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THE BRITISH JOURNAL OF PSYCHOLOGY

INSTINCT, ESPECIALLY IN SOLITARY WASPS.

BY CARVETH READ.

I. THE PLACE OF INSTINCT IN ANIMAL DEVELOPMENT.

§ 1. IN a recent number of the *British Journal of Psychology*, the relations between Instinct and Intelligence were fully discussed and illustrated in various aspects by several distinguished writers. There was general agreement that these functions are not opposed one to another in any exclusive way but co-operate in the life of animals. This is so plain to anyone who considers the facts that we wonder how other views should still so widely prevail. The popular belief, shared by some scientific men, seems to depend upon three influences: (1) the doctrine of special creation, according to which each kind of animal has its own endowment of instincts, infallible, invariable, and universally present in the individuals of each kind; (2) the Cartesian doctrine that every animal is a machine, whilst man alone is intelligent; (3) Spencer's theory of the development in animals of adjustment to external relations from irritability and contractility in zoophytes, simple reflex action in creatures that are first found with distinct tissues of nerves and muscles, through compound reflexes to instincts, in which consciousness first appears, and finally intelligence when instinct breaks down because the increasing complexity of the conditions which an organism has to deal with can only be met by a hesitating reaction. Romanes criticised Spencer's theory effectively at many points; maintaining that instinct and intelligence are not successive manifestations of conscious life; but that, while "secondary instincts" are due to inherited habits and "lapsed intelligence" (Lewes), and therefore later than intelligence, the greater number of instincts, the "primary" on the one hand, and intelligence on the other, are, by natural selection, differentiated from the common ground of perception. He did not, however, realise how early the signs of consciousness and intelligence are manifested in animal life, and the doctrine of inherited habits has become unpopular amongst Biologists.

Even popular observation knows that instincts are not infallible, that birds sometimes fail to migrate in time, that the hosts of a cuckoo cannot distinguish the parasite's egg or fledgling from their own, that cattle sometimes devour poisonous herbs, and so on; and it is plain that, if animals had once been infallibly endowed, nothing but a perpetual miracle could have kept them adapted to the ever-changing conditions of their habitat. The variability of instincts, as in the nesting of birds, is also in fact popularly known; and if they were not variable, their origin could never be explained. That instincts and all animal activities may be explained by comparing an animal with a machine has a certain value by way of generalisation; and it is better than any hypothesis of vitalism, that merely gives a general name to the activities of living things without explaining any of them; but as the notion of mechanism takes no account of all the *propria* of living things in which they differ from machines, it can only stand in the background as a type of the precision and rigour of causal explanation that is to be sought for in Biology[1].

[1] See the discussion of this matter by H. S. Jennings in the *American Journ. of Psych.*, July, 1910.

The "universality" of instinct amongst animals may be understood in two ways. First it may mean that every individual of any species exhibits the same instincts, if not always in the same perfection. But this is not true wherever dimorphism appears, whether sexually or in the more special forms that are found in ants and termites; since each sex- or "caste-" form (soldier or worker) has its own instincts. But the "universality" of instinct may be understood in another way, namely that instinctive action is found throughout the animal kingdom (except in man), or even that it is the sole adaptive function of animals. But when we spread out the facts before us three observations are almost unavoidable. The first is that the order of development indicated by Spencer—irritability, reflex action, compound reflex action, instinct—has some validity. It seems at least highly convenient to confine the term instinct to certain complex modes of reaction which have in general the aspect of compound reflexes, but may be distinguished from other compound reflexes by involving the adjustment—apparently the attentive adjustment—of the animal as a whole, and thereby exhibiting what has been called "behaviour" (Lloyd Morgan). [Other differences will be mentioned below.] But then innumerable animals below the Coelenterata, having no nervous system, and therefore no reflex action, and incapable of what we call instinct, nevertheless exhibit behaviour—adjustment of the animal as a whole. To describe it as irritability and contractility gives no adequate impression of the unity of their life. Their activities have been aptly termed "tropisms," being in close analogy with the reactions of plants to light, contact, gravity; and how high such reactions can rise without a nervous system may be read in many passages of Darwin's botanical writings. On the other hand, when we look at the higher levels of vertebrate life, especially amongst monkeys, we see that their behaviour rarely exhibits the regularity and predictability that are usually considered to be appropriate marks of instinct.

The second observation which on a fair view of the facts we can hardly help making is, that the behaviour of even the simplest animals, exhibiting only tropisms, is not entirely predictable; does not depend merely upon stimuli; but also upon each animal's internal condition; and moreover that it shows plasticity, reactions to stimuli that are not prearranged, "fatal" adjustments, but in the nature of trials; and finally that their behaviour is not without intelligence, but evinces (*a*) discrimination and recognition by pursuit and avoidance, and (*b*) memory, or learning by the experience that results from its trial-movements.

My third observation is that the tropisms and intelligence that appear in the earliest forms of life never afterwards disappear. For we cannot suppose with Spencer and Romanes that consciousness is first found at some stage in the development of animals, as a result of the complexity of organisation, hesitancy or delay of reaction, seeing that the signs of consciousness are present in the simplest; nor do we find that discrimination and memory are at any stage lost, to be afterwards recovered. There is, no doubt, opposition between the complete organisation of a reflex arc, and the manifestation of intelligence; and we are apt to assume that the whole life of a lowly organism is similar to the more completely integrated structures of our own bodies, which seem to us unconscious. But this is a very disputable analogy; for in complex organisms with division of labour the lead in adaptive variation is restricted to special organs. As intelligence persists, so also do the tropisms at all stages of development; originally independent of a nervous system, they are taken up by such systems as soon as these are formed, and extensively influence the conduct of all animals, including ourselves. I am inclined to believe that the explanation of every instinct will lead us back to a tropism or tropisms.

§ 2. Instinct, then, is a name for certain complex functions that help to maintain the life of animals at certain stages of development, but at earlier stages do not yet exist, and at the latest stages are superseded, or rather so modified by intelligence as to deserve another name—instinctive dispositions. To see the full meaning of this it may be well to distinguish two kinds of instincts: (*a*) the impulsive kind such as pugnacity, flight, gregariousness, which are satisfied each by complex activities of similar quality having one end—to overcome, to escape, to mingle with the herd; and (*b*) chain-instincts, such as migration and spawning, nesting and incubation, etc.; which, though tending to one result, yet advance by a series of diverse actions each of which probably has its own satisfaction. It is of these chain-instincts that solitary wasps present the most astonishing examples. And when we say that instincts fail to develop in the higher vertebrates, it is of the chain-instincts that this is most nearly true. For the impulsive instincts, though normally modified or restrained by an intelligent appreciation of circumstances, yet sometimes, when the stimulus is strong (relatively to the individual) and the conditions of inhibition are weak (or faintly appreciated), display themselves even in civilised men with all the features of brutal incontinence. The chain-instincts everywhere subserve especially the reproduction of the species; and in the higher vertebrates that stage of the instinct which is concerned with the rearing of offspring is carried out with such intelligent variety of adaptations to circumstances that its instinctive character is almost completely disguised. Yet when we consider that the end in nature of all such activities is the ensuring of future generations, it becomes plain that no animal can understand this, and that therefore the activities remain essentially instinctive until some pretty late condition of savage humanity; and even then perhaps may be possible for "intelligence," not by a representation of the end in nature, but only through an idea of the necessity of having successors to perform one's funerary rites; for a knowledge of the end can be of no avail if it be not strongly desired. Indeed the desire for posterity in most of us would not stay the world from depopulation without the instinctive affection of mothers.

Whilst, on the one hand, instincts are only masked by intelligent activities (to which they impart all their vigour), on the other hand, they cannot, I believe, be marked off from compound reflexes definitely, but only by considerations of more or less through a series of distinctions. (*a*) Instincts, as we have seen, are connate connections between stimuli and reactions of the organism as a whole; but this definition includes the action of a dog scratching himself, of a child writhing on being tickled; and such actions are usually considered to be compound reflexes. (*b*) Probably instincts are accompanied by the fuller consciousness of some object and cravings aroused by it, of effort and excitement in working themselves out, of satisfaction in the fulfilment; but without much forcing these features may be found in the scratch-reflex. (*c*) In reflex action the purpose served is often immediate, as in blinking to moisten the eye, withdrawing one's hand from a thorn; whereas many instincts have remote ends, as in making a nest, or storing nuts for the winter: yet the instinctive action of lying quite still may have the immediate effect of escaping an enemy; whilst the reflex of swallowing has the remote end of digestion and assimilation: but blinking and swallowing are not actions of the organism as a whole. (*d*) Instincts are the more complex; they often consist of many diverse yet co-ordinated actions, whether in chains like nesting, incubation, etc., or in the miscellaneous distribution of a fight; whereas even a chain-reflex, such as swallowing, consists in the repetition of the same action; or if it be said that swallowing must be taken with the ensuing diverse processes of digestion, still there is nowhere the activity of the whole organism. (*e*) Whilst reflex action is often, instinct is always, concerned with external relations: thus eating is the instinctive beginning which swallowing gives effect to. And it is this concern with external relations, even to remote events, that excites our wonder, because in this it is like reason; whereas the complex internal physiological processes, common to us with the animals and "unconscious" in both, are taken as a matter of course, though no less wonderful. (*f*) Whilst in most animals the reflexes are organised at birth and in none are long deferred, the instincts may not appear until late in life; their action is then a mark of maturity, constitutes a crisis in the animal's life-history, and alters its whole character. (*g*) Moreover, being later acquired than reflexes, the instincts are less strictly organised and more variable or plastic; and it is on this ground that C. S. Myers[1] (as I understand him) regards instinct as expressing a rudimentary intelligence. Of course, I agree with him in supposing all animal activities to have some degree of consciousness. On the whole, if the division between compound reflexes and instincts and intelligence is still indefinite, it is such as we should expect in Biology. Their differences are enough to make the terms intelligible: remembering that in Biology and Psychology we may distinguish but cannot separate, since no function or faculty has independent existence. As there are real resemblances between the meanings of "tropism," "reflex," "instinct," "intelligence," an ingenious man can stretch any one of these words until he seems to make it cover the whole field of animal activity; but in doing so he must efface all differences. The reasonable course is to distribute these, or other suitable words, over the field so as to divide it amongst them in the most convenient way on the whole; that is, according to the most important differences that can be discovered.

§ 3. The prominence or importance of instinct in an animal's life varies greatly from one order or division to another. It seems to reach its greatest development in certain insects and in spiders. Its utility consists in preparing for unforeseen and often remote events: when a spider first spins its web or constructs a trap, it cannot foresee that flies or other insects will fall into it; when a sand-wasp digs its nest it cannot foresee the processes which will ensure the existence of another wasp next summer. In fishes and birds the most remarkable instincts are concerned with the perpetuation of the species by means whose operation the animals cannot foresee; and the same thing is true of mammalia. In some cases provision is instinctively made for unfore-

[1] I have to thank Dr C. S. Myers for some valuable suggestions in the course of this paper.

seen wants of the individual, as in the storing of food for winter by squirrels and some rats and by the curious bird *Colaptes mexicanus* (allied to the woodpecker), which lives on insects when he can get them, but stores acorns against the time when insects will be scarce. But the life of fishes, reptiles, birds and mammals, for most of the year, seems to depend upon getting food and shelter by a sort of haphazard intelligence. It is only when intelligence has been enriched by the growth of effective memory and accumulated experience that it can make any approach to the superseding of instinct by preparing for remote ends. Even then it may be said to be an extension of instinct; for, first, its effectiveness for conduct (apart from which it could never develop at all) depends upon the excitability of instinct by ideas (which intelligence presents) instead of by objects as in the lower animals; and, secondly, the wide adaptability of intelligence by acquired knowledge of the properties and relations of things depends upon the development of a special instinct of curiosity. Instinct is an organisation for the attainment of ends before there is a long enough chain-memory and knowledge of conditions to adjust means to ends according to experience.

The growth of intelligence in the higher mammalia, and especially in monkeys and man, depends, as W. McDougall observes (*B. Journ. of Psych.* Vol. III. No. 3), on a prolonged youth and parental care. We have seen that intelligence in one of its functions consists in trying one action or another and remembering the consequences—effectively remembering them, that is, in the sense that behaviour is modified for the future, though there may be no memory in the shape of "images." An animal will try more, the greater its activity and the greater its plasticity. Plasticity implies, first, an incomplete organisation such that, instead of fixed or nearly fixed reactions to an object, some variety of impulses is evoked; and, secondly, a more or less lasting modification of the organism by any impulse and its successful or unsuccessful result, without which there is no memory. The young of the higher mammalia are born in a state of helplessness; they present, as it were, a continuance of foetal conditions after birth, a generalised type of organisation to be completed by experience. Experience is to be gained by trying; but trying is dangerous; so that, in the first place, in order that they may try their powers with the least risk, they are soon ready to play; and their play is an anticipation of adult activities in an ineffective form (except for the sake of play); an anticipation which seems to resemble the shortening of phylogenetic processes that sometimes takes place in embryonic development. The ineffectiveness of play activities (except for play itself) depends partly on the immaturity of the organism, as in the courtship-play of some young animals; or, in other sports, on the feebleness of teeth, claws, etc., but chiefly on limitations involved in the utility of the activity, which would fail if play turned to fighting. It is needless to suppose with W. McDougall that this limitation depends on the impulse of rivalry (*Social Psych.* Chap. IV.). In the second place, young animals are protected from danger at play by parental care actuated by parental instincts. Thus in their plastic youth they learn by experience; and the fact of being born undeveloped makes the growth of definitely adapted chain-instincts impossible: the neural connections do not at first exist in the brain, and experience intervenes before they can be formed.

Amongst gregarious animals, parental care may be supported by the protection of the herd; and this seems to me to throw light upon the nature of the intelligence attributed to ants, sociable bees and termites. Taken singly, such animals do not display much intelligence; whilst the co-operative work of the hive or nest is amongst the greatest wonders of nature. This perhaps may be best explained by the incessant trying of all the operative ants, or bees, or termites at their several tasks, in which individuals often fail, but have their work made good by the trying of others. An instructive paper by Turner on "The Homing of Ants," in the *Journ. of Comp. Neurology and Psychology* (Sept. 1907), concluded that the instincts of ants are not definitely adjusted to special tasks, but "generalised": to go out foraging; to carry out of the nest dead or useless things; to bring home pupae, lost ants, food; and to do such things as best they can, not in uniform ways; and that the appearance of *concerted* division of labour amongst them is probably deceptive, and rather due to accidental coincidences; as when some ants hide pupae under a stone, knowing no better place to put them, and then others, who know the road home, carry them there. The success of such societies, then, is due to incessant free trying under conditions in which not much harm can be done by individuals, because their mistakes will be rectified by others so far as to maintain the life of the nest as a whole; each experiments under the protection of the rest, just as the young of the higher vertebrates experiment under the "protection" of the limitations of play and (more literally) of their parents. Against the great danger of dispersal in the course of independent trying, ants, bees, and termites are protected by the gregarious and homing instincts, chiefly controlled by the odours of the nest and of one another; and one may observe that their communities are rather families than societies. The most difficult thing to understand in these creatures is the plan of the dwellings of South African termites—if they are really always constructed in the elaborate way of those that I have seen described; for I cannot attribute the plan to a termitic "over-soul."

