[·13 ± ·11]	·35 ± ·10	—	[0]	[0]
,, (acc.)	·30 ± ·08	[0]	[0]	—	[– ·26 ± ·10]	[0]
Addition (speed) and						
Addition (acc.)	[·13 ± ·08]	[·24 ± ·10]	·33 ± ·10	·43 ± ·07	·38 ± ·09	·35 ± ·12
Motor (all letters) and						
Letters e r	·53 ± ·06	·49 ± ·08	[·25 ± ·10]	—	—	—
[,, a n o s............	[·21 ± ·08]	[·21 ± ·10]	[0]	—	—	—]
Addition (speed).........	·25 ± ·08	·33 ± ·10	[·20 ± ·10]	—	—	—
,, (acc.)	[0]	·30 ± ·10	[0]	—	—	—
Müller-Lyer Illusion and						
Drawing	—	– ·44 ± ·09	[– ·19 ± ·10]	—	—	—
V.-H. Ill..................	—	[– ·21 ± ·10]	[·29 ± ·10]	—	—	—
M.V. of M.-L. Ill.	—	[– ·21 ± ·10]	[0]	(– ·57 ± ·14)	—	—
M.V. of V.-H. Ill	—	—	·32 ± ·10	—	—	—
Letters e r	—	[– ·20 ± ·10]	– ·31 ± ·10	—	·42 ± ·09	– ·42 ± ·11
,, a n o s............	—	[– ·21 ± ·10]	– ·35 ± ·10	—	—	—
Addition (speed).........	—	[·12 ± ·11]	– ·32 ± ·10	—	[0]	– ·66 ± ·08
Drawing and						
V.-H. Ill..................	—	[·27 ± ·10]	[0]	—	—	—
Addition (speed).........	—	– ·40 ± ·09	[·14 ± ·10]	—	—	—
Vert.-Hor. Ill. and						
M.V. of V.-H. Ill.	[·21 ± ·08]	—	·33 ± ·10	—	—	—
School marks	—	[– ·15 ± ·11]	– ·30 ± ·10	—	—	—
General intelligence ...	—	[0]	– ·30 ± ·10	—	—	—
Combination	[0]	[·22 ± ·10]	[– ·11 ± ·11]	—	—	—
M.V. of Vert.-Hor. Ill. and						
Letters a n o s............	– ·31 ± ·08	—	[– ·16 ± ·10]	—	—	—
M.V. of M.-L. Ill. and						
Memory for poetry......	—	—	·52 ± ·08	—	—	—
Mech. memory	—	[– ·16 ± ·11]	[·29 ± ·10]	[– ·21 ± ·20]	—	—
Drawing and						
School marks	—	[0]	·51 ± ·08	—	—	—
General intelligence ...	—	[0]	·42 ± ·09	—	—	—
Memory for poetry......	—	—	·44 ± ·09	—	—	—
Mech. memory	—	[0]	·39 ± ·09	—	—	—

might be expected, the two groups of boys correspond more closely with one another than either does with the group of girls.

The method of multiple or 'partial' correlation[1] may be very advantageously employed to investigate the way in which the correlation coefficients are related to one another. Thus, taking the *three* variables e r, a n o s, and motor, and using the formula

$$r_{12.3} = \frac{r_{12} - r_{13} r_{23}}{\sqrt{1 - r_{13}^2}\sqrt{1 - r_{23}^2}},$$

I get the following values for the 'partial' correlations in the first three Groups:

	Group I	Group II	Group III
e r a n o s =	·80 ± ·03	·82 ± ·04	·76 ± ·04
e r motor =	·59 ± ·05	·55 ± ·08	·37 ± ·09
a n o s motor =	− ·38 ± ·07	− ·35 ± ·09	− ·28 ± ·10

Thus the original positive correlation between a n o s and motor is due entirely to the correlation of each with e r. For 'e r constant' the correlation is large but *negative*, in all three cases. The relation here brought out is one very different from that of a central factor.

Employing the *four* variables School Marks, General Intelligence, Combination and Mechanical Memory (Group II), and using the formula

$$r_{12.34} = \frac{r_{12}(1 - r_{34}^2) - r_{13}(r_{23} - r_{24} r_{34}) - r_{14}(r_{24} - r_{23} r_{34})}{\sqrt{1 - r_{13}^2 - r_{14}^2 - r_{34}^2 + 2 r_{13} r_{14} r_{34}}\sqrt{1 - r_{23}^2 - r_{34}^2 - r_{24}^2 + 2 r_{23} r_{34} r_{24}}},$$

I find the correlation between Combination and General Intelligence, assuming constant ability in Mechanical Memory and as shown by School Marks,

$$= 0{\cdot}11 \pm 0{\cdot}11.$$

The 'entire' coefficients $= 0{\cdot}43 \pm {\cdot}09$. It is interesting to note that there is still correlation, though very slight, after the effect of memory and school industry and ability is eliminated.

A more thoroughgoing application of the method of partial correlation

same age limits) in Mr Cyril Burt's research, published in the last number of this *Journal*, and find that $\rho = {\cdot}56$. Perhaps the small size of this coefficient might be regarded as being slightly adverse to any view which would make the hierarchical orders, which Mr Burt obtains, evidence of any fundamental and, if I may so express myself, *essential* law of the inter-relation of coefficients between mental abilities.

[1] See G. Udny Yule, "On the Theory of Correlation for any Number of Variables, treated by a New System of Notation," *Proc. Roy. Soc.* Vol. 79 A, pp. 182–193, 1907.

was made in the case of Group I for the tests e r, a n o s, Combination, and Mechanical Memory. The results were:

Tests correlated	'Total' coefficient	Partial coefficient
e r a n o s	0·78 ± ·03	0·73 ± ·04
,, combination	0·45 ± ·07	0·01 ± ·09
,, mech. memory	0·40 ± ·07	0·26 ± ·08
a n o s combination	0·48 ± ·07	0·27 ± ·08
,, mech. memory	0·29 ± ·08	−0·15 ± ·08
Combination mech. memory	0·52 ± ·06	0·44 ± ·07

The *regression equation* for the calculation of ability in combination from abilities in the other three tests is

$$x_c = \cdot 002 x_{\text{er}} + \cdot 099 x_{\text{anos}} + \cdot 703 x_{\text{mech. memory}},$$

x in each case denoting deviation from mean ability.

For a larger number of variables than four the arithmetic of partial correlation becomes extremely lengthy and rather fatiguing, but there can be no doubt whatever that this is the one sound method to adopt in investigating the relations between coefficients. Working with a large number of variables is only satisfactory when the original coefficients are *large* compared with their P.E.'s, since *as a rule* (though not universally) the partial coefficient is smaller than the total coefficient. The *formula* for the P.E. is the same in both cases.

Tests were made of the applicability of Spearman's correction formula, with results which precluded the use of the formula[1]. The question of correlations of errors of measurement with the true values of the variates and with one another has been made the subject of a separate piece of research by the present writer. The results of this investigation will be published shortly. For the present the following brief discussion must suffice.

Dr Spearman deserves the credit of being the first to draw attention to the need of a formula for the 'elimination of observational errors.' Obviously, errors of observation must make any correlation, worked from measurements containing them, different from (generally, though not universally, *less* than) the true value of the correlation. The formula, in its full form, which Spearman has proposed as a means of correcting for this, and of which he has given a proof in the *Am. J. P.*[2], is as follows:

$$r_{XY} = \frac{\sqrt[4]{r_{X_1Y_1} r_{X_1Y_2} r_{X_2Y_1} r_{X_2Y_2}}}{\sqrt{r_{X_1X_2} r_{Y_1Y_2}}},$$

[1] See the note at the end of my article on "An Objective Study of Mathematical Intelligence," *Biometrika*, Vol. VII. Part 3, April, 1910.

[2] C. Spearman, "Demonstration of Formulae for True Measurement of Correlation," *American Journal of Psychology*, Vol. XVII., 1906.

where X, Y are the *true* values to be correlated, and X_1, X_2, Y_1, Y_2 are two pairs of *obtained* values.

$r_{X_1X_2}$, and similar coefficients, are called by him 'reliability coefficients.' They represent the correlation of two distinct series of measurements of the same mental capacity.

The proof of Spearman's formula is only valid on the assumption that the errors of measurement are uncorrelated with each other or with X or Y[1]. Thus, if x, y, δ, ϵ represent deviations from means,

and $$x_1 = x + \delta_1 \qquad x_2 = x + \delta_2$$

$$y_1 = y + \epsilon_1 \qquad y_2 = y + \epsilon_2,$$

then $$S(x\delta) \text{ etc.} = 0, \qquad S(\delta_1\delta_2) \text{ etc.} = 0;$$

also $$S(x_1y_1) = S(xy) = S(x_2y_2).$$

[All this is involved in Mr Yule's proof.]

Now, these are very large assumptions to make. Even in cases where the quantities δ, ϵ are genuine errors of measurement, there are strong reasons for assuming (on general principles and also from experimental evidence[2]) that they *will* be correlated. But in the case of almost all the simpler mental tests the quantities δ and ϵ are not errors of measurement at all. They are the deviations of the particular performances from the hypothetical average performances of the several individuals under consideration. Thus they represent the *variability* of performance of function *within* the individual. When an individual in the course of three minutes, succeeds in striking through 100 *e*'s and *r*'s in a page of print on one day, and 94 under the same conditions a fortnight later, there is no error of observation involved. The numbers 100 and 94 are the actual true measures of ability on the two occasions. The average or mean ability, which is the more interesting measure for the purposes of correlation, is doubtless different from either, but that does not make the other two measures erroneous. Evidently in these cases δ and ϵ represent *individual variability*, and to assume them uncorrelated with one another or with the mean values of the functions is to indulge in somewhat *a priori* reasoning.

There are two comparatively simple ways of testing the assumption.

[1] See Mr G. Udny Yule's short proof of the formula, quoted in my pamphlet on "Some Experimental Results in Correlation," *Comptes Rendus du VI^me Congrès International de Psychologie*, Genève, Aout, 1909.

[2] See Karl Pearson, "On the Mathematical Theory of Errors of Judgment, with special reference to the Personal Equation," *Phil. Trans.* A, Vol. 198, pp. 235–299.

(1) $$S(x_1y_1) = S(xy) = S(x_2y_2)$$

therefore $S(x_1y_1) - S(x_2y_2)$ should $= 0$ within the limits of the probable error of the difference.

I have applied this test to the case of correlation between accuracy in bisecting lines and accuracy in trisecting them in 43 adult subjects (a mixture of Groups V (a) and V (b)).

Here $$S(b_1t_1) - S(b_2t_2) = 137780 - 60036$$
$$= 77744$$

$$\text{P.E. of } S(xy) = {\cdot}67449\sqrt{\frac{p_{22} - p_{20}p_{02}}{n}}, \text{ in Pearson's notation,}$$

$$= \frac{{\cdot}67449}{\sqrt{n}}\sqrt{\frac{S(xy)^2}{n} - \frac{S(x^2)S(y^2)}{n^2}}$$

$$\text{P.E. of } S(b_1t_1) = 687, \qquad \text{P.E. of } S(b_2t_2) = 365$$

therefore $$\text{P.E. of } S(b_1t_1) - S(b_2t_2) = \sqrt{687^2 + 365^2}$$
$$= 778.$$

Since 778 is less than one-third of 77744, the formula cannot be employed to obtain the correlation between mean abilities in bisecting and trisecting lines.

(2) $$r_{\substack{X_1-X_2\\Y_1-Y_2}} = \frac{S\{(x_1 - x_2)(y_1 - y_2)\}}{\sqrt{S(x_1 - x_2)^2 . S(y_1 - y_2)^2}}$$
$$= \frac{S\{(\delta_1 - \delta_2)(\epsilon_1 - \epsilon_2)\}}{\sqrt{S(\delta_1 - \delta_2)^2 . S(\epsilon_1 - \epsilon_2)^2}}$$

$= 0$ if errors are uncorrelated with one another (since numerator then $= 0$).

Applying this test to the same case of bisection and trisection, I get

$$r_{\substack{B_1-B_2\\T_1-T_2}} = 0{\cdot}30 \pm 0{\cdot}09$$

which proves once more the inapplicability of the formula. I applied test (2) also to the case of correlation between speed of addition of figures and accuracy of addition in a group of 38 school children (girls between the ages of 11 and 12, Group II) and found

$$r_{\substack{S_1-S_2\\A_1-A_2}} = 0{\cdot}35 \pm 0{\cdot}09.$$

Even when test (2) does give the value 0, we can only conclude from this that

$$S(\delta_1\epsilon_1) + S(\delta_2\epsilon_2) = S(\delta_1\epsilon_2) + S(\delta_2\epsilon_1);$$

we cannot conclude that the formula is applicable, unless we have further independent evidence.

For the reasons presented above, I should prefer to avoid the use of Spearman's formula—by increasing the number of the original measurements of each ability[1]—and would also suggest that his so-called 'reliability' coefficients might in *most* cases be more appropriately termed 'coefficients of *individual* correlation,' since they are more analogous to Karl Pearson's 'correlation of undifferentiated like parts' than to anything else[2].

The above discussion raises the interesting question as to the relation between ability and variability, and the correlation coefficient between mean ability and the standard deviation would be the best measure of this relation. When the measurements have been made on only two separate occasions, the expression $r_{\substack{x_1+x_2 \\ x_1 \sim x_2}}$ might be regarded as a rough measure of the relation, and I would suggest that it be called a 'variability coefficient' $\left(\text{not to be confused with the 'coefficient of variation,' which } = \frac{100\sigma}{\text{mean}}\right)$. If x_1 and x_2 are chance values, and if the distribution of abilities at the given task within one and the same individual is approximately normal, then $x_1 \sim x_2 = \frac{26}{23}\sigma$ (approximately)[3], so that there is sufficient justification for this value.

Variability coefficients were obtained for Speed of Addition and Accuracy of Addition in Group IV, and were found to be

$$0{\cdot}33 \pm {\cdot}08 \text{ and } -0{\cdot}66 \pm {\cdot}05, \text{ respectively.}$$

A full analysis of the entire data in this and other additional ways must be reserved for a later paper, since it would unduly swell the volume of the present account.

In the case of the Vertical-Horizontal Illusion Test, it is perhaps of interest to note that if subjects showing a *negative* value of the

[1] This plan would also have the advantage of keeping the probable error low. Correction by Spearman's formula while 'raising' the value of the coefficient raises the size of the P.E. in the same proportion.

[2] *Grammar of Science*, 2nd edit., pp. 393, 397.

[3] Karl Pearson, "Francis Galton's Difference Problem," *Biometrika*, Vol. I. p. 399.

illusion are excluded the value of the correlation between this Illusion Test and the Combination Test becomes positive and appreciable—in Group IV $r_{\text{V.-H. Ill.}}$ combination $= 0{\cdot}24 \pm 0{\cdot}09$ (*one* test only of V.-H. Ill.) in Groups I + III, it $= 0{\cdot}26 \pm 0{\cdot}08$ (this latter value probably includes some 'spurious' correlation: see table, p. 309).

If negative values are included, we have the results:

$r_{\text{V.-H. Ill.}}$ combination: Group I, 0; Group II, $0{\cdot}22 \pm {\cdot}10$; Group III, $-{\cdot}11 \pm {\cdot}11$.

$r_{\text{V.-H. Ill.}}$ Gen. Intelligence: Group I, 0; Group II, 0; Group III, $-{\cdot}30 \pm {\cdot}10$.

In conclusion, it may be stated that the results of the present research, so far as they have yet been worked out, are in some conflict with Spearman's theory[1] and to some extent confirm Thorndike's views upon the nature of psychical correlation.

[1] Dr Spearman, who has read the above article in proof, kindly sends me the following criticism, which seems to be so important that I quote it verbatim: "The only comment I would make is that (like Thorndike) you have not noticed that the hierarchy was only meant by me to be applied to performances of considerable dissimilarity. This interpretation is, I must admit, quite excusable, owing to a verbal slip, *Am. J. Psych.* Vol. xv. p. 273, where 'at all dissimilar' should have been 'sufficiently dissimilar'; for some unexplained reason, the American papers were published without ever sending me any proofs. But other passages, for instance, *Zeitschr. f. Psych.* Vol. xliv. pp. 102, 103, put my real meaning beyond all doubt.

"Really to test my theory upon your results, it would be necessary to 'amalgamate' all your tests obviously related to one another, as the three tests of erasing letters, the two of memorizing, the two of addition, and the two of illusion. Or it would do equally well to omit all related tests except one; for instance, to omit two out of the three erasing tests, one out of the two memories, etc. It would further be necessary to see whether any still remaining discrepancies from the hierarchy were large as compared with the p.e.'s involved. Then, and then only, would it be possible to see whether your results really show any conflict with mine."

I hope to consider this objection fully in a future publication.

SOME PROBLEMS OF SENSORY INTEGRATION[1].

By HENRY J. WATT.

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A. Demonstrations of uniocular stereoscopy by means of

(1) *interruptedly disparate contours in alternation,*

(2) *progressively disparate contours in succession.*

B. The independence and probable primacy of uniocular or progressive stereoscopy.—Binocular or static stereoscopy an adaptation to the needs of static vision.

C. Depth-effect is a secondary attribute of visual sensation, derived from the integration of differences in the order aspects of its uniocular components.

D. Theory of stereoscopic vision from interrupted stimulation.—Presumption that the mechanisms of uniocular and binocular stereoscopy are similar.

E. Further theory of A.—The psychical determinateness of the object of regard determines the ambiguous uniocular stimulation.—Experiments with the stroboscope and various ambiguous stimulations.

F. Size as the special determinant in E.—General treatment of the result.—Ambiguous stimuli and their determinants.—Inadequate stimuli have a meaning only in relation to integrative processes.—Central and peripheral determinants.—Presumption that the latter are, for sensory integrations at least, consensuous with the ambiguous stimuli.—Parallelism of physiological and psychological investigation.

THE presence of a multiplicity of sense-organs of the same and of different kinds presents a number of interesting problems to the psychologist and to the physiologist.

An intimacy of connexion between *nerve-paths* or impulses emanating from different sense-organs is, of course, recognized in many forms.

[1] The following paper was read in a somewhat modified form before the British Psychological Society in London, on March 12th, 1910.

But this connexion has been somewhat exclusively considered to consist in a mere *coordination* or association of afferent or efferent impulses with one another. Sufficient attention has hardly been paid to the possibility that upon these afferent impulses an afferent structure might be raised which is dependent upon but essentially an addition to these. To distinguish it from mere coordination, such a structure might well be called *integration*.

The *psychical* elaboration of sensations is also generally recognized by psychologists. But here again there has been a strong tendency to emphasize the process of coordination of sensations with one another and to ignore the presence of any features of the elaborated state which were not original to the primary sensation from a single sense-organ. Any apparently new product of elaboration has been generally attributed to the action of a central unifying power, the mind. Reasons for this are not far to seek. It is the aim of science to free the individual from the primitive nomadic view of the world and to create a knowledge of things as they are independent of all particularities of cosmical position and psychophysical distortion. The nearest psychical correlate of a real thing is the sensation from a single sense-organ. If, then, the sensory effect of coordination of sense-organs is markedly different from such simple sensation, it is not surprising that the activity of the mind should be made responsible for the change; the more so, since the first attempts to establish a mental chemistry or the features of mental elaboration intended in that notion failed to find sufficient factual or speculative support to establish them truly. This is commonly expressed by saying that a particular effect, not obtained from a single sense-organ, is due to judgment, or is perceptive in character. Much ingenuity has often been required to overcome this conclusion in particular cases, for example in that of Helmholtz's theory of colour-contrast. When the change of view has been obtained, however, proof to the affirmative or contrary is found to be almost superfluous. The mind is then released from the power of its judgments and the observation takes on the character of self-evidence, because it becomes a matter of pure introspection. In fact, to call for an introspective observation from a mind that is carefully prevented from obtaining a knowledge of the real character of the contrasting surface in the example given, is the best means of refuting the judgment theory. The incognitive procedure now demanded in psychology is merely a generalisation of this.

But the thought of the bare realities of analysis and induction is still strong enough in many psychologists to blind the eye to the actualities of experience. A sensationalistic theory of experience is well able to hide many incompatible forms of experience completely from view or thoroughly distort their actual character. The fatigue of inventing forms of judgment and their more obvious futility may even lead to the statement that the muscular response to simultaneous stimulation of individual sense-organs is responsible for the apparent integration of experience resulting therefrom.

It will therefore be the aim of the following pages to emphasize the purely sensory character of some forms of integration involving a multiplicity of sense-organs.

Towards this end, however, a renewed examination of these forms must be the first contribution.

A. Complete and stable depth-vision can be obtained uniocularly in a variety of ways. These may be classed in two groups. In the first the single eye is stimulated in alternation by such disparate contours as suffice to produce ordinary stereoscopic vision binocularly. In the second, the eye is stimulated by contours which change by gradual and progressive disparity, whether this be produced by the continuous displacement of the eye over against the field of visual objects or by suitable displacement of the field of objects relatively to the eye. It seems better to begin with the first because of its greater resemblance to stereoscopic vision in its familiar binocular form. It will, however, become apparent that the second method represents the normal form of uniocular depth-vision. It may even be that this is the primitive form of stereoscopic vision in general.

1. Uniocular depth-vision can be easily obtained and demonstrated, if the right- and left-eye pictures of a stereoscopic view representing a good landscape or other group of natural objects are projected in alternating succession so that their far points and their coordinates exactly coincide. This can be done by using two simple projecting lanterns set side by side or above one another. Where Ives' apparatus for demonstrating printing by the three-colour process is available, good results can be got very easily, by taking out the front plain glass reflector and the coloured filters, so that the middle and right hand fields of projection are alone in use and colourless. The stereoscopic diapositives (¼ plate) are then inserted in the apparatus in the place in which the Ives special triple plates are usually inserted. Then the two views will be projected upon the same field of the screen. Fine adjustments are present in the Ives apparatus for obtaining complete

overlapping of the far points of the two pictures which should give clear images when both pictures are projected upon the screen simultaneously. This fine adjustment can be best carried out when the rotating disc standing in front of the two paths of light emerging from the apparatus is slipped down into place and set in rotation. The disc should have alternate segments of 45° cut out, leaving a steady carrying centre and a continuous rim. A radius of 14 cm. gives a good size and allows of easy adjustment of the projecting light-pencils on equivalent points of open and close segments. Any circular deviation of the two diapositives from common coordinate axes is very hard to deal with, but it can be rendered negligible, if the diapositives are carefully made. Slight errors of adjustment in the horizontal plane also detract from the stereoscopic effect, because they induce an apparent oscillation of the landscape round the virtual fixation-point, i.e. round the objects whose images in the projection exactly overlap. This apparent oscillation is rather disturbing and does not allow the depth-effect to develop clearly. The alternating disc should rotate so that there are some two or three cycles, i.e. successive projections of right- and left-eye pictures, per second, each picture thus lasting about 0·2 sec.

It is obviously immaterial whether the right- or left-eye views stand so in the apparatus or reversed; for the two series *r*, *l*, *r*, *l*, *r*, *l*... and *l*, *r*, *l*, *r*, *l*, *r*... are identical as soon as they have started.

Under these conditions a brilliant stereoscopic effect is obtained with uniocular vision. It is so clear that it could easily be demonstrated to a large audience. In the first few seconds, the effect is not quite so marked as afterwards, when, if the observer be at the proper distance from the projection, it is quite startling. The average time spent in the full development of the uniocular depth-effect might be some 10–14 seconds. But it must not be thought that the depth-effect as such is weak or absent at the beginning; it is brilliant at the very first change of view after the rotation of the interrupting disc is begun. Continuous fixation of far points of the landscape does not reduce the effect, but rather to enhance it, especially if a number of points unequally distant surround the far point fixated in the projection. The unavoidable flickering of the field of projection is rather unpleasant, but it is not so marked when the rate of rotation is as slow as possible. It is surprising how slightly it disturbs the observer, if the adjustments are good and when the depth-effect is fully developed. The rocking of the foreground is of course very curious, but it does not hinder full appreciation of depth-effects.

The depth-effects got in this way were compared with those got from the same views (natural water-cuttings in rock) in the stereoscope in the ordinary way and were found to be quite as good, if not superior to these. The much larger size of the projected view may be responsible for any superiority of the projection. It may be remarked in passing that the well-known stagey appearance of stereoscope pictures can be removed, if the two pictures are set further apart, and are seen under higher divergence than is usual[1]. When the field of projection is viewed with two eyes instead of one, the depth-effect is not nearly so good, although it is undoubtedly present, while the flicker and the oscillation of the foreground are much more pronounced and disturbing. Nor is the depth-effect obtained from the single resting projection in any way to be compared with that obtained uniocularly from alternate projection. The stereoscopic effect is also got uniocularly with inverted head, although the strangeness of the picture does not allow of full scenic interpretation.

It is also possible to get uniocular depth-effects from real as distinct from projected objects. If an observer, looking with one eye upon a natural scene containing marked depth-differences shakes his head from side to side, so that the eye moves from right to left and receives successively at the point of change of motion of the head disparate images similar to those received by the two eyes normally, a very pronounced stereoscopic effect is obtained. An instrument may also easily be devised whereby stereoscopic vision of a natural scene is made possible to uniocular observation. The principle of construction is that of the demonstration described above. A mirror at an angle of 45° to the line of vision reflects its light upon a transparent mirror set in front of the observing eye at an angle of 45°. The latter reflects this light into the eye and at the same time transmits light from a natural scene directly. Stereoscopic effects will be got if transmission and reflection are made to alternate by the rotation of a suitable disc set up in the paths of light entering the silvered mirror and the transparent mirror. I have not made any such instrument, but I have satisfied myself by rough trial that it is possible.

2. In the second of the two main methods, marked depth-effect is obtained when the eye is in motion over against a field of natural objects. The view from a moving train or steamer provides an excellent opportunity for observing this in all its degrees and variations. The depth-effects of the landscape can then be observed to be identical

[1] Of this, more later.

in all essentials, with those presented to binocular vision. Even a single line, group or pair of light-points on a dark background, such as is often seen from the train on approaching a town from a slight elevation, is present in perfect depth to the eye. Fixation of relatively distant points does not lead to any decrease or deterioration of depth-effect, but rather enhances it. I have seen these effects beautifully reproduced by the cinematograph. The pictures had evidently been taken from the deck of a vessel sailing on rapids in a well-wooded country. Stereoscopic effects are of course always seen with uniocular vision, when views of persons or animals in motion are projected. Quite a number of interesting demonstrations on vision are given in any cinematographic exhibition. These are also obtained in a much rougher form with the help of the stroboscope, as described by Straub and Brown (cp. below, p. 340). They, indeed, lay stress upon the degree of stereoscopic effect obtained, although the novelty of it and probably various theoretical views, incline them to urge that it is illusive or at least of another mental order than is binocular stereoscopy. But it must be clear that the psychical identity of uniocular and binocular stereoscopy, while it can be emphasized by theory or quantitative treatment of judgments, cannot be refuted by these. It must, anyhow, first be established by observation for itself. A report of experiments in the uniocular observation of moving objects will follow.

B. Uniocular depth-vision must be quite a normal process and must be habitual with all those animals whose usual state of activity involves more or less rapid motion and whose eyes project laterally. Overlapping of the fields of vision is either entirely absent or it is very limited in extent amongst these animals, while the increase allowed by convergence of the eyes is often very small and seldom employed, if indeed it be present at all.

Tschermak[1] is inclined to believe that some slight amount of binocular vision, however limited, is possible through overlapping of fields of vision in all the vertebrates. Harris, on the other hand, maintains that "in graminivorous and fruit-eating birds, as the parrots, pigeons, fowls, ducks, swans, many finches and others, the eyes are set laterally on the head, no attempt at binocular vision being possible[2]."

[1] A. Tschermak, "Studien über das Binokularsehen der Wirbelthiere," *Pflüger's Archiv*, xci. p. 13.

[2] Wilfred Harris, "Binocular and stereoscopic vision in man and other vertebrates, etc." *Brain*, xxvii. p. 115.

I have, however, frequently observed momentary convergence of the eyes of no mean extent upon a piece of food held in the claw-fist in a grey-rose cockatoo. In attaining convergence the bird seemed each time to make a sudden effort. Similar movements, perhaps much smaller, have been observed by A. Tschermak[1] in fishes and by Th. Beer in birds[1].

Birds are undoubtedly well accustomed to uniocular observation, for they adopt it regularly during near vision in a state of rest. Everyone is familiar with the peculiar position of a bird's head when it is looking from a cage downwards at some object. There can therefore be no doubt but many birds and other animals in rapid motion can have and observe depth-effects on either side of the head at will. It would also follow that their whole field of uniocular vision can be filled with depth-effects, no matter what the direction of parallactic displacement may be. Whether in these animals there is not also some further integration of vision involving the impressions or the depth-effects of both eyes at once is another matter, but they evidently do not need both eyes at once for the appreciation of relative distance as such. A suggestion of such further integration of vision is found if we ask why a bird on the wing should not be able to appreciate the relative distances of entirely different objects on each side of the head, on the basis of the different rates at which the depth-effects of the two visual fields would change, if the objects in each were at different distances from the head. The coordination or integration of relative speed of displacement and relative depth-effect would be possible in entire independence of any relative or apparent size of the objects of vision. Such correlation of visual fields should be a comparatively simple matter. It should, for example, be simpler than the coordination of tactual direction with visual direction or than the commonly postulated coordination or integration of movement of eye or hand with visual order. Unless some such integration as this is present, it is hard to see how a bird can fly securely between two trees or other obstacles.

One might indeed well go so far as to maintain that stereoscopic vision, far from being dependent upon coordinated use of two eyes for its first occurrence, is primarily uniocular[2]. That it is only obtained

[1] *loc. cit.* p. 18.

[2] Cf. A. Kirschmann, "Die Parallaxe des indirecten Sehens," *Philos. Studien*, IX. p. 492. "Die Parallaxe des indirecten Sehens, d.h. die Incongruenz zwischen Gesichts- und Drehungswinkel des Auges, ist von erheblicher Grösse und bewirkt bei Accomodationsänderungen und Bewegungen des Auges (bezw. der Objecte) Veränderungen in den relativen

uniocularly when the eye or the objects of regard are in motion, is no serious objection to this view, because it is well known that very many animals notice well defined near objects when these are motionless, important or dangerous though they be, just as little as we notice motionless objects situated in the periphery of our uniocular field of vision. Binocular stereoscopic vision would then be an adaptation of vision to the demands of a life of comparative or frequent bodily repose. The life of a beast of prey would also seem to bring many moments which demand long-continued observation of objects towards which it must turn its head with little or no progressive or regressive motion. Herbivorous animals and birds on the other hand, however swiftly they may move, seldom need to observe near objects in front of the head for any length of time, and are more dependent upon a wide field of vision for secure motion and feeding. Simultaneous use of both fields of vision would therefore seem to be necessary if motionless depth is to be obtained. Binocular depth-vision might, accordingly, be called static in distinction to uniocular or progressive depth-vision.

Comparative study of the vision of animals lead Harris to recognize the influence of the carnivorous habit upon the position of the eyes. "Binocular vision is originally associated with carnivorous habits, and is found to a moderate degree amongst carnivorous fishes in a few of the sharks and rays, in some amphibia, as the toad, which lives on flies and insects and in many carnivorous birds, especially the larger gulls some penguins, hawks, owls and vultures. Amongst mammals binocular vision is especially developed in the carnivora and in the primates[1]." Many animals besides the chameleon[2], may make a momentary convergent movement of the eyes at the moment of striking their prey, especially if their mode of pursuit happens to be that of rapid flight or chase, without long fixation or combat. "Though many of these animals have fair binocular vision, yet in all vertebrates below mammals there is total decussation of the optic nerves at the chiasma[3]." It is obvious then, that if the presence of total decussation is no barrier to the occurrence of static stereoscopic vision in the chameleon and owl, it can also be no argument against the presence of progressive stereoscopic vision in the birds and in all animals with fixed or laterally projecting

Lageverhältnissen der Netzhautprojectionen. (Diese) stehen in eindeutiger und ganz gesetzmässiger Beziehung zur Tiefendimension und werden wahrscheinlich vom Gesichtssinne als Hilfsmittel zur Gewinnung einer monocularen Tiefenwahrnehmung verwandt." Cf. also the same author, *Philos. Studien*, XI. 1895, p. 188.

[1] *loc. cit.* p. 108.

[2] *loc. cit.* pp. 113, 114.