§ 4. As to the physiological correlative of instincts, considered as complex reactions of the whole organism to external conditions, if we consider only vertebrates, it seems to lie in the highest regions of the nervous system prior to the growth of the cerebral cortex. For Edinger has pointed out that the principal animal instincts, fear, anger, gregariousness, etc., are shown by fishes (*Journ. of Comp. Neur. and Psych.*, Vol. XVIII.); McDougall has shown that the emotions are phases of the manifestation of instincts; and F. W. Mott and Pagano have given reasons for thinking that the emotions and instincts are organised in the optic thalami and corpora striata. Goltz's dog, which had lost a large part of the basal ganglia, still showed anger, but no sign of pleasure, fear or affection. The cry of distress, which must be considered an instinct (being related to possible aid from others), though a very simple one, has been located in the posterior corpora quadrigemina. Lower still, in the medulla oblongata, come the centres for the compound reflexes of sneezing, coughing and sucking; whilst various reflexes more or less simple are co-ordinated through single segments or certain lengths of the cord.

All reflexes and instincts, so far as the striated muscles and the joints are concerned in their expression, are reflected in the kinaesthetic area of the cortex. Hence the outward expression of emotion is under the control of will; and this explains how an actor (according to Diderot, the highest kind of actor) can play a pathetic rôle without feeling it. The basal ganglia are not involved: nay, their action must be inhibited; else they would become active through their associations with the skeletal movements that normally accompany their action. The inhibition may be supposed to result from concentration of attention upon the details of minute expression. But whether the mimicry can be complete in tone, gesture and facies, without the primitive instinctive fervour, is questionable. However, the cortex, on the one hand, gives an immense extension to instinctive activities by awakening them in relation to remote objects[1], and later, through ideas, and by enabling

[1] Sherrington has shown the importance of the distance senses in developing the cortex, *The Integrative Action of the Nervous System*, Lect. IX.

a representation of the vast array of means that may be necessary to their satisfaction—as may be seen in the human development of acquisitiveness, constructiveness, curiosity. Indeed, it seems possible that in the cortex a higher order of quasi-instinctive motives may be organised, such as the passions of romantic love, of liberty, of justice. On the other hand, the cortex checks and qualifies the instincts by spreading the effect of whatever excites them through ideas of circumstances and consequences, which tend to excite other instincts and so to establish inhibition and deliberation. Deliberate action through means to ideal ends we call rational. As we watch a wasp eagerly digging a hole it knows not why, catching and burying a spider there with no further purpose, laying an egg upon the spider without any foresight of the consequences, covering up the hole as if that were an action for its own sake, we may call all this instinct, without a knowledge of the end. Making elaborate preparations for our own children whose birth is foreseen, then providing nurture, education, outfit, we have an end in view; but if any sceptic asks us why we pursue this end, we can give no more answer than the wasp. The end in nature, so to speak, is in both cases life, more life; and reason supersedes, or (rather) it partially enlightens instinct, because at the present stage of the world it generally ensures more life. But if here or there in the world the reproductive instinct fails us (as it does), intelligence cannot make good the deficiency.

II. The Nesting of Solitary Wasps.

§ 5. To illustrate the nature of instinct, I will take Solitary Wasps which have been so fully and delightfully described by Dr and Mrs Peckham[1]. Beginning with *Ammophila* as one of the best types, we find that she leaves her cocoon in the early summer, and spends two or three weeks flying about, feeding on flowers and mating, sleeping under the shelter of a leaf or in long grass. Then comes the time for laying her eggs. She first makes her nest in the ground slantwise, about an inch deep with a pocket at the end, and covers it up. Next she goes to find a caterpillar, seizes it, stings it in six or seven places, pinches the neck, and drags it off to her nest, which may be more than 100 feet distant. Arrived in the neighbourhood, she drops the caterpillar and opens her nest; then fetches the caterpillar, drags it into the

[1] *Wasps, Social and Solitary.* All page references with no mention of a book will be to this book. I have used it extensively, as the most trustworthy collection of observations known to me, and as the best proof of gratitude to the authors.

nest, lays her egg upon it about the middle of the side, and covers up the hole. She then makes another nest.

This is a typical case; but the nesting instinct is so far from being invariable that, though we may suppose all these solitary wasps to have begun in much the same way, each species now exhibits some modification of it. Thus some catch their prey before making a nest: others make a nest, and then hunt for prey. Some species build nests of mud; some use crannies in a wall to lay their eggs in, or holes in a tree, or the stalks of plants (boring through the pith), or the open ends of straws in a stack; some resort to human dwellings, chimneys, eaves, door posts. Some do not even make nests at all, but lay their eggs upon living caterpillars (*Microgaster nemorum*), or on chrysalises, the larvae pupating in the shell (*Ichneumon pisorius*). The gall-wasps are an allied family.

Different species attack different kinds of prey: flies (*Bembex*), beetles (*Cerceris*), bees (*Philanthus*), spiders (*Pompilus*); and, as a rule, each species confines itself to one kind of prey or to nearly allied kinds. Some species are content with one victim for their larva; others take two, and so on up to thirty or more. This depends partly upon the size of the prey—five large spiders may serve as well as ten small ones—partly upon the size the larva is to attain, or perhaps upon the nutrition obtainable from various kinds of prey. Whilst most solitary wasps stock their nest with food and then close it, *Lyroda subita* and *Bembex* feed their young from hour to hour until they reach the pupa stage.

Of species that burrow, some make a straight incline (*Ammophila*), some an incline leading to a level gallery (*Philanthus punctatus*); others a tortuous burrow (*Cerceris*). And some leave their burrows open until they are completely stocked, or even until the cocoon is spun (*Bembex*); others cover them up more or less every time they leave them.

Species also show various degrees of care in studying the position of their nests, flying about them more or fewer times before going hunting; and they have various degree of success in finding again their nest, or their prey, after leaving it.

Not only species but individuals of the same species show remarkable variations. In making their nests they bore to different depths, or in different materials; in covering up their prey, they display more or less care; in finding their way home, they are more or less skilful; in stinging their prey, they deliver more or fewer stabs, in different places, and sometimes kill, sometimes paralyse; in the number of victims the same wasp has been found to take for one cell of its nest fourteen, for another ten, for a third eight (*Trypoxylon rubrocinctum*) (p. 186).

No step of the nesting-instinct is infallible. Wasps of different species have been seen to begin to make several nests before succeeding. *Sphex ichneumonica* began, on stony ground, one nest, and gave it up, then tried another, finally completing a third (p. 56). *Aporus fasciatus* began and abandoned five nests, finishing the sixth. *Pompilus quinquenotatus* began eight nests before succeeding. Probably the reason for such failures is that the wasp comes upon a stone; for *Sphex ichneumonica* is said to have been working on stony ground; and in another case *Pompilus quinquenotatus* is said to have paused in the midst of digging, and then pulled out a pebble. Had the pebble been too large to pull out, she must have given up that spot. Wasps, then, have no infallible instinct where to dig.

Having prepared a nest and caught prey, they cannot always bring it home. Peckham saw *Amm. gracilis* carry a caterpillar for two hours a distance of 261 feet through all kinds of difficult ground, and then give up (p. 45). *Amm. vulgaris* was also seen to fail. Peckham says, "The affairs of Ammophila must often go wrong." *Pomp. scelestus* also failed to get home. Having brought their prey close to the nest, wasps cannot always find the hole. *Trypoxylon rubrocinctum,* building in a straw-stack, usually has to hunt about before recovering its nest (p. 186). *Pomp. fuscipennis* rarely circles about when leaving, and on returning hangs her spider in a crotch before opening her nest: she nearly always loses track of either the nest, or the spider or both (p. 221).

Having stored prey in the nest, a wasp sometimes forgets to seal it up. Of seventy-six nests examined, seven were prepared and sealed up empty (*Tryp. rubrocinctum*). The blue mud-dauber not infrequently makes the same mistake (p. 189). At every stage, therefore, the instinct is fallible.

§ 6. Intelligence assists this instinct in various ways. We do not see how else to explain the fact that burrows are shaped differently according to the nature of the soil; or that the same species will use a hole in a wall or post, or a straw, or a shell, as may be most convenient. The most undeniable proof of intelligence is given by their knowledge of locality, which is very variable, but nearly always wonderful. They gain this knowledge, no doubt, first of all, whilst flying about in the neighbourhood of their birthplace, before the time of nesting comes. But having made their nest, they fly about it in circles, making what seems to us a deliberate study of the locality, before going hunting; and if, having caught prey, they leave it for an interval to revisit their nest or what not, they sometimes study the locality before going away. Dr Peckham has given several diagrams of these studies.

This knowledge of locality used to be put down to a "sense of direction," or to a memory of all the turnings of a journey. It is also found in bees and ants and limpets and snails, in fish, birds and mammals, and probably in far more than have been observed. Some of the stories told about this faculty are certainly inexplicable, or perhaps incredible. But experimental evidence indicates that it is based on genuine memory and often, apparently, of definite objects. Avebury's experiments on ants, Fabre's and Romanes' on bees, and the Peckhams' on wasps, all go to prove this. The same explanation is given of the behaviour of carrier-pigeons. The Peckhams on three occasions caught some social wasps, the first that left the nests in the morning, and then stopped up the nest, and liberated the wasps a good way from home; the first lot from two positions a furlong out on a lake; the second within a barn, having windows at each end, one toward the nest, the other away from it; the third 300 yards away in the country. Of the whole, fifty to seventy per cent. returned to the nest. Of those liberated in the barn twenty-two, showing no sense of direction, flew toward the distal window, which was best illuminated; sixteen to the window in the direction of the hive. Peckham concludes that to find their way home they rose higher and higher in the air, flying in circles, until they saw some object they knew, and then made for it (p. 278). This is what carrier-pigeons do; but it implies a surprising keenness of vision in wasps.

At any rate, other experiments confirm the view that they identify a position by its relation to known objects. Bouvier cut away the plants around the nest of *B. labiatus*, and the wasp was confused, and spent a long time in finding the hole; he left a stone close to her nest for two days, then moved it 8 inches, and the wasp tried to find her nest under it as before (p. 124). Marchand observed *Bembex rostrata* leaving her nest on a stony hillside; he moved a swallow-wort that grew about 20 inches from the nest, and placed it 2 feet further off. *Bembex* returning flew to a spot in the same relation of distance and direction to the plant as her nest had formerly been, and could not find her nest. He then frightened her away and replaced the plant, and *Bembex* returning easily found her nest (p. 125). *Ammophila* deserted a nest in front of which some lines had been drawn in the dust.

But some indications of much higher intelligence than a knowledge of locality implies, are given by wasps in what Peckham calls their "use of the comparative faculty." He had several times seen wasps enlarge their holes when "a trial had demonstrated that the spider would not go in": and reports a case of this at page 303. Wasps have also been seen to bite off the legs or wings of victims too large for the nest. But once *P. scelestus* was seen to bring home a large spider, and on looking at her nest she "seemed to be struck with the thought that it was decidedly too small to hold the spider. Back she went for another survey of the 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" (p. 238). Such phrases as "measured with her eye," "drew her conclusions," may add too much to the observation; but reducing the observation to its lowest terms, it still seems to describe an act of comparison. Huber has recorded an observation on bees that has a similar implication (quoted by Houssaye: *Industries of Animals*, p. 241). One summer when the hives were much worried by unusual abundance of the death's-head moth (which will penetrate a hive to feast upon the honey), some bees blocked up the doorway of their hive, so that it was too small for the moths; but others built parallel walls of wax in front of the door-way, leaving between them a zig-zag passage too narrow for the moth to turn in; so that if he entered at one end of the parallels he had to go out at the other. Some bees, then, viewed the moth in breadth, others took his measure lengthwise. Less striking signs of a wasp's having some sense of quantity are shown by their supplying their grubs with approximately the same numbers of caterpillars, flies, etc.; whilst (as mentioned above) if the usual number is much departed from, it is when the victims in one set are decidedly larger than in the other, so that the amount of nutriment provided is about the same.

Something like intelligence appears also in the occasional abbreviation of a chain-instinct; as when *Sphex Ichneumonea*, which on bringing home a grasshopper, habitually leaves it a little distance from the nest, runs into the nest, returns for the prey and carries it to the edge of the nest, then goes in again and once more returning drags it in after her; yet when Peckham, whilst *Sphex* was in the nest the second time, removed the prey again and again, the wasp after the process had been repeated five or six times, at last dragged it straight into the nest (p. 304). Perhaps this is to be explained by fatigue; which often makes us when writing or talking drop syllables or words. An allied species observed by Fabre persevered, however, in following all the links of custom forty times.

The limitations of such intelligence as these wasps have are seen at every step. Although they remember the place of their nest, they may be unable to find the opening under the slightest concealment. *Aporus fasciatus* lost her nest when a leaf that covered it was broken off, but at once found it again when the leaf was replaced (p. 286). Loeb relates (*Comparative Physiology of the Brain*, p. 226), that *Ammophila* could find her nest in his garden, when unable to climb a wall that stood in the direct path, by going around through a neighbour's garden and through the fence, yet could not again find the opening of her nest when, in her absence he covered the hole with a clover blossom, though she found it as soon as the blossom was removed. Little intelligence seems to us to have been needed. Had she no experience of falling leaves and blossoms and of the changes thus made in the appearance of the ground? As to their powers of observation, why, on bringing prey to the nest do they so often enter themselves before interring the prey? We are apt to suppose that they go in to see that all is safe. But the most dangerous enemies may be there and yet pass unnoticed. In the nests of *Bembex* certain flies (*Miltogramma*) lay their eggs on the food provided by the wasp for her own young; yet when the parasitic grubs appear, she continues to feed them though her wasplings starve. Fabre, having seen a Sphex carry her prey into the nest, return and prepare to close it up, drove her away and took the prey, which had an egg attached to it; he then allowed her to return; when she went down into the empty cell, came up again, and stopped up the opening as if it had been "all safe" (quoted by Avebury: *Senses of Animals*, p. 253). Wasps that make their own nests rarely attack parasites, though they sometimes attack ants and other wasps that attempt robbery. If in some cases they drive away parasites, the rest of their conduct shows that this is not intelligent action: they do not know what the danger is. The whole family of *Chrysididae* seems to be parasitic upon burrowing wasps; yet no warfare is made upon them: the danger of their presence is not understood, and no effective defensive instinct has yet developed. The behaviour of *T. rubrocinctum* (p. 181) may be the beginning of such an instinct. Similar failures of intelligence were found by Fabre in mason-bees.

§ 7. In attempting to explain the origin and development of these nesting instincts in solitary wasps, I shall assume the general principles of Natural Selection; namely, that variations of behaviour that are advantageous to the species may be inherited, and accumulated by inheritance, and fixed: so that in course of time very complex activities may, through the survival of those individuals that inherit them, and the failure in competition of individuals less well-endowed, become characteristic of the species as a whole. How useful these nesting habits are is shown by an interesting fact. The species of solitary wasp keep up their numbers century after century, age after age, although each female wasp has (compared with most insects, most fish and even with many mammals) very few offspring—lays very few eggs—less than a score (as well as I can judge); and this suffices in spite of many parasites and other enemies. The fact illustrates the general rule of animated nature, that the greater the care taken of offspring, the fewer they are. It is an economy of physiological energy; and the rule is correlated with another rule, that the fewer the offspring, the higher is individual development; and every observer attests that the activity, adroitness and distinctness of character to be found among these wasps are astonishing. The rule is further illustrated in the case of *Bembex spinolae*, who, instead of storing her nest once for all with flies, laying her egg, closing the nest, and leaving it, attends to her offspring after it is hatched, and feeds it day by day until it reaches the pupa stage. She does this for one larva at a time, and each takes a fortnight, so that she cannot have more than five or six offspring in a season. Her method has a certain disadvantage; it gives more opportunities to *Miltogramma* to invade the nest, in which she is very successful. Peckham opened ten or twelve nests; only one was free from parasites: the others contained from two to five maggots of *Miltogramma*; yet *Bembex* flourishes. *Lyroda subita* likewise feeds her young from hour to hour, but I do not know that the number of her offspring has been observed or calculated.

But this nesting instinct is a chain-instinct, a series of totally different actions, and to explain it we must consider each step, and also the order in which the steps occur. In the first place, then, we observe that (1) to hide an egg in a hole or other shelter is plainly useful; (2) so it is to hide or cover up the opening of the hole; (3) to lay food by the side of the egg, or the egg by the side of the food, is useful even if there is no nest; (4) to bring more food is useful, if it enables the larva to attain a better growth, or development or potentiality, before the pupa stage; (5) to kill living food, or paralyse it, is useful that it may not injure the egg or young larva; (6) to inspect the nest from time to time is useful, in spite of actual shortcomings;

(7) to explore the neighbourhood and to identify the spot are useful actions, if the egg and larva are afterwards to be provided for; (8) to return (or "home") is useful.

Such actions, being useful and immediately useful, will, if they occur in any individual, be perpetuated and tend to become specific: but, in the second place, how do they occur? Let us begin with the making of the nest. To dig a hole in the ground, or in the stump of a tree, or in the stalk of a shrub, or to build a mud cell, as some wasps and some bees do, without knowing what is to be done with it (for plainly they cannot know)—is not this an extraordinary operation? To understand it we must show how it may arise from simpler actions more commonly performed by animals, especially by insects, if possible by hymenoptera, and particularly wasps. We may assume that the peculiar actions characteristic of species of wasps, have been differentiated from a common ground. To find a shelter of some kind for itself or its progeny is an action common to most kinds of the higher animals and to very many insects. Amongst wasps it sometimes takes the form of creeping into a crack in a cliff, or wall (since walls have come into existence), or into a hole in a tree, whether the crack or hole has been made by the ordinary wear and tear of nature, or by some other animal. Whereas Peckham observed that the *Pompilidae* near Milwaukee dig their own nests, Fabre reports that in France the *Pompilidae* do not make their own nests, but lay their eggs in crevices, selecting a suitable crevice before catching their prey (p. 197). Near Milwaukee *Odynerus capra* has this habit (p. 94). The utilisation of a crevice in a wall, or a hole in a tree, already existing, I take to be the beginning of both masonry and pit-digging. *Trypoxylon rubrocinctum* (which preys on small spiders) was found by Peckham to be nesting in the cracks of a brick wall; but as the cracks were too deep for their purpose, the wasps "built a mud partition across the opening about an inch from the outside of the wall" (p. 178). This must be a useful protection against possible enemies on the other side. Wasps that nest in the hollow stems of plants act similarly, making partitions at such depths as suit their purpose. Slight variations of this practice in a wider crack would result in building a mud-partition all round; and that is to make a cell. Having made a complete cell in a crevice, it might be made in the hang of a cliff or on the bough of a tree, if there was anything to gain by it. On the other hand, suppose the natural crevice to be too small, then to kick out loose grains of earth or rubbish—or at a further stage, to bite away some of the wall, or the wood of a tree—would be a useful practice, and if inherited would be an occasion of selection. But this is nothing else than to dig. Another slight variation would transfer the work to the ground: first using holes already existing, then enlarging them, then digging fresh ones. *Cerceris deserta* was traced by Peckham to its nest in a crevice amongst some lumps of earth (p. 152); *Pomp. marginatus* was traced to a crevice amongst some lumps of earth where she was making further excavations (p. 229). Nests vary in depth from 1 inch (*Trypoxylon*) to 22 inches (*Phil. punctatus*). Few wasps can have such an opportunity of economising labour as that which is taken advantage of by one described by Hudson (*Naturalist in La Plata*, p. 181). This wasp preys on a spider, whose habit is to lurk in ambush in a hole whence it rushes out to seize any passing practicable insect. The wasp tempts this spider out, kills it, lays an egg upon it, and buries it in its own den.

In this instinct then we find every stage of development still represented by the habits of extant species, from the use of a crack to the making of a burrow 22 inches long. Let us next consider a certain variation of this instinct in relation to the taking of prey. Whilst *Ammophila*, *Cerceris*, *Sphex*, *Ichneumonea* and most solitary wasps make their nests first before taking prey, there are some—five species of *Pompilidae* observed by Peckham, *Aporus fasciatus* and others—that first catch their prey and then construct their nest. The latter course has certain disadvantages; for whilst the nest is being dug, the prey is liable to be carried off by ants, or by robber wasps, or to be attacked by parasites; and to guard against this the wasp frequently leaves off digging, to see that her prey is safe. Fabre thought that one Sphex that dug her nest in the neighbourhood of prey already captured, did so because the prey was too large or heavy to be carried far. But the practice seems not to be confined to wasps that take heavy prey; it does not hold with many that do take heavy prey (*Ammophila*); and Peckham saw *Aporus fasciatus* drag for some distance a spider much larger than herself, and deposit it on a melon leaf whilst she dug a nest (p. 81). The danger of leaving the prey whilst making a nest is sometimes partly avoided by *Pomp. quinquenostatus* in a remarkable way. Instead of leaving it on the ground, she hangs it in the crotch of some plant-stem; but even then it is not safe from robbers. This device, however, is not constantly followed: the wasp sometimes leaves her prey on the bare ground.

It has often been objected to the theory of evolution that we never see any species in process of changing form or colour; and the same objection might be urged against the evolution of instincts. It depends on an illusion similar to that implied in the term "fixed-stars": we cannot see the stars move, but we can calculate the direction and velocity of the movement of very many of them from facts that can be seen. So no doubt species and their instincts are always changing, but much too slowly for us to notice it within the limits of our short lives. We can, however, sometimes find in natural history, without appealing to embryology, evidence of the changes that have probably been undergone, and may sometimes find a condition of things that seems to imply that a change is now in progress. In such a condition perhaps are these instincts I have just described. When a wasp catches its prey before digging a nest, the simplest supposition is that, at first, the prey was left meanwhile upon the ground. It was an improvement upon this when prey was first hung upon a plant until its grave was ready, but not so great an improvement as quickly to exterminate the other practice; and so we still see them existing side by side in the life of the same species. Naturalists who live 50,000 years hence may find that the more careless practice has been entirely lost, or only occurs by atavism in idiot-wasps. But, further, the whole double process of either capture and digging, or digging and capture, may be in a state of change: the latter seems to be the commoner; and since it gives greater safety and economises time and energy, it may be gradually exterminating the former course.

To understand the matter we must try to find how both processes arose. To begin with the case in which capture precedes digging. The capture and killing, or paralysing, of prey in order to lay an egg upon it, is itself a complex process, which must have had a history. We know two simple cases: first, the depositing of eggs upon animals already dead, as by the Blow-fly (*Calliphora erythrocephala*), and by carrion-beetles (*Necrophorus*), and by parasitic wasps (*Cerophales* and *Pomp. subviolaceus*); and, secondly, the depositing of eggs upon living prey. The latter course is adopted by many flies; and amongst wasps by various species of *Ichneumonidae*, which lay their eggs upon living caterpillars on whose juices the larvae feed, and by some *Braconidae*, a closely allied family; and *Pompilus trivialis* oviposits on living spiders. Since a living caterpillar upon which an egg has been laid is still exposed to the attacks of other parasites and enemies, it may give greater safety to kill it; and this is shown to be probable by the

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comparatively great number of eggs deposited by *Ichneumonidae* and *Microgaster*. But much greater safety is obtained if the prey is not only killed but also hidden in a hole or cell. *Necrophorus* buries the carrion on which its eggs are laid; and parasitic wasps lay eggs on victims already hidden, or about to be hidden, by other wasps. With one utility depending on another, the combination of killing or paralysing the prey with the hiding of it is not more improbable than the combination (say) of imitative colouration with imitative flight in some butterflies.

If prey, killed or paralysed, is to be hidden, there are two ways of doing it: (1) by finding then, for the first time, a suitable hole, or by enlarging one, or by digging one; (2) by carrying it to a hole or cell already known or prepared. The former course may seem the simpler, involving the less imitation of foresight: in a wasp (we may say) flying about in summer weather, the need to oviposit matures; this excites the impulse to catch and kill a certain bee, and then comes the impulse to hide it and, therefore, to search for or dig a nest. But this only seems the simpler course if we suppose the wasp not yet acquainted with any suitable hiding-place. In many cases, however, they may have, or may anciently have had, a place ready, namely, their sleeping place; or the cell from which they themselves emerged, which may also have been their sleeping place. At present some wasps (including males) dig holes to sleep in; some "congregate at night in convenient crevices"; some, after the nest for their eggs has been made, sleep in it themselves (p. 117). Others sleep under leaves, or in long grass. Perhaps these last show the greatest deviation from original habit.

Peckham gives this very curious account of *Philanthus punctatus*. Her nest is a long gallery with pockets, in each of which an egg is laid with food for the larva. When the wasps emerge from their cocoons they live together for a time in the parent nest, flying abroad by day and returning to it at night. Such a family was found to consist of four females and three males. One of the females, the first maturing, enlarged the old nest for her own eggs; the second, a day later, left the nest, and made a new one close by; five days later, a third female, having already left the first nest and lived for two days in the second, made another for herself. The three males still lived on in the old nest with two females, one of which (as far as observation went) remained barren. One at least of the females that matured later than the first was seen to work at the old nest along with her sister, the new owner; and all, including the males, seemed to keep guard over it. Here, then, in the case of the first maturing female, there is a plain example of a wasp that before killing its prey (*Hallictus*), has a nest ready; and to bring the bee to it is merely the homing instinct that is found in animals of all orders. If few wasps are known to use their old nests, it may be that the practice is insanitary, and so has been generally eliminated. And for an earlier breaking up of the family (which is usual) than occurs in this case of *Phil. punctatus* there is a good cause, namely, the advantage of cross fertilisation.

The second female to mature made a new nest; and, at first glance, this seems to be an original action; but it is not. For the first one enlarged the old nest; and to make a new nest from some neighbouring crack or hole in the ground, is only to do the same thing a little more thoroughly. We can, however, see the opportunity for a different order of actions to arise at this point. Suppose the second female to begin, like the first one, by catching a bee, and to return with it to the old nest, and to be driven away. She must then make a new nest after catching the bee, or else her offspring perishes. If, however, she does make a new nest, and her offspring survives, and inherits this variation of first taking prey and then digging its grave, the species in each generation will consist more and more of wasps that follow this practice; since the offspring of the first maturing female will be fewer than those of the more numerous later maturing ones; and in some species this practice has become the rule.

Before leaving this case of *Philanthus punctatus*, I will venture to suggest that it points to a possible origin of sociality in wasps, bees, ants. Several species of wasps are semi-social in the sense of making their nests near together: *Bembex*, for example; but these are apt to quarrel and rob one another; there is no co-operation. Wherever we find true co-operation amongst these Orders there is "caste" or dimorphism. Now if the fourth female of *Phil. punctatus* that was not seen to leave the nest, was actually barren, and if she assisted at clearing out the old nest, as one of the sisters of the owner was seen to do, we have here the beginning of a composite nest. It was already strong in drones. That a barren female may become a worker, it is a necessary variation that the impulse to work at the nest should be stimulated by something else than the need to oviposit—say by the sight or smell of the nest, or of another at work. The only other approach to co-operation observed by Peckham was made by *T. rubrocinctum*: the male sat in the opening of the nest and drove away intruders during the absence of the female; and, on her return, made way for her, and sometimes carried in and stored the prey, whilst she flew away for more (pp. 181–2).

§ 8. To return to the nesting instinct: why do wasps seek any shelter for their eggs; why construct cells, or creep into holes or crannies? Do they foresee that their progeny have enemies; do they understand danger and safety? We cannot suppose so much. Probably to explain this matter we must fall back upon primitive tropisms—phototropism, or thermotropism or stereotropism. These impulses in such highly evolved creatures as the wasps, may date from remote ancestors in an age before our wasps had become wasps, and may remain active in existing species under conditions in which they are still useful and so far as they are useful. Wasps love sunshine and warmth; shun cold and wet; as the shadows of the afternoon lengthen, nearly all of them seek some sort of shelter, some being content with leaves or grass, others requiring more substantial protection. Peckham indeed mentions (p. 108) one species, *Crabro stirpicola*, that worked through the night at excavating its nest in the stem of a plant; but as it was under the shelter of a glass bottle provided by the naturalist, the bearing of the observation is a little doubtful. How little the seeking of shelter upon the impulse of a tropism implies any idea of danger, is shown by Loeb's amusing observation upon a butterfly, *Amphipyra*. Placed in a box, half of which was darkened and afforded concealment, whilst the bottom of it elsewhere was strewn with small glass plates, raised upon blocks just enough to allow the insects to creep under them, some specimens of this butterfly "collected under the little glass plates, where their bodies were in contact with solid bodies on every side, not in the dark corner where they would have been concealed from their enemies" (*Comparative Physiology, etc.*, p. 184). They did this both in direct sunlight and when the whole box was darkened. Hence stereotropism which normally gives concealment, remains compulsive when it gives no concealment. If, then, the nesting instinct of wasps be traceable to a tropism, we need not suppose that it implies any idea of the safety of the egg or larva.

But granting an original tropism as the basis of nest-making, we are still required to explain why at a certain time the behaviour originally determined by this tropism becomes effective, contrary to the animal's usual habits, in the morning or middle of the day, so that it suddenly begins to burrow in the ground or to wall up a cranny.

This is an example of a great class of problems presented by the existence of critical points in the life-history of animals. Why do birds in autumn feel the impulse to migrate, in spring to build nests; why does a caterpillar at a certain time begin to spin its cocoon, knowing nothing of the pupa stage upon which it is about to enter? And so on. Such changes seem to depend (*a*) on external conditions of temperature, food-supply, etc.; (*b*) on internal conditions, a certain maturing or modification of the organism, producing perhaps an uneasiness that is relieved by a certain action. In wasps the approach of the time for laying an egg brings on a complete change of behaviour, so that instead of sporting about amongst the flowers, paying no attention to insects or animals of any other species, she begins to burrow, or to catch bees or spiders. By merely natural-history methods we cannot explain this: it is intrinsically a physiological question. But perhaps Psychology will help us to something better than mere blank astonishment.