[3] *loc. cit.* p. 108.

eyes. Decrease in decussation would seem to involve very radical neural rearrangements, so that the easier means of obtaining static stereoscopic vision by connexions between the hemispheres is adopted when a sudden need for it arises.

I have observed that stimulation of a portion of one retina to the right or left of the macula by alternating disparate contours suffices to produce depth-effects. Depth-effect can therefore be produced by one-sided uniocular stimulation.

C. The stereoscopic effect of the double Ives' projection is still quite as clear to an eye paralysed with atropine, although the picture is not so sharply defined. It has also been noted that uniocular stereoscopy is present in full degree when a far point is fixated if only there is sufficient alternately interrupted or continuously progressive disparation of contour. It is therefore clear at least that uniocular depth-vision is not dependent on convergence, eye-movement, or accommodation or on any judgments based upon the presence of these.

Besides, no judgment or inference can be observed to intervene between the vision of the oscillating field of projection and the subsequent depth-effects. I have demonstrated uniocular depth-vision to a number of different observers and none of these gave utterance to anything which would show that inference or judgment was responsible for the depth-effects they observed. The usual exclamation of the unprepared observer was "Oh! it seems quite solid" or the like. In any case, argument seems quite irrelevant, because it needs no knowledge of psychology or any experimental inference to observe that depth-effects are presented directly to uniocular vision just as much as colour- or breadth-effects are. This is surely a matter of introspective comparison, not of the quantitative analysis of judgments. Nor does depth-vision involve any sort of judgment or consciousness of meaning, signifying that point A is nearer than point B, as its classification under Perception would suggest.

Depth-vision does not occur without a complex of sensational data. In what relation, then, does it stand to these? Primitive sensation may be defined as the simplest change in experience which is immediately and regularly dependent upon the stimulation of a sense-organ. Depth-vision, however, is not a simple, but a rather complex change in experience, involving more than one sensation as defined and more than one sense-organ in the strict sense, according to which the eye consists of a vast number of juxtaposed visual sense-organs. If it is not a perception or a process of judgment, it is certainly sensational in character, in so far as it is regularly and immediately dependent upon the stimulation of sense-organs. Yet depth-effect clearly cannot itself be thought to be even an elaborate sensation. For where are its attributes or aspects—its quality, its intensity, its extent or its position? Amount of depth could of course be called the extensity or the intensity of depth-effect, according to inclination; and the localisation of depth-effect might be found to be particularised, if imagination invented other forms of depth-effect peculiar to ultra-geometrical worlds.

Besides, depth-effect can obviously not itself be a sensation, because it cannot occur alone. It must be carried by more than one colour-sensation. Its nearest relative is the plane local character of every visual sensation, of which it forms a kind of continuation, and from the diversities of which it is evolved. Is depth-effect itself perhaps an attribute of visual sensation? It is certainly attributive in its general character, but it shares only one—the less strict—of the two peculiarities of the usual features of sensation, viz. independent variability. It might therefore well be called an occasional, additional, secondary or derived attribute of sensation[1]. Its separability would, however, not be demonstrated by the effect of flatness evoked by exactly similar stimulation of both retinas. That is, of course, itself a case of depth-effect, for the effect of flatness is much more compelling when both retinas are stimulated than when only one is affected. If we argue by analogy from the fact that cutaneous space is practically devoid of any clear element of solidity, it might be maintained that primitive normal and resting uniocular vision gives no direct or sensational sense of flatness. Besides, the loss of depth-effect, which may be observed in the transition from uniocular or binocular stereoscopic vision to motionless uniocular vision, is so enormous, that it may fairly be argued, that any primitive form of the latter is quite devoid of depth-differences. Any semblance of stereoscopic vision in resting uniocular vision may be properly put to the account of indirect or (in respect of stereoscopic vision, based on disparity of impressions) heterogeneous indications of depth. The presence and action of the latter naturally form an important problem for investigation.

D. What theory can be offered to account for the occurrence of

[1] This view seems to be in no way opposed by the fact that a certain amount of depth-effect may be evoked by double images of an object that is too far or too near for proper binocular stereoscopic fusion, v. Tschermak u. Hoefer, *Pflüger's Archiv*, XCVIII. pp. 299—321. Such a fact indicates, however, that the basis of the integration of depth is broader than that of stereoscopic fusion. We, too, found distinct depth-effect even in the case of the oscillating, unfused objects in the foreground of the landscape-projection.

depth-effect under the circumstances of uniocular observation described in section A.

A reason must first be sought for the presence of stereoscopic vision under the conditions of alternation of disparate contours, when each stimulation lasts about 0·2 sec. We have shown that uniocular stereoscopic vision, in so far as it is evoked by gradual disparation of contours, for example when the eye is moved over against a group of objects, is quite a normal occurrence; it is perhaps even the commonest and primary form of stereoscopic vision. It is, however, not quite clear how sudden and considerable disparation of contours in alternation should evoke depth-effect.

Certain observations of Guilloz[1] are of considerable interest in this connection. He found that "the sensation of relief is easily evoked through successive vision of the two eyes, by very slow alternation, without vision being at any moment binocular." A disc was rotated before an ordinary stereoscope or before real objects, so that only alternate uniocular and never binocular vision was possible. The duration of total eclipse was very short however. Under these circumstances perfect stereoscopic vision demanded at the most ten successive stimulations of each eye per second, on an average six per second, and at least two per second. I obtain the full effect myself with some four or five successive stimulations. As M. Guilloz noted, parallactic displacements of the various objects seen become rather pronounced with the slower rate of vibration, as in the case of the Ives projection also. That is as it should be; for if the integrated effect is just at or below its threshold for any given circumstances, the order-aspects of the two integrating sensations should then force themselves separately upon our notice. When the integration is complete, they disappear psychically in the integrated or derivative aspect of depth. Even in normal forms of stereoscopic vision, however, a slight effort of attention will easily discover the order-differences of the component sensations[2].

Guilloz's observations point to a feature common to both binocular and uniocular stereoscopic vision. Both of these are possible in the absence of synchronous disparate stimulations[3] and the rates of alterna-

[1] *Comptes rendus de la soc. d. biologie*, 1904, I. 1053–4. The same observation was made by Lohmann, *Ztsch. für Psych.* XL. p. 191, by alternately closing each eye before the stereoscope.

[2] Cf. W. Lohmann, "Ueber den Wettstreit der Sehfelder und seine Bedeutung, etc." *Ztsch. für Psych.* XL. p. 191.

[3] Cf. Stevenson and Sandford, "A preliminary report of experiments on time-relations in binocular vision," *Amer. Journ. of Psych.* XIX. pp. 129—137, 1908. The matter needs further investigation.

tion of stimulations are in both cases similar. Now Sherrington has shown "that only after the sensations initiated from right and left 'corresponding points' have been elaborated, and have reached a dignity and definiteness well amenable to introspection, does interference between the reactions of the two (right and left) eye-systems occur. The binocular sensation seems combined from right and left uniocular sensations elaborated independently[1]." It has also been shown by Sherrington for flicker in relation to brightness value that the state of the latter is relatively independent of the laws of the former so far as they are valid for identical tracts (uniocular). Guilloz's and our observations show that the continuity of stereoscopic effect is also to some extent independent of the continuity of its integrating stimulations. If the physiological bases of binocular stereoscopy are separate and distinct for each eye, it would seem to follow that in uniocular stereoscopy produced by alternate disparate stimulation two separate and distinct physiological bases, one for each of the disparate stimulations, may be presumed to exist. Further it may be presumed, that if binocular stereoscopy involves a special apparatus for the integration of its separate bases, so also should uniocular stereoscopy. It would therefore seem that binocular and uniocular stereoscopy are in no essential way different from each other. The physiological and psychological devices of both mechanisms are essentially the same. Hereby we feel confirmed in our view that binocular stereoscopy is an adaptation to a life of comparative repose (cf. p. 330) of a mechanism which was primarily developed to suit a life of rapid progressive motion. Binocular vision does not add anything essentially new to the physiological or psychological equipment. It only does in a slightly different manner what uniocular stereoscopy did before it.

There is furthermore a clear parallelism between the integrative process of stereoscopy and that of motion. In so far as vision is impossible during rapid and extensive movements of the eye, which are of the commonest occurrence, the effect of motion must be elaborated out of successive, interruptedly different visual impressions, in which any changes which might be brought about by movement of the eye or of the objects of regard, are reduced to a minimum. The stroboscopic representation of motion is the experimental statement of this fact. But although there is an obvious parallelism between the two integrative processes, their differences are also patent. For motion is just not depth-effect and the one can be present without the

[1] "The integrative action of the nervous system," p. 381.

other. Besides there is no special binocular form of the integration of motion.

It is of interest to make further comparisons between the mechanisms of uniocular and of binocular vision. Binocular vision is accompanied in the mammals by semi-decussation at the chiasma, so that the left halves of each retina are connected with the left hemisphere, and the right halves with the right hemisphere. These double left and right halves must therefore be associated with one another by connexions between the hemispheres. In birds and many other animals, on the other hand, there is total decussation at the chiasma, each nerve passing over completely to the opposite hemisphere. The cerebral connexions between the two fields of vision will, of course, not produce overlapping or rivalry of these fields, but will tack the one on to the other in the way most compatible with the distance between the front edges of the fields of vision or with the overlapping of the fields and with the other peculiarities of the animal's vision. One can imagine an animal with eyes so set that the edges of the two fields of vision should just meet or just overlap all the way round. There would then be no confusion or rivalry of vision but a visual panorama in all directions. Nothing would prevent the animal from being aware of the whole of this steady continuous panorama at one time or of attending to points in it on both sides of itself, i.e. in both fields of vision. It is very hard for us to accustom ourselves to this obvious arrangement. We always feel there should be some rivalry between the two fields of vision and find it curious that the chameleon should direct one eye forwards and the other backwards. That is not more curious than it is that we should hold one arm forward and the other behind and feel with both at once without confusion. We ourselves also enjoy this panorama to some slight extent; for in so far as our two fields of vision do not overlap they extend the total field of vision and we can attend to objects lying in the uniocular left portion of the left field of vision and to others lying in the uniocular right part of the right field of vision simultaneously without confusion or rivalry. Let our two fields be stretched out to the side till they meet behind and till they hardly overlap at all in front and the bird's vision is realised.

Thus it is possible to picture all stages between pure uniocular vision and almost pure binocular vision by supposing one field which just touches another field slid over on to it progressively until they overlap. We must remember, however, that the bird is surely just as unconscious of two eyes, of two fields of vision as we are usually. It sees, just as we feel, continuously, and better in some parts of the field of visual sensation than in others. Birds' vision is therefore also cyclopean, as, after all, all experience is. Vision therefore has to remain cyclopean however much overlapping of fields there may be. That could only be done by virtually eliminating this overlapping. Two ways are possible. In one, such connexions are made between the hemispheres as will eliminate the order-differences of the overlapping parts by the excitation of certain afferent impulses, e.g. sensations of eye-movement. One may compare this form of integration to that which we find in ourselves in touch, when distances are discriminated at one time with the fingers placed together, at another time with the fingers apart. There can, however, be no doubt but the visual impressions of the chameleon are modified in some apparent way—depth-effect—when the two eyes are directed upon one object amongst others. There are many other kinds of these modifications or derived attributes, which I shall treat by themselves. In the other of the two ways, such connexions are made between the retinal elements and the hemispheres as will procure visual identity of the overlapping parts. This can be done only if the relations between the eyes are of a fixed and unfluctuating nature or if certain points in the two overlapping parts are always used and stimulated identically. This we find realised in our own vision, for if the two foveas are not directed upon one object, allowing for very slight deviations within the fovea, and stimulated almost identically, we see double. Such a fixed relationship within binocular vision is, of course, a corollary of the very function of binocular vision, namely to give static depth-vision. Now, as a jointure has to be effected between the two fields of vision somewhere, it is quite the most natural and economical arrangement to split each of the overlapping areas into two halves, and to splice the two left halves by the shortest neural paths, as also the two right halves, the rest of the two retinas remaining in *status quo*. The split must obviously be in the vertical direction, since the two binocularly used eyes are upon a horizontal plane and the method of decussation is economical because thereby the neural paths between the hemispheres are dispensed with.

We need a large range of eye-movement in order to follow the motion of the objects we fixate binocularly and to bring our identical (or "corresponding") points of vision always to bear on the object of binocular regard. This fact suggests two extremes, between which all actual forms of vision may find a definite place. An ideal progressive vision, on the one hand, would show practically no overlapping or distance between the two fields of vision at any point of their whole circumference, equally clear vision at all points and therefore a complete visual panorama and consequently no need for any eye-movements whatsoever. The possessor of this form of vision would fly in a spherical panorama, hardly conscious of his own body visually. The vision of some of the birds may approach this, if we neglect for a moment the obstruction of the outstretched wings. And even these may be just as transparent as are our eyelids. The more an animal, on the other hand, becomes binocular and static, the more it must need eye-movement. The highest conceivable degree of eye-movement would be needed by an animal which could not move and had only the tiniest fields of vision completely overlapping one another. If it wished to view its whole visual environment, it would have to have universal eye-movement. The fingers of the blind are eyes of this kind.

Since the one retina in binocular vision is practically converted into a part of the other, it is quite evident that there should be a slight amount of difference of localisation to distinguish confused stimulations of identical points of the two retinas from one another. For in so far as the localisations of the two eyes are not specially identified, that of the left should be somewhat to the right, that of the right somewhat to the left, just as they are for disparate points in one eye, whereby uniocular progressive stereoscopy is accompanied by apparent motion[1]. For if we presuppose the left field of vision, the view of the object obtained in the right field would uniocularly only be got by a relative motion of the object towards the left, i.e. the presupposed left-eye view is to the right of the right-eye view. All these things argue still more strongly, that the integration of stereoscopy is achieved uniocularly and binocularly by similar mechanisms. The connexions of disparate points should be much the same whether some of them are in one eye and some in the other, or all of them in one eye.

Stereoscopic vision may therefore be defined as the integration of the order aspects of successive stimulations of one or of simultaneous stimulations of both retinas, under the familiar conditions regarding disparation, of which integration a new attribute or modification of visual experience—depth-aspect—is the psychical equivalent.

E. It has already been noted that it is a matter of indifference which of the two stereo-diapositives stands right or left, above or below in the methods of stereoscopy by projection; for the series of expositions given in both cases is identical, as soon as the series has been started.

[1] Cf. Witasek, *Ztsch. für Psych.* Abt. I. Vol. XL. p. 217.

But binocular depth-effect is essentially dependent on the projection of the right-eye image into the right eye and the left into the left. If the projection is reversed, the depth-effect is usually reversed as well. How then can the one ambiguous series of the Ives projection give a perfectly unambiguous depth-effect to monocular observation? Does it always do this?

To test this, one naturally turns to the simplest case of stereoscopic vision, that in which one unsuspended point is seen in front of another. The two images consist each of two dots at unequal distance from one another, thus: . . and . . . But if these are projected successively upon a screen as above described and observed uniocularly, no proper depth-effect is obtained. Either one point is seen to oscillate laterally or both do so, according to the manner in which they are adjusted in projection. Either can be *thought* to be behind the other, but neither is *seen* to stand in front of the other. There is nothing present which could give rise to depth-effect.

Nor is the matter essentially altered by the projection of a large number of points in two successive pictures corresponding to the left- and right-eye views of a number of points (walnuts) suspended on fine invisible threads before a white background. Several interpretations of such a projection are, however, possible. If the adjustment is so arranged that all the points except a few which do not move, oscillate simultaneously in one direction, the oscillating points can be interpreted or more or less clearly seen as in front or behind the steady points. If some points oscillate in opposite directions, while others remain steady, they must be interpreted by one of two opposite systems, which are defined by the interpretation of any one oscillating point as being in front or behind, while the steady points are at half-depth. Therefore it should be possible that different observers should chance upon each of these opposite systems of interpretation of the same projected views.

Such differences of interpretation do actually occur. They can be obtained in greatest variety when successive views of a schematic object are exposed in a stroboscope in periodic series, in which a circle A moves within a circle B from the point of concentricity along the horizontal diameter of B towards L and back again to concentricity in some 12 steps (cf. Fig. 1). The horizontal tangential points of the two circles are joined by lines in each case[1].

[1] Cf. Straub, *Ztsch. für Psych.* 20 Juli, 1904, Bd. xxxvi. p. 435, upon one of whose figures the series indicated in the text was modelled.

When the stroboscope is rotated, successive pictures falling on one eye are seen momentarily and induce thereby the perception of a single object in which a certain spatial change is taking place. For the above series, the only one I have examined by Straub's method, the following different interpretations were the first given by different observers: (1) The circle A seems to move on the plane of the paper laterally towards L and back to the middle position periodically; (2) The circle A stands nearer than the circle B and suggests the figure of a truncated cone which is making a periodic oscillation towards L and back to middle position, as if the body were moving on a vertical axis lying half way between the plane of A and the plane of B. These two are the forms in which the figure appears first to most observers; (3) The figure can be seen with the depth relations of (2) reversed, i.e. surface A lying farther away than surface B, and forming the figure of a hollow truncated cone; (4) It may appear that the surface A rises from the plane of B periodically towards the

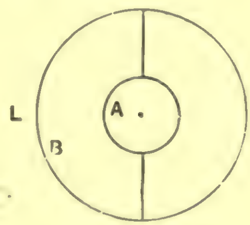

FIG. 1.

observer and back to the plane of B; or (5) the reverse, the surface A may appear to sink periodically away from and back to the plane of B. The latter two forms are those which appeared to the writer first, who made the figure series and owing to the suggestion exerted upon him by Straub's paper expected to see these forms. Straub notes[1] all these forms of interpretation and they can be readily seen by all observers. These interpretations are, of course, not usually steady or permanent in any one form. When all of them are familiar to the observer, they displace each other continuously. They can even be combined together to larger periods, as when forms (4) and (5) alternate rhythmically and give the appearance of a body collapsing from above to the level and then out behind and back again. There can be no doubt about the depth-effect presented to uniocular observation by any of the forms except the first (1).

[1] *loc. cit.* pp. 435–6.

This method of demonstrating uniocular stereoscopy was, it seems, devised independently and simultaneously by Straub and Brown[1]. The pictures used by the latter consisted of series of photographs of a group of small objects upon a table, which was turned through a small angle before each view was taken. Elschnig remarks in his discussion of these stroboscopic observations that because of the persistence of visual perceptions the single phases fuse to a unitary and therefore solid visual perception[2] and otherwise emphasizes the stereoscopic effect of uniocular change of parallax. He maintains however in general that "monocular stereoscopic vision with Straub's stroboscope is as little real stereoscopy, as is the apparent plasticity which a photograph viewed through a convex lens or two identical photogrammes seen binocularly in a stereoscope or any perspectival drawing or a well shaded photograph shows." He cites in evidence of this the fact that the depths in the stroboscope are so often reversed quite suddenly even when they seem clearest. In real stereoscopic vision, he says, only one interpretation of what is seen is possible. Straub goes so far as to maintain that there is no difference between uniocular and binocular stereoscopy in respect of the depth-effects in each[3]. He concludes, that both of these processes are inferential (in bieden Fällen ist die Tiefenvorstellung ein Schluss). Elschnig gives a further instance of illusory (vorgetäuschte) depth-effect; it is obtained very often in cinematographic projections especially in respect of the apparent approach or retreat of men, horses, waggons and the like. Finally he speaks of the "radical (himmelhoher) difference between apparent and real stereoscopic vision."

Evidently it is hard to see the facts clearly, so great is the reluctance to let uniocular stereoscopy pass. The stroboscope presents to us several different and very unstable forms of depth-effect from the same stimulus and there must be some reason for their presence. To call them illusory is no explanation and to compare them to a less degree of their own appearance, is not to discredit that appearance, but to emphasize the need for an explanation of the presence of any degree of stereoscopic effect. On the other hand, processes which might be called inferential may certainly often be involved in am-

[1] Theod. Brown, "Direct Stereoscopic projection," *Photography*, 23 July, 1904. The title and the description of the experiments I take from Prof. A. Elschnig's "Ueber monokulare Stereoscopie und direkte stereoskopische Projektion," *Jahrb. für Photographie*, 1905, pp. 103—108. I have not seen Brown's paper.

[2] *loc. cit.* pp. 104, 105.

[3] *loc. cit.* p. 432.

biguous presentation of depth, as will become more apparent in the experiment next to be described. But no reason has yet been given why inferential or any other hetereogeneous processes should be able to create or induce an actual presentational depth-effect, such as is observed in these experiments. This applies also to all "experience" theories of stereoscopic vision, which are not essentially nativistic or physiological. These theories do not call for discussion here, for the burden of proof lies upon them. They must show how their "experience" is capable of producing what careful introspective comparison shows to be a distinctly presentational state.

Further light is thrown upon uniocular stereoscopy by a form of experiment similar to that of Straub but of a much less regular and unitary nature. This consists in projecting through Ives' apparatus two pictures of a group of words set up and printed twice over as nearly as possible alike, cf. Fig. 2[1].

STEREOSKOPI-
SCHE UNTER-
SCHEIDUNG EI-
NES DRUCKES
VON SEINEM
NACHDRUCK.

FIG. 2.

In the single still projection of such a plate, there is of course hardly even a suggestion of depth-effect. The printing seems in no way to differ from the usual form of letters printed on a plane surface. Even if all the letters do not seem to be of the same height and at the same distance from one another, it is not easy to detect any difference between the right- and left-eye pictures, as these are found, when prepared specially for stereoscopic demonstration; and in this experiment no differences *can* be detected, because these plates are not projected at once, but successively.

When the projections are given successively in the way described, the depth-effect is found to be good, especially if the maximal oscillation is kept small. The letters are seen to stand well forward and behind. Too large a degree of oscillation spoils the depth-effect. But

[1] This stereoscopic slide is one of a set accompanying a small book, *Das Stereoskop und seine Anwendungen*, by Th. Hartwig-Teubner, Leipzig, Stereogramm v.

although all observers see depth-effects throughout the field of projections, they differ in their reading of these; they do not even each maintain one system of interpretation, as in the case of the experiment with the stroboscope. The variation in the observations of different observers may be exemplified by the following table in which the signs > and < stand for the expression "in front of" and "behind" in the direction in which the table is read. Three lines of the slide are given and, amongst the letters of these, signs are to be found indicating the depth-effect got from the slide in the stereoscope. It will be noticed

TABLE I.

Stereoscope	S	>	T		E		R	<	E		O		S		K		O	>	P		I
O_1		<	>					<													
O_2								>										<			
O_3		<						>							<		or	<			
O_4		<	>				<	>			>		>		=		>	<			
O_5		<						<										<			

Stereoscope	S	·	C	H	E	>	·	U	>	N	<	T	<	E	R
O_1		>							=		=		<		
O_2											>		>		
O_3						>					<		>		
O_4											<		>		
O_5									>		<		<		

Stereoscope	S	C	H	E	I	D	<	U	N	G	>	E	I	·
O_1							>		<		>			
O_2							>				<			
O_3							>				<			
O_4							>				<			
O_5							>				<			

The table indicates by the signs >, "in front of," and <, "behind" in the direction in which the table is read, the relative depths at which the letters opposite "Stereoscope" are seen when the right and left halves of this familiar stereoscopic slide (*v.* Fig. 2) are presented alternately to uniocular vision. The signs between the printed letters indicate the depth-effects seen through the stereoscope. A long sign > means "much in front of" a small one >, "slightly in front of."

that though there is much agreement amongst the five observers, no one of them remains consistently either in the system of depth-relations of the stereoscope or its reverse. A number of observations made by two observers seem to suggest that for the most part the positive or negative character of the depth-effect seems, as in the case of Straub's pictures, to follow Wundt's rule for plane optical illusions that the point fixated appears the nearer. An exception to this is found in the case of the three lines given in the table in the letters U N T E R.

These can be seen in various ways. If the observer looks at the letters S C H E, the letters U N stand well behind these and the T. There is a large space between these sets of letters and this it is, probably, which gives the impression that one is looking through between two different layers of letters of which U N forms the more distant. If the observer, on the other hand, fixates the E of U N T E R, the T stands forward, it may be, in front of both E and N. If the letters U N T E R are held visually in reference to the line above or the line below, they may be seen in front of or behind the latter, according as the position of the observer gives him the impression of looking down or up through the space between the lines. Curiously enough the depth-effects in this picture are also changed when the head from being allowed to hang down over the left shoulder is moved to hang down over the right shoulder. Finally, it may occur in this experiment that, if the observer in doubtful cases is pressed to decide upon depth-relations, inferential processes proper may make their appearance.

There are therefore a number of indirect factors which help to determine whether the depth-effects shall be positive or negative in the case of successive views which if reversed in the stereoscope would give an equally coherent object. These factors are presumably identical with those operative in the case of the ordinary visual illusions and in the reading of plane pictures. I have observed whether a number of points (walnuts) suspended on invisible threads were capable of giving systematic and unitary depth-effects, when the points were surrounded by a well-defined room-picture, as against the depth-effect produced by their projection alone. But while the depth-effect of the latter followed that just described and was best when the points at middle distance are made stationary in the projection, the scene did not seem to help, because such suspended points were, normally, separated by the attention demanded by close observation from the room-picture which surrounded them. But it is to be emphasized that in spite of this, the depth-effect, wherever it occurs, is direct and proper, and not essentially different from that first described. There can then be no doubt but the reason why the immediate depth-effect got in the projection of the landscape is permanent, unitary and unambiguous, is that a landscape is not capable of the double interpretation, positive and negative, which the projection of the series of views *r*, *l*, *r*, *l*, *r*, *l*, *r*, *l*, *r* makes possible. The landscape cannot be inverted in regard to its depths mentally. Even when the retinal images are reversed in the pseudoscope, or other similar appliances, the depth-effect is not coherent or unitary. Much of the usual effect is lost, what remains is awkward and puzzling; but there is certainly no extensive reversal of effect in a natural landscape.

F. What is there, then, in the landscape that excludes the double interpretation of its depth-aspects which is theoretically possible? After the treatment of stereoscopy in the foregoing pages, it would, of course, be absurd to suggest that the irreversibility of the landscape makes up the depth-effect derived from the successive projection of its disparate views. Stereoscopy is a sensory integration which is realisable in independence of all other visual integrations or integrative influences.

There is obviously one feature of the landscape that is not present in the ambiguous figures—the relation between the size of projected objects and their distance as indicated by their disparity of retinal position. This is the only afferent element which could account for the stability of the depth-aspects of the landscape. There is a perfectly unambiguous relation between apparent size and distances, as soon as any one apparent size and its psychical distance are given or are known by associative coordination. We have therefore to consider it probable that this unambiguous relation between size and distance acts as a determinant upon the ambiguity of the system of depth-aspects presented to uniocular vision by the successive projection of disparate views. Other factors might be suggested to account for the determination of the ambiguity, e.g. the psychical unity of or familiarity with the system of things in a landscape; and factors like these may very well act sometimes as determinants, as in the case of the stroboscopic stereoscopy where expectation kept the ambiguous figure determinate for some little time. In the case of pseudoscopic observation we find however that the relations of retinal disparity artificially reversed are in general unable to integrate the depth-effects for which they form the adequate stimuli, when the given relations between size and distance are familiar. For the latter, not having been altered, of course, run against the former and are able to disintegrate or suppress their effects. If relations of size to psychical unity and familiarity are able to disintegrate a complete process, it is not surprising that they are also able to determine the ambiguity of integrative stimuli. Psychical unity and familiarity and relations of size must usually accompany one another, as in the above two cases; so that it is, so far, impossible to exclude the one or the other. It must be left for other cases or general treatment to indicate which of the two is the actual determinant. It is only important to notice that a determinant, be it peripheral or central in origin, may support or encounter an integrative process, ambiguous or fully determinate.

A new interest attaches itself immediately to all forms of sensory integration which can be rendered ambiguous and their discovery is a matter of importance. We must also know what circumstances can determine their ambiguity and why.

Many of the familiar visual illusions show marked ambiguity quite apart from their use in the stroboscope or in double projection. If the outline of the cube-figure is drawn even quite roughly on a sheet of paper sufficiently large to fill a considerable part of the field of vision and is regarded uniocularly, the stereoscopic effect so obtained is, for moments, almost as good, as that of a cube in the stereoscope or elsewhere[1]. It can also be observed, as is well known, that with change of fixation all the depth-effects of the figure change consistently and almost instantaneously to their opposites. The stereoscopic effect is so pronounced that, on moving the head or the figure, the cube seems to shift to keep its near and far points in a line with the line of vision, just as has been observed with Rollmann's colour-stereoscope, although no two systems of disparate points are stimulated. Whence this effect?

The visually presented set of lines would naturally remain mere lines psychically, if some determination were given to the mind concomitantly to take them as such. But as we have seen stereoscopy and probably many other processes are integrated upon various primitive visual presentational complexes. Now the stimulus to a primitive sensation can not be inadequate, as no psychical change less than the primitive sensation is possible. *Only in respect of sensory integrative processes can a stimulus be said to be inadequate*, if the word is to convey any useful meaning. But if such an inadequate stimulus be moderately vigorous, we must not be surprised if it exert some stimulative effect upon the integrative mechanisms attached to the incoming paths of primitive sensations, unless we can show that the latter have certain main thoroughfares provided for them, from which they have to be specially deflected to reach these integrative mechanisms. This is, for vision at least very unlikely, in face of the facts. One may of course assert with confidence, that inadequate stimuli will not stimulate integrative processes, if they cannot, nor are they likely to do so, if the latter have not yet been roused by the corresponding adequate stimuli. In the latter case even the cube-figure might appear as a mere group of "insignificant" lines, although this seems rarely to occur for that figure. With many other figures it will occur readily at times with everyone. But the cube-figure may also arouse those integrations

[1] Except, perhaps, where the lines of the figure cross each other, especially if attention is not generally, but specially directed.

immediately coincident upon the presentation of lines of whose adequate stimulus the latter can form and have often formed a part. These integrations are the two cube-forms and the truncated many-sided pyramid, although everyone finds that even the help of attention is seldom able to integrate the latter. All these forms are possible, if the given stimulus suffices to evoke them. If they are found to have any tendency to arise, they can, of course, only be excluded, if one particular integration is so adequately determined that it will suffice to maintain itself steadily, besides excluding the others. Such a limitation, say to flatness, is often provided by binocular identity of stimulation. This might not however necessarily be a sufficient limitation, because it is conceivable that the attractive power of one of the solid forms and the chance support given to it by various circumstances might be great enough to break down the limitation to flatness.

What are these circumstances which act as *determinants* upon ambiguous stimuli, whether these be inadequate or under normal circumstances adequate? The investigation of the illusions of reversible perspective attempts to give an answer. Fixation of certain points and the movement of fixation along certain lines, e.g. are well established as peripheral determinants; but there are also central determinants which may support it or suppress it, for a reversal of perspectival illusion will take place even during fixation. Here we have a case in which probably a central determinant may act with or against a peripheral determinant[1]. The natural form which a theory of these determinants will take is that such and such an one determines an ambiguity, say of depth, because it is always or very usually found that fixation of a certain point involves a better view of farther points surrounding it, than of nearer points, perhaps because there is naturally and usually nothing opaque between us and our object of regard. Or it might be said that a determinant acts because it itself actually integrates partially or wholly one of the forms which the ambiguous stimulus integrates, or that it acts because it arouses centrally by memory-image or associative recall or the like one of these forms and therefore facilitates the passage of the inadequate stimulus by that way. In regard to *peripheral determinants* there is a presumption in favour of their being *consensuous* with the original ambiguous or inadequate stimulus, for it does not readily appear how a stimulus through

[1] Cf. Meumann, "Ueber einige optische Täuschungen," *Archiv f. d. ges. Psych.* Bd. xv. p. 405. In these illusions irradiation acts as a determinant within the details of a depth-integration. It strains the facts however to talk about "conflict" in this case, between irradiation and perspective.

another sense, e.g. the afferent products of muscular activity should produce a change in an integrative effect in a given sense. This is borne out by the results of the examination of the eye-movements accompanying visual illusions by means of cinematographic photography[1]. Central determinants are intelligible already in so far as they act as facilitants to the course of the original stimulation.