We observe, then, in the first place, that, when the impulse to make a nest is felt, there seems to be a sudden narrowing of consciousness, such as occurs in ourselves in the attitude of close attention; so that the wasp becomes interested only in a certain feature of the ground, or of a tree stump—if preparation of the nest be the first link in its chain-instinct. Conceivably, such a restriction might begin in the receptor-organs, their range being limited to the important object, so that the wasp can see nothing but that feature of the ground or tree. But more probably, it is due to the opening of an internal door that gives a certain perception access to the sources of certain motor activities. The sense of hearing seems to be strangely specialised in some animals: Edinger says (*op. cit.*) that a lizard may give no reaction to the most violent noises, such as loud singing, or banging a stone, and yet be at once on the alert at a slight rustling in the grass; there is mutual recognition of calls between lamb and dam; bees seem deaf to most sounds, but are said to be immediately affected by a peculiar cry of their queen; Peckham reports that social wasps took no notice of various noises he made, but seem to be affected by their own buzzing (p. 9); and elsewhere he speaks of *Clorion* as apparently listening to a cricket and being guided to its capture by the noise (p. 256). It is difficult to understand how in any of these cases the range of a sense-organ can be restricted to one sort of object. A more reasonable supposition is that the conditions of reaction are central; that in the case of wasps that begin to burrow, the internal maturing of the organism accompanying the development of eggs, releases an impulse when the ground, tree-stump, or whatnot is seen; that, in fact, such an object then for the first time becomes interesting in a peculiar way. This is analogous to the interests that from time to time possess ourselves, especially during childhood, and often predominate for a time almost to the neglect of everything else: such as babbling, running, climbing, stone-throwing, collecting. The wasp also seems suddenly to have no regard for anything but digging or plastering. And in her unconsciousness of purpose there is a further analogy to the play of children; for this we know has, *in ordine ad universum*, the great utility of developing their powers of perception, activity, imagination; but they think no more of that than the wasp does of the egg she is about to lay or of the imago that will sport in the sunshine next summer. Indeed the absolute detachment of play-interest, absorbed in itself, seems to be a survival of the original instinctive form of all activity, undisturbed by intelligent appreciation of further ends.

This interest lasts until a certain result is attained, and the wasp is then diverted by another critical change to another activity. The attainment of the result of digging or plastering cannot be measured by the time taken or the energy expended; for observations show that these vary greatly; and, yet, can it be the *form* of the work, that satisfies her, as it does an artist? The nest completed, the wasp hides it, or not; flies about to study the locality, or not; then whirls away, hunting it knows not what; and presently the sight of a bee or spider, its special prey, excites a new interest and a new impulse. Again it attends to that only. The victim is seized and stung; and then the homing impulse awakens; and so on until the whole task is finished.

§ 9. The remaining links of the chain-instinct are much easier to understand. Take first the amount of food supplied to the larva. Whilst *Ammophila*, capturing good-sized caterpillars, can sufficiently provision her nest with one of them, other wasps, capturing smaller prey, bees or flies, provision their nest with many victims, half a dozen, a dozen or more. Such a number is necessary for the development of the normal imago. For every species of animal there is, in a given environment, a certain normal stature, which few fail to approach and few exceed. In insects that undergo metamorphosis, the size of the imago depends upon the nutrition of the larva. If, then, we suppose a certain species of wasp to vary in such a way that, instead of taking large prey, one of which would suffice, it turns to smaller prey, it must also vary in the direction of supplying its larva with more victims, or else the species must dwindle proportionally in magnitude, or even perish. And a change of size must often lead to extinction, unless accompanied by further changes of habit.

We are thus led to a very interesting question—why does each genus or species of wasp confine itself (with few exceptions) to one kind of prey, or to closely allied kinds? Addiction to one kind of food is very common in the animal kingdom, and amongst insects: lepidoptera lay their eggs, species by species, each upon one kind of plant; gall-wasps frequent the same trees or shrubs. Some advantage is implied in this, in spite of the disadvantage that the flourishing of each kind of plant varies from year to year, and that therefore the food-supply is sometimes relatively scarce. The same thing is true of animal prey. As to wasps each kind of prey must be hunted in its own habitat, must be seized in the most advantageous way, must be stung in the most advantageous way, must be stored in the most advantageous numbers; and it plainly needs a much simpler adjustment to deal in the most advantageous way with one (or with closely allied) species of prey than with many different ones. It is the utility of all specialisation to do one thing well. There is physiological economy, and it is marked by anatomical adaptation—very apparent, for example, in *Ammophila*.

The killing or paralysing of prey is a very variable action. *Ammophila's* caterpillars are sometimes killed, sometimes paralysed; and it makes no difference to the grub whether its food be dead or alive. Some flies stored by *Crabro* were so slightly injured that they flew away when the nest was opened (p. 101). Energetic movements of wounded prey are dangerous to the egg, as seems to be shown by the practice of some wasps of suspending their egg by a thread from the roof; yet *Aporus fasciatus*, taking spiders, probably "depends upon packing her victims in tightly in order to keep them quiet" (p. 83). *Tachytes* is the most perfect paralyser of all; but so short a time elapses between the laying of the egg and the spinning of the cocoon, that its adroitness is of no use to it (p. 252). How shall we explain all this? By economy. To paralyse or to kill is indifferent; but poison is a physiological expense; and the tendency is to administer no more than will just serve the purpose. What may be the feelings of the wounded victims we can but faintly surmise: in the arrangements of nature they seem not to have been much considered.

The repeated exploring of the nest by some wasps seems a useless action, when we find that, after all, they do not notice the presence of parasitic eggs, nor even the absence of their own eggs. It is clear that they have no idea of the dangers that beset them, nor of the biological purpose of their actions. Perhaps the sole use of exploration is to provide that the nest, as a nest, shall be intact and adequate, that it shall be large enough, and that the roof shall not have fallen in.

The use of covering the nest and concealing it, though comparatively neglected by some wasps (*Bembex*), and only partially performed by others (*A. polita*), seems pretty plainly to consist in the exclusion of parasitic flies and wasps; which, if they get the chance, enter nests and leave their own eggs there. In one species, at least (*Aporus fasciatus*), this practice of covering the nest has become so fixed, that if they begin a nest and find the place unsuitable, they fill in the hole before beginning another (p. 82). It is done, therefore, without conscious purpose, yet sometimes with extraordinary perfection: as by *P. fuscipennis* (p. 218) that was seen to pound the earth over the mouth of her nest with her abdomen, sweep it smooth with her legs, and finally bring small objects to conceal it—"a little stick, the petal of a faded flower, a scrap of dead leaf, and so on until 10 or 12 things had been collected." Surely a work of supererogation, which no other wasp of the species was seen to emulate. *Ammophila* is sometimes careless but usually very careful in closing her nest.

The most unsatisfactory part of the wasps' nesting instincts appears in their behaviour to parasites. They sometimes drive them off in a feeble manner, or try to avoid them, but have never (I believe) been observed to attack and kill them; it is as if they regarded the parasites as annoying but not dangerous. Their behaviour to ants is very different. One wasp (*Pomp. scelestus*) was seen to try to kill an ant by seizing it furiously and throwing it back against its sting (p. 238): others (e.g. *Pomp. fuscipennis*), on the approach of ants, "make off with every sign of terror" (p. 219). Yet *Aphilanthops* preys upon winged queen ants, and *Fortonius* upon workers. Probably the relations of wasps to ants are more ancient than to any sort of parasite, and therefore the adaptation to them is more complete. We may assume that parasitic wasps once had the same habits as the rest, and that their degeneration is comparatively recent. In relation to parasites, then, the instincts of wasps are still in course of development. And plainly, considering the matter psychologically, the narrowness and intensity of any instinctive interest are opposed to variations enabling them to deal effectively with new conditions: it is the opposition of organisation to plasticity.

If it seems difficult to develop such chain-instincts as these wasps display by natural selection of occasional small variations, or even of considerable variations, such as the bringing of a second or third fly or bee, there has been plenty of time to do it in. Hymenoptera are found throughout the Tertiary strata, perhaps even in the middle of the Secondary (Jurassic)—a good many million years ago. Similar species of wasps and ants with similar habits are found in North America and in Europe, and must be supposed to have spread when the Arctic regions were viable; for so many resemblances can hardly be accounted for by such methods of migration as the occasional transportation of colonists by floating timber.

§ 10. The Intelligence shown by solitary wasps may be considered under three heads:

(1) The nature of their Memory, which is conspicuous in relation to locality. Locality-memory is widely distributed throughout the animal kingdom. A horse often travelling the same road comes to know (I am told) every object by the wayside, and is uneasy if anything is added or taken away. Snails, far removed from both horses and wasps, know the way back to their shelters. So do limpets to their old scars. Lloyd Morgan (*Animal Behaviour*, p. 156), experimenting upon limpets, found that of twenty-one moved a distance of 18 inches, eighteen (nearly ninety per cent.) found their way back; of thirty-six moved 24 inches, five (about fourteen per cent.) got back. Amongst Hymenoptera, ants find their way for considerable distances, guided apparently by the direction of light and by odours. Knowledge of locality is very important to animals, especially to all that have homes. Some migrating birds are said to return year by year to their old nesting haunts. The carrier-pigeon is notorious. No animals need it more than these wasps.

Amongst mankind, savages acquire a minute knowledge of locality, sometimes over wide areas. To townsmen exact knowledge of locality is so unimportant that they have difficulty in understanding how it can be acquired. We attend to and remember, at most, conspicuous landmarks and general directions. Usually in going about we think of something else. If we notice particular objects, it is not in their place relations, but as of this or that class, or as presenting some unusual feature; and they start in us trains of thought.

It may help us to understand the memory of wasps and bees if we consider that the context of place is all-important to them; that they live on the perceptual plane, and are not distracted by concepts or trains of thought. Hence it is almost or quite impossible for us to see a group of objects as they see it. Perception is only the starting point of our knowledge; for them it is all in all. To appreciate the difficulties that beset a homing wasp, we must remember, that whilst some species seek their prey on the wing and fly home with it, others that seek their prey on the wing take victims of such a size that they are obliged to drag them home along the ground, perhaps through stubble or brushwood. In the latter case they cannot see their way beyond a few inches, and all their landmarks are hidden. Then Peckham reports several cases in which the wasps leave their prey from time to time and return for it. We may surmise that the intervals are spent in flying up into the air to locate known objects, or in actually revisiting their nests: in either way reascertaining the direction. And whilst dragging their victims along through stubble they may find their way less effectively by the direction of the light. If the journey takes a good while, as it sometimes does, the direction of the light becomes misleading. They do not always get home. In fact in this way those wasps that make their nests first, and then go hunting far afield, are at a disadvantage compared with those that first catch their prey and then make a nest near at hand. The former need greater powers of observation and memory.

But further although several wasps before leaving the nest fly in circles and seem to study the spot, yet these circles of direct study do not seem to be very wide. They then fly to a distance from which the near neighbourhood of the nest may be hidden: in returning they must trust at first to other landmarks. But how difficult it is to recognise an object—say a tree, that has not been specially circled—from a new point of view. It seems to imply the sufficiency of very partial and something like analogical recognition, and without ideas. In ourselves it is plain that recognition does not always involve ideas, may depend upon features that cannot be precisely indicated, and indeed primarily consists (as it seems to me) in exciting a certain mode of reaction. In wasps, it determines them to fly in a certain direction. This may be the essential character of their memory. Further investigation is needed as to the areas within which their recognition of locality is effective, and as to their means of orientation, whether by the direction of the light or what else, when beyond those areas. Guidance by the direction of the light may be connected with their preference for working in clear weather.

(2) Their behaviour in respect of quantity and form. Each

burrowing species (and these remarks apply equally to the masons) has its own customs of always making a nest nearly the same size, of nearly the same depth, sunk nearly in the same direction, with one or more pockets, and of storing it with nearly the same amount of food. Its first nest is made by each individual nearly according to type. What determines such uniformity of behaviour? If it were merely a question of the amount of excavation accomplished, we might suppose that the critical change that releases the burrowing impulse also supplied energy for just so much work. But this is incompatible with the fact that wasps often begin one, two or more nests and abandon them, and yet complete a nest at last. They have therefore more than enough energy for one nest. But at any rate a definite supply of energy would not explain the nearly uniform shape, and pocketing of the nest. Can they have observed the shape of the nest from which they themselves emerged? This seems very unlikely; and the parallel explanation of the specific architecture of birds' nests has been refuted by the observation that birds bred in a felt nest will nevertheless next year build according to the ancestral pattern if they get the materials. Each wasp works alone: we cannot see any explanation analogous to that which Darwin gave of the cells of the hive-bee. And I cannot perceive any correlation between the shape and structure of a wasp and the form and direction of its burrow. We may suppose a certain satisfaction when the work is done, but what is the ground of the satisfaction?

Similarly as to the amount of food stored, in each case it seems to be about enough. And the size of the prey brought home is usually such that it can be carried into the nest: *T. rubrocinctum*, says Peckham, never takes too large a spider for the calibre of the straw in which its nest is made (p. 184). This indeed may be understood as a necessary mutual adaptation between the size of the nest made or selected, and the size of the prey that excites the impulse to attack. But with some species the prey brought home is not infrequently too large for the nest. In one case already mentioned, Peckham thought that this situation incited the wasp to a definite act of comparison. Was it the first time that that wasp had met with the same difficulty? If not, its behaviour may have been due to the effects of former experience. But we had better not theorise on such a unique observation. Generally, when a wasp brings home prey, it first tries to pull or push it into the nest; if it will not go in one way, it tries another; or it bites off legs or wings to reduce the size; or it enlarges the nest.

There are these three courses open to it, and it may try them all; an admirable proof of the plasticity of its organisation, but not requiring ideas or anything else beyond the impulse to get the prey in, which is the instinct itself. Perhaps the wasp does not display greater resource than Darwin reports of earthworms when dragging leaves into their burrows (*Vegetable Mould*, etc. Chap. II.). Upon this he comments as follows: "If worms acted solely through instinct or an unvarying inherited impulse, they would draw all kinds of leaves into their burrows in the same manner. If they have no such definite instinct, we might expect that chance would determine whether the tip, base or middle was seized. If both these alternatives are excluded [as he had already shown], intelligence alone is left; unless the worm in each case first tries many different methods, and follows that alone which proves possible or the most easy; but to act in this manner and to try different methods makes a near approach to intelligence." We may say the same of wasps. Trying is not intelligence, for this implies at least a scintilla of foresight; but it is the necessary preparation for intelligence.

(3) The adaptation of means to ends. Anything in the nesting of wasps that looks like intelligence under this relation must be confined in each case to single links of the chain of activities. The chain is complete from the first; but the first time it is run through, a wasp cannot possibly foresee the next link, still less the final purpose or use in nature. Each instinctive activity contains its own purpose as an activity. The above cases of dealing with prey too large for the nest are a good example of this: the activity itself includes the end of getting a spider, or what not, into the nest; and for this they discover means by trying different courses.

Another case that excites admiration is the way in which *Ammophila* stings her caterpillars just between the segments where the ganglia lie, so as to obtain the greatest effect with least expenditure. She cannot possibly understand the anatomy of caterpillars. If we look at the illustration of this action given by Peckham (p. 27), we see the wasp with long legs, bestraddling the caterpillar, holding it fast by gripping the back with its mandibles, and tucking its long abdomen under the caterpillar so as to reach the ventral chain of ganglia. If this is the way in which Ammophila always stings caterpillars, its whole structure is adapted to such a method. But how does it find the position of a ganglion? A good many caterpillars (of which the one in the illustration is an example) have their segments plainly marked by external constrictions of the skin. Bestraddling such a caterpillar and drawing the tip of its abdomen along the side, it must catch in a fold of the skin, and that may (by a reflex) discharge the stab of the sting. In other caterpillars, however, the skin hangs loose, and would give no guidance to the stinging operation. Does *Ammophila* ever take such caterpillars; and, if so, does she find the ganglia with any precision? That she may act in the way I have described may seem probable when we consider the following observation of Fabre's (quoted by Houssaye: *Industries of Animals*, Chap. v.). Once when *Sphex flavipennis* brought a paralysed cricket to her nest, whilst she as usual entered the nest before dragging in her prey, he took it away and substituted a live cricket, hoping to see her method of attack; and in this he was not disappointed. After a struggle the cricket was turned on its back. Sphex seized with her jaws the end of the cricket's abdomen, placing her legs on its belly and with her two hind-legs holding its head turned back so as to stretch the underside of its neck. "The cricket is unable to move, and the conqueror's sting *wanders over the horny carapace* seeking a joint, feeling for a soft place in which it can enter to give the finishing stroke. The dart at last reaches, between the head and the neck, the spot where the hard portions articulate, leaving between them a space without covering, etc." The words I have italicised express the analogy to what is suggested above as the possible action of *Ammophila.* Not knowing *S. flavipennis,* I cannot judge whether her figure is specially adapted to her dealings with the cricket: it may be worth the observing.

Again, some wasps, as we have seen, catch their prey before making a nest; and whilst burrowing some of them leave their prey on the ground, some lay it on a leaf, some hang it in the crotch of a branching shrub. The last plan strikes us as a remarkable refinement upon the second, which itself seems to be an intelligent means to the end of putting prey out of the way of ants. But, really, the prey must be put somewhere; and these are almost the only places in which it can be put. Is it necessary to see more in such behaviour than three variations of placing the prey, none of which hitherto has had sufficient survival value to extinguish the others?

Take, finally, the case of covering up the nest whose use, unknown to the wasp, is protection against ants and parasites. We have seen that this is done with all degrees of care. Peckham reports (p. 38) an interesting observation upon *A. urnaria*: one individual of this species was seen to finish the closing of her nest by picking up a small pebble in her mandibles and using it as a mallet to harden and smooth the surface. He quotes W. S. Williston as having observed the same action as specific (apparently) in *A. yarrowii Cres.* If the action is specific, it is less probable that it is intelligent. We must consider that these wasps are accustomed to pull pebbles out of holes and to carry them in their mandibles, and also that they are accustomed to smooth the earth by striking with their abdomens and butting with their heads; and now some have taken to using pebbles instead of their bodies. This is all the variation; and what most puzzles me is that it should be useful enough to become specific; but there are numerous cases in nature in which the development of structure and function seem to have been carried to an unnecessary pitch of perfection, and for which nevertheless we can at present assign no other cause than natural selection.

The methods of Natural History and Psychology can only make a first approximation to the explaining of tropism, instinct and intelligence. Movements of pursuit and avoidance, plasticity, critical changes in the life-history of an animal or plant, original tryings, analogical recognition, memory, must all have grounds in the intimate constitution of living things: there must be a *latens schematismus* and a *latens processus* of these things, that for the most part remain to be explored and promise a boundless field for experimental industry.

OBSERVATIONS ON THE COLOUR VISION OF SCHOOL CHILDREN.

By A. WINIFRED TUCKER.

Object: the comparison of the colour vision of English children with that of primitive peoples. Methods: matching with Holmgren's wools, naming, tintometer. Resemblance of children to primitive peoples in character of discrimination. With tintometer general raising of thresholds, but not more for blue than for other colours. Influence of pigmentation and general intelligence.

THE observations, of which an account is to be given in this paper, were made on children from two different elementary schools in Cambridge ranging in age from 10 to 5 years. The work was suggested by Dr Rivers who has helped me all through with it, and I should like to thank him here for the interest he took in the work and the kind assistance he was always ready to give me. He thought that it might be interesting to have a series of experiments on children of different ages, similar to those which have been made on various primitive peoples, to find out whether any light could be thrown on the peculiarities of colour vision found among the latter, or at any rate to see whether these peculiarities exist in European children even though they are absent from adults.

Ever since the question was first raised by Gladstone[1], it has been a matter of debate whether the capacity to discriminate colours has improved in historical times, and whether there are not even now some peoples in whom the perception of blue is very poorly developed. From a study of philological evidence various writers asserted that this must be the case, and experiments undertaken with a view to finding out the nature of the colour vision of living people low in the scale of civilisation may support the conclusion of the earlier writers, though they are capable of receiving another interpretation which rests on the fact that the experiments were performed on dark skinned peoples. Thus Dr Rivers says[2], "we may suppose that the defective sensibility

[1] *Studies on Homer and the Homeric Age*, 1858.

[2] *Cambridge Anthrop. Exped. to Torres Straits*, Vol. II. Part I. 1901, p. 79.

to blue is due chiefly or altogether to the influence of the macula lutea. It is well known that owing to the yellow-red pigmentation of the region of direct vision, blue and green rays are absorbed more strongly than in the extra-macular region of the retina. On this account blue is a less intense colour to the macula region of the normal eye than it is to the extra-macular region." If then the yellow pigmentation of the macula is greater among black-skinned than among white-skinned peoples, the absorption of green and blue rays will be greater than in the European eye, and the insensitiveness to blue in direct vision may be due to this cause.

If however the same defects exist in European children, it seems more likely that some explanation which will account for these in the children will also account for them in the primitive peoples. In both cases the facts would probably be due to the lack of development of the physiological mechanism which acts as the basis of the sensation blue.

The children, who came to me one at a time during school hours, were tested first with Holmgren's wools; then the names of the chief colours were obtained by showing them papers and wools of various colours, and finally the thresholds for the colours red, yellow, and blue, were found by means of Lovibond's tintometer.

Colour Discrimination.

Sixty-three girls and sixty-five boys were tested with Holmgren's wools. In addition to the three test wools three others, used by Dr Rivers among the Todas and the Papuans[1], were also compared, namely a yellow, a blue, and a violet of medium saturation. The wools were presented on a white background in the order red, green, pink, yellow, blue, violet; red was given first partly to keep the conditions the same as those which had been observed when testing the natives, and partly because it is by far the easiest colour for children to match.

Of the sixty-five boys who were tested two were found to be definitely colour-blind, while two more were distinctly weak for red. An account of their matches may be left till later. Only three of the ten-year-old girls made any mistake at all with the wools, and then it was simply to put blue wools with the violet test wool, a mistake which became universal among the younger children, and one which is made by all primitive peoples. The ten-year-old boys were not

[1] A full description of the methods used both with the wools and with the tintometer is given in *Reports, Cambridge Expedition to Torres Straits*, 1901, Vol. II. p. 49.

quite so good as the girls, for three of them put distinctly bluish wools with the green test, while three others put very unsaturated pinks with the blue-pink test wool; blues and violets were also put together.

The eight-year-old children matched the red wool quite correctly except that four of them compared a very reddish brown with the test wool, though none of them finally accepted it as a match. Green was correctly matched by all, as was pink. Two compared a very faint, salmon pink with the yellow which was a rather dull colour; blues and violets were mixed and one or two children also put green-blues with the blue test.

The seven-year-old children made all the mistakes of the older children and showed far less hesitation in doing so. The range of colours accepted with the test wools was much larger, *e.g.* with red, dark blue-pinks were often put and often dark browns too. All shades of blue-green were chosen with the light green test, though dark pure greens were mostly avoided. All degrees of saturation were accepted for the pink test though the older children always refused a wool unless it was fairly dark. The yellow wool was well matched as a rule though in one case a light green was put with it, and in another case a grey; light pink wools were also compared with it.

The five- and six-year-old children chose a very wide range of colours, only a few being as consistent as the older children in their matches with red, pink and yellow.

Generally we may say that blue and violet tend to be confused by all children, then the matches with green extend their range, and finally those with pink, red and yellow.

The comparisons made by the colour-blind boys are those normally associated with red-green blindness, *e.g.* browns were compared with red; light pinks, light yellows, and neutrals with green; a blue-green was compared with pink, as also were purple and violet wools; blue-pinks were put with blue, and with violet. Yellow and blue were never confused. Of the two boys who were weak for red, one made one or two slight mistakes with the wool test but he might easily have been passed as normal, and it was only when he came to do the tintometer test that he failed to discriminate red. The other boy made several curious mistakes not usually made by the colour-blind or colour-weak person, and it is possible that these were due in part to his defect of general intelligence, *e.g.* he put light pinks, neutrals and yellows with the green test wool, but he also chose a blue-green wool and a light blue which a red-green blind person would not do. With the red he matched brown wools as usual but also a dark pink containing blue. The test with the tintometer showed that he was weak for red, for his threshold for this colour was 300, but some of his mistakes are probably due to mental defect rather than to sensory deficiency.

Colour nomenclature.

A great deal of work has been done on the colour names of school children and this subject is only mentioned here because, though in these experiments vagueness about colour names often went with defective discrimination, yet there was no strict relation between the two. Earlier observers based their conclusions as to the colour vision of children on their power to name various colours, while later ones have often gone to the opposite extreme and asserted that colour discrimination is quite independent of colour naming and much earlier. The truth seems to lie between the two statements. The name is probably at first attached only to well-saturated shades of the colour and its exact application only learnt by degrees.

Very interesting results have been obtained lately by Housten and Washburn[1], who experimented on 1000 individuals of different ages. They suggest as others have done that the completeness of the colour vocabulary depends on the extent of the social needs of designation, and they think that colour vocabulary is no test of discrimination. Twenty-four spectral colours were chosen which were exhibited against a grey background for two seconds one at a time. Their results are given in graphic form in a diagram which clearly shows, that in naming, green is sharply marked off from red, but has a wide range on the blue side, blue and purple or violet overlap, while blue and yellow are strongly marked off from one another. These results are exactly similar to those obtained from the children with Holmgren's wools.

Mr Winch[2] has lately published results, obtained from much younger children ranging in age from three to five, which he thinks are in favour of the view that colour sensation is at first unitary and only gradually differentiates. He found that these children used red and blue correctly far oftener than any of the other colour names, while very many of the children did not use the others at all. This fact led

[1] *American Journal of Psychology*, 1907, Vol. XVIII. pp. 519–523.

[2] *American Journal of Psychology*, July 1910, "Colour names of English School children."

Mr Winch to the conclusion that discrimination and naming of colours are independent. "I have no doubt at all[1]," he says, "both from my own observations and from the work of others that the discrimination of colours is a much earlier thing than the correct naming of them." This is probably true for the well-saturated colours, but the responses obtained from the Cambridge children show that even when the name is known it is only very gradually that its exact application is learnt, and further that discrimination is faulty in some cases where the name is correctly applied to the saturated colour. Thus where the name violet is not known this colour is always confused with blue, but so it often is even when the name is known. Blue was correctly named by every child, yet the range of colours regarded as similar to blue (as shown by the wool test) was very wide. Frequently too a light violet was correctly named while a dark violet was called blue and matched with it.

The children of ten who gave me colour names made no mistakes with the principal colours. They were shown a series of coloured papers which included white, black, grey, red, dark pink, orange, yellow, dark green, light green, blue-green, indigo, violet. I also showed them a dark brown wool, a light pink one, and often a light violet wool when they had called the darker violet blue. By seven-year-old children orange was often called light red, or dark yellow; violet they called dark blue, and the pink paper often red or mauve. In these cases a lighter pink was correctly named. Many of them made the curious mistake of calling the dark colour, light, and the light, dark; similarly in the tintometer test they confused right and left. By the younger children pink was called red or they did not know the name. Grey was invariably called brown. Blue-greens and green-blues were always called by the predominating colour except by the ten-year-old children and one or two of the more intelligent younger ones; this may account to some extent for the matches that were made with these colours, blue-greens often being put with pure green wools.

Dr Rivers found exactly the same thing among the natives. He says "there was a natural tendency to put together all the wools to which the same name was given[2]," and very often the native could be heard murmuring the name as he picked up a wool which differed in tint from the test wool. This was more particularly the case with unsaturated wools.

[1] *American Journal of Psychology*, July 1910, p. 476.

[2] *Torres Straits Reports*, p. 49.

Tintometer tests.

The results so far obtained seem to show that English children make exactly the same kind of mistake as the natives, while the younger ones often show an even less highly developed power of colour discrimination than they.

The method used with the tintometer was exactly the same as that used by Dr Rivers[1]. Three series of carefully graded glasses red, yellow, and blue were used, and the threshold for each was obtained. The thresholds obtained are relative, not absolute, but as it is only the relation between the threshold for any one colour and that of another that is required, the results are sufficiently definite for the present enquiry. In obtaining these thresholds two wrong answers in ten were allowed; a glass for which more mistakes than this were made was regarded as being below the threshold for that colour. In replying the red glass was often called pink, and the yellow one green, but a mistake was only recorded when the coloured glass was definitely called white or when both glasses were said to be the same.

Very often simultaneous contrast was so great that it was difficult to obtain a threshold. When the coloured glass presented was well saturated it was clearly recognised, but the opposite side was seen in the contrast colour, and when the colour presented was of low saturation it was often not recognised as coloured at all, while the opposite side which contained no glass was seen in the contrast colour. This happened in most cases when the threshold for the colour was nearly reached, and often a rest would make the contrast less intense, but in one or two cases these colours remained even when the threshold had been reached. The yellow glass caused most difficulty, a vivid blue contrast being obtained, though in the case of one little girl it was a pink that was seen, due perhaps to the large amount of green in the glass. With the red glass many of the boys and a few of the girls saw the contrast colour, green. Sometimes yellow was seen opposite the blue glass but not often.

Strangely enough many of the children saw the contrast colour with the pale yellow glasses vivid enough to make them call the opposite side blue long after they had passed below their threshold for the blue glasses themselves.

Successive contrast occurred at times. The child would name the glass correctly but would also say there was a colour in the other side,

[1] See *Journ. Anth. Inst.* 1901, Vol. xxxi. p. 239.

and this very often was not the complementary of the glass shown but of the one before. This usually happened when the white glass was shown, but it occurred also with the coloured glasses.