The psychological aspect of these problems is perfectly parallel to the physiological basis indicated. What relation is there between the sensations which form the basis of integration, be this basis perfectly or imperfectly determinate and the psychical state which represents the integration? It may be that the systematic integration of order-aspects in uniocular stereoscopy by alternate projection is made determinate by the order-relations which apparent sizes of familiar objects have developed, because the latter is parallel or psychically identical with one of the former. Similarly in the case of the illusions, the order-aspects inherent in the reversible figure might be determined psychically by the progress of a psychical action of examination from one point common to the order-aspects onwards in a certain progression, whether this progression be determined by habituation or by choice of a certain sequence. There is of course no reason why this psychical action should not be called a process of apperception or assimilation or production provided one does not imagine that these processes manufacture or create the psychical appearance which results upon them, whether a special process be supposed to intervene or not. Such terms as these can surely only indicate that a variety of distinguishable primitive presentational complexes have merged into one unitary and in some aspect at least, unique state. Neither for physiological nor for psychological consideration can it be well maintained that central determinants complete presentational complexes by creation of the missing parts. It must be shown that the given and the determining parts together provide a sufficient integrative basis for the final result. We suggest therefore that the order-aspects of component parts, both in a physiological and in a purely psychological sense, provide a sufficient basis for the determination we find.

[1] Judd and others, *Psychol. Rev. Monogr. Suppl.* Vol. VII. No. 1. Neither a certain amount of correspondence between eye-movement and illusion, as shown to be present by Judd, nor a complete correspondence would suffice to establish the influence of impulses towards or of afferent effects of movements of the eye upon visual complexes, unless the latter were themselves visual. For there would always remain the greater probability that the illusions produced the eye-movements.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

Dec. 18, 1909. Statistical Methods in Psychology, by W. Brown.

Jan. 22, 1910. On the After-effect of Seen Movement, by A. Wohlgemuth.
The Calculation of Correlations, by C. Spearman.
Some Observations on the Relation between Memory and Intellectual Capacity, by J. H. Wimms.

March 12, 1910. On Stereoscopic Vision, by H. J. Watt.
On the Theoretical Aspect of the After-effect of Seen Movement, by A. Wohlgemuth.

May 7, 1910. A Possible Factor in the Monocular Appreciation of Spatial Depth, by F. Gotch.
The Psychology of Freud and his School, by B. Hart.
Some Methods for the Study of Psychological Contrast with Demonstration of Apparatus, by J. C. Flügel.

EXPERIMENTS ON MENTAL ASSOCIATION IN CHILDREN.

BY ROBERT R. RUSK.

From the Psychological Laboratory, Cambridge.

Review of previous work.

Procedure: arrangement of series; method of timing; form of instruction.

Results. Association times: rate of reaction; order of difficulty of various series; practice effects; comparison of rates of reaction in the case of sthenic and asthenic terms.

Analysis of responses: verbal reproductions; relation of superordination responses to intelligence; perseverance and persistence.

Analysis of introspection: kinds of imagery exemplified; verbal imagery; displacement in imagery; relation of part and whole; generic imagery; types of subjects; endowment in respect to imagery and correlation with intelligence; vividness of imagery and relation to perception; self-projection in imagery; relation of imagery to thought.

Summary of conclusions.

THE first important work on Mental Association in school children was undertaken by Ziehen at Professor Rein's Seminar School at Jena about 1898[1].

Ziehen's conclusions regarding the rate of the association-processes are that the rate is quicker with the adult than with the child[2], and that the speed of association increases markedly year by year[3]. When

[1] *Sammlung von Abhandlungen aus dem Gebiete der pädagogischen Psychologie und Physiologie*, I. Band, 6 Heft and II. Band, 4 Heft. A summary of Ziehen's experiments is given in Meumann's *Vorlesungen zur Einführung in die experimentelle Pädagogik*, Bd. I. S. 220 ff.

[2] II. 57.

[3] II. 50. Wreschner however found that a child of 3⅔ reacted much more quickly than a child of 5⅔ (*Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, I. Abteilung, Ergänzungsband 3, "Die Reproduktion und Assoziation von Vorstellungen" (Leipzig, Barth, 1907—9)).

the various processes are arranged according to their speed to form a scale of difficulty, the quickest being placed first, the result is[1]: (1) Word associations; (2) Homosensorial associations; (3) Partialising associations; (4) Heterosensorial associations[2]; (5) Totalising associations; (6) Pure contiguous associations without partial connection; (7) Relational associations.

Regarding the content of the child's imagery, Ziehen states that he was prepared for the preponderance of individual presentations in the child, but the extent to which this occurred astonished both himself and all to whom he communicated his results[3]. He consequently concluded[4] that in this respect the mental association of the child differs *toto coelo* from that of the adult.

Verbal associations were but seldom found with children[5]; and when found as a rule took the form of word-completion. Such verbal association appears to increase with age and is most frequent with adults[6]; rhyme associations also occur less frequently than with adults; similarly associations of words commonly connected, e.g. hand, foot[7]. With more intelligent children of the same class, pure individual associations preponderate, and a premature approximation of the child to the type of the adult is said to indicate intellectual inferiority. The percentage of pure individual associations decreases with increasing age but in no regular proportion; in comparison with the total number of individual associations, the number of time-defined individual associations decreases in like manner; the relation of time-and-space defined associations to purely space-defined associations appears to vary little with age[8].

The factor of "perseverance" was also noticed by Ziehen: he observes that it decreases with increasing age[9], but he does not attach to it so much importance as later writers have done.

In 1903 Meumann in his investigations in connection with the intelligence of children of the elementary schools of Zurich[10] employed certain Reproduction tests. The total number of children examined

[1] II. 49.

[2] Ziehen omitted (4) doubtless accidentally. We conclude it should be Heterosensorial. Meumann, *Vorlesungen*, I. S. 228, has not taken account of this apparent omission.

[3] According to Wreschner (S. 153) the degree of particularisation is 8·09 °/₀ cultured, 6·65 °/₀ uncultured, 43·93 °/₀ children.

[4] I. 32. [5] I. 26. [6] I. 28. [7] I. 29.

[8] I. 45. [9] I. 26.

[10] "Intelligenzprüfungen an Kindern der Volksschule," *Die experimentelle Pädagogik*, I. Band, S. 86 ff.

was over 800 and the method employed was that of mass experiments in class. The reproductions applied were "free" associations. The class teacher read aloud lists of words and the pupils were required to write down the first other word that occurred to them. A few examples were previously given, and little difficulty was experienced by the pupils in understanding the requirement. On the completion of the test, the papers were collected, each child having entered at the top of his sheet its name, age, class and the date of the test.

In these mass experiments the times could not be determined, but in the older classes the whole investigation went appreciably quicker than in the lower classes, and with fourteen-year-old children, frequently only half the time taken by eight-year-old pupils was required. Meumann noted that not infrequently highly intelligent children reproduce slowly, whereas children of weak intelligence reproduce quickly and fitfully[1], showing less power of mental effort and indicating less attention to the content of the stimulus word, together with a tendency towards less resistance in reproduction[2].

Meumann concludes that wealth of imagery and originality of reproduction, without however any departure of the subject from the type proper to his stage of development, are characteristics of intelligence. The former is displayed in change of categories; object-reproductions alternate with qualities and processes. The latter is shown in the response of a word like "fuel," instead of the usual response "black," to the stimulus word "coal."

Associations which arouse object-images preponderate with young children; verbal changes preponderate with older pupils[3]. When about the thirteenth or fourteenth year concrete imagery fails, the pupils take to reproducing the logical opposite[4]. This form of reproduction especially the use of correlatives is usual with the less intelligent children[5], and is found to be the quickest form of association with adults[6]. Unintelligent children reproduce words frequently named together, and depend much on what has been learnt at school for the material as well as the form of reproduction. Their imagery is not so concrete as that of the intelligent child; they also use verbal imagery to a larger extent, thus tending to approximate to the adult type of thinking and consequently appearing in this respect precocious[7].

With younger children visual reproductions preponderate and auditory are remarkably few in comparison: with older children the

[1] I. 90. [2] I. 91. [3] I. 100. [4] I. 100.
[5] I. 91. [6] I. 94 and 99. [7] cf. I. 95.

images derived from the various spheres of sensation are more evenly distributed[1].

Of "perseverance" Meumann distinguishes three forms: (1) where a previous stimulus or reaction word repeats itself when inappropriate, (2) when a certain type of relation of images one to another determines the reproduction, e.g., opposition, (3) when a form of expression keeps recurring, e.g. a mere intensification like the addition of the word "very" to the stimulus word. This phenomenon he regards as a definite characteristic of low intelligence[2].

In his investigations on the subject of "endowment" in 1904–5 Winteler tested individually for reproductions both free and constrained, eight boys about ten years of age[3].

In free reproduction two pronounced types manifested themselves. One class reacted to substantives with an attributive adjective, to adjectives with a complementary substantive; this constitutes the perceptual or describing class. The others responded to a substantive with another substantive standing in logical relation, to an adjective with a contrary adjective, to a verb with a contrary or a synonymous verb; this constitutes the comparing or relating type. No relation was apparent between the type and the degree of intelligence.

Winteler also employed constrained reproduction tests in which the choice of the response word was limited to a certain class of terms. Five different tests were applied, to find (1) a superordinate concept for a given concept, (2) a subordinate concept, (3) a coordinate concept; to give (4) an example or species of a genus, (5) an opposite.

Winteler's conclusions are[4]: (1) Of the three tests, to find a superordinate, a coordinate, or a subordinate concept, the first test implies substantially the greatest logical effort, and the last test the least logical power. (2) The more intelligent children give evidence of better defined logical capacity than the less intelligent; the first test shows the greatest divergence, and the third the closest approximation between the two groups. (3) Consciousness of opposites is better defined than that of identity. (4) Children of opposite grades of intelligence are more alike in the reproduction of opposites than in the reproduction of identical concepts. (5) The shortest reaction times occur with the more intelligent subjects, the longest with the less

[1] I. 100. [2] I. 96.

[3] *Die experimentelle Pädagogik*, II. Bd., "Experimentelle Beiträge zu einer Begabungslehre."

[4] S. 239.

intelligent, yet the rate of reproduction possesses only secondary value for estimating the grade of intelligence.

OBJECT OF INVESTIGATION, APPARATUS, AND PROCEDURE.

A reinvestigation of mental associations in children (both free and constrained) appeared to the writer to be desirable. The following points seemed to require special attention, viz. the relation of the speed of reaction to the age of the subject; the effect of practice on the rate of reaction; the order of difficulty as judged by the speed of reaction of the various processes; the relation, if any, of the nature of the responses in the case of superordination to standard of intelligence; the various forms and the degree of perseverance; the nature of the child's imagery, its relation to age and intelligence; and the part played by imagery in constrained association.

To determine the principles governing free associations, five series of experiments were arranged; viz. the presentation of:

(1) A series of disconnected concrete terms;

(2) A series of disconnected abstract terms;

(3) A set of concrete terms comprising two series of five terms each, the five terms being connected one with another.

Then in order to test the effect of practice,

(4) A series of disconnected concretes similar to series (1); and

(5) A series of disconnected abstracts similar to series (2).

These last two tests were applied at the close of the experiments in each case, i.e., after the preceding free associations and the whole of the constrained associations had been completed.

The constrained associations were analogous to those employed by Watt in his "Experimentelle Beiträge zu einer Theorie des Denkens[1]," viz. to find (1) a superordinate concept, (2) a subordinate concept, (3) a whole, (4) a part, (5) a coordinate concept, (6) another part, of a common whole. In the present investigation, the last series was omitted and a series requiring the subject to find "a cause" substituted. The following was the arrangement adopted:

Series	I.	Free associations:	Concretes disconnected.
,,	II.	,, ,,	Abstracts ,,
,,	III.	,, ,,	Concretes connected.
,,	IV.	Constrained associations:	To find a superordinate concept.
,,	V.	,, ,,	To find a subordinate concept.
,,	VI.	,, ,,	To find a coordinate concept.
,,	VII.	,, ,,	To find the cause.
,,	VIII.	,, ,,	To find the whole for a given part.
,,	IX.	,, ,,	To find the part for a given whole.
,,	X.	Free associations:	Concretes disconnected.
,,	XI.	,, ,,	Abstracts ,,

[1] *Archiv f. d. ges. Psychologie*, Bd. IV.

Each series comprised ten terms; and alternative lists of words were prepared so that, if an effort were made by subjects to prepare for the test by ascertaining from one another what words were being employed, they would be misled and give up the attempt. The alternative series and the original series were made as far as possible of equal difficulty. The subjects were also asked not to disclose the words employed, and this request was, so far as the experimenter could determine, complied with.

The words used were as follows:

Series I. Horse, Lamp, Hat, Village, Rain, Market[1], Post[1], Cherry[1], Board[1], Red: *alternatives*, Cow, Watch, Hill, Box, Snow, Castle[1], Boot[1], Picture[1], Butter[1], Blue.

Series II. Drink, Hunger, Justice, Health, Work, Pity, Joy, Cruelty, Praise, Grief: *alternatives*, Food, Thirst, Mercy, Illness, Play, Hope, Sorrow, Kindness, Pride, Blame.

The abstract words were selected to discover also whether pleasureable or painful affects were better known (i.e. had the shorter reaction times).

Series III. Shop, Window, Street, Town, Country: Field, Game, Team, Ball, Goal: *alternatives*, Desk, Seat, Room, School, Road: Bridge, River, Boat, Race, Bump.

Series IV, Superordination. Apple, Pansy, Cabbage, Shilling, Bread, Doll, Gold, Green, Bitter, Reading: *alternatives*, Pear, Rose, Potato, Penny, King, Cab, Silver, Yellow, Sour, Writing.

Series V, Subordination. Tree, Fish, College, Battle, Picture, Tool, Hero, Lesson, Taste, Wrong: *alternatives*, Bird, Leaf, Game, Poem, Song, Toy, Hobby, Book, Smell, Virtue.

Series VI, Coordination. Hand, Penny, Pencil, Moon, Baker, Triangle, Silver, Blue, Fear, Smell: *alternatives*, Foot, Shilling, Pen, Sun, Butcher, Square, Gold, Red, Anger, Taste.

Series VII, Causal. Steam, Wind, Thunder, Daylight, Rainbow, Clouds, Tides, Weight, War, Evil, Death.

Sufficient suitable words to form an alternative series could not be found.

Series VIII, Part-Whole. Ear, Wheel, Beak, Inch, Platform, Mast, Branch, Kernel, Funnel, Buckle: *alternatives*, Mouth, Handle, Claw, Ounce, Pavement, Sail, Stem, Core, Boiler, Knob.

Series IX, Whole-Part. Chair, Boot, Sword, Comb, Watch, Basket, Glove, Egg, Umbrella, Flower: *alternatives*, Table, Bottle, Gun, Knife, Door, Kettle, Cup, Clock, Bed, Tree.

[1] These words were selected in order to admit of word completion.

Series X, Free Concretes. Purse, Stamp, Piano, Barrel, Student, Scissors, Envelope, Cigarette, Coal, Brown: *alternatives*, Motor, Hymn, Hail.

Series XI, Free Abstracts. Danger, Honour, Hatred, Honesty, Sadness, Beauty, Liberty, Deceit, Gladness, Greed: *alternatives*, Disgrace, Bravery.

The tests were undertaken during the Michaelmas and Lent Terms 1909–10 at the Morley Memorial School, Cambridge, and thanks are due to the managers, master and teachers for their kindness in permitting this work to be carried out. The mornings, from 9 a.m. to 12 noon, were invariably chosen for the tests and only one set of ten words was given at a sitting. The examination of each subject consequently extended over eleven sittings. The time occupied with one series was usually from 30 to 40 minutes, varying according to the amount of introspection given. Each individual was taken, as far as possible, about the same hour of the day.

Twenty-two boys, from 7½ to 14½ years of age, were examined. The school is a middle-class (mixed) elementary institution, and the pupils are of much the same social status as the children that attend Professor Rein's Seminar School at Jena, where Ziehen's tests were applied. The classes are graded from the top, the pupils of Class I being about 14 years of age. The general intelligence of the subjects as estimated by the class-teacher and head-master is indicated thus:

High position in class = a,
Medium position in class = b,
Low position in class = c,

and in two cases it was considered advisable to mark the subject as "d" indicating that he was of very low intelligence although not of feeble mind or mentally deficient. The ages of the pupils were calculated as at the commencement of the tests. The youngest subject was a Scots' boy; it was thought expedient to test him in preference to a Cambridge boy as he was familiar with the voice of the experimenter who knew intimately his life-history.

The words were presented visually, but, as the two subjects of intelligence "d" evinced difficulty in reading them, the words were simultaneously spoken and presented visually to these subjects. The words were spoken only to the youngest subject.

The apparatus employed for the purpose of word presentation was a blackboard 58 cm. broad and 41 cm. high with an open space 12½ by 6½ through which the stimulus word could be seen when a shutter was released by the experimenter pulling a cord. The stimulus words were printed by hand on cards 17·5 cms. by 9·5 cms., which were held in position with a clip similar to that ordinarily used for gripping papers. The letters were 15 mm. in height. The apparatus rested on a drawing-table which could be adjusted until the stimulus word was level with the eyes of the subject, who during the experiment was seated on an ordinary chair. The room was well lit, and, save in exceptional circumstances, only the subject and the experimenter were present. Each subject was, at the commencement of the tests, familiarised with the working of the apparatus.

The reaction times were measured with a stop-watch marking fifths of seconds. As Ziehen used a Hipp's chronoscope[1] and was at some pains to point out its various possibilities of error, the use of a stop-watch, indicating only fifths of seconds, might be thought to require some justification. The stop-watch has however the advantage of enabling the experimenter to measure the times while the subject is unaware that he is being timed; and it also allows the subject's attention to be concentrated solely on the test without his having also to attend to the process of reacting.

The use of the chronoscope with children is liable to introduce a sense of mystery which causes them to reflect upon the object of the experiment, and as a result adopt an unnatural and suspicious attitude. The simpler and more natural the conditions under which such tests are applied the more valuable are the results likely to be.

Wreschner with children as subjects employed a stop-watch[2], and Meumann in determining the difference between two forms of instruction was led to reject the chronoscope in favour of a stop-watch indicating quarter seconds[3]. The times here given will, owing to the fact that the action of the watch is less delicate and consequently responds less quickly to the touch of the experimenter, be longer, probably about ⅖ secs., than those of the chronoscope, but as the difference is constant the relative value of the times will not be affected to any considerable degree.

After a little practice no difficulty was experienced by the experimenter in starting the watch at the exact moment the shutter was released.

In reducing the reaction times of the various series for the different

[1] Ziehen, II. S. 7.

[2] Wreschner, S. 23.

[3] *Archiv f. d. ges. Psychologie*, Bd. IX. S. 128.

subjects, it was decided to take the median[1] rather than the arithmetical mean, thus eliminating extremes which are probably due to interfering factors. For example, one subject took 17 secs. to respond to the word "Rain." His average for the series, excluding this reaction, was 6·6 secs. On being questioned he admitted that he had thought first of the head-master of the school whose name was Mr Rain, and considering this impertinent was led to seek another response. The same subject took $13\frac{4}{5}$ secs. to respond to the word "Beauty." Suspecting that something other than the reply given had first occurred to the subject, the experimenter challenged the response and received the admission, not without some blushing and stammering, that the boy had first thought of one of the girls in his class. Exceptionally quick reactions occurred in some cases, owing to the presence of some object in the field of perception, which suggested the response.

To indicate the range of each series the semi-interquartile range (i.e. half the difference between the second above and the second below the median) is also given.

With regard to the form of instruction, Meumann has pointed out in his article "Ueber Assoziationsexperiment mit Beeinflussung der Reproduktionszeit[2]," that two quite different methods of reaction are followed when different forms of instruction are given to the subject. In one case the reaction is effected as quickly as possible, whereas in the other the subject takes his own time to comprehend the content of the stimulus word and to connect it with the reaction word. The former Meumann describes as the "A" instruction, the latter as the "B" instruction.

When the quickness of the response is the controlling factor, the stimulus word is, according to Meumann, comprehended after the most fleeting fashion; the reproductions are of little value, e.g. rhymes, word changes, opposites, etc.; the times are shortened; the statements regarding the processes intervening between the apprehension of the stimulus and the response are incomplete, and often the subject knows nothing of how he arrived at the reproduction. With the other form of attunement, however, these characteristics are reversed. Meumann maintains, therefore, that for experiments with a problem to be solved the instruction "as quick as possible" is detrimental.

In the tests with constrained associations, the "B" instruction was adopted in this investigation. The subject was invited to take his own

[1] The fifth from the bottom was taken as the median in each case.

[2] *Archiv f. d. ges. Psychologie*, Bd. IX.

time, but to give the answer immediately it came into his mind; he was also warned that an introspective account of the process would be required, and was kept in ignorance of the fact that he was being timed.

In the tests on free associations a new form of instruction was employed.

In some preparatory experiments on adults it was found that in free associations the stimulus word frequently aroused in the mind of the subject images which were rejected owing to the momentary difficulty of finding the appropriate term by which to characterise them. The so-called free associations consequently tend to some extent to be constrained. Dr Myers accordingly suggested to the writer that it might be expedient to give the subject the option of responding with a word, or of saying "Now," immediately an image appeared in consciousness. The second alternative was more frequently adopted.

For purposes of comparison, the old instruction, viz. "Give as quickly as possible the first word that occurs to you on seeing the given word," was used with certain subjects; in these cases the subjects were aware that they were being timed.

By matching pupils of similar age and ability, the times (the median in each) resulting from the two forms of instruction may be compared thus[1]:

Subject	New or "Now" Instruction			
	Series I	Series II	Series X	Series XI
F	2	2·8	2·2	7·4
I	1·4	2·8	3·6	3
H	10·4	7·4	9	19·4
O	2	3	2·4	4·4
P	2·4	2·4	2·4	3·8
T	1·6	2·4	2	2·2

Subject	Old Instruction			
	Series I	Series II	Series X	Series XI
E	2·8	3·6	4·4	5·4
K	5·6	7·2	2·4	5·6
J	6·4	4·2	3·2	3·4
Q	1·8	2·4	3·4	5
R	2·4	2·4	5·6	6·2
S	4·8	5·6	4·6	4·4

[1] For age, class, and standard of intelligence of subjects see table given below.

The gain derived from answering as quickly as possible is, it would thus appear, counterbalanced by the time taken to find the word by which to characterise the image.

The instructions given for the various series were as follows:

Series I, II, III, X, XI.	"A word will be shown, e.g. dog. When anything comes into the mind either name it or say 'Now': then tell as exactly as possible what passed in the mind. Take your own time but answer immediately the first idea comes into your mind."
Series IV.	*Superordination:* "An example of a class will be shown. Name the class to which the thing belongs, and then state what passed in your mind, e.g. dog—animal."
Series V.	*Subordination:* The name of a class will be shown. Give an example of the class and state what passed in your mind, e.g. animal—dog."
Series VI.	*Coordination:* "An example of a class will be shown. Give another example of the same class and state what passed in your mind, e.g. knife—fork."
Series VII.	*Causal:* "Name the cause of the thing shown and state what passed in your mind, e.g. smoke—fire."
Series VIII.	*Part-Whole:* "The name of a part will be shown. You are to name the whole to which it belongs and to state what passed in your mind, e.g. leg—table."
Series IX.	*Whole-Part:* "The name of a whole object will be shown. Name a part which belongs to it, and state what passed in your mind, e.g. chair—leg."

The subjects found little difficulty in understanding the instructions. One example usually sufficed and in some instances the subjects themselves suggested an example while the instruction was being given. In exceptional cases two or three examples were necessary, but no record of these was taken. It would perhaps be useful to keep a record of such cases in future as there are doubtless individual differences in this adaptation to new forms of mental activity, and it would be as well to determine them in order to ascertain whether there is any correlation between this capacity and general intelligence. Occasionally the instruction was forgotten, and, for example, a superordinate sought when a subordinate or a coordinate concept was required.

Immediately the response was given, the subject was asked to state what came into or passed through his mind. This also the various subjects accomplished with little difficulty. Then the experimenter cross-examined the subject and elicited further details. The result of this cross-examination was kept apart from the subject's own statement.

Lest it should be thought that the details were suggested by the experimenter's inquiries, it may be mentioned that negative answers were given to the questions as frequently as affirmative answers, and that the subjects were able to distinguish and to state definitely whether an image arose in consciousness at the appearance of the stimulus word or at the question of the experimenter: in the latter case the images are not included in the analysis of introspection here given.

Results, Association Times.

The following table shows the results obtained from the different subjects under the various series.

From the foregoing it would appear that Ziehen's statement[1], repeated also by Meumann[2], that the speed of association increases with age, is not justified. Wreschner found[3] that a child of 3¾ years reacted much more quickly than a child of 5¾ years; and if we compare the results of the youngest child in the classification here given with those of the oldest, who was almost twice his age, we find that the youngest has in the majority of the series the shorter reaction times. There is no constant increase of rate year by year as Ziehen affirms, at least if different children are compared. Ziehen tested the same children with the same words at different periods of time, in these circumstances practice must have played a part. If there is any considerable alteration in the form of reaction with increasing age, it must be found in the qualitative and not in the quantitative aspect of reaction.

Order of difficulty. To calculate the order of difficulty for the various series two methods may be employed, viz. either the arithmetical mean and the mean variation of the results of the various subjects, denoted by A.M. and M.V. respectively[4]; or the results of the different tests for each individual may be arranged and numbered in order of speed, 1 being assigned to the quickest, 2 to the next and so on, and the resultant numbers for all the individuals then added[5]. Both methods are here employed, and in the latter, when the medians in two series were the same, that series with the least semi-interquartile range was preferred; when median and semi-interquartile range were the same, the mean of the two numbers was assigned to each, e.g.

Series	IV	V	VI	VII	VIII	IX	X	XI
Subject L (omitting Series I—III)...	7	4	3	8	5½	5½	1	2

[1] II. 50. [2] *Vorlesungen*, I. 231. [3] S. 49.

[4] Symbols suggested by Dr Rivers, *Br. J. of Psych.* I. 354, 355.

[5] This method is used by Winch in determining the colour preferences of school children, *Br. J. of Psych.* III. p. 44.

Subject	Age Yrs.	Age Mths.	Class	Grade of intelligence	Free associations: Concretes I	Free associations: Abstracts II	Free associations: Concretes connected III	Constrained associations: Super-ordination IV	Constrained associations: Subordi-nation V	Constrained associations: Coordina-tion VI	Constrained associations: Causal VII	Constrained associations: Part-whole VIII	Constrained associations: Whole-part IX	Free associations: Concretes X	Free associations: Abstracts XI
A	14	9	I	*a*	4·8 (*2·1*)	3·4 (*1·0*)	3·8 (*1·0*)	3 (·*4*)	3 (·*8*)	1·8 (·*2*)	7 (*3·2*)	2·6 (·*2*)	2·8 (·*4*)	2·3 (·*5*)	5·4 (*1·8*)
B	14	1	I	*a*	8·2 (*2·7*)	7·4 (*1·6*)	4·2 (·*6*)	3·4 (*1·3*)	3·2 (·*7*)	2·4 (·*4*)	2·6 (·*3*)	2·2 (·*5*)	1·6 (·*2*)	2·4 (·*2*)	3·2 (·*6*)
C	13	8	I	*b*	40 (*15·6*)	32 (*12·0*)	22 (*15·0*)	9·8 (*1·8*)	11·4 (·*9*)	9 (*2·2*)	11·2 (*4·0*)	5 (·*6*)	4·8 (*1·7*)	5·8 (*1·1*)	7·6 (*3·1*)
D	12	6	IV	*c*	2·2 (·*2*)	5 (*1·1*)	3·4 (·*6*)	2·8 (·*9*)	2·4 (·*4*)	3 (*1·0*)	15·4 (*7·7*)	3·2 (·*6*)	3 (·*6*)	2·4 (·*1*)	3·4 (·*8*)
E	12	5	III	*b*	**2·8 (1·3)**	**3·6 (1·3)**	**2·4 (·5)**	3 (*1·3*)	3·6 (*1·1*)	3·4 (*1·2*)	5·4 (·*8*)	3 (·*8*)	3·4 (·*6*)	**4·4 (1·2)**	**5·4 (1·1)**
F	12	4	II	*b*	2 (·*3*)	2·8 (*4·3*)	2·6 (·*6*)	2·6 (·*5*)	2·4 (·*4*)	2 (·*2*)	3·4 (*2·5*)	3 (*1·0*)	1·4 (·*4*)	2·2 (·*8*)	7·4 (*3·8*)
G	12	3	II	*b*	2·8 (·*3*)	3·4 (·*2*)	1·8 (·*2*)	1·8 (*1·0*)	1·6 (·*3*)	1·8 (·*3*)	3·4 (*1·1*)	1·6 (·*2*)	1·6 (·*2*)	1·4 (*0*)	1·8 (·*3*)
H	12	1	I	*c*	10·4 (*9·0*)	7·4 (*3·3*)	10·8 (*2·0*)	4 (*2·1*)	6·4 (*4·2*)	2·8 (·*7*)	4·4 (*4·7*)	2·4 (·*8*)	2·4 (·*4*)	9 (*5·0*)	19·4 (*9·7*)
I	12	1	II	*a*	1·4 (·*3*)	2·8 (·*5*)	2·4 (·*2*)	2·4 (·*5*)	3 (*1·0*)	2·4 (·*7*)	4·6 (·*6*)	2·2 (·*4*)	4 (*1·0*)	3·6 (·*3*)	3 (·*4*)
J	12	0	IV	*d*	**6·4 (2·8)**	**4·2 (1·4)**	**10·6 (3·4)**	5·2 (·*9*)	4 (·*8*)	5·6 (*1·1*)	7 (*11·3*)	3·6 (*1·3*)	2·4 (·*2*)	**3·2 (1·3)**	**3·4 (1·6)**
K	11	10	III	*a*	**5·6 (2·2)**	**7·2 (2·9)**	**5·4 (1·0)**	4·4 (·*8*)	2·2 (·*2*)	2·8 (·*5*)	13 (*4·4*)	3·6 (*1·1*)	5·4 (*1·0*)	**2·4 (·4)**	**5·6 (1·5)**
L	11	7	IV	*b*	2·2 (·*3*)	1·6 (·*3*)	1·2 (·*1*)	1·8 (·*3*)	1·6 (·*3*)	1·3 (·*1*)	2·4 (·*4*)	1·8 (·*1*)	1·8 (*1·0*)	1 (·*1*)	1·2 (·*1*)
M	11	2	IV	*d*	**4 (·8)**	**3·4 (·4)**	**4·2 (1·0)**	4 (*1·0*)	2·6 (·*9*)	3·8 (*2·2*)	6·4 (*3·3*)	2·6 (·*6*)	2·2 (·*1*)	**2 (·2)**	**3·4 (1·3)**
N	11	0	III	*a*	2 (·*8*)	5·8 (*1·5*)	4·2 (·*1*)	3·2 (·*4*)	4·4 (*1·3*)	4·6 (·*8*)	9·8 (*2·9*)	4 (·*9*)	3·8 (*1·0*)	4 (·*4*)	8 (*1·9*)
O	10	8	IV	*a*	2 (·*3*)	3 (·*2*)	1·8 (·*1*)	3 (·*8*)	3·8 (*1·0*)	2 (·*4*)	4·4 (·*6*)	2·2 (·*4*)	1·8 (·*3*)	2·4 (·*3*)	4·4 (·*7*)
P	10	6	IV	*b*	2·4 (·*2*)	2·4 (·*5*)	1·6 (·*2*)	3 (·*5*)	2·6 (·*4*)	2·6 (·*1*)	4·4 (*2·4*)	3·4 (·*6*)	2 (·*3*)	2·4 (·*5*)	3·8 (*1·0*)
Q	10	6	IV	*b*	**1·8 (·5)**	**2·4 (·5)**	**1·8 (·3)**	3·2 (*1·2*)	3·8 (*1·4*)	2·8 (*1·1*)	7·6 (*6·8*)	2·4 (·*6*)	2·4 (·*4*)	**3·4 (1·1)**	**5 (1·7)**
R	10	5	IV	*b*	**2·4 (·1)**	**2·4 (·2)**	**7·4 (2·0)**	2·8 (·*3*)	4·4 (*2·5*)	2·4 (·*7*)	6·8 (*1·6*)	3 (·*6*)	2·4 (·*2*)	**5·6 (2·7)**	**6·2 (4·0)**
S	10	3	IV	*a*	**4·8 (·5)**	**5·6 (1·5)**	**5 (2·7)**	3·4 (·*8*)	3·6 (·*9*)	2·8 (·*7*)	4·8 (*2·1*)	2·6 (·*8*)	2·8 (·*4*)	**4·6 (·8)**	**4·4 (2·3)**
T	10	3	IV	*b*	1·6 (·*2*)	2·4 (·*5*)	1·8 (·*2*)	2·2 (·*1*)	3·6 (·*3*)	2·4 (·*6*)	4·4 (*5·3*)	2 (·*1*)	1·8 (·*4*)	2 (·*1*)	2·2 (·*2*)
U	10	0	IV	*a*	**2·4 (·3)**	**3·6 (1·0)**	**2·6 (·5)**	3·6 (*1·5*)	3·8 (·*5*)	3·6 (·*5*)	9·8 (*3·3*)	2·4 (·*3*)	2 (·*1*)	**4 (·5)**	**5·6 (·3)**
V	7	6	VII	*a*	2·4 (·*3*)	2·4 (·*3*)	1·4 (·*2*)	3 (·*3*)	3·2 (·*6*)	2·2 (·*4*)	3 (·*2*)	2·2 (·*2*)	2 (·*2*)	2·4 (·*2*)	1·6 (·*4*)

NOTE. The times are given in seconds.
The larger number is in each case the median, the smaller number in brackets indicates the semi-interquartile range.
The numbers in black type indicate that the old instruction, viz. "as quick as possible," was employed with these subjects for the free associations.