The actual thresholds obtained for the three colours red, yellow, and blue are given in the following table together with the mean variations which show the extent to which the individuals of the different groups varied from the average, and the relation of the mean variation to the average which enables us to compare the variability of one group as compared with another. The figures obtained by Dr Rivers from various primitive peoples and from a number of English men are also given for the sake of comparison.

The results obtained from the colour-blind boys and those who were weak for red have not been included in the table as it was impossible to get accurate results for them and the high figures would only have given a false value to the average.

TABLE I.

Group	No.	Red					Yellow					Blue				
		Av.	Max.	Min.	MV.	MV./A.	Av.	Max.	Min.	MV.	MV./A.	Av.	Max.	Min.	MV.	MV./A.
English Adults	41	27·5	—	—	17·9	·65	16·7	—	—	7·2	·43	30·8	—	—	11·2	·36
Boys 10 yrs.	10	31	50	15	9·4	·30	29	80	10	15	·52	52	80	30	18·4	·35
Girls ,,	10	32	40	10	8	·25	30	40	10	4	·13	52	80	30	16·4	·32
Boys 8 yrs.	16	35·6	60	10	11·2	·32	29·7	50	15	9·8	·33	75·3	150	15	29·1	·39
Girls ,,	18	34·2	50	15	8·1	·24	30	80	10	10	·33	71·6	140	20	26·1	·36
Boys 7 yrs.	15	41	80	15	8·8	·21	40·6	80	30	10·4	·26	78	100	40	23·6	·34
Girls ,,	14	44·6	100	15	17·4	·39	37·9	70	20	12·2	·32	80·7	140	40	22·4	·28
Boys 6 yrs.	10	59	100	20	22·8	·39	54	100	30	18·8	·34	93	110	80	10·4	·11
Girls ,,	11	53·6	80	40	13·7	·29	40	80	20	12·7	·35	87·7	150	50	26·1	·29
Boys 5 yrs.	10	54	70	30	12·8	·24	60	100	40	18	·33	101	170	70	27·4	·27
Girls ,,	10	69	150	40	27	·39	67	100	30	21	·36	112	180	70	42	·37
Todas	47	32·1	100	10	18·1	·56	29·2	120	5	15·6	·53	53·3	100	15	20·7	·38
Uralis and Sholagas	14	31·1	70	15	13·5	·43	26·4	40	15	7·9	·29	66·4	100	40	15·5	·23
Egyptians	26	28·7	60	10	14·2	·49	26	50	5	10·5	·40	85·4	200	20	34·3	·40
Murray Islanders	17	17·6	40	5	7·5	·44	26·5	40	10	9·7	·37	60·6	130	30	16·5	·28

From this table it will be seen that there is a progressive increase in all three thresholds as we go from the older to the younger children, while the average threshold for any one age is much the same for boys and for girls. If we compare the results with those obtained from the

English men it will at once be seen that there is a very large difference, even between the ten-year-old boys and the 41 men who were measured, while for the other ages the differences become increasingly large, the thresholds for yellow and blue being slightly higher than those for red. The mean variations are not large when compared with those of the adults and of the various groups of natives, while in some cases they are remarkably small. Unfortunately the numbers experimented on were too small to permit of the standard deviation and the probable error being calculated, but the increase is so regular and so consistent for boys and girls that it seems legitimate to assume that it is real and not due to chance.

On the other hand there is little if any change in the ratio of one colour to another for the different ages as will be seen from Table II.

TABLE II.

	Age	B./R.	B./Y.		Age	B./R.	B./Y.
Boys......	10	1·68	1·79	Girls......	10	1·63	1·73
,,	8	2·12	2·75	,,	8	2·09	2·38
,,	7	1·90	1·92	,,	7	1·81	2·13
,,	6	1·57	1·72	,,	6	1·63	2·19
,,	5	1·87	1·68	,,	5	1·62	1·67
,,	all	1·84	1·91	,,	all	1·79	2·18
English	men	1·12	1·84				
Todas		1·66	1·83				

In this table the results obtained with the tintometer test have been expressed in a different way, *i.e.* as ratios of blue to red and yellow respectively, the latter being in each case taken as unity. The figures show that the progressive raising of the threshold by the children in the decreasing age is not accompanied by any raising of the threshold for blue, beyond that for other colours. In comparing the thresholds of the children with the adults, the figures for red are not comparable, owing to the fact that one or two cases of special weakness for red were included in the latter. Taking the ratio of blue to yellow, it will be seen that there is a slight raising of the threshold for blue in the children as compared with the adults, but so slight as to be well within the limit of the varieties among the children themselves.

The quantitative results thus show that though there is a raising of the threshold all round among children, the general character of their colour vision is in no way different from that of adults, the increase for blue being no greater than that for yellow and very little greater than that for red.

The ratio between yellow and blue in the case of the Todas shows that in these people also there is no decisive raising of the threshold for blue. When the results are expressed in this way as a ratio between blue and yellow the Todas fall into line with the Europeans, both adult and children. It has been suggested[1] that the high threshold for blue with the tintometer depends on the amount of pigmentation of the yellow spot, and since so far as pigmentation of the skin is concerned Todas would be classed with Europeans rather than with Negroes, it might seem that the hypothesis of pigmentation receives some support. The high threshold of the Egyptians is however a difficulty, and all that can be said at present is that comparative observations on members of different races allow no definite conclusions to be drawn concerning the cause of the high threshold for blue which is found in some of them.

If however the pigmentation theory is correct, we might expect to find slightly higher thresholds for Europeans with deeply pigmented eyes, and in order to see if this were so in the case of the children, the colour of the eyes was in each case noted down together with the colour of the hair and other details. The thresholds for children with light eyes and for those with dark eyes were then worked out separately so that the two groups can be compared. Among the light eyes were classed the blue, both light and dark, grey, and green; among the dark ones were classed all the brown eyes. The averages for the 49 children with light eyes were: yellow 62·3, blue 89·9, while those for the 14 children with dark eyes were: yellow 52·1, blue 89·3. In one case the ratio is 1 to 1·5, in the other 1 to 1·7, so that the difference between the two is very slight; that is the thresholds are equally high whether the eyes are light or dark. We can therefore say that there is no relation between the amount of pigmentation of the iris and the threshold for blue. It seems probable that pigmentation of the iris and of the macula may go together, and if this should be so, these results would indicate that such differences of pigmentation of the yellow spot as occur among English children have no apparent influence on the threshold for blue. It must however be remembered that the number of children examined who had dark eyes was small, and that the difference of pigmentation was not very great.

Another argument which seems to favour the pigmentation theory is based on the fact that among natives blue is discriminated quite as well in indirect vision as in direct, or possibly even better in the former, thus indicating that in direct vision the macula lutea influences the

[1] See p. 34.

threshold for this colour. That the children were often able to perceive blue in indirect vision or at any rate to see blue contrasts, after they had passed below the threshold for blue in direct vision, suggests the same conclusion, and therefore an attempt was made to get quantitative results with indirect vision. Only two boys (both of whom had brown eyes) could be satisfactorily tested, for it was very difficult to get a threshold at all with the tintometer, as the children did not like the procedure, and one could not be sure that the eye did not move so as to bring the glasses into the macular field of vision. The results obtained, such as they are, do not support the suggested explanation. One boy had a threshold of 100 in direct vision while in indirect vision he failed to see blue at 100, three times out of five, the other had a threshold of 70 in direct vision while in indirect it was 100.

Since the quantitative results seem to show clearly that European children differ in no wise from adults in their perception of blue, the qualitative results which showed that they tended to make the same mistakes with Holmgren's wools as the natives, probably depend on quite other conditions than the degree of development of colour perception; both among children and natives these mistakes in comparison are probably due to faulty discrimination based on psychological rather than on physiological grounds.

As the thresholds of the children were in all cases so much higher than those of the adults, it seemed worth while to see whether the intelligent children had lower thresholds than the less intelligent. In a good many cases I had made notes about the children when they seemed especially quick or otherwise in their methods of responding to various tests, but to obtain a more satisfactory estimate of their general intelligence, I asked the head-mistresses of the two schools at which I had been working if they would be kind enough to grade the children for me into three classes, bright, moderate, and dull, adding a note if they wished to say anything particular about any of the children. They did this, and all the children seem to have come under these heads without any difficulty, except one who was called defective. The classification is rough, but probably sufficient for the purpose. The lists for the two schools have been kept separate, so as to avoid any complication there might have been due to the differences in the methods of classification which are bound to be present owing to the fact that the lists were drawn up by two mistresses.

The results given in Table III do not seem to show that intelligence has much to do with the highness or lowness of the threshold. This is in agreement with my own observation while I was doing the work; I noted that the children who seemed particularly dull might have quite low thresholds. The difference between the children showed far more in the speed with which they grasped the method and gave their answers, and also in the influence of fatigue upon them. Some would go right through the tests without showing any fatigue at all, while others would require two or three rests before they reached their threshold.

TABLE III.

	Bright				Moderate				Dull			
	No.	Red	Yellow	Blue	No.	Red	Yellow	Blue	No.	Red	Yellow	Blue
School *A*...	13	50	50	87·7	12	63	42·1	86·6	9	47·2	50·5	103·9
School *B*...	34	38·2	36·9	82·4	30	40·5	34	72·5	24	45	38·3	75·6

However this may be, the difference between the qualitative and quantitative experiments in the case of the children suggests that there may be two factors present in the explanation of the peculiarities found among primitive peoples; the one, psychological, depending on the stage of development of the powers of observation and thought, leading to mistakes similar to those made by European children; the other, physiological, remaining in whatever stage of development the people may be. If similar observations could be made on people of high civilisation, with dark skins, it might help towards the solution of the problem.

THE FALL-HAMMER, CHRONOSCOPE AND CHRONOGRAPH.

By KNIGHT DUNLAP.

[*From the Psychological Laboratory of the Johns Hopkins University.*]

Criticisms on the Wundt fall-hammer.—Defects in the Zimmermann model and their correction.—Reliability of the break spark in chronograph records.—Latency of magnetic markers shown by spark method.—Timing fall-hammer from break to break of current.—Timing from break of current through supporting magnet.—Effect of occasional reversal of current on Hipp chronoscope.—Spark method of recording movements of chronoscope armature.—Effects of unreversed current on chronoscope.—Advisability of never reversing.—Lower winding for Magnets.

THE following observations on the testing[1] and manipulation of the Wundt fall-hammer, Hipp chronoscope and certain chronograph accessories are offered on account of their interest in connexion with the operation of other pieces of apparatus of analogous structure, as well as because of their importance for the accurate use of the instruments mentioned.

The large model fall-hammer has been orally condemned by various American psychologists as being difficult of adjustment and inaccurate in operation. In print, Cattell[2], while not certifying to the unreliability of this particular model, states that he believes the fall-apparatus designed by himself to be a more accurate instrument. "Ich bezweifle zwar nicht, das der von Külpe und Kirschmann verschriebene Fallhammer recht wohl zur Regulierung des Chronoscopes dienen kann, aber ich glaube, ein Fallschirm ist besser." Edgell and Symes, in their

[1] The tests reported herein were made with the assistance of George R. Wells.

[2] J. McK. Cattell, "Chronoscope und Chronograph," 1894, *Philos. Studien*, IX. 307—310.

first statement[1] concerning the large fall-hammer, declare it to have a negligible variation in its fall-times, and imply that it is a very satisfactory instrument; in their second note[2], a few months later, they virtually recant this commendation, saying that the instrument did not wear well, and that the methods by which they had timed it were quite unreliable.

The most interesting data on the operation of the fall-hammer are those supplied by Külpe and Kirschmann in their original account of the instrument[3]. They find for the average fall-times, ranging from 616σ to $56{\cdot}6\sigma$, mean variations ranging from 2·8 to 0·6, respectively. This is remarkable, inasmuch as the fall-hammer is virtually a pendulum, and ought to be operable with practically no mean variation, unless there are grave defects in its construction.

In using the large model fall-hammer made by Zimmermann (No. 1300, price list No. 20), we have found no serious difficulty in obtaining fall-times practically without variation, when measured from make to break, although careful handling of the instrument was necessitated by structural defects which will be detailed shortly. When we attempted to time the fall from break to make, using magnetic markers, we found a large variation, and on this account have made a careful study of the instrument's action, and have made some changes which seem decidedly good.

In the following descriptions we shall refer by letter to the cuts of the fall-hammer and contacts given in Wundt's *Physiologische Psychologie*, 5th ed., Vol. III., Figures 358, 359 and 360, pages 397, 398 and 399. These cuts are identical with Figures 1, 2 and 3, in the article by Külpe and Kirschmann.

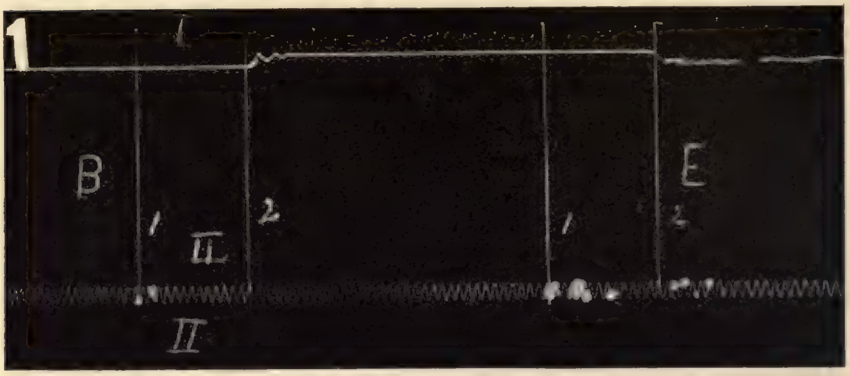
There are some differences between the Krille instrument and the one made by Zimmermann, but these will be specifically mentioned, in so far as they are important for the present discussion.

The magnet (*E*, fig. 358) is held in position on the transverse rod by a simple sleeve and set screw. When adjusted for short or medium falls, the magnet is necessarily placed at a considerable angle from the vertical, and we found it impossible, under these conditions, to prevent it from gradually swinging toward the vertical, thus appreciably changing the total fall-time if frequent readjustments were not made. The set

[1] B. Edgell and W. L. Symes, This *Journal*, 1906, II. 58—88.

[2] *Ibid.* 281—283.

[3] O. Külpe u. A. Kirschmann, "Ein neuer Apparat zur Controle zeitmessender Instrumente," *Philos. Stud.* 1893, VIII. 145—172.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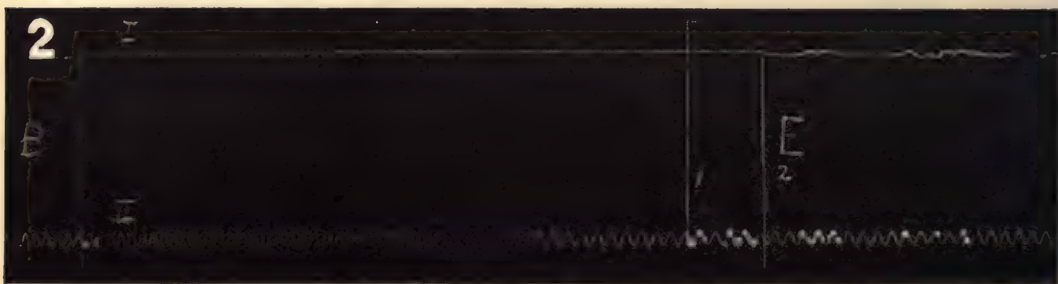
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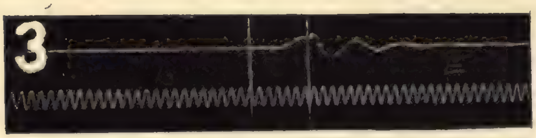
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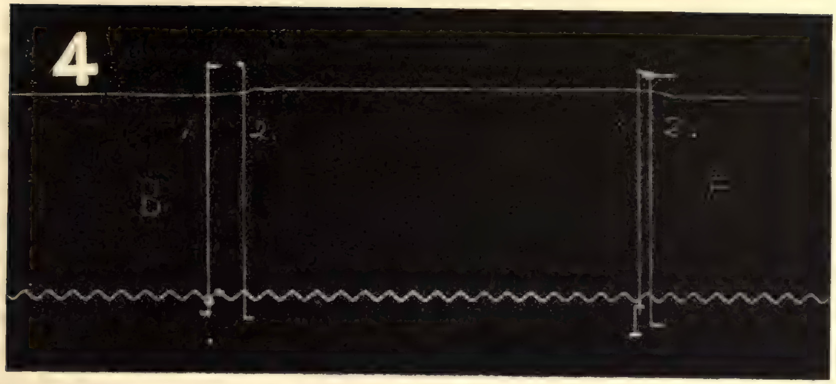
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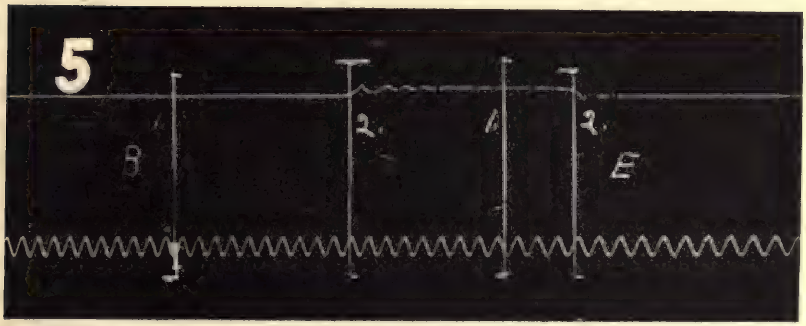
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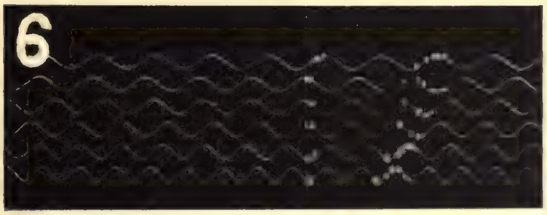

6

screw could not be tightened enough to prevent the rotation of the sleeve on the transverse rod under the influence of the heavy jar of the fall. We have had the simple sleeve replaced by a split sleeve, with lugs projecting above the transverse rod, and a heavy clamping screw through the lugs. This sleeve may now be clamped so tightly on the rod that it is absolutely immovable.

In timing from break to make, by the spark method, we found a clear break-spark, a fair make-spark, and then a succession of sparks, trailing out over an interval which was often more than 100σ. This after-sparking we soon found to be due to the bouncing of the floating lever of the lower compound contact-key (the lever bearing the post *sq* and contact plate *p*, fig. 360), caused by the jar of the fall. Thus are explained at once the variations in the times recorded by magnetic marker and by chronoscope: the irregular and rapid interruptions of the current prevent the uniform re-establishment of the magnetic field.

The spring r' (of a different type in our instrument) could not be given sufficient stiffness to stop the bouncing. The floating lever was about five times as heavy as is necessary, and this exaggerated the trouble. There was even a heavy binding post on the lever. It was necessary to reduce the weight of the lever, and it was desirable to add a spring which could be adjusted to give adequate tension.

We had the floating lever reduced in width, the binding post removed, and a light flexible conductor run from the lever to a post on the base (*b*). We also had another binding post mounted on the base, and a light flexible conductor run to the lever (*l*). In the original form of the machine, the leading wires were necessarily connected directly to the levers, so that the slightest disarrangement of the wires changed the action, introducing serious errors in the readings. It was absolutely necessary to have this defect remedied—to have the additional binding posts on the base.

We have not yet had the spring added. We found that a strong rubber band could be satisfactorily used, and have so far used one on the lower compound key. Although we have had the upper compound key modified in the same way as the lower, we have not had to increase the spring tension on it, as the bouncing of this lever makes no difference, whether we are using the hammer from make to break or break to make. If it is ever necessary to use the hammer from make to make, an additional spring would be needed on the upper lever also. The simple break contacts of the instrument (fig. 359) have also been improved by a single binding post on the base, with light flexible conductor to the lever (*l*).

The figures 1 to 6 (pages 46, 47), are tracings taken with the 250 *d. v.* fork on the modified Schumann chronograph. These records are to be read from left to right, *B* indicating beginning and *E* ending. The upper lines in figures 1 and 5 are the tracing of one stylus of the *Doppel-markiermagnet* made by Zimmermann (No. 1802, List 20). The upper tracings in figures 2 and 4 are made by the *Federsignal nach Pfeil modif* (Zimmermann, No. 1830, List 20). These are all records from break to make, taken with the fall-hammer. The sparks on the accompanying fork tracings are also break to make, the circuit through the spark-coil primary having been broken and made simultaneously with the break and make of the marker current. The vertical lines were drawn by disconnecting the gearing of the chronograph, clamping the drum in position, and drawing the fork along the drum in the normal way by rotating the carriage-screw. The points of the fork and the magnetic marker in each case had been adjusted on a similar line before the record was taken. The lines marked (1) are drawn through the sparks marking the actual break or make of the current. The lines marked (2) are drawn near the points of inflection of the marker tracing, but no attempt was made to get these placed accurately. The upper tracing in figure 3 is from one of the magnetic markers supplied with the Schumann chronograph (Spindler and Hoyer, No. 17, List 21; see F. Schumann, *Philos. Stud.* IX. pp. 260—262). In this record, which reads from make to break, the current was made and broken through the marker by an automatic key attached to the chronograph. Many other records were taken from all three markers, both with fall-hammer and with the drum-key, using varied current strength. The records given are strictly representative.

The effect of the improvement of the contact-keys of the fall-hammer is marked. Compare for example the make-latency of the marker in figure 1 with the same latency in figure 5. Although in the latter case the current is weaker and the spring stronger, the latency is markedly reduced. The effect on the Pfeil marker is of the same order, but shows rather as increased legibility.

Before the floating lever was reduced in weight, tests were made on the chronoscope, with constant adjustment of fall-hammer, chronoscope and current, but in one case without, and one case with, the rubber band. The following two series, taken in succession, are typical; the figures representing thousandths of seconds (σ):

A (Rubber band on floating lever of fall-hammer)	*B* (Without rubber band)
305	358
306	342
306	357
305	361
307	350
306	344
307	361
306	358
307	356
307	343

Suppose, now, an approximately correct timing of the fall-hammer had been obtained (as might easily have been done by taking the record from make to break), and the hammer had been used to control the chronoscope from break to make, without regard to the effect of the bounce; an error of over 40σ would have been introduced.

We have adopted the spark-chronoscope as the only adequate means of testing the fall-hammer or other control instrument, where no direct tuning fork record is available such as is possible where an arc is attached to the fall-hammer. We use preferably the break-spark only, as that is perfectly reliable, whereas the make-spark, although sufficiently reliable for most practical purposes, does show slight variation at times, and in order to get a good make-spark it is necessary to use a current which gives a break-spark so heavy as to burn the carbon from a considerable area around the actual perforation of the paper.

That the break-spark is reliable, even with low current, we have abundant evidence. The record in figure 6 is one instance. The current was broken by our automatic key attachment to the chronograph, and the vertical line in the record shows the position of the tuning-fork point when the arm just touched the lever and the contact was still unbroken. It can readily be seen that the delay in the spark is negligible.

Similar results were obtained with currents ranging from 1·9 amperes to 10 amperes, with voltage not exceeding 10 volts with current broken.

The sparks following the break are due to the remake of the current and the vibration of the key. This could be easily obviated, but was of no importance in this particular record. It will be noticed that occasionally there are two break-spark perforations. Nevertheless, the first of these is always correctly placed and the second can therefore be neglected. This doubling of the perforation is a function of the length of the spark discharge, which, under our working conditions, was sometimes 6σ or 8σ.

Of course, apparatus may be so arranged that considerable delay in the spark occurs. It is necessary to make tests under the exact working conditions, as we have done, to certify that these conditions are appropriate. The spark method cannot be successfully applied by an experimenter who has not considerable practical knowledge of the action of electrical currents.

On the question of the availability of magnetic markers when the spark method cannot be used, the records here presented are illuminative. In obtaining record 1 the strongest tension of the spring was employed, with relatively high current (·57 amp.) and minimal movement of the armature; in obtaining record 5 the spring was cut, shortening it so as to obtain greater tension. By no adjustment of spring could we obtain a more satisfactory tracing than in record 5.

Records 2 and 4 were obtained from the Pfeil marker, with 1·0 ampere of current. The magnet is lower wound than that of the double marker. This instrument is very sensitive, but no great amplitude of movement can be obtained without sacrificing accuracy; hence, at high speeds of the drum the points of change in the line are almost undiscoverable.

The marker on the Pfeil principle supplied on the Schumann chronograph (figure 3) is not as reliable as the small Pfeil marker, principally because the magnet is wound too high. We do not use the Schumann markers at all, but have removed them from the chronograph and substituted the Pfeil marker. When we consider the use which in the past has been made of unsuitable markers in all sorts of time work, we must have grave doubts of the reliability of much of the resulting data.

One particularly bad feature of the Zimmermann fall-hammer, which is inexcusable, is the absence of lock nuts from the adjusting screws. The Krille instrument had lock nuts on the contact screws, but not on the screws which adjust the springs. All these screws must have lock nuts, as a slight change in the contact screws changes the time, and a change in the spring screw of the upper contact key may also produce a time change. The lock nuts are replaced in the Zimmermann instrument by split ends on the levers, but these are not satisfactory substitutes.

In timing the fall-hammer from break to break, the primary current is arranged through the upper contact and the coils of a Cattell relay, the two coils of the relay being connected in parallel to reduce the resistance and reluctance. When the upper contact is broken, the relay armature moves over, and completes the primary circuit through the lower contact, so that it may be rebroken by the hammer. The action of the relay may introduce some sparks between the initial and terminal break-sparks, but these do not cause any confusion. In theory, there should be more delay of the spark when the first circuit is interrupted than when the second is interrupted, but practically there is no difference; there is no appreciable delay in either case.

In attempting to time the fall-hammer from the break of the current through the supporting magnet to the break of a second current through one of the contact-keys, large variations were found. In one series, for example, the recorded times ranged from 171σ to 178σ, inclusive, and this is typical of series in which the counterpoise was set near the pivot. With the counterpoise set far out, so that the downward pull at the magnet is small, the variation may be as much as 20σ. There is rather obvious bearing of these variations on the question of accuracy in certain models of the pendulum chronoscope, in certain types of fall-apparatus, and such reaction-times as those obtained by Froeberg[1], where the time was measured from the break of the current through the magnet holding a ball, the computed time of fall of the ball being subsequently subtracted from the measured time.

In regard to the Hipp chronoscope itself, the remarkable work of Symes and Edgell has confirmed and added to the results obtained by Külpe and Kirschmann, Müller and Schumann, Kraepelin, and Ach, and conclusively proved that the Hipp is not influenced by the length of time during which the current runs through the magnets for timing of extents as small as the shortest simple reaction, or longer. So far nearly all experimenters have assumed the necessity of reversing the current, frequently, if not after each reaction, through the chronoscope magnets; and no one has made a real test of the advantages or disadvantages of not reversing at all. Edgell and Symes state that "occasional omission to reverse, gives rise to no appreciable irregularity in a series of chronoscope readings of equal periods" (p. 76).

This statement is not in agreement with my own observations. I have found it much better to keep the current flowing constantly in the same direction through the magnets, and have found uniformly that if in a series in which the current is reversed after each record, reversal is omitted once, the time following the non-reversal is longer than the other times. The next reversal, however, is followed by a reading of the same length as the normal reading following reversal. This is for readings from make to break. If the chronoscope is set up for break to make, it makes absolutely no difference how regularly or irregularly

[1] Froeberg, Sven, *Archives of Psychology* (Columbia University), No. 8.

the current is reversed. The effect of the reversals on the chronoscope running from make to break is clearly shown by the results of the series taken in a way which will now be described. The magnet having been depolarized, the fall-hammer set for 106σ, the hammer was dropped twice and the readings not recorded. Readings were then taken from the succeeding falls, the direction of the current being (1) reversed, (2) reversed, (3) reversed, (4) not reversed, (5) reversed, and so on, the current therefore being reversed before each fall except the 4th, 8th, 16th, and so on. Fifty-two readings were taken, 13 for each of the four cases. The averages and mean variations for the four cases are here given. (4) is the average of the readings before which the current was not reversed, and (1), (2) and (3), respectively, are the averages for the first, second and third readings after the non-reversal.

(1)	Av. 111·7	m.v. 1·0
(2)	„ 111·7	„ 1·4
(3)	„ 111·9	„ 1·0
(4)	„ 118·2	„ 1·2

The variation is a little high in these series, owing to the particular setting of the fall-hammer. The variation arises from the errors in the reading (an error of 1σ is always possible in this particular chronoscope, although not highly probable); the actual error in the chronoscope; and the adaptation error due to the fact that the virtual chronoscope time is (in the first three cases) between 111σ and 112σ, a time which the chronoscope is of course incapable of recording. In most cases, adjustment of the chronoscope springs or current which brings the average recorded fall-time as near as possible to an integral value, also reduces the mean variation to a minimum value.

The obvious interpretation of the results, of which those given above are typical, is that a slight magnetization is left by the current, and that the next current, if in the opposite direction, has to overcome this remanent magnetization before inducing the opposite polarity. If successive currents are in the same direction, the remanent magnetization is in the direction of the second polarity, which is therefore established a little quicker than in the case where the polarity is opposed. In any case, the remanent magnetization at the end of a current period of 100σ is independent of the conditions at the beginning of that period.