Calculated according to A.M. and M.V. the following order of difficulty results:

	A.M.	M.V.
Causal	6·42	2·71
Series I	5·2	4·36
,, II	5·18	3·1
,, XI	5·06	2·27
,, III	4·65	3·02
Subordination	3·66	1·15
Superordination	3·43	·9
Series X	3·34	1·3
Coordination	3·06	1·05
Part-Whole	2·76	·63
Whole-Part	2·62	·81

Omitting Series I, II, III, the results of which are probably affected by improper adaptation on the part of some of the subjects to the work, as is evident from the large M.V. of these series, the order is as follows: Causal, Free Abstracts, Subordination, Superordination, Free Concretes, Coordination, Part-Whole, Whole-Part. The order is the same if we ignore the subjects who followed the old instruction and consider only those who followed the new.

If we calculate the order of the series omitting I to III for all subjects, according to the second method the order is: Causal, Free Abstracts, Subordination, Superordination, Coordination, Free Concretes, Part-Whole, Whole-Part, i.e., Free Concretes are reduced one place. Again, considering only the subjects who followed the new instruction in the Free Associations, the Free Concretes are brought still lower down: Causal, Free Abstracts, Subordination, Superordination, Coordination, Part-Whole, Free Concretes, Whole-Part.

The three scales may be compared thus:

Ziehen	Winteler	Present investigation
Partialising	—	Whole-part
Totalising	—	Part-whole
Contiguous	—	Coordination
Subsumption	Subordination	Free concretes
Generalisation	Coordination	Superordination
Causal	Superordination	Subordination
		Free abstracts
		Causal

It will be observed that the most trustworthy order in the present investigation agrees in the main with Ziehen's, although all his associations were free or undirected. Winteler found subordination easier than coordination or superordination, but this was doubtless due to the assistance given in this test by his instruction "Name a —," instead of: "Give an example belonging to the class —."

It is thus agreed that to find a part for the whole is the easiest test; next comes finding the whole for a given part (only one subject experienced any complication of a part attaching to many wholes). The Causal is undoubtedly the most difficult[1], and there is little difference between subordination and superordination. The scale might be said to follow generally the degree of the subject's familiarity with the terms employed in the various series.

Subjective Preferences. Each subject was asked, at the termination of his task, to state which series he preferred. There was no uniformity in the replies, but usually the more difficult series, e.g., the Causal, were chosen by pupils of high intelligence, and the easier, e.g. Part-Whole, by those of low intelligence.

Practice Effects. By comparing the results of Series X with those of Series I, and those of Series XI with those of Series II, the effect of practice can best be judged. It is evident from the A.M. of the series that there was greater improvement with concrete than with abstract terms:

Series I, 5·2; Series X, 3·34;
„ II, 5·18; „ XI, 5·06.

Pleasurable and Painful Reactions. Meumann states[2] that what excites pain appears to be better known than what excites pleasure. This he based on the examination of the mental contents of children entering school.

Wreschner[3] shows that with the two children he tested, words which denote things that excite pleasure, and those which represent what is actually pleasurable, cause quicker reactions than words which denote what excites pain or represent what is actually painful (2874σ and 3914σ as against 3686σ and 4081σ).

The terms used in Series II and Series XI of this investigation were arranged in two classes, viz. the sthenic, Justice, Health, Joy, Praise, Hope, Kindness, Pride, Honour, Honesty, Beauty, Liberty, Gladness, Bravery, Play; and the asthenic[4], Pity, Cruelty, Grief, Illness, Sorrow, Blame, Danger, Hate, Sadness, Deceit, Greed, Disgrace. Comparing the reaction times (calculated according to the arithmetical

[1] The M.V. of the Causal series shows that the children tested do not differ much from one another in their ability to give some sort of cause.

[2] *Vorlesungen*, I. 151.

[3] S. 63.

[4] For terms see Stout's *Groundwork of Psychology*, p. 191.

mean) it appears that of the fourteen subjects who followed the new instruction, six reacted more quickly both in Series II and in Series XI to sthenic terms, three in both series to asthenic terms, and five more quickly to sthenic in one series and to asthenic in the other. Of the eight subjects who followed the old instruction (i.e. replying with a word as quickly as possible) five reacted more quickly to the sthenic, two to the asthenic in both series, and one to the sthenic in one series and to the asthenic in the other. Ignoring the difference of instruction, 11 reacted in both series more quickly to the sthenic, and 5 reacted in both series more quickly to the asthenic, and 6 reacted in one series more quickly to the sthenic and in the other series to the asthenic. It appears therefore that no general rule can be formulated with respect to these classes of terms.

Analysis of Responses.

Verbal Associations. Verbal associations either in the form of word completion or in the form of a response word similar in sound to the stimulus word, occurred but seldom in this investigation. The reasons for this are that the limitation in the choice of response terms implied in the constrained associations excludes the possibility of the introduction of merely verbal reproductions, and that the use of the "new" instruction in free associations removed the necessity of answering with words at all. With subjects tested with the old instruction for free associations the average number of verbal associations was about four.

Superordination Series. Ziehen working with free associations and Winteler working with constrained associations have both come to the conclusion that, in finding the superordinate to a given concept, the more intelligent children give the genus immediately above the given concept, whereas the less intelligent give a genus more remote; this is no doubt due to the fact that with the former the system of knowledge is better articulated than with the latter. An analysis of the results of the superordinate series in this investigation tends to confirm this view.

Employing symbols used by Winteler, viz. R_1 = immediate genus, e.g. violet—flower; R_2 = a genus other than immediate, e.g. violet—plant; R_3 = a logical relation other than the superordinate, e.g. violet—rose; R_4 = no logical relation, e.g. violet—garden, or no reply[1],

[1] Winteler made R_5 = no reply, but this does not seem necessary.

and allowing $R_1 = 1$, $R_2 = 2$, $R_3 = 3$, and $R_4 = 4$, we can compare the relative values of the reactions of the various subjects thus:

Age	Intelligence	Subject	R_1	R_2	R_3	R_4	Total
13–14	*a*	*A*	7	2	1	—	14
	a	*B*	9	1	—	—	11
	b	*C*	6	3	—	1	16
	c	*D*	6	4	—	—	14
12	*b*	*E*	4	6	—	—	16
	b	*F*	8	1	1	—	13
	b	*G*	6	3	—	1	16
	c	*H*	7	3	—	—	13
	a	*I*	4	5	1	—	17
	d	*J*	4	3	1	2	21
11	*a*	*K*	6	4	—	—	14
	b	*L*	6	4	—	—	14
	d	*M*	—	10	—	—	20
	a	*N*	7	3	—	—	13
10	*a*	*O*	7	3	—	—	13
	b	*P*	7	2	1	—	14
	b	*Q*	6	1	2	1	18
	b	*R*	8	2	—	—	12
	a	*S*	4	5	1	—	17
	b	*T*	3	6	1	1	22
	a	*U*	4	6	—	—	16
7	*a*	*V*	4	2	1	3	23

In the first three groups of ages it will be observed that the subject with the highest aggregate is also the subject with the lowest intelligence; and amongst the subjects aged 10 the one with the highest aggregate is a subject of the second grade of intelligence.

Perseverance and Persistence[1]. As already mentioned, Meumann distinguished three forms of Perseverance which we may denote P_1, P_2, and P_3 respectively:

P_1 = where a previous stimulus or reaction word repeats itself;

P_2 = where a certain form of expressions keeps recurring;

P_3 = where a certain type of relation, e.g. oppositional or adjectival, determines the reproduction.

To these might perhaps be added as P_4 the repeated suggestion of the same image by different stimulus words. To denote this perseverance of content in imagery, and to distinguish it from the mere perseverance of form, the term Persistence will here be employed.

Meumann regards both perseverance and persistence[2] as indications of low intelligence, but from the present investigation it would appear

[1] Logically Persistence should be considered along with Imagery but it seems more convenient to deal with it along with Perseverance.

[2] *Experimentelle Pädagogik*, I. S. 97 and S. 91.

that the former is a much more trustworthy index of intelligence than the latter.

The subjects who were required in the free associations to answer with a word or words naturally show a higher degree of perseverance than those who were only required to indicate when an image was aroused.

The various types of perseverance and persistence may be illustrated by the following cases occurring in this investigation:

P_1. *Subject S: Series IX*, Basket—Handle, Sword—Handle, Door—Handle, Cup—Handle: *Subject E: Series III*, Shop—People, Street—People, Game—Boys, Ball—Boys: *Subject D: Series IV*, Cabbage—Vegetable, Green—Vegetable.

P_2. *Subject M: Series III*, Work—No work, Pity—No pity for you, Hope—No hope for you, Mercy—I have no mercy: *Subject Q: Series XI*, Bravery—Being brave, Gladness—Being glad, Greedy—Being greedy, Hate—Being nasty. The extreme case of this occurred with the youngest subject owing to his helplessness in the face of abstract terms. In *Series II:* Hunger—I am hungry, Illness—I am ill, Work—I am working, Pity—I am pitiful, Joy—I am full of joy; and in *Series XI*, Disgrace—I am in disgrace, Greed—I am greedy, Sadness—I am in sadness, Danger—I am in danger, Deceit—I am full of deceit, Liberty—I am full of liberty, Honest—I am full of honesty, Honour—I am full of honour, Beautiful—I am beautiful, Bravery—I am in bravery.

P_3. *Adjectival Form. Subject U: Series I*, Hat—Black hat, Lamp—Brass lamp, Cherry—Red cherry, Hill—Green hill, Castle—Big castle, Boot—Black boot, Snow—White snow, Butter—Yellow butter, Board—Brown bread board.

Oppositional Form. Subject R: Series XI, Gladness—Sadness, Honour—Dishonour, Honesty—Dishonesty, Disgrace—Graceful, Sadness—Happiness, Greedy—Ungreedy. The last example indicates the power of the type of relation over the nature of the reproduction.

Of P_4 or Persistence, with Subject C, an image of the outcasts of London on the Embankment by night, a scene which the subject had not witnessed but only heard described, did duty for Health, Joy, in Series III, and more than a week later for Sadness, Gladness, in Series XI.

With this same subject the image of a school companion in playground "screwing up his face" when eating a sour apple was reinstated in response to Bitter (Series IV), Taste (Series V), Taste (Series VI), Mouth (Series VIII). An image of a village also occurred four times with this subject.

With Subject D, the space-index was identical in twenty of his images, although the images, most of which were constructive, were different. The place to which they referred was the corner of Cowper Road, and on being questioned, the subject stated that he was seldom near this spot, but about three years before, had broken his leg there.

The various cases of Perseverance and of Persistence for the different subjects are shown in the following table.

Subject	Age		Class	Intelligence	Perseverance			Persistence
	Yrs.	Mths.			P_1	P_2	P_3	P_4
A	14	9	I	*a*	1	—	—	3
B	14	1	I	*a*	—	—	—	10
C	13	8	I	*b*	1	—	—	23
D	12	6	IV	*c*	2	—	—	4
(*E*)	12	5	III	*b*	15	—	—	1
F	12	4	II	*b*	4	—	—	6
G	12	3	II	*b*	1	—	—	4
H	12	1	I	*c*	3	—	—	—
I	12	11	II	*a*	4	—	—	4
(*J*)	12	0	IV	*d*	10	8	6	—
(*K*)	11	10	III	*a*	6	4	—	—
L	11	7	IV	*b*	7	—	—	3
(*M*)	11	2	IV	*d*	5	12	5	4
N	11	0	III	*a*	2	—	—	5
O	10	8	IV	*a*	—	—	—	2
P	10	6	IV	*b*	3	—	—	5
(*Q*)	10	6	IV	*b*	11	4	—	4
(*R*)	10	5	IV	*b*	7	—	5	6
(*S*)	10	3	IV	*a*	8	—	—	5
T	10	3	IV	*b*	7	—	—	1
(*U*)	10	0	IV	*a*	3	—	30	—
V	7	6	VII	*a*	6	9	6	4

NOTE. The letters in brackets indicate that those subjects were tested with the old instruction for free associations.

In calculating the above, the first instance was not reckoned, e.g. a series of ten in which adjectives were always applied would give only nine cases of perseverance as the first occasion cannot be regarded as a perseverance case.

ANALYSIS OF INTROSPECTION.

In this investigation the subjects experienced no difficulty in giving introspective detail. Even the youngest subject (7½ years of age) could state clearly what had passed in his mind. For example, in response to "Cruelty" he gave "Teacher is sometimes cruel to X and Y" (fellow-scholars), and stated that he had no picture of Teacher but only of X and Y: "They seem to be in school with red faces—the two of them got a good scolding yesterday." In the example "Silver—Gold" he stated that he thought of the song:

> "I'll give you silver
> And I'll give you gold."

The variety and detail in the imagery of children which so astonished Ziehen were also evident in the present investigation, and to those who witnessed any of the tests, it was the surprising feature of the experiments. The examples quoted throughout this Section illustrate this, but the following will indicate the detailed nature of the imagery frequently occurring:

Subject C. Series X. Stimulus Word, "Scissors."

Introspection: "Saw scissors in box in class-room cupboard; there was red paper on the top of the box with the word Borax printed on it. It was rather a tall cupboard and scissors were on the second shelf from the bottom. There were a lot of scissors in the box, some open some closed, not new ones, rather dark in colour."

(No definite occasion recalled—lid of box was open at angle of about 30°[1].)

Subject B. Series VIII. "Pavement—Street."

Introspection: "Saw pavement in front of Roman Catholic Church cracked all the way up."

(On examination of the pavement referred to, this was found to be the case.)

Subject O. Series IV. "Shilling—Money."

Introspection: "Saw a shilling about the size of a half-penny only silver. It seemed to be on a mantle-piece at home with a few coppers underneath. Could read what was on it—1909 and crest. Only saw shilling at first but when "money" was said the pennies underneath appeared."

Every conceivable description of imagery was exemplified throughout the course of the investigation. The following are examples:

[1] What was elicited by cross-examination is placed in brackets.

VISUAL. *Subject B. Series V. "Poem—Gray's Elegy."*

Introspection: "Just formed picture of what is given in the first verses—a ploughman going home and the sun setting."

AUDITORY. *Subject B. Series VII. "Thunder—Electricity."*

Introspection: "Heard roll of thunder then saw vivid flash of lightning."

OLFACTORY. *Subject A. Series X. "Cigarette."*

Introspection: "Saw brother's; he has a boxful of them. He told me to fetch some on Sunday evening and I took them into the drawing-room between 8.45 and 9 p.m. Smelt the cigarettes plainly, the same tobacco smell as I felt when getting them out of the box."

GUSTATORY. *Subject D. Series V. "Taste—Orange."*

Introspection: "Can see myself eating orange and feel the taste and see it as well. It seems as if it was last night."

TACTUAL. *Subject J. Series III. "Team" misread "Tame."*

Introspection: "See a boy with a black mouse and I asked if it was tame. Then he told me to hold it and I seem to feel it in my hand."

TEMPERATURE. *Subject L. Series IV. "Shilling—Money."*

Introspection: "Seemed to see a shilling in my hand at home—feel the shilling on it—it is cold and smooth."

STRAIN OR TENSION. *Subject O. Series IV. "Cabbage—Vegetable."*

Introspection: "Could see cabbages in garden with outside leaves all fading away and cabbages were getting eaten by caterpillars—feel the touch and the crackling of the veins breaking when bent. They have a cold feel—seems as if I have been pulling them up—can feel the strain in the arms."

KINAESTHETIC. *Subject S. Series III. "Ball—Round."*

Introspection: "Could see a ball going up in the air in the house—felt just as if I had been throwing it up."

(The subject's arms went up when he responded.)

ORGANIC. *Subject B. Series II. "Thirst."*

Introspection: "Recollect in Lancashire at Pendle Hill when half-way up and sitting down to tea we forgot to bring water; see heather and landscape plainly—had parched feeling in mouth."

(The thought of the scene brought this into the mouth.)

Subject V. Series II. "Hunger."

Introspection: "I am kind of hungry."

(Subject said he felt hungry at the moment then it went away.)

PAIN was also reinstated on sight of stimulus word, e.g.

Subject L. Series X. "Scissors."

Introspection: "See myself cutting a picture out last night and I cut my hand with the scissors. Felt the cut worse just now when I thought of it."

The experience of "fulness[1]" was exemplified in one instance.

Subject R. Series VIII. "Ear—Head."

Introspection: "Saw a lot of people in a crowd looking all different ways, and see and feel myself in the middle of the crowd at time of election."

(The experience was described as "a close feeling".)

The images were usually direct reproductions of percepts with quite definite limits, e.g.

Subject K. Series II. "Grief—Love."

Introspection: "See picture in front of paper with a girl stabbing a man. See my own hands holding the paper; they seem cut off at wrists."

In some cases the images were constructive never having been to the subject's knowledge previously experienced, e.g.

Subject C. Series I. "Lamp."

Introspection: "See a lamp lying in gutter in a street and water running down washing it and making it a bad colour."

(Had never actually seen this—"was like gutter in Trumpington Street".)

Images which had been previously constructed were also reproduced on occasion, e.g.

[1] Mentioned *Br. J. of Psych.* I. pp. 137, 138.

Subject P. Series X. "Barrel."

Introspection: "Seem to have a picture of Guy Fawkes putting all barrels of gun-powder into cellars and seem to see his friend outside and Guy just getting through window."

(Had not seen this in picture but had seen it in the mind when it was described by teacher a long time ago.)

Even dream-imagery was reproduced, e.g.

Subject K. Series I. "Castle."

Introspection: "Had a picture of a castle down the garden and there were some men with shields fighting, and they were burning the castle and I could smell the burning and hear the clashing of swords. I had dreamt about it last Saturday night. That Saturday I had had soldiers out playing with them down the garden. (The dream came first then picture of playing afterwards.) Castle in dream seemed to be on a hill, it looked near yet small and the men were small. Everything looked small yet it was near."

The vividness of the imagery is evident from the foregoing example. Even in constructive imagery and in the reproduction of pictures, sounds are quite commonly heard, e.g. one subject, *R*, said he seems to hear the guns when he looks at the picture of the battle of Waterloo in his home. Cf. also:

Subject Q. Series IV. "Gold—Mineral."

Introspection: "Could see men digging gold; it did not look like real gold but was brownish-coloured. Seemed to be in a foreign country. Felt the heat of the furnace where it was melted and weight of gold."

(Had seen it in a picture about a month ago.)

Verbal imagery was not uncommon and it appeared quite frequently as visual imagery, e.g.

Subject K. Series VI. "Anger—Cross."

Introspection: "Seemed to see word 'Cross' underneath (stimulus word). It appeared to be written in capitals."

It may here be remarked that verbal association does not necessarily imply verbal imagery. In fact verbal association is the type of reproduction most frequently unaccompanied by any imagery whatever, e.g.:

Subject A. Series II. "Work—Woodwork."

Introspection: "Nothing passed in the mind."

Concrete imagery may accompany verbal reproduction, e.g.:

Subject I. Series I. "Boot—Bat."

Introspection: "Saw a boy with a bat in a field."
(Thought of Bat because it sounds nearly like Boot.)

Verbal imagery may also be present without verbal association, e.g.:

Subject C. Series VIII. "Ounce—Pound."

Introspection: "Saw words Ounce and Pound in arithmetic book."

Many apparently word completions are found on cross-examination not to be real verbal associations, e.g., "Post—Post Office" might appear to be a word completion, whereas "Post—Letter Box" would not be regarded as a verbal association, yet the word "Post" in both cases aroused the same image, viz. a Post Office Pillar Box on Hill's Road near Homerton College; and in the former it was asserted by the subject that the image of the Post Box came into the mind first and then the name was sought. Again, in the case of "Boot—Lace" the image of a boot was seen and the part of it definitely chosen. In mass experiments when the subjects are not individually cross-examined, such responses would be wrongly attributed to verbal associations.

There is in some cases a displacement of part of the image or of a word, e.g.:

Subject G. Series IX. "Comb—Teeth."

Introspection: "Seemed to see a white comb right along and one of the teeth seems to be below the comb and dropping out."

Drawn thus by the subject:

$\overline{\text{VVVVVVV}_{\text{V}}\text{VVVV}}$

C O M B—(Stimulus word).

Subject F. Series X. "Barrel."

Introspection: "Remember reading it in book yesterday; see it in sentence thus:

'You're like a $^{\text{barrel}}$ on two smaller ones.'"

Series XI. "Disgrace."

Introspection: "Remember reading it in some book last night: see it and hear it:

'You are a $^{\text{Disgrace}}$ to your form.'"

In the "Part-Whole" and "Whole-Part" series the part of an object was not unfrequently reinstated independently of the whole. In some cases too the part appeared in the mind first and the whole seemed to form itself about the part.

Subject V. Series VIII. "Mast—Flag."

Introspection: "See Juno's mast. Know it is Juno's mast because I know what kind of flag the Juno has." (Had image of mast only, not of whole boat.)

(The "Juno" is the name of a pleasure-steamer calling daily at the seaport town of which the subject is a native.)

Subject L. Series VIII. "Branch—Tree."

Introspection: "Could see beech-tree and I was getting beech nuts. Saw branch up in the air first without any tree and then the tree came."

Subject J. Series VIII. "Knob—Door."

Introspection: "See a knob on class-room door—saw knob first then door seemed to grow about it."

The imagery was usually static, but one, *subject H*, was unable to image anything which did not represent action. He said that the pictures in his mind ran into one another and moved about like a cinematograph, e.g.:

Series II. "Field."

Introspection: "Saw myself walking along a field watching two men on horseback galloping round the field with some hounds following them, then they jumped a hedge and ran round next field and then they came back and took the dogs home."

(Four pictures in mind: (1) walking in field watching them run round about, (2) when they jumped one hedge and went round next field, (3) when they came back again and went home, (4) putting dogs in shed.)

The only cases resembling generic imagery, and they were very few in number, were of the following nature:

Subject G. Series IX. "Umbrella—Handle."

"Seem just to see handle—no definite handle."

Subject F. Series VIII. "Mouth—Face."

"Small picture of face, no definite face—passed through the mind."

Subject S. Series VIII. "Beak—Bird."

"Saw any kind of bird sitting on hedge; saw beak then directly afterwards saw bird."

Less than ten such images occurred in the total number of tests, 2420, and unless mere vagueness is held to constitute generality, it may almost be said that with the children tested, no generic imagery exists[1].

The time-index did not occur so frequently as the space-index, and where the latter was absent, the image was usually spoken of as appearing "in the air," or "anywhere."

The imagery was attached in some cases solely to the stimulus word, in some solely to the response word, in others to both. Sometimes the space and time indices were the same, sometimes they were different. The former are called by Ziehen judgment associations: for these the term uni-referent might be employed, and to denote the latter, the term bi-referent. "Butcher—Baker" may induce an image of two shops, a butcher's and a baker's standing together; this would be a uni-referent association. Or the butcher's shop might appear in one part of the town and the baker's in another; this would be a bi-referent association. When the image attached itself to only one word, it was usually to the particular rather than to the general term, e.g., to the stimulus word in Series IV and to the response word in Series V.

To analyse the introspective data the following table was employed:

Subject:	
Series:	
Words inducing	No imagery Images
Words inducing	Verbal imagery Object imagery
Words inducing	Constructive images Reproductive images

[1] Cf. however Galton, *Enquiries into Human Faculty* (Everyman Library, p. 77) for what *ought* to exist in such cases.

Kinds of imagery
- Visual
- Auditory
- Olfactory
- Gustatory
- Tactual
- Temperature
- Tension
- Kinaesthetic
- Organic

Indices
- Space
- Time

Images attaching to
- Stimulus word
- Response word
- Both bi-referent
- Both uni-referent

Frequency of self-projection.

Although the following table can hardly claim mathematical exactitude since the characterisation of the images had sometimes to be determined arbitrarily and in some cases even hypothetically, yet the results cannot but be regarded as more valuable than mere general impressions.

The totals for all the series in the case of each subject are given; it should be remembered that the total number of tests set to each was 110.

The main conclusion to be drawn from the results here tabulated is, that the chief differences in mental imagery revealed by association tests are individual, and not proper to any age or class. The children of most fertile imagery are not those of highest school intelligence, e.g., the subjects E, L, M, O, Q, R, T and U are best endowed with imagery, but only two of them are of intelligence "*a*," the others, with the exception of M who is marked "*d*," being of intelligence "*b*."

The following are examples of the most noteworthy types:

Excessive constructive imagery. Subject D.

This subject has a very limited range of experience and although 12½ years of age has not seen the sea or ever been in a railway train. But for this narrow range of experience he would doubtless stand much higher in the grades of intelligence.

Pronounced verbal-visual-imagery. Subject F.

The degree is evident from the table, but the vividness of the imagery may be judged from the undermentioned examples:

Subject	Grade of intelligence	Words giving: No imagery	Words giving: Verbal imagery	Words giving: Constructive imagery	Kinds of imagery: Visual	Kinds of imagery: Auditory	Kinds of imagery: Olfactory	Kinds of imagery: Gustatory	Kinds of imagery: Tactual	Kinds of imagery: Temperature	Kinds of imagery: Tension	Kinds of imagery: Kinaesthetic	Kinds of imagery: Organic	Images attaching to: Stimulus word	Images attaching to: Response word	Images attaching to: Both separately (bi-referent)	Images attaching to: Both having same space and time index (uni-referent)	Index: Space	Index: Time	Frequency of self-projection
A	*a*	6	—	7	94	5	2	3	—	—	—	—	1	67	15	4	18	86	43	7
B	*a*	8	—	3	100	3	—	1	—	—	—	1	1	77	9	6	10	76	56	16
C	*b*	2	1	9	103	4	—	1	—	—	—	—	—	84	5	2	17	76	25	—
D	*c*	—	—	39	110	7	—	1	—	1	1	—	—	75	20	—	15	89	57	53
E	*b*	—	1	1	110	49	7	3	2	—	—	—	—	9	30	1	70	105	95	—
F	*b*	5	16	1	103	16	2	3	—	—	—	—	—	60	8	10	27	86	76	17
G	*b*	—	1	2	110	5	1	—	—	—	—	—	—	61	27	1	21	88	81	10
H	*c*	6	1	1	103	21	—	2	1	—	—	—	—	58	16	1	29	98	82	93
I	*a*	12	1	11	97	—	—	—	—	—	—	—	—	55	10	12	21	29	10	1
J	*d*	—	—	2	110	42	7	4	5	—	—	—	—	26	9	1	74	109	104	34
K	*a*	8	11	5	104	10	5	1	—	2	—	—	—	23	26	—	53	77	64	53
L	*b*	—	—	1	109	36	13	4	17	3	8	9	—	59	14	1	36	108	104	51
M	*d*	1	—	1	108	27	3	2	1	1	1	—	—	30	12	1	66	108	105	34
N	*a*	—	—	—	110	10	—	1	—	—	—	—	1	42	5	—	63	94	90	16
O	*a*	3	—	—	107	43	17	8	18	10	4	2	—	52	2	1	52	105	96	14
P	*b*	3	1	3	107	9	—	1	2	1	—	—	—	59	15	2	31	102	99	6
Q	*b*	4	1	3	108	34	4	4	13	2	3	1	—	28	20	—	58	105	94	75
R	*b*	4	1	3	106	48	5	2	—	2	—	1	—	33	26	1	46	104	103	34
S	*a*	1	13	13	109	12	—	—	—	—	—	1	—	26	25	4	54	63	30	57
T	*b*	2	—	6	108	20	5	2	2	1	1	1	—	49	9	2	48	104	99	87
U	*a*	—	2	23	110	13	1	2	4	2	—	—	—	38	13	59	—	97	64	3
V	*a*	11	2	3	99	5	1	2	—	—	—	—	2	35	7	1	56	82	42	12

Series II. "Justice."

Introspection: "Could see newspaper and word 'Justice,' 'Court of Appeal,' with Lord Chief Justice underneath—could hear myself reading words as well as see them."

Series III. "Game."

Introspection: "Read in book last night tale of some boys who had gone out to the colonies and were shooting: about $\frac{3}{4}$ way down the page were the words

'Big Game Shooting.'"

Series XI. "Greed."

Introspection: "Had picture of word underneath an illustration in a book read last night.

'You can't have any more Greedy.'"

(Only part of illustration was imaged and word 'Greedy' was displaced downwards.)

Series XI. "Disgrace."

Word was seen thus:

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            | | | | | | | | | | |
| | | | |        DECEIT        | | | | |
          | | | | | |   | | | | | | |
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There was space all round the word: about five letters of previous word could be seen and five of following word. Letters appeared above, and below the ending of one word and the beginning of next.

Extreme Visualisation.

The following examples from Subject G will illustrate this:

Series IV. "Shilling—Money."

Introspection: "Seemed to see sovereigns, half-sovereigns and shillings and copper: all seem to be plastered on (stimulus) card."

Series V. "Pen—Pencil."

Introspection: "See pencil across letter E of (stimulus word) thus

P $\cancel{\text{E}}$ N.

Series VI. "Foot—Arm."

Introspection: "Seem to see a man raising his arm up

F O O T.

Man is policeman seen at Hills Road stopping cart to let lady cross."

The images were always about the size of the letters on the stimulus card. The subject stated that when his teacher described any incident[1] he saw it against the wall, e.g., in history lesson about the "Boston Tea Party" he could see the boat on the wall and Indians throwing tea overboard. When he sings he can see the notes in staff notation against the wall. Also when he wants to explain anything he pictures it, and can remember and describe it best from the picture. The subject has sometimes an abstracted look which might be taken for inattention, whereas it is really due to concentration on his own imagery. So vivid is his imagery that in some instances the image obliterates whatever is in the field of perception against which the image is projected.

Imagery in motion. Subject H.

Practically every image was of some action, and this subject could not keep an image fixed in his mind. The images kept dissolving into one another, but when he saw himself he was usually in a standing position. This characteristic was also observed in isolated cases with other subjects but was infrequent. As it is customary to divide the stages of development of observation into (1) the substance stage, under 8 years of age; (2) the action stage, 8 years old; (3) the relation stage, from 10 to 11 years of age; (4) the quality state, about 14 years of age, the imagery of the subject in question would correspond to the second stage.

[1] The subject used the word "we," evidently under the impression that all persons image in the same way as himself.

No other pronounced type of imagery was found although the imagery of Subject S was extremely vague and indeterminate. Some of his images he described as "like a film."

Relation of Imagery to Perception.

In the child's thinking the percept and the image have practically the same characteristics. The abruptness and consequent degree of "vivid intensity," which, it has been maintained[1], are the distinguishing features of the percept, are also characteristic of the imagery of the child, as is evident from examples given above. The two processes however would seem to develop along different lines; for, whereas the percept acquires increasing definiteness, the image becomes more and more vague and fragmentary, losing the space and time indices which belong more properly to the perceptual than to the ideational aspect of experience. This may be accounted for on the principle of the dissociation of the perceptually excited elements of consciousness from the ideationally excited factors and may be looked upon as but an instance of the differentiation of function which is characteristic of most forms of development. The adult when he works perceptually is not consciously much troubled with associations, and when he works ideationally he can largely ignore the presence of perceptual factors in the field of consciousness: the child however lives on the ill-defined borderland of both worlds, and easily passes from one to the other: he likewise readily confuses the two spheres of perception and imagination.

Self-projection.

A fact which has not been noted in previous investigations is the self-projection of the subject into the image. R. L. Stevenson describes it so well in a passage referring to the imagery in children's thinking that we make no apology for quoting it.

"Rummaging in the dusty pigeon-holes of memory," he writes[2], "I came once upon a graphic version of the famous Psalm 'The Lord is my Shepherd,' and from the places employed in its illustration, which are all in the immediate neighbourhood of a house then occupied by my father, I am able to date it before the seventh year of my age, although it was probably earlier in fact. The 'pastures green' were

[1] Carveth Read on "Percepts and Images," *Br. J. of Psych.* II. 324.

[2] *Essays of Travel: Random Memories.*

represented by a certain suburban stubble-field, where I had once walked with my nurse, under an autumnal sunset, on the banks of the water of Leith; the place is long ago built up; no pastures new, no stubble-fields; only a maze of little streets and smoking chimneys and shrill children. Here in the fleecy person of a sheep, I seemed to myself to follow something unseen, unrealised, and yet benignant; and close by the sheep in which I was incarnated—as if for greater security—rustled the skirts of my nurse. 'Death's dark vale' was a certain archway in the Warriston Cemetery: a formidable yet beloved spot, for children love to be afraid,—in measure as they love all experience of vitality. Here I beheld myself some paces (*seeing myself, I mean, from behind*) utterly alone in that uncanny passage; on the one side of me a rude knobby, shepherd's staff, such as cheers the heart of a cockney tourist, on the other a rod like a billiard cue, appeared to accompany my progress: the staff sturdily upright, the billiard cue inclined confidentially, like one whispering, towards my ear."

This reflexive attitude in children's imagery—the seeing oneself from behind—as Stevenson describes it, is surprisingly common in the imagery of the subjects tested in the present investigation. From the youngest to the oldest it appears in all the subjects save two, and with some it occurs with almost every image. It was first noticed in the second series put to the first subject, i.e. on the second day of investigation. The image which the term "Cruelty" aroused was that of the subject standing glancing at the headings of the *Weekly News* for the previous Friday. (One of the articles dealt with a man who was summoned for ill-treating a horse.) On the same day the third subject tested, in response to the term "Illness," imaged himself sitting in bed reading after an attack of measles. This self-projection was taken as a matter of course by the subjects; the youngest alone remarked upon it on its first appearance. In reply to the word "Butter" in the first series he said he saw a dairy and "me going away with butter," and remarked that "it was a funny thing to see—me going away with butter."

The following indicate the various positions which the self assumes in the images:

Subject D. Series I. "Cherry."

Introspection: "Can see a cherry tree and me and Mr W——. Mr W—— is picking cherries and I am holding the basket for him."

(About a month ago this happened. Saw front view of self half-way up ladder. Seemed to feel weight of basket.)

Subject F. Series XI. "Liberty."

Introspection: "Remember speaking about it this morning at Scripture lesson. Can see and hear myself talking in answer to teacher's questions. (Front view of self as if in teacher's place.)

Subject F. Series X. "Envelope."

Introspection: "Had picture of one lying on the floor at breakfast time. Had picture of myself going to pick it up."

(All one picture. Side view of self.)

Subject G. Series III. "Shop."

Introspection: "Can see Hallack and Bond's and a lot of people buying things. Can see mother and myself inside buying things. Saw it on Saturday evening.

(Appeared just as it was on Saturday evening. Can only see back of myself. Can feel smell of shop, of bacon and of coffee, but can hear nothing.)

Subject J. Series IX. "Sword—Handle."

Introspection: "See a sword: it has a handle that was crooked. I took it up yesterday afternoon at home and found it was heavy. See myself" (back of myself).

Subject K. Series VI. "Sun—Moon."

Introspection: "Seem to be looking up into the sky and to see the moon. The sun was setting and the moon was coming up. It was last Thursday night. See myself as I was looking up" (back of self).

There does not seem to be any relation between this self-projection and age or intelligence; the table given above indicates the number of times it occurred with the 110 tests to each subject.

Owing to the novelty of the self-projection in the imagery of the children tested at the Morley Memorial School, it was deemed advisable to confirm the results by testing the pupils of another school. The writer was kindly permitted by the head-master to put tests to some of the boys of the Milton Road School, Chesterton. This school is a mixed institution and the pupils are of about the same social status as the Morley Memorial School children. Six boys of from 12 to 12½ years of age were tested with free associations; five boys with ten examples each and one boy with twenty examples.

In the case of four of the boys self-projection did not occur in the images aroused by any of the ten words presented. On cross-examination at the conclusion of the test, these subjects admitted that in dreaming they frequently see themselves, but they denied that they ever see themselves in their imagery.

With one of the other subjects self-projection occurred with the fourth example put to him, viz. Scissors: "Can see myself using scissors cutting a piece of cloth. Have cut my finger and am cutting piece of rag to bind it up, etc." (Front part of self seen.) With this subject self-projection was present in five out of ten terms presented, and in one case he saw his back, viz. Rain: "See myself standing in street and all of a sudden the rain starts coming down and I start to run home." (See back of self.)

With the sixth subject self-projection occurred in the first example after the explanatory test, and in fifteen out of the twenty terms presented. In seven cases the subject saw his own back.

With a Scots' boy of nine years of age to whom the whole 110 tests were put, self-projection was illustrated in the following cases:

Series II. "Hunger."

Introspection: "When I came out of bed this morning I was hungry. See me jumping out of bed." (See whole of self, front view.)

Series VIII. "Platform—Station."

Introspection: "See friends coming from station and me and my father going to meet them." (See whole of front of self.)

Relation of Imagery to Course of Thought.

In some cases, as already stated, imagery attached to only one term of the association. For instance, it might attach itself to the stimulus word only, as:

Subject I. Series XI. "Honour—Truth."

Introspection: "See honour board in school with names on it (had no image of truth).

(Could not trace connection between honour and truth.)

Or to the response word:

Same Subject. Series VI. "Pencil—Pen."

Introspection: "Did not see pencil—thought only of ordinary pen." (Saw pen—no special place.)

The frequency with which this occurred makes it impossible to explain it as failure to introspect adequately.

In some cases (75 out of 2420) no imagery at all was present, e.g.

Same Subject. Series IV. "Pansy—Flower."

Introspection: "Just thought of pansy then of flower, but of no particular flower."

(Had no image of pansy and no image of flower.)

Or, *Subject H. Series VI. "Taste—Smell."*

Introspection: "Nothing seemed to pass in mind."

The image may attach itself to neither the stimulus word nor the response word, but to some intermediate thought, e.g.

Subject K. Series IV. "Apple—Fruit."

Introspection: "See a small apple tree in garden and I was putting a stake to it."

(The tree was bare—had no image of apple or of fruit.)

Or a better example:

Subject F. Series VII. "Evil—Temptation."

Introspection: "Had picture of the word sin printed 'SIN.'"

(Thought of Evil—Sin—Temptation.)

Not infrequently the course of imagery was opposite to the course of thought, e.g.

Subject C. Series VI. "Pencil—Pen."

Introspection: "Could see some pens lying on teacher's desk in class-room in pencil box along with some pencils. Saw pens before the pencils."

Subject E. Series X. "Hail—Rain."

Introspection: "See it pouring rain on Hills Road about two or three days ago—hear it—see hail after the rain."

Subject B. Series VIII. "Boiler—Steamship."

Introspection: "Saw steamship at Felixstowe then big boiler in Pumping House at Stopford."

These examples tend to support the view held, e.g. by Binet[1], that thought without images is possible. He also states that the complete absence of the image is very rare in the cases where the end avowedly is to provoke images, but that what is more often produced is a lack

[1] *L'Étude experimentelle de l'Intelligence*, Cap. VI. La Pensée sans images.

of correspondence between the thought and the image; he also states, however, that the rule is not the incoherence of the image but its correspondence with the thought[1].

In this investigation the images aroused could in no instance be characterised as wholly irrelevant. In fact the irrelevance of imagery frequent in adult thinking[2] is probably a feature distinguishing the thinking of the adult from that of the child. But it is not the case, as stated by Ziehen[3], that the association of ideas of the child actually differs *toto coelo* from that of the adult.

Meumann discovered[4] amongst adults, chiefly students, a certain percentage with whom abstract terms were always accompanied by an astonishingly lively concrete content; and in the present investigation some trial tests made with certain students produced similar results. Meumann maintains that this concrete content has with the adult quite another value than with the child. Whereas it often forms the only content of the words for the child, for the adult it serves only as a connecting link and support of the logical relations which constitute the essential content of the word. This distinction might be made clearer if the imagery attaching to connected discourse were investigated in children.

Conclusions.

The main conclusions may be recapitulated thus:

Whereas Ziehen affirms that the speed of association increases with age, the present investigation confirms the view of Wreschner and Winteler that for different children the speed of association bears no direct relation to age, and has little value as an indication of the standard of intelligence of the subject. No conclusion however can be drawn from the present results as to the relation of the speed of association to age in the case of the same child.

The order of difficulty, judged by the rate of reaction, of the various processes agrees generally with the order suggested by Ziehen, although Ziehen's results refer only to free associations. The order in this investigation is as follows, the easiest being placed first: whole-part, and part-whole, coordination, free concretes, superordination, subordination, free abstracts, and, most difficult of all, causal relations.

The more intelligent subjects generally prefer the more difficult series, the less intelligent the easier series.

[1] *L'Étude exp. de l'Intelligence*, Cap. VI.

[2] Cf. Stout, *Analytic Psychology*, I. pp. 82–85.

[3] p. 32.

[4] *Archiv*, Bd. IX. S. 133.

The speed of free associations increases with practice, but the effect is greater with abstract than with concrete terms.

No principle can be established as to the relation of the rates of reaction in the case of sthenic and asthenic terms respectively.

The new instruction for free associations employed in this investigation may be regarded as successful in that the attunement which results gives more satisfactory responses, and enables the subject to give a better introspective report than is the case with the old instruction, and at the same time the speed of reaction is not appreciably altered.

Examination of the responses shows that when a superordinate to a given concept is required, the more intelligent children give a proximate genus and the less intelligent a more remote genus. This agrees with the conclusions of Ziehen and Winteler.

The degree of Perseverance varies inversely with age and intelligence. The present investigation confirms the results of previous workers on this point.

Persistence, by which is here meant the repeated reinstatement in consciousness of the same image, and which in this connection does not seem to have been previously investigated, is not so trustworthy an index of intelligence as Perseverance.

The present investigation confirms the statement of Ziehen as to the astonishing definiteness and vividness of the child's imagery which has much the same characteristics as its perceptual experience, viz. vividness and definite spatial and temporal localisation: it also supports the general view as to the great predominance of concrete visual imagery over other forms of imagery accompanying the thought-processes of the child.

The differences in the types of imagery appear to be individual differences rather than, as Ziehen suggests, correlated with age.

Children who are best endowed with respect to the various forms of imagery do not, it would appear from the analysis of introspection here undertaken, necessarily stand highest in school.

The factor of self-projection in children's imagery appears to have been neglected by previous investigators.

Thought as implied in constrained associations is, in the case of children, possible without imagery, as Binet contends. Where imagery is present the image may attach to only one term of the association, and the course of imagery may be opposite to the sequence of thought. The imagery present can in no single case be described as wholly irrelevant.

THE TRANSFER OF IMPROVEMENT IN MEMORY IN SCHOOL-CHILDREN[1]. II.

By W. H. WINCH.

I. The problem stated.—II. First Series of Experiments, School "E.F."—III. Second Series of Experiments, School "E."—IV. Third Series of Experiments, School "I."—V. Summarized Conclusions.

I. The Problem Stated.

In a previous piece of work dealing with this subject[2], I endeavoured to show that children whose rote memories had been improved by training in one medium—for example, in poetry—became thereby more proficient in rote memory for subject matter of a different kind—for example, for geography or history.

Rote memory, doubtless from its previous excessive use and estimation, has been educationally under a cloud for some years past, perhaps unduly so. But whether that be the case or not, it is commonly thought to be of more importance to carry away accurately the *substance* of what is heard or read than the actual words. It is not indeed uncommon to find the one capacity placed, as it were, in antithesis to the other. It is often supposed that proficiency in rote memory is not positively related to proficiency in substance memory, and that improvement in the former brings with it no improvement in the latter. It becomes a matter of concern to the educationist, since it is of importance to him whether the exercise of the rote memory in the acquisition of certain educational elements furthers or hinders the

[1] An outline account of the above experiment with the suggested conclusions was given in a paper read at a joint meeting of the Aristotelian and British Psychological Societies and of the *Mind* Association, in London, in July, 1910.

[2] *British Journal of Psychology*, Vol. II. p. 284.

growth of that kind of memory which we all desiderate, namely, an accurate substance memory.

The following experiments were undertaken with a view of helping towards a solution of some of these questions. An attempt was made to discover

1. Whether there is any transfer of improvement in rote memory for meaningless things to substance memory for stories (i) by an auditory method, (ii) by a visual method.

2. Whether there is any transfer of improvement in rote memory for things with meaning—for example, poetry—to substance memory for stories.

II. First Series of Experiments. School "E.F."

A first series of experiments was carried out in a municipal school situated in a good neighbourhood. The school was organized as a Junior Mixed, and the whole of the first class, both boys and girls, comprising the upper division of a well-advanced but old Standard III, whose average age was 10 years 1 month, was set to do the work.

On one day in three successive weeks preliminary tests were given in substance memory. Side by side with these were given some tests in rote memory for meaningless things. The latter were done in order to provide material for finding the correlation between the two sorts of memory. In the fourth week, the class was divided into two equal groups on the results of the tests in substance memory. One of the two groups was practised once weekly for three successive weeks in rote memory whilst the other group drew difficult geometrical designs. In the seventh week the two groups were placed together again and worked a further test in substance memory. The results of this final test may enable us to tell whether the group practised in rote memory does better work in substance memory than the group not thus practised.

i. *Chronology of the Series.*

The following is a brief chronology of the work :—

Jan. 15th, 1909. 11.13 a.m. to 11.32 a.m. The whole class worked a test in substance memory. After a short interval, during which the room was cleared and the children walked quietly to and from the playground, a test was worked of rote memory for meaningless things from 11.47 a.m. to 11.57 a.m.

Jan. 22nd. 11.5 a.m. to 11.23 a.m. The whole class worked a second test in substance memory. After an interval occupied as on the previous occasion, a second test in rote memory was done from 11.42 a.m. to 11.52 a.m.

Jan. 29th. 11.20 a.m. to 11.38. The whole class worked a third test in substance memory, and after an interval, occupied as before, worked a third test in rote memory from 11.45 a.m. to 11.55 a.m.

The class was now divided into two groups equal in substance memory.

Feb. 5th. 11.15 a.m. to 11.35 a.m. One group, hereafter called the practised group, worked a practice exercise in rote memory for meaningless things, whilst the other group drew geometrical designs in another room.

Feb. 12th. 11.21 a.m. to 11.41 a.m. The practised group worked a second exercise in rote memory, whilst the non-practised group drew designs as before.

Feb. 19th. 11.20 a.m. to 11.40 a.m. The practised group worked a third exercise in rote memory, whilst, as before, the non-practised group drew geometrical designs.

In all respects other than in the exercises specifically mentioned above, the curriculum for the two groups remained the same during the period of the experiment, and on

Feb. 26th, 11.10 a.m., the whole class worked a final test in substance memory.

A reference to the calendar will show that all the tests and exercises were done on Friday. It is of advantage to the regularity of the results if they are obtained on the same day of the week and after the same school lessons.

ii. *Tests employed and Method of Marking.*

(*a*) Specimen substance memory test.

The following passage was read to the children three times slowly: "Nero is a large Newfoundland dog. He lives at a farm-house, and is more useful than many boys are. Indeed his master says that he really could not get on without his faithful helper. Nero's master and mistress are very proud of him, and often tell people what a useful and clever dog he is. One day there was a visitor, who could hardly believe the stories they told about this wise animal."

The first reading occupied 1½ minutes, the second 1 minute, and the third 1 minute. The children were told that they were not required to remember the actual words; they could write out afterwards all they could remember in their own words. Just before the papers were collected, they were told that if they found they had omitted anything, they could write at the bottom of their papers, "I forgot to say so and so." In 15 minutes from the time of commencing writing, all the children had finished writing all that they could remember.

The tests were marked on a system of mnemonic units which depends upon the mental stage reached by the class experimented with. I have explained the method in my research on "Fatigue in Evening Schools[1]," so that a brief reference here will be all that is necessary. Speaking generally, we aim to estimate as units those pieces of the total story which go in and out of consciousness together. We make a preliminary analysis, and work tentatively on that. In the course of our marking we are fairly certain to come upon the work of some child who has divided something which to us seemed unitary, that is, he has remembered one piece of it and forgotten the other. We add one to our units and go back again through the papers we have already marked. After a few experiences of this kind, we usually find ourselves in possession of an analysis, which, so long as we do not use it for classes of very inferior mental ability, will serve our purpose quite well. Speaking broadly, it is true to say that subjects, predicates and objects (where there are any) are, at this mental level, remembered or forgotten as a whole, and receive one mark; adjectival or adverbial adjuncts receive one mark; and connecting words other than "and" receive one mark. It is not theoretically satisfactory to regard these various constituents of substance memory as quite equal; the adverbial relations and the connectives indicating relationships between clauses are usually more difficult to remember than adjectives and subject—predicate—object. If many thousands of these papers were done under very varying conditions of teaching and the errors tabulated for various ages, we could obtain a scale of marks more satisfactory theoretically. But, in practice, it is quite possible to get valuable results with the system of marking above outlined. And, of course, it must be remembered that the reproduction of meanings only is required, not the words, nor the order of the words actually read to the children. A mark is allowed for any expression which conveys the requisite idea; indeed, I go further, and allow a mark for

[1] *Journal of Educational Psychology*, 1910, p. 85.

anything which *to the child's mind* is mentally equivalent to a unit in the test.

The following piece of analysis may be of service:

Nero is a dog
large Newfoundland
Nero lives at a farm
Nero is useful
more than
boys are
many
Indeed
Nero's master says
sometimes
that he could not do without him
really the helper
faithful

In one case above, where one unit only is given for "Nero lives at a farm," it appears that the general direction to count adverbial adjuncts as separate from subjects and predicates has been disregarded. It was thought, however, that "lives" was so existential a predicate that the real predicate in that case was not "lives," which might, indeed, have been taken for granted, but "lives at a farm." No additional meaning appears to be conveyed by the word "house" attached to "farm," so that its presence or absence was disregarded in the marking. If a child wrote, "Nero was a farmer's dog," it was of course held to be equivalent to the first and third units of the analysis given above.

(*b*) Specimen test of rote memory for meaningless things.

v j d m
g c w s

The children first learnt how to write down the letters they could remember when tests like the above had been read aloud to them. Then *five* of these as tests were given on each morning when the preliminary substance memory tests were also given; and *ten*, as training exercises, on the three occasions in subsequent weeks to the practised group only.

Each test was called out twice in 30 seconds; after which, as much of it as could be remembered was written down, 1 minute and 30 seconds being allowed for this. No apparent or audible articulation was permitted. The worked papers were marked on the following scale: three marks were allowed for each consonant correctly remembered and correctly placed, two marks if it were remembered but was one place out, and one mark if it were two places out. It would have been more satisfactory, theoretically, if eight marks had been allowed for each consonant correctly remembered and correctly placed, as it is possible for a consonant to be remembered seven places out, and the remembrance even in such a misplaced way is worth something; whereas, on the system of marking actually used, any consonant displaced more than two places received no mark at all, even though it really occurred in the test. In practice, however, it was extremely rare to find a case in which any injustice was done by the easier system of marking actually adopted. It was useful to request the children to insert a "nought" in any position when they knew they had forgotten the letter which stood in that place. I will give a marked paper as illustration:

v	j	m	d
3	3	2	2
g	o	t	c
3	0	0	1

in which a total of 14 marks is obtained out of a possible maximum of 24.

iii. *Results.*

The first step in the analysis of the result was the division of the class into two equal groups for substance memory. An indication of the division follows:

Group A	Total marks for 3 stories	Group B	Total marks for 3 stories
1. K. A.	67	1. R. F.	67
2. S. D.	58	2. G. A.	63
3. S. V.	57	3. H. M.	56
....	...	....	...
23. C. M.	30	23. F. W.	29
	1067		1068

Then, in the list of children in Group B, the total mark for the three tests in rote memory, which were done *pari passu* with the substance memory tests, was inserted and the two series correlated. I was aware from previous work that rote memory was to some degree positively correlated with substance memory in school children, and this work was done as a means of ascertaining whether low positive correlation would be accompanied by a transfer of improvement from one of the correlated functions to the other and, if so, to what extent.

The correlation was not worked out for Group A, since this group was to form the half of the class unpractised in rote memory, and consequently no question of transfer could arise.

TABLE I.

Showing the correlation for Group B only in
(*a*) *Substance memory.*
(*b*) *Rote memory for meaningless things.*

Marks for substance memory	No. of children	Av. mark per child, substance memory	Av. mark per child, rote memory
Over 55	3	62·0	235·0
55 to 50	5	53·6	190·0
50 to 45	4	47·0	215·5
45 to 40	5	43·4	230·8
40 to 35	2	39·0	185·5
Below 35	4	32·8	172·5

An inspection of the above table shows that some positive correlation is probable, but that it is likely to be small in amount. Worked out from the 23 individual cases by the Pearson formula $r = \frac{\Sigma xy}{n\sigma_1 \sigma_2}$, r is found to be $+\cdot 262$, which, with such a small number of cases—the probable error being more than $\cdot 1$—must be regarded as a decidedly low value.

iv. *Improvement within the practice medium itself—rote memory for meaningless things.*

All the children, it will be remembered, had worked three series of 5 tests each *pari passu* with the tests in substance memory; the first week's average mark per child per test was 11·9, the second week's mark was 14·3, the third week's mark was 13·8. Then Group A discontinued memory work, whilst Group B worked three series of 10 tests each in three consecutive weeks. The average mark per child per test for the first week was 15·1, for the second week was 15·0, and for the third week 15·8.

It is a long way from 11·9 to 15·8 marks out of a maximum of 24, but it must be remembered that *both* groups worked the first three sets of tests, the *additional* practice of the practised group was that done during the last three weeks only. How far has the improvement due to this *additional* practice been transferred to the final exercise in substance memory?

Unfortunately, three children in the non-practised group were absent on the occasion of the final test. C. C. had left the school, M. S. was suffering from a protracted illness, and K. H. was staying in the country. This group was now reduced to 20. To balance this loss, L. M., P. B., and E. B. in the practised group, who in the preliminary tests were equal to C. C., M. S., and K. H. respectively, were omitted from the final table of results, which follows.

TABLE II.

Showing the practised and non-practised groups compared in
(*a*) *The preliminary tests in substance memory.*
(*b*) *The final test in substance memory.*

	Non-practised group			Practised group		
Marks for 3 prel. tests in substance memory	No. of children	Av. mark per child per test in 3 prel. tests	Av. mark per child in final test	No. of children	Av. mark per child per test in 3 prel. tests	Av. mark per child in final test
Over 55	3	20·2	21·3	3	20·7	22·0
50 to 55	4	17·9	20·0	4	17·9	22·7
45 to 50	4	15·7	17·2	3	15·8	18·0
40 to 45	4	14·5	16·5	5	14·5	17·8
35 to 40	—	—	—	1	13·0	17·0
30 to 35	5	11·1	13·8	4	10·9	14·2

Is is obvious that steady improvement has occurred in all sections both of the practised and non-practised groups. Much of this is doubtless due to growth; some of it may be due to the influence of the other subjects of the school curriculum; but the point of interest lies in the comparison of the corresponding sections in Groups A and B, the non-practised and practised groups. It will be seen that, in every case, when the corresponding sections are compared in the work of the final test, the section belonging to the practised group is superior. There is a steadiness and constancy about the transferred improvement which the low correlation of + ·262 might not have led us to expect. Let us make some further rough quantitative comparisons. The additional practice of Group B in rote memory caused an advance from 14·0 (the third week's average mark per exercise for the members of Group B) to 15·8 (the average mark per exercise in the sixth week). This is a rise of approximately 13 °/ₒ. But some of this was doubtless due to growth and would have occurred *without* the additional practice of Group B over Group A.

Comparing the results for substance memory in the non-practised group, we see that without intermediate memory practice and after three weeks interval each section shows improvement in the final over the preliminary tests. This we shall consider an improvement due to growth. Turning now to the results for the practised group, we again find an advance in the final test on the results of the preliminary tests. There is every reason to suppose that there has been also in this case an improvement due to growth, but there is a bigger advance, section by section, on the preliminary work than in the case of the non-practised group. It seems fair to attribute this excess to the intermediate work in rote memory for meaningless things done by this group only. Group A rises from an average of 15·4 in the preliminary tests, with a mean variation of 2·5, to an average of 17·0 in the final test, with a mean variation of 3·2. Group B—the practised group—rises from an average of 15·5 in the preliminary tests, with a mean variation of 2·5, to 18·7 in the final test, with a mean variation of 2·9. There has been an improvement of approximately 10 °/₀ in Group A which we have decided to consider as an improvement due to growth. Let us suppose that 10 °/₀ of the improvement shown in Group B is also due to growth. This is not a very unreasonable supposition, since the groups as a whole, and indeed section by section, were very much alike to start with. Adding 10 °/₀ to 15·5 we get 17·05. Actually we have an average mark of 18·7, an excess of 1·7 over the 17·05. Let us regard this, as indeed we must do, unless it is a chance result, as due to the intermediate practice in rote memory. Now 1·7 on 15·5 gives a percentage improvement of 11 °/₀. The improvement in rote memory for the same group was, we may remember, 13 °/₀—a figure which included improvement due to growth. Just how much of this 13 °/₀ is due to growth without practice there is no means of telling from the data of this experiment, it is probably more than 2 °/₀ at any rate. So that we are faced with a rather striking result, namely, that about as much or more improvement, reckoned in percentages, as has been made in the practice medium itself—rote memory for meaningless things—has been transferred to the substance memory.

There is an alternative—the higher average in the final test of Group B as compared with Group A may be a *chance* result.

But we need merely to estimate the chances against *all* the averages for Group B, section by section, varying in one and the same direction as compared with the averages of the corresponding sections of Group A, to see how unlikely this is.

III. Second Series of Experiments. School "E."

The second series of experiments was carried out in a municipal girls' school situated in a very poor neighbourhood. The children were rather backward for their ages, though proficient in school work of a mechanical kind. All the children in Standard IV (of an average age of 10 years 8 months at the commencement of the exercise) did the work, which, in this case, consisted of a series of visual tests and exercises in rote memory for meaningless things and substance memory for stories. Four tests in substance memory were given—two in a week—and on the results of these tests the class was divided into two equal groups. Then, for a period of several weeks, one of the groups did exercises in rote memory, whilst the other group worked exercises in arithmetic. In all other respects the school curriculum of the two groups was exactly the same. At the end of the practice period the two groups worked four more tests in substance memory.

i. *Chronology of the Experiment.*

A brief chronology of the experiment follows:

On Oct. 20th, at 11.20 a.m., all the class worked a test in substance memory: then on Oct. 22nd, a second; on Oct. 27th, a third; and on Oct. 29th, a fourth. All the tests were taken at the same time of day. The practice exercises in rote memory were taken by one group only, and began on Nov. 10th. Further exercises were worked on Nov. 12th, 17th, 19th, 24th, and 26th. Then, finding that the exercises succeeded each other too rapidly for the satisfactory production of what is known as improvement by practice, the intervals between the exercises were changed, and the exercises were taken one in each week instead of two. Further exercises were given on Dec. 1st, 8th, 15th, and 22nd. The Christmas holidays then intervened. The exercises were resumed and continued on Jan. 12th, 19th, and 26th. In all, thirteen exercises in rote memory were given, always at the same time of day. At the conclusion of this practice period, four more tests were given in substance memory for stories to both groups, to the group practised in rote memory and to the group not thus practised. The substance memory tests were given at 11.20 a.m. on Feb. 2nd, 4th, 9th, and 11th.

ii. *Specimen tests and exercises.*

(*a*) Test in substance memory.

"Two boys lived in a cottage by a river. Their father made them a little boat with a white sail. He promised he would come home early one day and they would sail the boat on the river. Not far away there was a mill and the miller had a big dog which looked very fierce. Several days passed but the boys' father could not leave his work. So the eldest boy said, 'Let us go and sail the boat by ourselves.' They went to the river and placed the boat on the water. The wind caught the sail and blew the boat along. The boys ran by the side shouting with glee. Suddenly there was a loud splash and the miller's dog sprang into the water. He was going to swim across to the other side but the little boat was in his way. He snapped at it with his strong teeth and smashed the pretty toy so that it would no longer float. The boys sorrowfully gathered up the pieces and went home to tell their father."

The stories were given to the children in writing and they were required to study them visually without audible articulation. Six minutes were allowed, after which they wrote down immediately what they could remember. They were given to understand that the exact words were not required, that they were to think of the meaning, not the words. They were allowed to insert, after reading their written work before giving it up, anything which they were aware they had omitted. The system adopted in analysing and marking tests in substance memory has already been explained—identical procedure was adopted in this case.

(*b*) Exercise in rote memory for meaningless things.

Eight consonants arranged in two lines were exposed for 30 seconds and the children were required to learn them visually without audible articulation. They were directed to fix their attention on the consonants successively from left to right, on the four consonants in the upper line first, then on the four in the lower line, and to keep on doing this until they were told to write them out. One minute 30 seconds was allowed for writing them down. Nine of these exercises were given on each occasion. The worked papers were marked exactly like those of the auditory exercises in the preceding series of experiments.

iii. *Results.*

I will show first the general correlation between rote and substance memories for the practised group only. It will be remembered that both groups of children worked all the tests in substance memory, whilst only the practised group worked the exercises in rote memory. It is not quite satisfactory to calculate correlation from exercises that are not worked during precisely the same period. In this case the rote memory exercises were done in the period intervening between the four preliminary tests in substance memory and the four final tests; but since there are 8 substance memory tests and 13 exercises in rote memory the resulting correlation cannot fail to have some value. No preliminary exercises preceded the first of the thirteen, so that it was thought fairer for correlation purposes to omit the first one; the correlation is therefore worked on the results of twelve exercises in rote memory.

TABLE III.

Showing the general correlation between
(*a*) *eight tests in substance memory,*
(*b*) *twelve sets of exercises in rote memory.*

Marks for substance memory	No. of children	Av. mark per child in substance memory	Av. mark per child per exercise in rote memory
Over 340	1	342·0	199·3
320 to 340	3	327·3	148·5
300 to 320	2	313·5	161·8
280 to 300	6	287·3	156·7
260 to 280	5	266·4	146·5
Below 260	4	245·0	150·6

It is probable from inspection of the table that some positive correlation exists, but the sequence for the marks for rote memory is too broken to allow us to feel much confidence. Worked out from the individual cases by means of the Pearson formula, the coefficient is found to be $+{\cdot}208$, which, with a probable error of more than $\cdot 1$, scarcely gives us a result from which we can argue with any confidence except that we should be entitled to say, if positive correlation exists, that it is very small. Two children, L. R. and E. C., omitted two of the four final tests in substance memory; they are excluded from the above table.

iv. *Improvement within the practice-medium itself.*

I will now show the improvement made by the practised group in rote memory for meaningless things. In the first set of exercises the average mark per child was 148·2; but, for the reason just given, I do not propose to include this set in calculating the improvement made. I shall measure this by taking the average of the 2nd, 3rd, 4th and 5th sets of exercises as indicating the initial position of the children, and the average of the 10th, 11th, 12th and 13th as indicating their final position. The average of the former series is 153·6, with a mean variation of 15·3, whilst the average of the second series is 164·2, with a mean variation of 19·5. If the percentages of improvement are calculated for each individual, it is found that, of the 23 cases, 16 show improvement, one remains as at first, and six show a loss. Of the 16 cases which improve, 1 improves 23 %; 1, 18 %; 1, 17 %; 3, 16 %; 1, 15 %; 1, 12 %; 1, 11 %; 1, 9 %; 1, 8 %; 3, 7 %; 1, 3 %; 1, 2 %. Of those which show a loss, 1 loses 8 %; 1, 7 %; 2, 6 %; and 2, 2 %.

It is not asserted that the whole of this improvement—about 7 %—is a practice improvement; some of it is doubtless due to growth, by which I mean a natural change which would have taken place in the direction of greater facility even if no exercises had been given between the earlier and the later series. But there are no means of determining within the data of this experiment how great this change would have been.

v. *Transfer of improvement from rote memory to substance memory.*

Let us see how far, if at all, the improvement in rote memory is transferred to substance memory. I shall measure the improvement in substance memory by considering the average of the four preliminary tests in substance memory as the initial position of the children, and the average of the four final tests in substance memory as their final position. Without doubt we have a factor here also due to growth or the influence of the other subjects of the school curriculum; but in this case we have a means of estimating it. For we have two equal groups, equal, that is, in the initial series. Both show improvement, and it seems fair to suppose that the *excess* of improvement in the practised over the unpractised group may be a measure of improvement due to the special practice in rote memory, an improvement from which natural growth and the influence (if any) of the other subjects of the school curriculum has been eliminated.

We must, I think, be prepared to admit that some transfer of improvement has taken place unless we are willing to suppose that five sections out of the six into which Group B is divided come out higher in the final tests than the corresponding sections in Group A as a matter of chance only.

Let us consider the groups as wholes with a view of getting an approximate measure of general improvement. Thus, Group A scores a total mark of 2932 in the preliminary tests and 3394 in the final tests, whilst Group B scores 2944 in the preliminary tests and 3574 in the final tests.

TABLE IV.

Showing the practised and non-practised groups compared in
(*a*) *Four preliminary tests in substance memory.*
(*b*) *Four final tests in substance memory.*

	Unpractised group A			Practised group B		
Marks for 4 prel. tests	No. of children	Av. mark in 4 prel. tests	Av. mark in 4 final tests	No. of children	Av. mark in 4 prel. tests	Av. mark in 4 final tests
Over 150	3	153·7	172·7	3	155·0	178·7
140 to 150	3	144·3	158·5	3	145·0	166·2
130 to 140	4	134·5	145·0	4	137·0	151·7
120 to 130	6	124·8	150·6	6	123·8	154·5
110 to 120	3	117·0	145·0	4	114·7	139·7
Below 110	4	100·0	120·5	3	98·3	149·0

In Group A there is a percentage improvement of 16 °/ₒ, an improvement made without the special practice in rote memory.

In Group B there is a percentage improvement of 21 °/ₒ. We seem entitled to regard the excess of 21 °/ₒ over 16 °/ₒ as likely to be due to the special practice.

It has, doubtless, already been noticed that the fifth section of Group B compares unfavourably with the corresponding section of Group A in the final test. Of the six children in the whole of Group B who show no improvement in rote memory, it is surely significant that three belong to this section. The four children included in this section are M. G. who improves 2 °/ₒ, E. C. who loses 7 °/ₒ, L. R. who loses 8 °/ₒ, and F. T. who loses 2 °/ₒ. It is therefore quite in accordance with the hypothesis of transfer that this group does its final tests in substance memory *worse* than the corresponding sections of Group A.

It might be of some value to give the average percentage of improvement of each of the sections indicated in Table IV, together with the excess of the percentage improvement of the sections of Group B over the percentage of improvement of the corresponding sections of Group A.

TABLE V.

Showing percentages of improvement in substance memory of Groups A and B, and the percentage improvement of rote memory in Group B.

	Group A		Group B			Percentage gain in substance memory of Group B over Group A
Marks for 4 prel. tests in substance memory	No. of children	Percentage improvement in substance memory	No. of children	Percentage improvement in substance memory	Percentage improvement in rote memory	
Over 150	3	12	3	15	8	3
140 to 150	3	10	3	13	10	3
130 to 140	4	7	4	10	9	3
120 to 130	6	20	6	24	9	4
110 to 120	3	24	4	22	−4	−2
Below 110	4	20	3	52	9	32

I am at a loss to explain the quite unusual percentage of improvement in substance memory shown by the lowest section of Group B. It is scarcely due to the transfer of improvement in rote memory, since the percentage improvement in rote memory of this section appears quite normal. Considering the two groups as wholes, it would probably be fair to regard some 3 °/₀ of the excess improvement of Group B over Group A as due to transfer. The improvement in rote memory seems to be about 9 °/₀, if we disregard the group which shows a loss instead of an improvement. If we knew how much of this was a growth-effect for children of this age and mental status, we could subtract the amount from the 9 °/₀ and obtain the ratio of the percentage of improvement due to transfer to the percentage of improvement through practice in the practice medium. Such ratios are sorely needed all over the educational field; but, for the present, I fear I must be content with indicating the fact of transfer and expressing it in a roughly quantified way.

IV. Third Series of Experiments. School "I."

A third series of experiments was carried out in a municipal girls' school situated in a fairly good neighbourhood. The children were "old for their standard," a technical educational expression which would be commonly interpreted to mean "backward for their age," though the two phrases do not quite cover the same ground. The point of interest in relation to these experiments lies in the fact that the children were below "saturation point" for their age and ability in most, if not all, of their school subjects. In cases like this, considerable improvement is usually obtained, and transfer of improvement is much more probable than in schools where the native capacity of the children has already been employed to its utmost limit. The children who worked the tests and exercises comprised the whole of Standard V, which, at the time the experiment commenced, had an average age of 12 years 0 months. The ages were distinctly high, for the class was not yet half-way through its current educational year.

The general plan of the experiment was similar to that of the preceding ones. The class was divided into two equal groups on the basis of some tests in substance memory, and one of the equal groups was practised for a period in learning poetry by rote whilst the other group worked exercises in arithmetic. Very great care was taken that both groups should regard their work as of equal importance, though it was not intended that the sums should be so set and marked as to give an adequate basis for calculation of correlation between arithmetical and mnemonic functions. The precaution was taken since we are not unfrequently told that improvement in some mental function is not due to the exercise of that or of some allied function, but that it is due to the training of attention or will. I was anxious to give approximately as much training of attention or will training to one group as to the other, so as to preclude the reference of any subsequent differences in the groups to such a factor.

i. *Chronology of the Experiment. Tests and Exercises.*

All the tests and exercises were taken at 11 o'clock in the morning. On Nov. 23rd, 25th, 30th, and Dec. 2nd, 1909, the whole class worked tests in substance memory for stories. On the results of these four tests the class was divided into two equal groups.

On Dec. 14th one of the two groups (Group B), hereafter called the practised group, learnt by rote William Blake's "Nurses' Song," from "When voices of children...morning appears in the skies" (62 words); on this and the following occasions the remaining group worked "rule" sums whilst the practised group were learning poetry. On Dec. 16th, Group B learnt Fr. Dempster Sherman's "Daisies," from "At evening when I go to bed...into the meadows of the town." On Dec. 21st, Group B learnt "The Golden Crested Wren," by T. Miller, from "The smallest bird that can be found...rumpled, crumpled every feather" (79 words).

On Dec. 23rd, Group B learnt Longfellow's "Slave's Dream" from "Beside the ungathered rice he lay...among her children stand" (81 words).

The Christmas holidays now intervened, but the practice exercises were resumed on Jan. 11th, when Group B learnt Eliza Cook's "Cuckoo" from "How glad I shall be...so white and so pink" (88 words). On Jan. 13th Group B learnt Mary Howitt's "Sea Gull" from "Oh! the white sea-gull...an anchored boat" (83 words); on Jan. 18th, the remainder of Miller's "Golden Crested Wren" (91 words); on Jan. 20th, J. Taylor's "Snow" from "O come to the window" (98 words); on Jan. 25th, "I love the sunshine" by Mary Howitt (110 words); and on Jan. 27th, Mary Howitt's "Woodmouse" (104 words). Then the two groups were put together again and worked four more tests in substance memory, on Feb. 1st, 3rd, 7th and 9th.

The increase in the length of the poems was forced upon us as the exercises proceeded, otherwise several of the children would have reached perfect scores and so would have vitiated our results.

Specimens of the stories used and the analysis according to which the written results were marked have already been given. Six minutes exposure was allowed for each substance memory test and rote memory exercise. The work was done visually without audible articulation. The rote memory work in poetry was marked according to a very simple plan, namely, one mark was given for every word correctly given and correctly placed.

ii. *Results.*

I will indicate first the correlation between the two functions, rote memory for poetry, and substance memory for stories. The correlation thus obtained will not be based upon tests which are theoretically

TABLE VI.

Showing the general correlation between substance memory and rote memory for group B.

Marks for 8 tests in substance memory	No. of children	Av. mark in substance memory	Av. mark in rote memory
380 and over	8	384·0	787·0
360 to 380	2	369·0	756·0
350 to 360	2	353·0	764·0
340 to 350	6	345·5	726·5
330 to 340	2	335·5	766·0
Below 330	2	302·0	723·0

satisfactory from that point of view, since they were not done *pari passu.* The eight substance memory tests were done in two sets, four before and four after the 10 rote memory exercises, but, having regard to the fact that the number of tests in each function was considerable, the resulting correlation cannot fail to have some value. It is possible to show this correlation for the practised group only, since Group A did not work any exercises in rote memory.

An inspection of the table of averages gives some ground for supposing that positive correlation exists; and if the coefficient of correlation be worked out from the individual cases by means of the Pearson formula, r is found to be $+ \cdot 373$; but the probable error is decidedly over $\cdot 1$. We should be safe in concluding that if a positive correlation exists, it is a small one.

I next propose to set out the improvement made by the practised group in rote memory for poetry. The group will be classified on the basis of the marks obtained in the preliminary tests in substance memory.

TABLE VII.

Showing improvement in rote memory in Group B.

Marks for 4 prel. tests in substance memory	No. of children	Av. mark per child for 1st, 2nd, 3rd, 4th exercises in rote memory	Av. mark per child for 7th, 8th, 9th, 10th exercises in rote memory	Percentage of improvement to nearest unit
170 and over	2	292·5	357·5	19
160 to 170	4	278·2	333·0	19
150 to 160	5	273·4	329·4	19
140 to 150	3	282·3	331·7	18
Below 140	3	279·0	350·7	26

Not only do the averages of each section within the group show a considerable and steady improvement, but there is no individual case in which improvement is not shown. It is not contended that all this improvement is due to practice; the girls are getting older and would have become more proficient as a matter of natural growth; but there are no data available to indicate how much of the increased proficiency is due to the latter factor. I estimate the combined efforts of practice and growth roughly at 20 °/₀.

Let us now see how far, if at all, this increased proficiency transfers itself to the work of the final tests in substance memory.

Taking the unpractised and practised groups as wholes and working from the individual cases, we find that, whereas Group A obtains an average mark per test of 38·6 with a mean variation of 2·8 in the preliminary tests and an average mark per test of 46·6 with a mean variation of 3·1 in the final tests, Group B, the group practised in rote memory between the preliminary and final tests, obtains an average mark per test of 38·4 with a mean variation of 2·7 in the preliminary tests, and an average mark of 48·8 with a mean variation of 2·6 in the final tests. The smaller variation in the final exercises of the practised group may indicate that the practice exercises have brought the members of the group nearer together. Group A improves approximately 21 °/。 on its preliminary record, whilst Group B improves

TABLE VIII.

Showing Groups A and B compared

(*a*) *In four preliminary tests of substance memory.*

(*b*) *In four final tests of substance memory.*

	Unpractised group A			Practised group B		
Marks for 4 prel. tests in substance memory	No. of children	Av. mark per child per test in 4 prel. tests	Av. mark per child per test in 4 final tests	No. of children	Av. mark per child per test in 4 prel. tests	Av. mark per child per test in 4 final tests
170 and over	2	44·3	50·3	2	44·2	52·2
160 to 170	4	41·1	48·8	4	41·1	50·7
150 to 160	5	38·5	47·5	5	38·4	48·5
140 to 150	4	35·8	43·2	3	36·3	48·7
Below 140	2	33·6	42·6	3	33·2	45·3

approximately 27 °/。. We must clearly attribute to natural growth or the influence of the other school work much the greater part of the improvement shown in both cases; but the excess improvement of Group B—about 6 °/。—may perhaps be attributed to the effect of the practice exercises in rote memory. But it is advisable to show, section by section, that the averages of the whole groups do not unfairly represent the case.

TABLE IX.

Showing percentages of improvement in substance memory of Groups A and B, and the percentage improvement in rote memory of Group B.

	Group A		Group B			
Marks for 4 prel. tests in substance memory	No. of children	Percentage improvement in substance memory	No. of children	Percentage improvement in substance memory	Percentage improvement in rote memory	Percentage gain in substance memory of Group B over Group A
170 and over	2	13	2	18	19	5
160 to 170	4	18	4	23	19	5
150 to 160	5	23	5	26	19	3
140 to 150	4	21	3	34	18	13
Below 140	2	26	3	37	26	11

If we knew how much of the percentage improvement in rote memory was due to natural growth and the influence of work in other school subjects, we should have a means of estimating how much of the excess percentage of improvement in the sections of Group B over the corresponding sections of Group A is due to transfer of improvement from the one function to the other, but what the former factors amount to I do not know. As before, I must be content with indicating the fact of transfer in a rough and inexact way. Assuming that the natural growth of the two groups is approximately the same, as it probably is in groups so much alike at the outset, we have, as I have previously pointed out, obtained an *excess* percentage of improvement in *substance* memory for Group B over Group A from which that factor is eliminated. But we are not entitled to say that an improvement by practice in *rote* memory of some 20 % transfers to substance memory only a 6 % improvement, for much of the 20 % is a natural growth improvement and is not due to the special practice. The pedagogical question is: How much of the improvement *due to practice* in one function, if any, is transferred to another? The transfer seems clear, but the extent seems doubtful until we know how much of the improvement in the practice medium is itself an improvement through practice.

V. SUMMARIZED CONCLUSIONS.

1. It appears that improvement through practice in rote memory for things with and without meaning is followed by improvement in substance memory for stories. I have used the word 'transfer' of improvement, since transfer is the word commonly used in pedagogical discussions; but I am not unmindful of an explanation which would base the so-called transfer on the presence of elements common to the two functions. Pedagogically, the fact of 'transfer' should not lead us to adopt indirect or formal training methods unless the improvement transferred thereby is greater than that obtained by a direct attack on what is really required to be known.

2. If it is permissible to calculate correlations from such a small number of cases, it appears empirically that very low and indeed doubtful positive correlations between two mental functions, when one child is compared with another, may nevertheless be consistent with some real connection between those functions within the same mind; that is, improvement by practice in the one may produce an improvement in the other.

THE 'PERCEPTIVE PROBLEM' IN THE AESTHETIC APPRECIATION OF SIMPLE COLOUR-COMBINATIONS.

By EDWARD BULLOUGH.

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I. *The Object.*

THE object of this set of observations taken during 1909—10 has been to investigate the aesthetic appreciation of simple (dual) colour-combinations in the light of the conclusions reached in the course of some previous experiments with single colours, and published in the *British Journal of Psychology*, Vol. II. pp. 406 ff. under the title: "The 'Perceptive Problem' in the Aesthetic Appreciation of Single Colours." These conclusions suggested the possibility of applying the conception of the so-called 'perceptive types' also to the appreciation of colour-combinations, as an explanation of individual differences of appreciation.

The principal points of investigation, as they gradually formed themselves in the course of the experiments, were the following:

1. The question to what extent 'perceptive types' were traceable in the appreciation of colour-combinations.

2. The analysis of any further variations of appreciation not primarily due to differences in type; and the attempt to group such differences systematically. This task amounted practically to an enumeration as complete as possible of the criteria used in pleasant and unpleasant judgments, *exclusive* of those criteria which were *ipso facto* implied in the adherence to a definite type. Such additional criteria, to distinguish them from what might be called 'type-criteria,' I shall in the future, for convenience' sake, call 'combination-criteria.'

3. A third problem was presented by the probability that these 'combination-criteria' are not entirely independent of the perceptive types. For it may be supposed that adherence to a certain type predisposes to the use of certain 'combination-criteria' rather than of others. In short, the problem is that of the relation between the types and the 'combination-criteria.'

II. *Material and Procedure.*

1. The material consisted, not as in the previous experiments, of coloured papers shown under artificial illumination, but of small pieces of coloured silks (Liberty silks) of a size of about $1' \times 2'$. There were 38 pieces, comprising the following tones: 8 Reds, 8 Yellows (including orange), 8 Greens, 7 Blues, 5 Purples and 2 Browns. They were presented in ordinary daylight, which avoided the deadening effect of artificial light upon the colours. The silks had the further advantage of being much finer in colour and offering a greater variety of mixed and broken tones than the coloured papers. Owing to the material and the use of ordinary illumination, the whole experiment was much more akin to the natural situation of choosing and matching stuffs in the ordinary routine of life, and consequently the general conditions of the tests were far less strained and fatiguing than those of the earlier observations—a fact favourably commented upon by several subjects who had taken part in the experiments with single colours. No apparatus was required, besides the silks themselves and a sheet of neutral-tinted holland to serve as a background; the whole folded up into a small parcel and could be taken to the subjects, in their customary environment, instead of requiring attendance at the laboratory with its constraint and distracting influences.

While thus conforming to the natural conditions under which aesthetic appreciation normally takes place, one drawback should be mentioned, which lay in the very resemblance to the reality of selecting and combining stuffs for practical purposes. The nature of the material further emphasised this disadvantage: namely the introduction into the observations of a factor which I shall call the 'material complication,' i.e. associations due in part to the texture and nature of the silk, and in part to speculations about the applicability of the colours to certain purposes or about the uses to which the combinations might be put, in the shape of curtains, upholstery, drapings, dresses, ties, wall-decorations, etc.

The subjects numbered 40 (23 men and 17 women), all, with the exception of one girl of about 15, adults. They were for the largest part English, but numbered also some other nationalities among them viz. 9 Germans, 2 Italians and 2 Poles. This fact revealed some small points of interest in respect to differences of appreciation, none however of fundamental importance.

2. The procedure consisted (*a*) in inviting the subjects to state their opinion as to pleasantness or unpleasantness of 19 combinations which I had previously arranged with the 38 pieces of silk. These combinations had been arbitrarily selected by me and comprised what appeared to me both as distinctly pleasant and distinctly unpleasant combinations. The combinations as well as their order of presentment remained, of course, unchanged throughout. The subjects were especially asked to state, if possible, their reasons for pleasant or unpleasant judgments, in the same way and with the same end in view as in the previous experiments with single colours[1].

These combinations, offered for appreciation, constituted Series A.

(*b*) Further the subjects were given successively 6 colours—Red, Yellow, Green, Blue, Purple and Brown, and invited to select from all the remaining 37 colours any 3 colours which in their opinion "would go with" the 6 'standard'-colours, and to state, if possible, their reasons for the selections. I obtained thus $6 \times 3 = 18$ self-made combinations, which constituted Series B. My object was thereby both to increase the total number of observations as data for any conclusions; and to observe any differences that might arise between the *appreciation* and the *active production* of colour-combinations, especially as the latter would give the subjects considerably more freedom of choice.

The number of observations were accordingly $19 \times 40 = 760$ of Series A and $18 \times 40 = 720$ of Series B, or a total of 1480. This total was, however, not reached in reality, as not all the selections of

[1] *Brit. Journ. of Psychol.* 1907, Vol. II. pp. 411–413. All the remarks made there concerning the procedure and introspective evidence apply equally to these experiments.

Series B were completed, owing to the inability of many subjects to make their own combinations satisfactorily. Allowing for 53 such omissions the actual total amounts to 1427.

I subjoin a list of the combinations of Series A for reference, though the actual colour-material, as in the previous experiments, proved of only secondary importance.

1. Flat, slightly bluish green—dk. orange.
2. Sat. green—golden brown.
3. Vivid blue purple—pearl pink.
4. Pale red purple—sat. sky blue.
5. Deep nearly red orange—bluish pink.
6. Yellow red—vivid green.
7. Unsat. dark yellow—vivid green.
8. Flat grey blue—flat yellow green.
9. Dk. brown—sat. orange.
10. Dk. turquoise blue—sat. yellow.
11. Pale blue purple—pale pearl pink.
12. Dk. blue purple—pale blue.
13. Very vivid sat. red—dk. sat. green.
14. Sat. blue red—pale turquoise blue.
15. Dk. blue purple—dk. blue, navy.
16. Flat green—flat orange.
17. Dk. olive green—salmon pink.
18. Dk. red, rose—paler shade of same.
19. Flat dk. blue—sat. yellow, sulphur.

I should not like to let this opportunity pass without expressing my thanks both to Dr Rivers and Dr Myers for the advice and assistance, which they gave me in planning and arranging this set of experiments, with the same unfailing kindness as on earlier occasions.

III. *Results.*

I propose, in order to economize space, not to give the records of the observations *in extenso*. Their general appearance is the same as that of the records on single colours under Table II of my previous paper. For the convenience of the reader, I shall quote in full some representative cases, but shall otherwise confine myself to giving the conclusions drawn from the records.

I intend to deal with the conclusions, first of Series A, bearing upon the persistence of the perceptive types, and upon the 'combination-criteria,' secondly with the same points as illustrated by Series B, and finally with the relation of the types to the 'combination-criteria,' based upon Series A and Series B conjointly.

Series A.

(*a*) *Persistence of Types.*

A general explanation of the meaning of 'perceptive types,' together with a description of their respective criteria, a theory of their genealogy and discussion of other aspects has been given in detail in my previous paper[1]. I distinguished then, in the appreciation of single colours, four types which I tentatively called:

1. The *objective* type.
2. The '*physiological*' type.
3. The '*character*' type.
4. The *associative* type.

In the course of the observations on colour-combinations I have become more convinced than ever of the reality and the value of these types for the understanding and explanation of individual differences of appreciation. Judgments, so divergent as to appear at first sight hardly applicable to the same object, become easily intelligible by reference to such profound apperceptive differences as exist for instance between members of the objective and character-type. This I found to hold good also in the case of colour-combinations, though here the situation is considerably complicated by the addition of the 'combination-criteria,' which occasionally seem to supersede almost completely the criteria implied in the adherence to perceptive types. In view of this complication it is essential to state that all the subjects, previously to being shown the combination-series, were rapidly examined with single colours as to their respective types. As ten of the subjects had already taken part in the single-colour tests, this preliminary observation had the additional value to showing to what extent these subjects had adhered to their types. It proved a strong argument in favour of the type-theory that the majority of the subjects on re-testing appeared to have retained their particular type, even after a lapse of, in some cases, three years. It seems to me besides no mean evidence in support of the same theory, that the 30 new subjects ranged themselves without any difficulty under the four type-groups established upon the evidence of the previous 43 subjects.

The stability of the types, as shown by the re-examination of the 10 subjects just mentioned, seems to afford ground to the view that membership of some type is permanent, and, in some sense, funda-

[1] *Brit. Journ. of Psychol.* 1907, Vol. II. pp. 418–458.

mental. It is true that in three instances the subjects seemed to have changed; but the change was really due only to giving prominence to different sides of the same type. Thus one subject, who had previously appeared as a 'transition-case from the physiological to the character-type,' belonged more decidedly to the latter than in the previous experiments (No. 20). Another, mainly associative in the earlier tests, with slight physiological traces, showed the reverse in the colour-combinations (No. 36). In only one case, No. 16, a complete substitution of types took place. This was a particularly interesting instance as the change took place during the tests, as much to the subject's astonishment as to my own. The change itself was instructive, as it was from the objective to the character-type—this latter is much more in accordance with the general nature of the subject—and seems to give a striking evidence of the artificiality of the objective attitude, which I had suspected already in the earlier tests[1].

As a rule, the comments made upon the combinations further increased the certainty and clearness of the subjects' types. In a few cases only the 'combination-criteria,' which usually appeared side by side with 'type-criteria,' obscured the types,—very considerably in Nos. 13, 23, and 26, in two cases, Nos. 3 and 9, completely,—so much that, as in the latter, but for the preliminary test, I might have remained in ignorance as to which types the subjects really belonged.

In order to give some idea of the kind of evidence that is furnished by the records for the persistence of the types, I give the records of seven representative cases in full, marking the comments which indicate their respective apperceptive peculiarities. (Table I.)

Of these seven instances, No. 19 represents one of the purest objective cases in the records, as does also No. 16, as it stands, i.e. independently of Series B. The change of the objective to the character-type, that occurred in this subject (see above), shows itself only in the comment on combination 14, but became very marked in Series B. No. 22 is a good physiological specimen, No. 1 shows the transition from the physiological to the character-type, No. 20 the same in a more advanced state and No. 39 is one of the most consistent character-cases on record. No. 31 is a fair instance of an associative subject, as such showing some admixture from other types—in this case very slight physiological-character traces.

The following is a selection of the comments of the different types,

[1] *Brit. Journ. of Psychol.* 1907, Vol. II. pp. 450, 462.

TABLE I.

No. 19	No. 16	No. 22	No. 1
1. **unpl.** 'association'	1. **unpl.** cheap sofa hangings	1. **unpl.** singly *glaring*	1. **dist. pl.** *contrast of character*
2. **unpl.** Br. too light for G., *don't balance*	2. **pl.** G. *improves* Y.	2. **pl.** Br. alone *too strong*	2. **dist. pl.** fused, 'evening'
3. **unpl.** separately and together	3. **dist. unpl.** *bad mutual influence*	3. **dist. unpl.** *painful to eyes*	3. **unpl.** *sickly*
4. **unpl.** ,, ,, ,,	4. **pl.** P. *improved by* Bl.	4. **sl. pl.** better than 3	4. **pl.** *cheeky comb.*, *harsh* Bl. *compensates for insipid* P.
5. **unpl.** singly unpl.	5. **dist. unpl.** yells, Y. blazing, vulgar colour	5. ?	5. **unpl.** Y. very pl. *happy warmth*, but spoil each other
6. **unpl.** G. unpl.	6. **unpl.** Y. unpl., *though improved by* Bl.	6. **dist. unpl.** *hurts*, G. *too glaring*	6. **ind.**—**pl.** G. *too light for* Y.
7. **unpl.** singly unpl.	7. **sl. unpl.** *dull, depressing*	7. **unpl.** both *too light, loss of restfulness*	7. **pl.** *equal weights, fresh*
8. **unpl.** G. unpl.	8. **unpl.** comb. of Bl. with G. unpl.	8. **unpl.** Bl. nondescript, spoils G.	8. **dist. pl.** *reposeful* comb. and *freshness* of G.
9. **unpl.** *balance wrong*	9. **unpl.** Y. unpl.	9. **unpl.** Br. should predom.	9. **ind.**—**pl.** *weights* don't match
10. **sl. pl.** 'startling,' Y. *pl. pure*	10. **unpl.** *dislikes* Y.+Bl.	10. **unpl.** Y. unpl., spoils Bl.	10. **pl.** Y. unpl. but harmonizes with *cold* Bl.
11. **sl. pl.** *delicate*	11. **unpl.** Pink unpl. P. *washy*	11. **unpl.**	11. **pl.** rather *insipid*
12. **unpl.** P. unpl.	12. **unpl.** Bl. *too washy*	12. **unpl.** *too much contrast*	12. **unpl.** *contradiction in temperaments*
13. **unpl.** R. *kills* Gr.	13. **pl.** *? good comb. of a garish kind*	13. **pl.** if less R. than G.	13. **pl.** match and *balance*
14. **unpl.** singly unpl., *balance wrong*	14. **sl. pl.** *remarkable* R. *characterless* Bl.	14. **dist. pl.**, pl. singly	14. **unpl.** strange
15. **unpl.** P. unpl., Bl. pl.	15. **dist. pl.** Bl. much *improved by* P.	15. **unpl.** P. spoils Bl.	15. **unpl.** *melancholic*
16. **unpl.** Y. *impure*, unpl.	16. **pl.** G. *improves* Y.	16. **unpl.** *too contrasting*	16. **dist. pl.** *healthy*
17. **unpl.** *balance wrong*	17. **unpl.** colours don't suit?	17. **pl.** pl. singly	17. **dist. pl.** *similar in warmth*
18. **sl. pl.** better together than singly	18. **sl. unpl.** don't go together	18. **unpl.** pl. singly	18. **unpl.**
19. **pl.** *favourite* Y., *improves* Bl.	19. **pl.** Bl. *much improved* by Y.	19. **unpl.** Y. *spoils it*	19. **unpl.** *don't match in weight*

	No. 20	No. 39	No. 31
1.	**unpl.** *cold, cheerless*	**unpl.** *soft character*, G. spoilt by an *ambitious* Y.	**unpl.** dislikes all Y. and G.
2.	**pl.** more of an entity, balanced	**pl.** same character, *friendly* to each other	**pl.** '*autumn*'
3.	**unpl.** disharmonious?	**dist. unpl.** fight	**sl. pl.** '*Spain*'
4.	**dist. unpl.** Bl. *outweighs* P., no fusion	**unpl.** both *characterless*	**unpl.** cold
5.	**pl.** unique, *strength*, 'oriental'	**unpl.** Y. *hardhearted* thing	**sl. pl.** some *association*; 'curious'
6.	**sl. pl.** attractive from purely sensuous point of view	**unpl.** both *presumptuous* and hard, *trying to be what they are not*	**dist. unpl.** cheap
7.	**unpl.** *cold, weak*	**unpl.** something *sad* about them, *trying to be what they are not*	**dist. unpl.** cf. 1
8.	**sl. pl.** *sad effect, refined*	**unpl.** *very sad* Grey, G. *doleful*, total *sad*	**pl.**
9.	**unpl.** lack of harmony?	**pl.** Y. unpl. Br. full of *strength of character, independent*	**unpl.** too obvious
10.	**unpl.** dislikes Y., *weak*	**dist. unpl.** *happy* Y., spoil each other	**dist. unpl.**
11.	**unpl.** *very cold*	**ind.**	**unpl.** meretricious, flimsy
12.	**unpl.** *contradictory, weak + strong*	**unpl.** *contradiction of char.*, light music when one feels unhappy	**pl.** very odd but interesting
13.	**pl.** spectacular, fine, *bizarre, unique, masculine*	**unpl.** R. *ambitious*, smothering G., feels sympathy for G. (unpl.).	**unpl.** R. unpl. G. pl.
14.	**unpl.** suggestion of *refined cruelty, maliciousness*	**ind.**—**unpl.**	**pl.** '*oriental*'
15.	**dist. pl.** *sorrowful but strong*	**pl.** both *friends* to each other, similar *character*: *steady*	**dist. pl.** '*personal association*'
16.	**pl.** *refined, spiritual, non-sensuous*	**ind.**—**unpl.** *characterless*	**unpl.** G. harsh with R.
17.	**unpl.**	**unpl.** *not nice character*	**unpl.** '*Liberty*'
18.	**unpl.**	**unpl.** *not friendly*	**pl.**
19.	**unpl.** antagonistic, *cold, cheerless*	**unpl.** Bl. pl., Y. pushing itself, though *happy and well-meaning*	**sl. pl.** gay, *national*, '*flag*'

as they occur in the records, in the same manner as in the above representative instances:

(*a*) *Objective* comments: colours are washy; have or have not body; they are pure or impure, they improve each other by proximity or exercise a bad mutual influence upon each other; the combinations are "good"—without any further indication of what is meant by it—or are "good of their kind"; they are "unusual" (a strikingly objective criticism meaning "deviating from the accepted standard of the subject"). Some of the 'combination-criteria' obviously belong to the same group (see later p. 445), as for instance some of the remarks on balance or lack of balance of luminosity or saturation between the two colours, or the presence or absence of a common element; possibly what I shall call 'Harmonies of Shades of the same tone' should also find a place among the objective criteria.

(*b*) *Physiological* criteria are exceedingly numerous: combinations are cheering or depressing, fatiguing, glaring, cold, warm, heavy, light, strong, mournful, cheerless, calming, exciting, restful, fresh and cool, bright or staring—all remarks which were also applied to single colours[1] and are occasionally applied to the individual colours of the combinations.

(*c*) The *Characters* of combinations were described as: insipid, friendly, sad, fanatical, shallow, morbid, lovable, trusting, prudent, delicate, gushing, excitable, melancholic, gloomy, disagreeable (as character), quarrelsome, interesting, refined, cruel, stupid, brutal, elevated, childlike, pensive, peaceable, attractive, unique, malicious, spiritual, bad-tempered, contemptible, fresh and healthy, sober or flaunting, simpering or egotistical, assertive or vulgar. Here again the characters are the same as those attributed to single colours[2] and were sometimes ascribed to the individual elements of the combinations.

(*d*) *Associations* cover naturally as wide a range as those attaching to single colours: flowers (especially), pictures (Watteau), the sea, flags (very common), evening, autumn, spring, thunderstorms, music (rarely), persons, a jockey, a circus, a menagerie, seasickness, railway-signals.

Numerical distribution of Types.

I. Clear and consistent cases of the different types are naturally much rarer than scattered judgments indicating different types, as the

[1] *Brit. Journ. of Psychol.* 1907, Vol. II. pp. 418–419.

[2] *Ibid.* p. 439, Table IV.

latter may occur almost anywhere, objective and associative comments being found in the records of physiological or character subjects. If considered in a sufficiently large number, such apparently misplaced judgments seem, however, to follow a definite principle, which renders their occurrence intelligible, so far as they are not explicable by special circumstances.

Among the 40 subjects, the following clear cases of types were found:

1. Objective Type = 4.
2. Physiological Type = 16.
3. Character-type = 12.
4. Associative Type = 3.

The small number of objective cases is quite normal; they are exceedingly rare in any consistency and definiteness. Even among these four subjects, there are two whom I should on further experiments expect to break down and change into another, probably the physiological, type, as happened to No. 16, mentioned earlier.

The low number of associative cases was unexpected, but explains itself probably first by the competition with the 'combination-criteria' which are much more obvious and obtrusive than any association, and secondly by the fact that combinations offer much less play to association than single colours, since a combination, owing to its complexity, offers some difficulty to fit itself into an associative scheme.

II. Considering, instead of merely *pure* cases, *all* the evidence of criteria, attributable to different types, whether pure or impure, the records furnish the following figures:

1.	*Objective*	traces in	17	subjects,	in the shape	of	43 (11)[1] = 54	judgments
2.	*Physiological*	,,	32	,,	,,	,,	87 (58) = 145	,,
3.	*Character*	,,	19	,,	,,	,,	112 (31) = 143	,,
4.	*Associative*	,,	21	,,	,,	,,	62	,,

III. Analysing these figures further by taking into account not merely the numbers of type-criteria, but also the distribution of types among the subjects who employed them, the following figures are obtained:

1. The 54 *objective* judgments were given by 8 subjects of the objective and mixed objective-physiological types, and by 9 belonging to the physiological or physiological-character type. But 39 judgments

[1] The figures in brackets are judgments on individual colours, not on the combinations.

out of 54 were given by the former and only 15 by the latter. To put it into the shape of a convenient formula:

$$\frac{O, P^{1}}{P, PCH, CH} = \frac{8\ (39)^{2}}{9\ (15)}.$$

2. Of 145 *physiological* judgments 93 were given by 18 physiological and physiological-objective subjects; 52 by 12 purely objective, physiological-character and character types:

$$\frac{P, PO}{O, PCH, CH} = \frac{18\ (93)}{12\ (52)}.$$

3. Of 143 *character* judgments 140 were given by 18 character-subjects, 3 by 1 objective subject:

$$\frac{CH, PCH}{O, OP, P} = \frac{18\ (140)}{1\ (3)}.$$

4. Of 62 *associative* judgments 29 were given by 5 associative subjects; 33 by 16 subjects of the objective, physiological-objective, physiological, physiological-character, and character-types:

$$\frac{PA, CHA^{3}}{O, OP, P, PCH, CH} = \frac{5\ (29)}{16\ (33)}.$$

These figures appear sufficient to prove the intrinsic importance of the types. In spite of the fact that the subjects occasionally adopt criteria not strictly belonging to their type, on the whole the types remain, so to speak, the centres of attraction of their particular criteria, which group themselves more persistently round the subjects of their proper type than round the others. Each type maintains its numerical superiority either in regard to *number of subjects*, as in 2 or 3, or, if these are equal or nearly so, as regards the *number of judgments*, as in 1 and 2, or, as in 4, where the proportions are upset by the low total figure of associative subjects, as regards the *ratio of judgments to subjects*: $A = 1:6$, all the others $= 1:2$.

[1] I shall use the following abbreviations in the sequel:

O = objective type.
OP = objective-physiological type.
P = physiological type.
PCH or CHP = transition for physiological to character-type.
CH = character-type.
A = associative type; and the combinations AP, PA, CHA, etc.

[2] The figures in brackets are the judgments, those without brackets the subjects.

[3] The association type never appeared quite pure in the combination-tests, but always mixed with physiological or character-traces.

The point which actually requires explanation is the question, why one type should adopt the criteria of another type at all. Here the records offer suggestions of fundamental value regarding the whole nature of perceptive types, which the single-colour-test failed to bring out.

It is of interest to notice the direction in which such changes occur. That any subject may occasionally show traces of association hardly requires comment, owing to the accidental and unavoidable nature of associations. Already with the single colours I found that associations may be found scattered in almost all records and, in the present records, they are fairly evenly distributed all over the types. Putting the interchange of association and other criteria out of court, it is noteworthy that no subject ever adopts a character-criterion, unless he belongs to the character-type; on the other hand, character-subjects very frequently show physiological, less often objective traces. Physiological subjects very often use objective criteria. This seems to me to suggest an explanation of the changes of types, which I think may claim the value of a principle: especially under unfavourable conditions, *the changes may be expected to take place on the principle of a reversal to the next lower type.* On the assumption of the genealogy and aesthetic scale of the types, explained in detail in my previous paper[1], reversals ought to take place in the order: character to physiological; physiological to objective. The former transition is explicable by the genetic connection between the physiological and the character-type; the latter by the fact that the objective attitude is practically extra-aesthetic, and represents in most cases a mere makeshift-attitude or a *pis-aller*, usually admitted as such by the subjects themselves. This order of the changes is actually found in the records: under disadvantageous conditions—which are especially represented by a dislike for one colour or combination—character-types tend to revert to a physiological, and physiological to an objective attitude. There are none but character-types using character criteria; there are 12 character-types adopting physiological, 8 physiological types adopting objective criteria. Occasionally a character-subject may become objective, passing over the physiological attitude: 3 character-subjects have given objective judgments. Objective subjects are apt sometimes, but rarely to pass over into the physiological group. There are no instances of this in the records, except in the case of subjects who showed from the very

[1] *Brit. Journ. of Psych.* 1907, Vol. II. pp. 446, 461.

beginning some physiological traces. The reason I believe to be that the physiological is the original and fundamental type or attitude, from which the character-type is regularly developed, but which may also have been, accidentally, superseded by the objective, through habit, training or interest. The objective type represents therefore a side-development; but it is, though not genetically, yet aesthetically the lowest phase.

Instances of such reversals will be found in greater number in Series B.

(*b*) *Combination-criteria.*

By this term are meant those criteria which are not directly referable to the adherence to any type, but rather conditioned by the fact that the objects were combinations or collocations of two colours, suggesting thereby certain criteria based upon the relation of the two elements of the combination to each other. They are, if the distinction may be allowed, more of *objective* origin, i.e. directly suggested by the object, while type-criteria are of *subjective* origin, i.e. implied in the perceptive attitude of the subject.

The 'combination-criteria' which I have found in the course of the tests, are very numerous. It is essential to note that all those tabulated were explicitly stated by the subjects and that none are inferred merely by analogy.

They can be grouped under the following headings:

1. Conscious Unification and Dissociation.
2. Implicit Dissociation:
    - (*a*) Balance of saturation or luminosity.
    - (*b*) Balance of physiological effects.
    - (*c*) Compensation of physiological effects.
    - (*d*) Compensation of character.
    - (*e*) Dislike of one colour makes the combination unpleasant.
    - (*f*) Liking ,, ,, ,, ,, ,, pleasant.
    - (*g*) One colour 'spoils' the other.
    - (*h*) One colour improves the other.
3. Colours are liked singly but disliked together.
4. Colours are disliked singly but liked together.
5. Affinity (*a*) presence of a common element,
    (*b*) harmony of shades of the same tone.
6. Consonance and Dissonance.

I offer a few remarks, together with the numerical distribution, on each of these criteria.

1. *Unification and Dissociation.*

By Unification is meant that the combination is apperceived and appreciated as an entity, as a true *combination*—as distinct from a mere collocation of two colours. The fact is frequently described by subjects as "fusion" or "synthesis" and implies that the combination represents a total character and conveys a total impression, which is not equivalent to the character or impression of its two constituents alone, but is a new total effect due to the combination or addition of the individual effects, frequently modifying each other. A unified combination is, so to speak, a new colour, offering an individuality as a single colour.

Dissociation, on the other hand, means that the combination remains a *collocation* of two colours, each claiming its own effect and appreciation. The collocation is, therefore, accepted or rejected simply on the merits of its elements independently of each other or of their proximity. As a matter of fact, wherever it was explicitly stated, it was generally a reason for rejection, though dissociation need not actually lead to rejection, as is shown by the criteria, such as balance and others, which imply some dissociation, but may be positive criteria. The purest type of dissociation is to be found in some cases in which a combination is accepted or rejected on the ground of the pleasant, or unpleasantness of *one* of its elements.

The causes of Unification or Dissociation appear to be multiple. Few actual reasons for unification were stated, but those given for dissociation allow inferences to be drawn with regard to the former. Reasons given for dissociations were: lack of balance of heaviness, of luminosity, of strength or saturation, light-contrasts, pleasantness or unpleasantness of one colour to the detriment of the whole, general attractiveness of one, very frequently its undue predominance, usually the monopolising of the attention by it; on the other hand, balance, similarity of effect or character, similarity of the tones, the presence of a common element in the tones, especially a general impression, in one case the emergence of an association covering both elements, acted in the direction of unification.

As usual, criteria for unpleasant judgments are more frequently stated than the grounds for pleasant ones, the numbers for unification being 31, for dissociation 50, total 81.

2. *Implicit Dissociation.* (*a*) *and* (*b*) *Balance of saturation or luminosity and balance of physiological effects.*

(*a*) The first of these criteria means that a combination is approved of, if both elements appear to the subject as equally matched in point of saturation or luminosity or both; and is disliked, if this balance is absent, if one colour is "too bright for the other," as was commonly stated. It must be remembered that saturation and luminosity are up to a certain point interchangeable, and in any case are frequently confused by unsophisticated subjects. The point of importance is therefore that the estimates of saturation and luminosity fluctuate enormously, and that consequently the same combination may be and has been accepted for its balance by one, and rejected for its lack of balance by another subject.

The number of judgments for combinations—on the ground of balance—was 6, against them, owing to lack of balance, 34; total 40.

(*b*) Under the heading of 'balance of physiological effects' I have grouped all statements indicating a balance of heaviness, temperature, strength—as distinct from saturation[1]—stimulating or soothing effect. The reason for this distinction is that, as the tests with single colours had already shown, these physiological effects belong to the physiological type, whereas saturation and luminosity are essentially objective criteria, and it seemed to me valuable in view of the relation of 'combination-criteria' to type-criteria to keep these two classes distinct from each other at the outset. As might be expected, the numbers of judgments based upon balance or lack of balance of physiological effect are much higher than those of the last group, physiological criteria being much more common than objective ones.

Pleasant judgments on ground of balance, 34; unpleasant judgments on ground of lack of balance, 78; total, 112.

(*c*) *Compensation of physiological effects.* This criterion is a variety of the previous one. It differs from balance of physiological effects in affecting combinations in which colours not of the same, but of opposite physiological effects are combined. Thus the combinations red and blue as warm and cold, or yellow and purple as lively and depressing, or purple and green as sombre and refreshing, are liked on this principle as presenting in combination a happy means between two unpleasant extremes.

[1] Cf. *Brit. Journ. of Psychol.* 1907, Vol. II. p. 425.

The number of such cases is relatively small but very significant, as will appear in connection with Series B. It is also interesting to observe that with this criterion the number of explicit acceptances is larger than that of rejections.

Pleasant judgments, 18; unpleasant judgments, 13; total, 31.

(*d*) A similar criterion, genetically a development of the compensation of physiological effects interpreted as character-qualities, is the '*compensation of character*,' with '*contradiction of character*' as a special variation. For instance, a sad brown may be enlivened by collocation with a cheerful yellow (No. 29, 16), or a fresh blue relieves the seriousness of a purple (No. 24, 12). On the other hand, a "weak" pale blue together with a "strong" purple (No. 20, 12), or as another subject (No. 25) expressed the effect of the same combination, "the strong, egotistical, calculating purple overbearing completely the simpering mild blue," is taken as a contradiction of character. It is impossible to draw a definite line of demarcation between these two opposite forms of apperception, or to tell at which point a compensatory effect changes into a contradiction. On the other hand, it is easy to see that the ultimate point of difference lies in the degree of unification or dissociation imparted apperceptively to the combination: character-compensation represents a degree of unity, of total impression, after an initial dissociation of the elements, which character-contradiction has been unable to reach. No. 39, one of the most consistent character-types, quoted in full among the representative cases, offers ten instances of such a dissociation, nearly half of the total number of this criterion.

The figures for 'compensation of character' are 14; for 'contradiction of character,' 24. It is important to notice that the latter need not imply the dislike of such a contradictory combination.

(*e*) *The dislike of one colour renders the combination unpleasant.* This is an exceedingly frequent occurrence, but requires no explanation. The disapproval rests ultimately on some criteria implied in the adherence to one of the types.

Number of judgments (all unpleasant), 108.

(*f*) *The liking of one colour renders the combination pleasant:* the converse of the above. The pleasantness of one colour is able to outweigh indifference or actual dislike to the other. Cases of this kind are, characteristically, very rare, showing that dislike is a much more decisive factor than liking.

Number of judgments, 15.

(*g*) In a fair number of instances, exception is taken to a combination on the ground that '*one colour "spoils" the other.*' It is usually not, as might be supposed from the wording of the comment, the bad influence of one colour upon the other (for instance by 'simultaneous contrast') which is objected to, but the criticism expresses generally a distinct liking for one colour together with a kind of resentment at the presence of the other which alone is either indifferent or actually unpleasant. Especially if one of the colours happens to be a subject's favourite, he would rather have it alone, than be disturbed, and have it "spoilt" by the proximity of the other (17 cases). Occasionally, but rarely the *tones* 'spoil' each other as red and yellow (3 cases), one colour "kills" the other (lack of balance of strength) (6 cases); lack of balance of weight (3 cases) and twice a contradiction of physiological effect is meant by this "spoiling" effect. As this analysis shows it would be very rash to refer all instances of such impairment of one colour by the other to simultaneous tone-contrast, more particularly as even complementary tones are capable of "spoiling" each other, if the luminosity-contrast is too strong or if one interferes with the character or physiological effect of the other.

Number of judgments, 39.

(*h*) The converse of the above, viz. that *the colours "improve" each other*, is perhaps the rarest comment of all. The number of judgments to this effect is only 12, of which half may possibly be due to simultaneous contrast, affecting combinations 13, 14, 16, 17, 19. What makes simultaneous contrast probable is the statement, at least in two cases, that the one colour "shows off" the other. At the same time No. 16 who supplies 8 out of the 12 instances, maintained a mutual improvement in the combinations yellow and green, and blue and purple, where there is no question of a complementary relation between the colours.

3 and 4. Incidentally I was able to observe that *combinations may be unpleasant, though both their elements are pleasant singly* as well as that *combinations may be pleasant, in spite of their elements being singly unpleasant.* Neither cases are frequent; the latter is much rarer than the former, in fact it appeared to many subjects *a priori* impossible, until they had the actual experience. The number of the former are 29, of the latter 10.

5. *Affinity.*

(*a*) *The presence of the common element* is occasionally mentioned as a reason for either liking or dislike. Considering the importance which has been attached to this point as a species of the 'Unity-in-variety' principle, the infrequency of this criterion is instructive, and still more so the fact that it is more often stated as a ground for dislike than for approval. The numbers are, for approval 9, for disapproval 22, total 31.

It is very difficult to know what exactly the subjects meant by saying that two colours "belong together," that they are "related," "possess affinity" or a "common element." Where they meant to express more than the mere pleasantness of the combination, the affinity resolved itself usually into the common possession of some tone as of red shared by purple and pink of comb. 3 or of blue in comb. 4, 14, and 15, or of some other feature as darkness or flatness or an equal degree of saturation. A common tone-element was, however, more frequently a reason for unpleasantness than for acceptance. Two subjects were much astonished to find that they disliked comb. 3 and comb. 17 *in spite of* the obvious common element in these combinations. One subject at least accepted the almost universally rejected comb. 3 (somewhat resembling the Litt.D. gown of Cambridge University) on the ground of a common tone-factor. To judge by the unfavourable comments that some colours were "too near each other" or "showed too little difference," it is probable that a certain degree of differentiation varying with different subjects is essential to the pleasantness of such combinations.

(*b*) The same irregular attitude prevailed with regard to combinations of '*different shades of the same tone*,' viz. pink and red, light and dark blue, etc. I had purposely introduced two combinations of this kind, Nos. 9 and 18, to test opinions with respect to this point. Out of a total of 20 judgments bearing upon the question, 7 were pleasant and 13 unpleasant, all the latter, except one, affecting comb. 18, all the former affecting comb. 9, with two exceptions. Combinations of this kind were declined as uninteresting, insipid or obvious; comb. 18 especially as showing too little differentiation. Series B will supply some further observations on combinations of this type. Interesting is the fact that subjects 3 and 26 objected to them, on principle so to say, because they collided with their favourite criterion, namely, balance of luminosity or strength.

6. *Consonance and Dissonance.*

Under this heading I have classed a relatively small group of statements of a very obscure character, which the subjects were quite unable to elucidate in any way. They are all to the effect that the two colours did or did not "go together," without indicating any further what was meant by this expression or similar ones like: "the colours blend, fuse, or do not blend, fight, swear, scream, yell or clash."

There can be little doubt, on analysis, that these various expressions of approval or disapproval imply a great variety of criteria. Consonances may be due to balance of luminosity or saturation, to harmony of the same shade, to the presence of a common element either in tone or saturation, to compensation of physiological effects, to the emergence of an association or of a total effect, in six instances possibly to the complementary relation of the colours—or at least to the approximation to it, as no actually complementary pairs occur in Series A.

Similarly the greater part of dissonances were probably due to the objectionable form of affinity with too little difference in the elements, a smaller part to lack of balance, especially in the shape of too pronounced a light-contrast.

The numbers of consonances were 23, of dissonances 48.

N.B. The '*Material Complication*'; the '*Quantitative Complication.*'

Having been warned of the danger of using silk, owing to the possible introduction of disturbing factors connected with the material and its use, I was careful to watch for any evidence of such interference. I was, however, surprised to find what a small part associations with dresses, drapings, ties, etc. played in the appreciation of the combinations. It is true that some subjects admittedly suffered under such associations. Among the ten who did so very markedly were as many men as women, a result which contradicted the warnings that women would be more liable to such effects than men. In only five subjects was the complication persistent throughout and four of these were men.

The necessity of thinking of colours as always put to some definite use or as otherwise appearing in some concrete form, seemed in some cases almost constitutional, inasmuch as the subjects were unable to conceive of the colours in any other way. Occasionally this complication produced curious results: one subject, No. 22, tended to think of colour as permanently present, and judged it according as to whether he would be prepared to have it constantly about him, a consideration which naturally limited his range of appreciation very much; another, No. 13, declined the combination red and pink, on the ground that "he would not know what to do with it," as it was so obviously a "boudoir-combination."

On the whole, the material complication, wherever it appeared, had a very narrowing effect upon the appreciation and considerably increased the number of disapproving judgments.

The quantitative relation of the coloured areas to each other has always been considered of primary importance, and I therefore wished to leave it out of consideration for the present, as a separate problem to be studied later. It was this endeavour to exclude quantities as far as possible which led me to use silks rather than flat surfaces of coloured boards or papers, as I hoped that a pliable material, easily twisted and ruffled and intermingled, would prevent the emergence of the quantitative factor on the part of the subjects.

In general this attempt seems to have been successful. I found only eight subjects each of whom one to four times wished to alter the proportions of the coloured areas. The alterations were usually in the direction of assuring the lead to one element, either because it was liked, or because it was wished to drown an unpleasant colour by the predominance of the other. In the case of strong luminosity-differences especially there was a tendency either to establish a balance by the predominance of the lighter, or to subordinate the lighter to the darker colour so that it formed a mere decorative border or piping.

SERIES B.

The results of Series B differ in some respects very materially from those of Series A. These differences are almost wholly due to what proved to be the *unfavourable conditions presented by the method of production*.

After the very first few tests of this series it was easy to see that the production of combinations and their appreciation offered difficulties which were entirely absent from Series A. Clearly the making of selections for combinations called into play a quite new set of conditions and of psychical processes, compared to the mere judgment upon already made combinations, and I was surprised to find that some of the most appreciative subjects of Series A were almost comically helpless when asked to make combinations for themselves, and proved quite unable to reach the appreciative attitude which they had displayed quite naturally in the first series. Being asked wherein the difficulty lay, they were unable to give any answer but that the "creative" effort was beyond them. It may seem ludicrous to speak of a "creative" effort in the solution of so simple a problem as making a satisfactory colour-combination, but there is no doubt that the 'creation,' the active production of an aesthetic object, however simple and intrinsically unimportant it may be, does require an effort of which many subjects were incapable. The difficulty lay first in the selection of the colours to make the combination, then in the actual appreciation, when it was made, and especially in the transition from the critical attitude of the former process to the appreciative point of view of the latter.

The initial selection of the colour "to go with" the 'standards' embarrassed many subjects extremely. The majority stated that they had never before found themselves in this situation, at least under the necessity of choosing from so many colours, and without regard to some definitely practical purpose. Most of the subjects had no previous idea of which colours they would like to combine with the 'standards,' or what effect they should try to realize. It was by no means a lack of visual imagery, though in two cases—Nos. 16 and 18—this defect was an additional obstacle. On the whole the physiological group suffered least under this difficulty of selection, as the 'standard' itself, by reason of its particular effect, decided in most instances the choice of its companion. Similarly some of the best character-subjects were little troubled and, generally, enjoyed most of all the subjects the making of combinations; they found it an occupation full of interest and often of unexpected pleasures. Those who had the greatest difficulty were members of the objective group and transition-subjects from the physiological to the character-type—notably Nos. 20, 23, 24. This latter class of subjects especially seemed to find the transition from the critical to the appreciative attitude a source of much trouble.

The difficulty of obtaining a free and easy transition from the one point of view to the other, seemed to militate strongly against the production of satisfactory combinations in a great many cases. The critical standpoint appeared to paralyse the imagination and prevented the kind of impersonal detachment which would allow the combination, when made, to be regarded as *objectively given* and enjoyed as such, and *not as produced by the subject* himself. Owing to the inability of the subjects to shift their point of view, the critical attitude persisted into the aesthetic realisation of the colour, much to the detriment of its enjoyment, and interfered with the objectivation[1] of its content by laying an undue stress on its relationship to the subject. This objectivation of an experience is precisely the end of the 'creative' effort, in the making of colour-combinations no less than in any other and higher kind of artistic creation. In this case the result of such a stunted or imperfect objectivation was most evident in the decrease of character-judgments, which tended to remain in their non-objectivated form, viz. as physiological apperceptions. Hence a certain number of physiological judgments in Series B, which are genuine reversals to the next lower type. The critical point of view is in the same way responsible for a considerable increase of objective judgments, as reversals from the physiological group.

The helplessness of several subjects showed itself in the method which they adopted for making combinations. Instead of picking out the most likely colours to suit from the heap of silks, they were eventually obliged to use the *procedure of excluding the unsuitable* ones, by taking *all* the colours in turn, putting them side by side with the 'standards' and rejecting the unsatisfactory ones. This method was the only possible one in the circumstances, though it lasted instead of the intended half-hour sometimes three times as long, and proved a great strain and fatigue both to the subjects and the experimenter. The subjects affirmed that they were quite unable to conceive of a combination until they had actually seen it, although most of them were by no means deficient in visual imagery. It was the lack of 'creative' imagination which made the method necessary, as it practically did away with *active* selection and retained only the appreciative phase, when the combinations were made for them one by one.

It is only fair to say that women showed rather more aptitude in making combinations than most of the men. It is, therefore, likely that practice is a not inconsiderable factor partly by rendering the transition from the critical to the appreciative attitude easier and the process of selection more flexible, partly by increasing the subject's knowledge of what he or she really likes.

Many subjects found a great difficulty in the unpleasantness of some of the 'standards.' In such cases it was interesting to observe the means which the subjects resorted to to render the disagreeable 'standard' innocuous, provided they did not, as many of them did, abstain from making combinations with them at all.

[1] On the process of 'objectivation' of the colour-content see *Brit. Journ. of Psychol.* 1907, Vol. II. pp. 446–447.

TABLE II.

	No. 19		No. 16		No. 22		No. 1
R.7	+ G. 4 dislikes R. 7; something required to improve it; tried favourite Y. but failed as R. 7 spoilt it + – + –	R. 7	+ Bl. 5 disl. R. 7 + Bl. 4 + –	R. 7	+ Bl. 6 disl. R. 7; tries 'to keep it down'; navy blue required + G. 5 + Br. 2	R. 7	+ G. 4 damping excitement of R. 7 + P. 3 'sensual' combination + R. 6 subordinates itself nicely to R. 7
Y. 4	+ Br. 2 (unsatisfactory) disl. Y. 4 as 'impure'; tried P. to improve it, but improvement did not overbalance dislike of Y. Wanted a black + – + –	Y. 4	+ Bl. 6 'original combination' + G. 4 'not bad for a Y. + G. combination' + –	Y. 4	+ G. 5 tries to 'keep down' Y.; + Bl. 5 tries Bl. 6: horrified at + G. 8 intensity of Y. with it	Y. 4	+ Y. 8 'joy'; healthy + G. 6 'spring' + Br. 1 difference in weight not unpl., Br. motherly
G. 6	+ Y. 1 (favourite) + Br. 2 + Y. 6 (unsatisfactory)	G. 6	+ P. 3 improvement by proximity + G. 5 variations of same tone + R. 7 improvement	G. 6	+ G. 5 + Br. 1 + Bl. 7 balance in strength	G. 6	+ Y. 5 'spring' + Bl. 7 quiet elegance + Y. 6 different from 'spring': more massive
Bl. 4	+ Bl. 6 (attempt to enhance Bl. 4) + Y. 1 (favourite) + G. 5 balance	Bl. 4	+ Y. 8 'makes me laugh' + Y. 5 'stateliness; has a character of its own' + G. 5	Bl. 4	+ Br. 1 Bl. 4 too cold: required something to warm it + Y. 8 + R. 5	Bl. 4	+ G. 5 imposing strength; joyful + R. 8 audacious + Y. 8 'America'; strange, strong
P. 4	+ P. 2 + R. 7 (unsatisfactory) + Br. 2 (pis aller)	P. 4	+ Br. 1 + G. 8 + G. 5	P. 4	+ Br. 1 P. 4 unpl., gloomy: something cheerful wanted + R. 4 + G. 7	P. 4	+ Y. 4 royal + Br. 2 resigned; 'widow's dress' + G. 6 quiet, elegant, dignified
Br. 2	+ Y. 1 (favourite) first disl. Br. 2; + Bl. 7 begins to like it: 'sober' + – Wanted: grey to increase Br.'s 'sobriety'	Br. 2	+ G. 8 balance of tones: 'harmonious' + G. 4 + P. 4	Br. 2	+ R. 2 Br. 2 pl., a little gloomy, cf. above remark + R. 1 + Y. 6	Br. 2	+ R. 7 'late summer'; melancholic + Y. 6 to counteract 'unpretentiousness' of Br. 2 + G. 4 noble, serious, unpretentious

	No. 20		No. 39		No. 31
R. 7	+ P. 5 disl. R. 7: uninteresting, faded + Br. 1 + P. 3 (unsatisfactory)	R. 7	+ Br. 2 R. 7 being so strong in character, requires some + – stayed and subdued char. + – with it to steady and tone it down. Black would be ideal	R. 7	+ R. 5 + Bl. 3 + R. 6
Y. 4	+ Y. 6 disl. Y. 4 + Br. 1 + Y. 7	Y. 4	+ Br. 2 friendly to each other; + – really the same + –	Y. 4	+ Y. 8 'barbaric' + P. 4 'crocusses' + Bl. 5 'flag' Y. 4 unpl., wants to be kept quiet
G. 6	+ Br. 1 + Bl. 5 + Y. 6	G. 6	+ Br. 2 friendly, genial Br. + P. 1 + G. 4 same character	G. 6	+ Bl. 5 + P. 2 + –
Bl. 4	+ Y. 7 disl. Bl. 4 + Bl. 6 'elevates' Bl. 4 + Y. 4 spectacular; 'makes the vulgar more vulgar'	Bl. 4	+ Br. 2 + – + –	Bl. 4	+ P. 4 tried R. but Bl. too cold + Br. 2 + G. 3
P. 4	+ Y. 7 (comb. with P. 4 much easier) + Y. 4 + G. 4	P. 4	+ P. 1 + G. 4 + –	P. 4	+ R. 6 + P. 3 + Bl. 6
Br. 2	+ P. 5 gloomy, but pl. + Bl. 7 + – Required something rich and thoughtful to match the sombreness of Br. 2	Br. 2	+ G. + P. anything will go with it; + R. Br. 2 mere background	Br. 2	+ Bl. 6 + G. 3 + P. 4

One of the principal drawbacks of the method of production, from the point of view of the experimenter, was the marked loss of introspection on the part of the subjects, owing to the above-mentioned difficulties encountered in making combinations. The result is that the total number of data available in Series B, besides being cut short by omissions, is further limited to the relatively small percentage of selections which were accounted for introspectively by the subjects themselves. As in Series A, only *explicit* statements are used in drawing conclusions from the records of Series B.

I add the records of the same subjects quoted in full for Series A, by the way of illustration and comparison (Table II).

(*a*) *Persistence of Types.*

Numerical distribution. As the general remarks made in regard to the persistence of types for Series A apply equally to Series B, I give at once the figures for the latter.

I. The figures for *clear* types are the same as those given for Series A (see p. 415), as they were based upon the consideration of the subjects *in toto* rather than upon their behaviour in each separate series.

II. The first differences appear, on putting together *all* the evidence from the different types, and comparing it with the figures obtained from Series A (see p. 415):

1. *Objective* traces were found in 16 subjects in the shape of 77 (2) = 79 judgments.
2. *Physiological* traces in 31 subjects, in the shape of 173 (16) = 189 judgments.
3. *Character* ,, ,, 16 ,, ,, ,, 85 (3) = 88 ,,
4. *Associative* ,, ,, 7 ,, ,, ,, 24 ,,

Comparing these results with those obtained from Series A (1 : 54, 2 : 145, 3 : 143, and 4 : 62), the higher figures for the objective and physiological judgments and the lower ones for the character and associative criteria are very striking. The difference is the more noteworthy, as the numbers of the subjects are nearly the same, except those of the associative type which shows a decrease from 21 in A to 7 in B.

The differences become more definite in the further analysis of the judgments, distributing them according to the types to which the subjects belong:

III. 1. Of the 79 *objective* judgments, 48 were given by 8 objective and objective-physiological subjects; 31 by 8 physiological, physiological-character and pure character-subjects. Or in the shape of the formula used before:

$$\frac{O, OP}{P, PCH, CH} = \frac{8\,(48)}{8\,(31)} \text{ compared to A}: \frac{O, OP}{P, PCH, CH} = \frac{8\,(39)}{9\,(15)}.$$

2. Of 189 *physiological* judgments, 133 are due to 17 physiological and physiological-objective subjects; 56 to 14 objective, physiological-character and character-subjects:

$$\frac{P, PO}{O, PCH, CH} = \frac{17\,(133)}{14\,(56)} \text{ compared to A}: \frac{P, PO}{O, PCH, CH} = \frac{18\,(93)}{12\,(52)}.$$

3. Of 88 *character* judgments, 85 were given by 14 physiological-character and character-subjects, 3 by 2 objective subjects:

$$\frac{CH, PCH}{O, OP, P} = \frac{14\,(85)}{2\,(3)} \text{ compared to A}: \frac{CH, PCH}{O, OP, P} = \frac{18\,(140)}{1\,(3)}.$$

4. Of the 24 *associative* judgments, 11 were given by 3 associative subjects, 13 by 5 subjects of the other types:

$$\frac{PA, CHA}{O, OP, P, PCH, CH} = \frac{3\,(11)}{5\,(13)} \text{ cpd to A}: \frac{PA, CHA}{O, OP, P, PCH, CH} = \frac{5\,(29)}{16\,(33)}.$$

It is hardly necessary to point out again the predominance of the types. For the physiological and the character-type the numerical superiority of the respective judgments is quite definite. For the associative type, the ratio, as before, between the numbers of subjects and the number of judgments, is, owing to the small total figure, much less striking than in A, but may still be considered as sufficient evidence. Even with regard to objective judgments, the number of those given by objective subjects is higher than that for the other types, though the difference is much smaller than in Series A, owing to reversals. The only further point of interest is the addition, in the character-group, of one objective subject, No. 19, who gave one solitary character-judgment in Series B, the only one in his whole record.

The general increase of objective and physiological judgments, together with a decrease of character-criteria, is entirely due to the before-mentioned unfavourable conditions of the method of production, leading to frequent reversals or undeveloped realisation. The point is borne out by a closer examination of the increases or decreases among the different types.

The increase of objective criteria is brought about, in spite of a decrease of seven judgments, by the addition of 16 *OP*, 12 *P* and 4 *CH* judgments. These figures show even more clearly than those supplied by Series A, the direction of reversals as stated on p. 417. The increase of 44 in the physiological group is due to the addition of 39 judgments given by *P* and *PO* subjects, owing, for the greater part, to the extensive use of 'compensation of physiological effect' in Series B, but also to 5 *CH* judgments, which are instances of true reversals. The same disadvantage of Series B is responsible for the decrease of character-judgments (– 55) and of associations (– 38), neither of which was in the circumstances able to develop fully or to have free play.

(*b*) '*Combination-Criteria.*'

The combination-criteria, being all positive, are fewer in number than in Series A and somewhat different in their numerical distribution. They can be grouped as follows, corresponding to the grouping in Series A:

1. Unification and Dissociation.
2. Implicit Dissociation:
   - (*a*) Balancing of saturation or luminosity.
   - (*b*) Balancing of physiological effects.
   - (*c*) Compensation of physiological effects.
   - (*d*) Compensation of character.
   - (*e*) Enhancement of character.
   - (*f*) Mutual enhancement of the colours.
3. Affinity:
   - (*a*) Presence of a common element.
   - (*b*) Harmony of shades of the same tone.
4. Consonance.

1. *Unification and Dissociation.*

The evidence of Series B for both these points is exceedingly poor. There are in all only seven cases of unification, explicitly stated, of which two are due to balance of the colours, one to compensation of effect, one to presence of a common element, two unexplained and one accompanied by the comment: "sufficient contrast without falling apart." Of explicit dissociations there are no instances at all, except that one subject stated that his liking for separate colours was, he felt, influencing his choices. This I believe to have been characteristic for the selection of Series B as a whole. The whole series suffered from *too much* dissociation. I had originally designed the procedure to test the effects of dissociation: I expected that, if the subjects were given the colours separately to start with and asked to combine them themselves, it would afford an opportunity of observing different stages of dissociation and unification, from the initial objective dissociation to, in certain cases, a perceptive unification. As a matter of fact, very few subjects appear to have reached the latter, as is shown by the prevalence of dissociative criteria and the decrease of character-judgments.

2. *Implicit Dissociation.*

(*a*) *Balancing of saturation or luminosity.* The number of instances is 25, compared to 40 of Series A.

(*b*) *Balancing of physiological effects.* The number of instances is 70, compared to 112 of Series A.

(*c*) *Compensation of physiological effects.* The number of instances is 125, compared to 31 of Series A.

This figure is astonishingly high, but is explicable by the difficulty which the subjects frequently encountered in Series B of having to use an unpleasant 'standard' for making their own combinations. In such cases the compensation of physiological effects offered the easiest solution of dealing with such an unpleasant element. The usual expressions constantly recurring in the records, describing this process, were: "to tone it down," "to enliven it," "to brighten it up," "to steady it," "to keep it down," or "to kill it." Besides, as the combinations were in the making, the compensating effect of different colours, by means of trials, was much more easily observable than in Series A. The prevalence of this criterion is a striking testimony to the tendency to dissociation in this series.

(*d*) *Compensation of character.* The number of instances is 14, the same as that of Series A.

(*e*) *Enhancement of character.* This criterion does not appear in Series A, as conversely 'contradiction of characters' is missing in Series B. By enhancement of character the subjects meant the combination of two colours of similar character which by proximity reinforced each other. It is the opposite of character-contradiction, and something analogous to the balancing effects, though more unified than the latter. The number of instances is 11.

(*f*) *Mutual enhancement of the colours.* The number of instances is 41, compared to 11 cases in which the colours were said to improve each other in Series A.

Again, the making of the combination must have been a powerful factor in the working of this criterion, as the actual enhancement of colours could be more easily tested by trials than in Series A. As with the compensation of physiological effects it marks the pronounced dissociation in the combinations of Series B. These enhancements were, with few exceptions, instances of the use of complementary colours. There were, however, also combinations which, without being complementary in any way, were said to enhance each other. The following are the figures for this group of combinations:

Y. and Bl. = 8.	P. and R. = 2.
P. and G. = 6.	G. and Bl. = 2.
Y. and P. = 5.	G. and Y. = 1.
R. and G. = 4.	Br. and Bl. = 1.
Bl. and Bl. = 4.	G. and Br. = 1.
R. and Y. = 2.	Bl. and P. = 1.
Bl. and R. = 1.	

This question of enhancement I shall have to touch upon again later (see pp. 438—440).

3. *Affinity.*

(*a*) *Presence of a common element.* The remarks made on this point under Series A apply also to Series B. The affinity may consist in a common-tone element, in balance of luminosity or the equal degree of dullness or darkness. In three instances it may have been due to the complementary relation of the tones, at least No. 27 was quite satisfied that *P* and *G*, *G* and *R*, and *Y* and *P* possessed sufficient affinity to form a combination.

The number of instances is 29 compared to 31 of Series A.

(*b*) '*Harmonies of different shades of the same tone.*' As opposed to the small number of judgments based on this criterion in Series A, there is a very large number of such combinations in Series B, 93 in all. This high figure represents, however, many such combinations which were merely makeshifts, on the admission of the subjects themselves. On the whole the subjects may be divided into three groups. The first studiously avoids combinations of this kind, which they do not consider combinations at all, or object to them, as stated earlier, on the ground that they conflict with the principle of balance of luminosity or saturation. The second group has a special weakness for such 'harmonies of shades,' finding in them a particularly delicate attraction, due most probably to the fundamental unity imparted by the sameness of the tone, together with the suggestion of continuity implied in such gradations. The third group uses them as a *pis aller*, being unable to make combinations they really like. There is all the difference in seeing people of the second and third group at work.

The first go at once for combinations of this kind, the latter, after endless searches and trials, all unavailing, reluctantly pick out some darker or lighter tone of the 'standard.' This class is by far the largest and renders the high figure for this group of combinations somewhat fictitious.

4. *Consonances.*

A very small group of seven cases. They are, if anything, still more obscure than those of Series A, owing to the absence of any introspective explanations and the small number which renders any comparisons impossible.

For the purpose of a better survey, I arrange all the evidence for the persistence of the types and for the frequency of 'combination-criteria' in one table: Table III.

To Series B. N.B. 1. In regard to the *material complication* and *quantitative complication*, the same remarks apply as to Series A, except that, owing to the lack of introspection, the evidence is much poorer. There were three subjects who complained about the material complication or on whose selections it seemed to exercise a strong influence.

Four subjects occasionally expressed the wish to have the quantitative relation of the colour-elements altered, in the same manner and with the same purpose as in Series A.

N.B. 2. A matter of special interest is offered by the consideration of the *objective results of Series B.* Which colours have been most frequently combined? In particular, what is the evidence of the records, for or against, *complementary combinations*?

The analysis of the records of Series B from this point of view, furnishes the results shown in the appended Table IV, which gives the number and percentage of the selections from all colours to form combinations with the six 'standards.'

The nearest approximations to complementary pairs available among the material, would have been $R_7 + G_6$, $Bl_4 + Y_4$ and $P_4 + G_7$. One would, therefore, have expected (assuming the correctness of the theory that complementary pairs are generally preferred), the combinations to be selected from the tone-series $R + G$, $Bl + Y$ and $P + G$, allowing for fluctuations round the actually closest representatives of the complementary relations.

TABLE III.

a. **Persistence of Types**

1.	*Objective* Type:	Series A / Series B	Number of Judgments:	54 / 79	Total: 133
2.	*Physiological* Type:	Series A / Series B	Number of Judgments:	145 / 189	Total: 334
3.	*Character*-Type:	Series A / Series B	Number of Judgments:	143 / 88	Total: 231
4.	*Associative* Type:	Series A / Series B	Number of Judgments:	62 / 24	Total: 86

b. **'Combination-Criteria'**

1. *a.*	*Unification:*	Series A / Series B	31 / 7	Total: 38
b.	*Dissociation:*	Series A / Series B	50 / —	Total: 50

2. *Implicit Dissociation*

a.	Balance of sat. or lum.:	Series A / Series B	40 / 25	Total: 65
b.	Balance of phys. effects:	Series A / Series B	112 / 70	Total: 182
c.	Compensation of phys. effects:	Series A / Series B	31 / 125	Total: 156
d.	Compensation of Character:	Series A / Series B	14 / 14	Total: 63
	Contradiction of Character:	Series A	24	
	Enhancement of Character:	Series B	11	
e.	Dislike of one colour renders the combination unpleasant:	Series A	108	Total: 108
f.	Liking of one colour renders the combination pleasant:	Series A	15	Total: 15
g.	One colour 'spoils' the other:	Series A	39	Total: 39
h.	Mutual enhancement	Series A / Series B	11 / 41	Total: 52

3. *Affinity*

a.	Presence of a common element:	Series A / Series B	31 / 29	Total: 60
b.	'Harmony of Shades':	Series A / Series B	20 / 93	Total: 113

4.	*Consonance and Dissonance:*	Series A / Series B	Number of Judgments:	71 / 7	Total: 78

TABLE IV.

[The highest figures are italicised.]

	R.		Y.		G.		Bl.		P.		Br.	
	No.	p.c.	No.	p.c.	No.	p.c.	No.	p.c.	No.	p.c.	No.	p.c.
R.	18	16·66	7	6·48	22	20·37	21	19·09	15	13·39	13	11·30
$Y._1$	5	4·63	9	8·33	19	17·59	*23*	*20·90*	18	16·07	20	17·39
$Y._2$	1	0·92	6	5·55	13	12·03	6	5·45	3	2·67	3	2·60
G.	26	24·07	23	21·29	11	10·27	*23*	*20·90*	*27*	*24·10*	*33*	*28·69*
Bl.	*30*	*27·77*	*30*	*27·77*	*25*	*23·07*	17	15·45	14	12·50	20	17·39
P.	19	17·59	14	12·98	*25*	*23·07*	7	6·36	24	21·43	16	13·91
Br.	9	8·33	18	16·66	13	12·03	13	11·81	10	8·92	7	6·08

The results *confirm* this expectation in the combinations made with standards Y and P : $Y + Bl = 27·77\ ^\circ/_\circ$, and $P + G = 24·10\ ^\circ/_\circ$.

They *contradict* it, however, in the combination with standard $R : R + G = 24·07\ ^\circ/_\circ$ against the highest frequency : $R + Bl = 27·77\ ^\circ/_\circ$. In the combinations with standard Bl, the expected Y ties with $G : Bl + Y$ or $G = 20·90\ ^\circ/_\circ$; with standard G the combinations with P may fall under the class of complementary combinations, though R would have been nearer the mark, but it ties with Bl, which is certainly far from being in complementary relation to G.

The result is that complementary combinations are *not* generally the most frequently selected and that the complementary relation does not seem to be the decisive factor in the preference of colour-combinations.

Is it possible to obtain any information from the introspective evidence of the subjects, with regard to complementary combinations? Unfortunately, as stated before, the introspective testimony has been very poor in Series B. As far as any account has been given by subjects as to their motive in selecting complementary combinations—

most of them were unaware that they were complementary—the two principles of 'enhancement' (see p. 433) and of 'compensation of physiological effects' (see p. 433) have been stated to be the reasons of choice. The numbers of instances in which they have been given as explanation are:

(*a*) '*Enhancement*':

for	$R + G$: 3	times out of	26
	$Y + Bl$: 2	„ „	27
	$G + R$: 5	„ „	22
	$Bl + Y$: 5	„ „	23
	$P + G$: 4	„ „	27

(*b*) '*Compensation of physiological effect*':

for	$R + G$: 4	times out of	26
	$Y + Bl$: 7	„ „	27
	$G + R$: 3	„ „	22
	$Bl + Y$: 4	„ „	23
	$P + G$: 12	„ „	27

The remainder of instances after subtracting the cases under these two headings is for the greater part unaccounted for, though a certain number were explained by balance of luminosity or saturation, and a few by reference to 'type-criteria,' irrelevant to the present question.

There is a predominance of 'compensation of physiological effects over 'enhancement' (30 : 19), which would, I think, have been much larger, if *all* the instances had been introspectively accounted for. The reason for this I find in the enormous preponderance of the total number of 'compensation' cases over those of 'enhancement': 125 against 41 (see p. 436 Table of 'comb.-criteria,' 2, *c* and *h*), and in the generally more frequent use of physiological compared to objective criteria, to which enhancement undoubtedly belongs.

In short, I believe '*compensation of physiological effect*' and in a lesser degree '*enhancement*' to be *the psychological explanation for the combination of complementary colours.*

Then, what is the relation of complementary colours to these two principles?

The complementary relation I take to formulate primarily the *physical* fact that two complementaries, when mixed, produce white. This fact has been taken by many aestheticians to imply a fundamental *unity* between complementaries, the tone-differentiations of which are to constitute the *variety* of the aesthetic object, thus bringing combinations of complementaries under the favourite 'unity-in-variety' principle. There is no question that this interpretation is false, presenting, as do several other so-called aesthetic principles, a confusion between physical and sensational-aesthetic facts. This physical affinity is as little sensationally given or consciously existent, as the vibration-ratio in Lipps'[1] extravagant theory of our pleasure in single colours, or in Raymond's[2] equally contestable hypothesis concerning the consonance of vibration-ratios in complementary combinations.

Secondly, the complementary relation formulates a *psychological fact* which I divide into two kinds of effect, relevant in this connection: on the one side, by simultaneous contrast, two complementaries in juxtaposition cause each other to appear more saturated than they would be, when seen singly: this would correspond to '*enhancement*,' as used by the subjects in the records. On the other side, the eye finds a certain relief when passing from one colour to its complementary (successive contrast), corresponding, up to a certain point, to '*compensation of physiological effects*.' It is certainly striking that compensation is said by the subjects to take place between such colours as *R* and *G*, *Y* and *Bl*, *P* and *G*. It may be more than a coincidence that *R* is exciting and *G* restful, *Y* warm and *Bl* cold, *P* depressing and *G* refreshing.

At the same time it must be borne in mind that both 'enhancement' and 'compensation of physiological effect' have a much wider range than the complementary relation. Colours may enhance each other without being complementaries, as frequently happened in the records (see p. 434). It depends on what is to be enhanced: the luminosity of blue for instance may be enhanced by placing a darker shade of the same tone next to it. One subject (No. 36) tried to give the standard *P* the "right tone" by combining it with *Y* and *R*. The compensation, on the other hand, can be more easily obtained in some cases by other colours, than by the use of a complementary. Thus red is more effectively "cooled" or "kept down" by a blue—which explains the high figure for this combination—than by a green; a yellow may be more thoroughly "toned down" by a purple or a brown than by a blue.

To sum up: the principles of 'enhancement' and 'compensation of physiological effects' coincide, but only in part, with the comple-

[1] Th. Lipps, *Æsthetik*, I. Hamburg und Leipzig, 1903, pp. 27–28.

[2] G. L. Raymond, *Essentials of Aesthetics*, London, 1907, pp. 374–378.

mentary relation. The latter, if used as a *physical* fact, is a false interpretation; if used to express a psychological fact, it is insufficient, though the coincidence may be more than mere chance. I cannot escape the impression that, on the whole, as an *aesthetic* principle, it plays as unsatisfactory a part as that which the 'Golden Section' has proved to play in the explanation of our pleasure in linear complexes: it coincides with a certain *average* of our preferences, but it is too inflexible a theory to embrace and account for the fluctuations and subtler differences of aesthetic appreciations, which are not reducible to an average without losing their essence and their meaning.

(c) *The relation of perceptive types to the 'combination-criteria.'*

Series A and B.

The question as to what the relation between perceptive types and 'combination-criteria' is, presupposes the existence of such a relation. This, however, is by no means self-evident, since, as I pointed out earlier, the 'type-criteria' differ in origin from the 'combination-criteria' and need not, therefore, be assumed to cooperate or stand in actual relation to each other. It may be supposed, on the other hand, as I said in the beginning (p. 407), that adherence to a certain type predisposes to the use of certain 'combination-criteria.' This, it will be seen, is true in the case of some criteria which would be impossible without the existence of certain types; in other cases the 'combination-criterion' appears to be independent of the types, and called into activity solely by the features of the combination.

The manner of dealing with this question of the relation between types and 'combination-criteria' consisted in analysing the judgments based upon the latter according to the types of the subjects[1]. It is the same procedure which I used in dealing with the evidence for the persistence of types in Series A and B, under heading III. I shall deal with the 'combination-criteria' in the order of Table III, p. 436.

1. (*a*) *Unification.*

Series A: Total 31. Series B: Total 7.

The 31 judgments of Series A have been given by 5 *OP* and 5 *CH*-subjects; 8 by the former, 23 by the latter. The 7 judgments of

[1] For the purpose of these calculations I shall take *O* and *P* together as one group, and *PCH* and *CH* together as another group. The total number of the former is 19, of the latter (simply marked *CH*) is 21. In this way the two groups, being practically equal, allow of comparisons, which would otherwise be impossible.

Series B are shared by 5 subjects, so that 6 fall to 4 OP, 1 to 1 CH-subject. Or in the previously used formula:

$$A:\frac{OP}{CH}=\frac{5\,(8)}{5\,(23)},\qquad B:\frac{OP}{CH}=\frac{4\,(6)}{1\,(1)}.$$

Neither of these formulae represents, however, the actual facts, as practically all the character-judgments were unified. Though the subjects did not state it expressly, the very fact that the combinations possessed character, was tantamount to a unified apperception. At the same time, it is necessary to deduct from the character-judgments of Series A all the judgments of No. 39, viz. 15, and 14 'character-contradictions' (24 less 10 judgments included in the 15 of No. 39), as all these 24 judgments were dissociated. Similarly, on the side of the subjects, No. 39 and the five subjects included in the above formula must be deducted from the total number 19, leaving 13.

The formula for Series A must, therefore, be corrected by the addition of

$$\frac{OP}{CH}=\frac{0\,(0)}{13\,(89)}=\frac{5\,(8)}{18\,(106)}\text{ for Series A.}$$

Correcting the second formula in the same way by the addition of

$$\frac{OP}{CH}=\frac{1\,(1)}{12\,(61)}=\frac{5\,(7)}{13\,(62)},$$

we obtain a total for both Series of

$$\frac{OP}{CH}=\frac{10\,(15)}{31\,(168)}\text{ or }=\frac{1\,(1)}{3\,(11{\cdot}2)}.$$

It will be seen that there are three times as many CH-subjects as OP-subjects who unify and 11·2 times as many unified CH-judgments as OP-judgments.

Unification appears, consequently, to be more a feature of the character-type than of the objective or physiological type.

(b) *Dissociation.*

I have given the above calculation rather in full in order to show its working; henceforth I shall merely give the complete formulae, especially as the facts are much simpler than in the case of unification.

Series A: Total $50=\frac{OP}{CH}=\frac{9\,(17)}{12\,(23)}$ + dissoc. CH (not already included): $\frac{0\,(0)}{6\,(18)}=\frac{9\,(17)}{18\,(51)}$ or $\frac{OP}{CH}=\frac{1\,(1)}{2\,(3)}$.

∴ a predominance of dissociation as ground for rejection[1] in favour of character-type.

2. *Implicit Dissociation.*

(*a*) *Balance of saturation or luminosity.*

$$\text{Total: } 65 = \begin{cases} \text{Series A: } \dfrac{OP}{CH} = \dfrac{8\,(27)}{5\,(13)} \\ \text{Series B: } \dfrac{OP}{CH} = \dfrac{6\,(20)}{2\,(5)} \end{cases}$$

$$\text{Total: } \frac{OP}{CH} = \frac{14\,(47)}{7\,(18)} = \text{(about) } \frac{2\,(2{\cdot}5)}{1\,(1)}.$$

∴ a predominance of this criterion on the side of OP-subjects.

(*b*) *Balance of physiological effects.*

$$\text{Total: } 182 = \begin{cases} \text{Series A: } \dfrac{OP}{CH} = \dfrac{28\,(87)}{11\,(25)} \\ \text{Series B: } \dfrac{OP}{CH} = \dfrac{12\,(62)}{5\,(8)} \end{cases}$$

$$\text{Total: } \frac{OP}{CH} = \frac{40\,(149)}{16\,(33)} = \frac{2{\cdot}5\,(4{\cdot}51)}{1\,(1)}.$$

∴ a predominance of this criterion on the side of OP-subjects.

(*c*) *Compensation of physiological effects.*

$$\text{Total: } 156 = \begin{cases} \text{Series A: } \dfrac{OP}{CH} = \dfrac{11\,(24)}{5\,(7)} \\ \text{Series B: } \dfrac{OP}{CH} = \dfrac{17\,(88)}{9\,(37)} \end{cases}$$

$$\text{Total: } \frac{OP}{CH} = \frac{28\,(112)}{14\,(44)} = \frac{2\,(2{\cdot}54)}{1\,(1)}.$$

∴ a predominance of OP-types (especially as a number of CH-subjects and CH-judgments in Series B are due to reversals).

(*d*) *Compensation of character* (including contradiction and enhancement of character).

$$\text{Total: } 63 = \begin{cases} \text{Series A: } \dfrac{OP}{CH} = \dfrac{1\,(0)}{13\,(38)} \\ \text{Series B: } \dfrac{OP}{CH} = \dfrac{1\,(1)}{12\,(24)} \end{cases}$$

$$\text{Total: } \frac{OP}{CH} = \frac{2\,(1)}{25\,(62)}.$$

∴ practically an exclusive predominance of the CH-types.

[1] It is true that dissociated CH-judgments *need* not necessarily be negative, as stated on p. 421. As they, however, were sometimes negative, and certainly dissociated, I have included them here for simplicity's sake.

(*e*) *Dislike of one colour renders the combination unpleasant.*

$$\text{Total}: 108 = \text{Series A}: \frac{OP}{CH} = \frac{17\,(70)}{12\,(38)} = \text{about } \frac{OP}{CH} = \frac{1\cdot4\,(2)}{1\,(1)}.$$

∴ a slight predominance of OP-types.

(*f*) *Liking of one colour renders the combination pleasant.*

$$\text{Total}: 15 = \text{Series A}: \frac{OP}{CH} = \frac{5\,(12)}{2\,(7)} = \frac{OP}{CH} = \frac{2\cdot5\,(1\cdot7)}{1\,(1)}.$$

∴ a predominance of OP-types.

(*g*) *One colour 'spoils' the other.*

$$\text{Total}: 39 = \text{Series A}: \frac{OP}{CH} = \frac{14\,(26)}{7\,(13)} = \frac{OP}{CH} = \frac{2\,(2)}{1\,(1)}.$$

∴ a predominance of OP-types.

(*h*) *Mutual enhancement of colours.*

$$\text{Total}: 52 = \begin{cases} \text{Series A}: \dfrac{OP}{CH} = \dfrac{4\,(10)}{1\,(1)} \\ \text{Series B}: \dfrac{OP}{CH} = \dfrac{10\,(37)}{3\,(4)} \end{cases}$$

$$\text{Total}: \frac{OP}{CH} = \frac{14\,(47)}{4\,(5)} = \frac{3\cdot5\,(9\cdot4)}{1\,(1)}$$

∴ a marked predominance of the OP-types.

3. *Affinity.*

(*a*) *Presence of a common element.*

$$\text{Total}: 60 = \begin{cases} \text{Series A}: \dfrac{OP}{CH} = \dfrac{15\,(26)}{5\,(5)} \\ \text{Series B}: \dfrac{OP}{CH} = \dfrac{6\,(24)}{3\,(5)} \end{cases}$$

$$\text{Total}: \frac{OP}{CH} = \frac{21\,(50)}{8\,(10)} = \frac{2\cdot62\,(5)}{1\,(1)}.$$

∴ a marked predominance of OP-types.

(*b*) '*Harmony of shades.*'

$$\text{Total}: 113 = \begin{cases} \text{Series A}: \dfrac{OP}{CH} = \dfrac{15\,(15)}{5\,(5)} \\ \text{Series B}: \dfrac{OP}{CH} = \dfrac{16\,(54)}{12\,(39)} \end{cases}$$

$$\text{Total}: \frac{OP}{CH} = \frac{31\,(69)}{17\,(44)} = \frac{1\cdot8\,(1\cdot57)}{1\,(1)}.$$

∴ a slight predominance of OP-types.

N.B. The figures for Series A correspond probably more accurately to the facts, as in Series B the numbers are somewhat fictitious owing to reversals and difficulties of choice (see p. 434).

4. *Consonance and Dissonance.*

$$\text{Total}: 78 = \begin{cases} \text{Series A}: \dfrac{OP}{CH} = \dfrac{16\,(38)}{16\,(33)} \\ \text{Series B}: \dfrac{OP}{CH} = \dfrac{3\,(4)}{2\,(3)} \end{cases}$$

$$\text{Total}: \frac{OP}{CH} = \frac{19\,(42)}{18\,(36)}.$$

$\therefore$ there is practically no difference in the behaviour of the two groups of types in respect to this criterion.

Summarising this analysis, the following criteria appeared to be peculiar *to the physiological or objective type*[1]:

1. *Balance of saturation or luminosity* (probably predominantly objective).
2. *Balance of physiological effects* (predominantly physiological).
3. *Compensation of physiological effects* (predominantly physiological).
4. *Dislike of one colour renders combination unpleasant.*
5. *Liking of one colour renders the combination pleasant.*

[1] As the experiments discussed in this paper were undertaken independently of experimental work upon colours published by others, I have abstained from a discussion of their results. I should like, however, to illustrate the importance of perceptive types by indicating one consequence of their neglect in a recent publication on the Aesthetics of Colour (E. Utitz, *Grundzüge der Aesthetischen Farbenlehre*, Stuttgart, 1908). The author maintains the pleasantness of complementary colour-combinations on the ground of a set of experiments on 18 subjects, who more or less persistently preferred them. These subjects, he states (p. 40), he selected mainly from painters and persons studying Art, because the saturated colours he used 'have the disadvantage of appearing to the subjects as unusual and as too "glaring," "hard" or "brutal."...The results are more satisfactory, *i.e.* more judgments of approval are given, if the subjects are a little conversant with colours' (p. 38). A consideration of the perceptive types would have saved him from thus, unconsciously, manufacturing his evidence. I have frequently noticed that painters and subjects otherwise professionally interested in colour-combinations adopt, as a result of this interest, the objective attitude, and with it the criteria peculiar to this type. It is, therefore, not surprising, though hardly evidential, that complementary combinations have been preferred by Dr Utitz' subjects, because 'enhancement' is a characteristically objective criterion. Yet a professional interest is by no means identical with aesthetic appreciation, even among artists. Dr Utitz' results are due to the generalisation of one special type, a defect under which nearly all the writers on Aesthetics have suffered up to the present.

6. *One colour 'spoils' the other.*

7. *Mutual enhancement of colours* (predominantly objective).

8. *Presence of a common element* (predominantly objective).

9. *'Harmony of shades'* (predominantly objective).

The following criteria appeared to belong to the '*character-type*':

1. *Unification and Dissociation* (exclusive of implicit dissociation).

2. *Compensation of character* (including contradiction and enhancement of character).

3. Under unfavourable conditions *compensation of physiological effects.*

Common to both types:

1. *Consonance and Dissonance.*

2. Under unfavourable conditions probably also: *dislike of one colour renders the combination unpleasant.*

Conclusion.

A systematisation of the results reached in the last paragraph would, I think, be possible under the heading of *Unification and Dissociation.* Though this principle occurs separately as a special criterion, the range of its influence is much wider than would appear from the *explicit* use made of it by the subjects. I have, for instance, pointed out in discussing 'character-compensation' and 'character-contradiction,' that the ultimate difference lies in the degree of unity imparted apperceptively to the combinations. Again, I have indicated the importance which attaches to it, by grouping eight of the 'combination-criteria' under the heading of 'Implicit Dissociation.' In fact, *implicitly* it runs through all the various kinds of appreciation and is the only one capable of covering all its forms.

At the same time, unification represents an *aesthetic* principle of very definite value. It appears both as a feature of the aesthetic object, whether objectively existing or apperceptively imposed upon it, and as characteristic of the subject's own attitude, in the shape of the 'autotelicity' of the aesthetic experience. I have made use of unification in both these senses in endeavouring at the end of my previous paper, to fix the relative aesthetic values of the different types; and I am convinced that in a formulation of the 'aesthetic consciousness' as the central fact of all aesthetic experience, it must play, not perhaps the sole, but the predominant part.

An arrangement, therefore, under these two headings, of the types and the 'combination-criteria,' indicating by their relative position the degree in which they partake of either unification or dissociation, will at once serve the two purposes of attempting a systematic survey of the types and 'combination-criteria,' and of showing the relative aesthetic values which, I think, they may severally claim:

Unification		Dissociation
Character-type		
	Compensation of character	
Enhancement of character		Contradiction of character
		Physiological type
	Balance of physiological effects	
	Compensation of physiological effects	
		Dislike and liking of one colour
		Objective type
	Balance of saturation and luminosity	
		Mutual enhancement of colours
	Affinity	
		Common element
	Harmony of shades	
Associative type		*Associative type*

As a special point of methodological importance I should like to mention in conclusion the *inferiority of the method of production* compared to the method of appreciation, a matter of general psychological interest.

As a point affecting aesthetic methods in particular, I have been struck by the *inequivalence of negative and positive appreciation.* The practical expression of this fact has been the tendency to reversals of the types: in negative judgments especially a person will not reach the highest level of appreciation of which he may be capable, a fact which it is easy to observe as a matter of ordinary experience. It is, therefore, dangerous to conclude from negative criticisms to the possible positive grounds of acceptance, as their exact opposite. The positive appreciation will generally lie on a higher and more developed plane than such an inferred reason for preference.

As has been pointed out by Legowski[1], the experiences with which aesthetic experiments deal, are not merely *parts* of wider and fuller impressions, such as those realised before works of the different arts. But, however intrinsically unimportant the object may be, they are

[1] L. W. Legowski, "Beiträge zur experimentellen Aesthetik," *Arch. für d. ges. Psychol.* 1908, p. 311.

complete, though simplified, aesthetic experiences. The explanatory value of experiments does not consist in accounting for the effects of lines and colours, sounds and rhythms, in so far as they may be combined into a total effect of a picture or a piece of music, and enter as part-impressions into the appreciation of the whole. Their value lies in the light which they throw upon aesthetic experience in general, which they represent, so to speak, in miniature.

Thus these colour-tests afford no direct explanation of the appreciation of pictures, partly because the colours in paintings appear in a definite representative context, possessing a local meaning which obscures the pure colour-effects, partly especially because the object of the painter is not to paint colour but light, as has repeatedly been stated by artists and has quite recently been most convincingly expounded by M. Alfred Binet[1]. Experiments may, however, contribute to an indirect explanation of aesthetic appreciation as a whole by the study of one of its many forms.

While aiming on the one hand at the investigation of colour-appreciation pure and simple, I have been conscious at the same time of the bearings which such studies must have upon the understanding of aesthetic experiences in general, and I am confident that the explanation of the one will lead to general principles applicable to the other. This confidence I feel in respect of both the process of 'objectivation,' as a phase of artistic creation generally, and of the 'perceptive types,' which, I am sure, will prove of value in the study of other aesthetic impressions, after due deduction of the differences in presentment and material, introduced by an appeal to other senses and by differently constituted objects.

[1] A. Binet, 'Le Mystère de la Peinture,' *Année psychologique*, 1909, p. 300, '...il a un autre souci, c'est de faire de la lumière; il ne peut faire de la lumière qu'avec des couleurs; et pour y arriver, il est obligé à des sacrifices; *il est obligé de décolorer, pour faire de la lumière*' (p. 312).

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PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

June 24, 1910[1]. Instinct and Intelligence, by C. S. Myers, C. Lloyd Morgan, H. W. Carr, G. F. Stout, and W. McDougall.

June 25, 1910[1]. On Secondary Qualities independent of Perception, by T. P. Nunn and F. C. S. Schiller.

The 'Faculty' Doctrine. Outlines of some Experiments on School Children in Relation to the above Doctrine, by W. H. Winch.

The Aesthetic Appreciation of Simple Colour Combinations, by E. Bullough.

The Nature and Development of Attention, by G. Dawes Hicks.

Nov. 12, 1910. Some Features of the Visual Illusion of Filled and Unfilled Space, by E. O. Lewis.

The Relation between the Total Fusion of a Chord and the Relative Positions of its Constituent Intervals, by T. H. Pear.

[1] In conjunction with the Aristotelian Society and the *Mind* Association.

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