We next took records of the movement of the chronoscope armature, and these gave complete confirmation of our interpretation. The direct writing attachment used by Edgell and Symes is of course the most reliable method of recording the armature movements, but for convenience we adopted a different method. A light arm was attached to the armature, with platinum contacts touching contact screws above and below, so that when the armature was in either extreme position, contact was made. The current was made to flow through these contacts and the primary induction coil. The drop of the hammer (the chronoscope being connected from make to break) caused the armature attachment to move from the upper contact to the lower, and then return. Since the make-spark in no case penetrated the paper on the chronograph drum, the movement produced only two sparks, the first when the armature began its upward movement, and the second when it began its return movement. These spark records were taken in our usual fashion with the 250 d.v. fork, and with the record of the Pfeil marker alongside, indicating the points at which the hammer made and broke the chronoscope current. The Pfeil marker was slightly variable in its action, and not all the records obtained from it are legible, but we obtained enough records to make it perfectly clear that the variation in the latent period of the chronoscope at the break of the current, running from make to break, is independent of the preceding reversals of the current, whereas the delay at the make of the current when the flow was in the same direction as in the preceding record was shorter than when the current was reversed between records. The shortening of the latent period in this case corresponded with the lengthening of the chronoscope record.

We had earlier supposed that there might be a progressive change in the chronoscope error, when the current is never reversed, due to the increase in the amount of the remanent magnetization. Careful tests have shown conclusively that nothing of the sort takes place. Starting with the magnets demagnetized as far as possible, and neglecting the first record, we have found that the succeeding records show absolutely no progressive change.

The following are the averages of ten records, chronoscope make to break, springs 0 and 10, current ·08 ampere. The current was in the same direction throughout, and was allowed to run continuously for 19 seconds between the first and second, second and third, and third and fourth series. The magnets were practically demagnetized before the first series, as was indicated by specific tests.

Series	1	2	3	4
Average	192·6	192·1	192·3	191·8
m.v.	1·0	0·8	0·5	1·4

Another set was taken with conditions at start as in the preceding, but the current was in this set 0·145 ampere. Ten reaction falls were timed, and then the current was allowed to flow for two minutes, being rapidly interrupted by the vibrator of an induction coil. Then a second series of ten was taken, the current being in one direction throughout. The averages for these two series were 198·9σ and 198·2σ, respectively, with mean variations of 0·3 and 0·6 respectively. A number of other series was taken with different current strength and time relation, but in no case was any progressive error discoverable. There can be no doubt that progressive change in the error from day to day, when it occurs, is due to the progressive weakening of the chronoscope springs.

The reversal of the current through the chronoscope magnets is clearly useless, and is apt to introduce errors of appreciable amount, since the experimenter is certain to forget frequently to reverse the commutators. In the case of more sensitive magnetic apparatus, as for instance the Pfeil marker, reversal of the current, even when regularly done, introduces serious errors, the latent periods being different for the two directions of current. A similar effect is found with the electrically driven fork; seldom, if ever, can the reversals of the current be so made that the intensity of the sound is the same for the two directions.

The only suggestion for the improvement of the Hipp chronoscope is that the magnets should be wound with fewer turns of coarser wire, thus reducing the reluctance and shortening the latent periods.

THE EXPERIMENTAL EXAMINATION OF SOME DIFFERENCES BETWEEN THE MAJOR AND THE MINOR CHORD.

By T. H. PEAR.

Lecturer in Experimental Psychology, University of Manchester.

I. Two current explanations of the differences.—The possibility of testing the general validity of these explanations by the examination of the usual, and of some "unmusical," major and minor chords.—General plan of the experiments.—II. Preliminary experiments with intervals.—The employment of musical and unmusical observers.—The consistency of their judgments, and the results obtained.—III. Experiments with chords.—Further experiments with intervals.—IV. The classification of observers as "musical" and "unmusical."—V. (i) *The results obtained from each individual observer.*—(ii) *The general results and a discussion of their significance.—VI. Conclusions.*

I. Introductory.

The major and the minor chord systems, as they are employed in modern music, afford an interesting problem for psychological investigation[1]. Various attempts have been made to explain the difference between them, and two, at least, of these proposed explanations are open to examination by experimental means.

(1) The first of these two explanations refers the difference to differences of direct and indirect relationship possessed by the clangs in the chords.

A short statement of the nature of clang-relationship may not be out of place here[2]. Two or more clangs (*i.e.* complex sounds, each

[1] For remarks upon the "softer" impression produced by the major chord, and upon the use of the German terms *dur* and *moll* for major and minor respectively (a use which primarily indicated a difference in *scale*, not a difference in the *psychical effects* produced by the chords), see O. Külpe, *Outlines of Psychology*, trans. by E. B. Titchener, English edition, 1895, 297–8, and W. Wundt, *Physiologische Psychologie*, 1910, 6 Aufl. II. 428.

[2] See Wundt, *op. cit.* 408–429, Külpe, *op. cit.* 296–8.

consisting of a fundamental tone with its accompanying partials or overtones) may be related to each other in two ways, directly or indirectly. They are said to be *directly* related when they possess some of their partials in common. The degree of direct clang-relationship is proportional to the number and intensity of the coincident partials. Clangs are *indirectly* related when they are referable to a common fundamental, *i.e.* when they may be regarded as components of one and the same "fundamental clang" (*Grundklang*). Indirect clang-relationship is proportional to the nearness of the common fundamental to the fundamentals of the related clangs. Both forms of relationship may appear simultaneously in the same combination of clangs. Thus the tones *c* and *g*, forming the interval of the fifth, are directly related since several of their overtones, *e.g.* g^1, g^2 coincide; they are indirectly related because both of them appear as overtones in the clang *C*, which lies one octave below *c*. (This is at once obvious if we remember that the vibration frequencies of *C*, *c* and *g* respectively stand to each other in the proportion of 1 : 2 : 3. The coincident overtones of the fundamentals *c* and *g*, which fundamentals are respectively represented by the numbers 2 and 3, are those expressed by the numbers 6 and 12, or g^1 and g^2.)

Let us apply these criteria of clang-relationship to the major and minor chords. Taking, for example, the major chord *c–e–g*, and the minor chord *c–e♭–g*, formed from it, we find that the first common overtone of *c–e–g* is the ninth overtone of *g*; it lies three octaves and a third above the fundamental of the highest clang. The first common overtone of *c–e♭–g* is the third overtone of *g*, it lies a double octave above *g*. The distances of the common *fundamentals* of the two chords from *c* stand in the reverse relation[1]. In other words, the minor chord shows the greater direct, and the major chord the greater indirect, clang-relationship.

Upon the question of the relative importance of direct and indirect relationship we find some difference of opinion. Wundt says that, in the case of successive clangs, direct relationship is more important than indirect, since in the change from one clang to another the coincident partials remain in consciousness. In the case of simultaneous clangs, indirect relationship is the more important form. Here the common fundamental may arise in consciousness, either associatively, or directly as a difference-tone[2].

[1] On the complementary structure of the major and minor chords, see Wundt, *op. cit.* 425.

[2] *Op. cit.* 410, 415, 422.

The fact that the degree of indirect relationship found in the major chord is greater than that found in the minor, coupled with the statement that here indirect relationship is more important than direct, forms the basis for this suggested "relationship-explanation" of the difference between major and minor.

Külpe holds, however, that "the degrees of clang-relationship, and more particularly those of indirect relationship, seem to be intellectual constructions rather than sensible relations....The common fundamental, the nearness or remoteness of which determines the degree of indirect clang-relationship, can only be given as a difference tone, or as a concomitant tone-sensation associatively excited. These tones are invariably too weak, as the common overtones also very frequently are, to furnish a satisfactory theoretical basis for the obvious differences of harmony and disharmony[1]."

Külpe, while attributing less importance than Wundt to the effect of clang-relationship in general, considers, for the reason given above, that, in most cases, the degree of *direct* clang-relationship is more important than that of indirect, and adds: "It would follow that the minor chord, whose clangs are more closely related directly, is the better harmony. This conclusion is not borne out by the facts[2]."

Dissatisfied with the suggested "relationship" explanation of the difference between major and minor, he seeks for help from the laws of tonal fusion, and proposes an explanation which we may call

(2) *The "fusion-explanation."* These laws, he says, do not supply us with any valid explanation of the difference, unless we make the assumption (which is no more than an assumption) that the total fusion of a chord is influenced by the relative positions in it of the constituent intervals, *i.e.* by the pitch of their separate components. For we have precisely the same grades of fusion in the major chord, *c–e–g*, that we have in the minor chord *c–e♭–g*. The only difference is that in the chord *c–e–g* the major third, *c–e* (which possesses a higher degree of fusion than the minor third, *e–g*) occupies the lower position, while in *c–e♭–g* the minor third, *c–e♭*, takes this place, and the more highly fused major third, *e♭–g*, is now in the higher position. The total fusion of *c–e–g* is of a higher degree than that of *c–e♭–g*[3]. If, therefore, we judge

[1] *Op. cit.* 297. [2] *Op. cit.* 298.

[3] Experiments upon the time necessary to cognise the special character of the major and minor chord have shown that the reaction-time with the minor is constantly though but slightly shorter than that with the major. The general rule would be that the chord which is more quickly cognised is the worse fusion. See Tanzi, *Rivista di filosofia scientifica*, VI. 1887, 174 f.

from the impressions of fusion produced by the well-known major and minor chords and by the intervals which compose them, the assumption will be that the degree of total fusion possessed by chords of this kind increases when the more highly fused interval occupies the lower, and decreases when it is in the higher position in the chord. Or as Külpe says[1], "The assumption would be that the degree of fusion of a chord varies with the position of its constituent degrees of fusion within the tonal scale; decreasing when the worse degrees are the lower, and increasing when they are the higher."

It would seem that at present, as in 1893 when the *Grundriss der Psychologie* appeared, the above is a mere assumption. At least, one finds no record of experimental investigation in the matter. But in the spring of 1909, at the suggestion of Professor Külpe, and with his help and advice, an attempt was made to conduct such an experimental inquiry. The work was carried out between February and July, 1909, in the Psychological Institute of the University of Würzburg.

The experiments to be described below afford material for an examination of both the suggested explanations, viz. the "relationship-explanation" and the "fusion-explanation" which is based upon the assumption which we have just considered.

It will be convenient to consider the second theory first. The major chord, c–e–g, is built up by combining the intervals c–e (the major third, having the frequency-ratio 4 : 5) and e–g (the minor third, with the frequency-ratio 5 : 6). Keeping the terminal tones, c and g, constant, and inverting the positions in the chord of the major third and minor third, we obtain the minor chord c–$e^{\flat}$–g. If, in the observer's opinion, the major third possesses a higher degree of fusion than the minor third, then, as stated before, if Külpe's assumption is correct, the chord c–e–g will be judged as exhibiting a higher degree of fusion than the chord c–$e^{\flat}$–g. For a similar reason the chord c–g–c' will be more highly fused than c–f–c', which has been made by inverting the position of the fifth and fourth in c–g–c'. For the fusion of the fifth is greater than that of the fourth, and the fifth occupies the lower position in the chord c–g–c'.

However, the *general* truth of the assumption can be tested experimentally. Chords may be selected for judgment which are composed of (*a*) "musical" intervals, possessing very high and low degrees of fusion, and (*b*) "unmusical" intervals, possessing practically no fusion, *i.e.* chords which, unlike the above examples, are never or

[1] *Op. cit.* 298.

seldom met with in ordinary music. By taking a musical or unmusical chord of three tones, and (with the terminal tones constant) inverting the position of the intervals within the chord, we form a pair of chords, in which the relative grades of total fusion can be then determined. Knowing the relative fusion-grades of the upper and lower intervals in the chord, we can examine the influence of the relative position of the constituent intervals upon the total fusion of the chord which is formed by them.

It is evident that the results of such experiments will serve as criteria of the validity of the first suggested explanation (p. 56), based upon clang-relationship. This "relationship-explanation" may be further tested in the light of results obtained from "major" and "minor" chords, which, although conforming to the rules laid down on p. 59 for the construction of "musical" major and minor chords, do not appear in ordinary music. The difference in the degree of their total fusion will therefore not be so obvious or well known. It can then be seen whether the chords which possess a higher degree of total fusion are those the clangs of which possess greater indirect (or direct) relationship.

At the same time, we may also apply our results to (3) the examination of a law stated by Meyer[1]. (It must be added that he expresses it "provisionally, with all reserve," intending "to go more deeply into the question when a greater amount of material for observation permits.") It is to the following effect: "The fusion of a chord of three clangs (*Dreiklang*) is the higher, the simpler the ratios of its frequencies, whether the chord be considered as a whole or the tones be taken in pairs."

We return now to the examination of the alleged influence exercised upon the total fusion of a chord by the position of its more highly- (or less highly-) fused interval.

Although there is a certain amount of agreement in the existing literature concerning the scale of fusion-grades in intervals[2], fairly wide variations may exist[3]. It was therefore decided to establish for each observer such a scale. The intervals employed in making up the

[1] M. Meyer, *Ztschr. f. Psychol.* 1898, xvii. 421.

[2] C. Stumpf, *Tonpsychologie*, Bd. ii. 135, 142 ff., *Ztschr. f. Psychol.* 1901, xxvii. O. Külpe, *Outlines*, 286. A. Faist, *Ztschr. f. Psychol.* 1897, xv. 102. A. Meinong and S. Witasek, *ibid.* 289. M. Meyer, *ibid.* 1898, xvii. 401. R. Schulze, *Phil. Stud.* 1898, xiv. E. Buch, *ibid.* 1900, xv. 1. 183. E. B. Titchener, *Experimental Psychology*, Instructor's Manual, Qualitative, 1901, 329–337.

[3] See M. Meyer, *op. cit.* 411, etc.

chords were accordingly arranged in the order of their degree of fusion as judged by the observer, and this individual (or "private") scale of degrees was afterwards used in working up the results obtained by the experiments upon the fusion of chords.

Throughout the experiments, the method employed was that of "paired comparison[1]." The intervals, or chords, being presented successively in pairs, the observer was required (1) to judge directly upon the immediate impression of fusion, (2) to say whether the second stimulus possessed greater or less fusion than its predecessor. Judgments of "equal" and "doubtful" were also allowed and recorded.

Both musical and unmusical observers were selected; all, with the exception of Professor Külpe himself, being advanced students of psychology, conducting some research under his direction[2]. The tests of their musical ability will be described in a separate paper[3].

It was necessary to make perfectly clear to the participators in these experiments what was meant by "the immediate impression of fusion." To this end, preliminary experiments were undertaken. The instruments used in all the experiments were Appunn's Tonmessers[4] (tonometers) in the form made by Max Kohl of Chemnitz, giving a difference of two vibrations between each note. Within the octave c'–c'', the different criteria upon which a judgment might possibly be formed—*e.g.* (*a*) fusion itself, (*b*) analysability, (*c*) pleasantness and unpleasantness, (*d*) associations, were illustrated by taking pairs of intervals and of chords in which the members of each pair possessed one of these different criteria in obviously different degrees. The observers

[1] E. B. Titchener, *op. cit.* 331, 332.

[2] They were—Miss Helen D. Cook, and Herren Bernhard Köhler, Friedrich Hacker, Gustav Walle and Ernst Westphal. To them the writer offers his warmest thanks for their kind services. To Professor Külpe he is especially grateful for his generous and unfailing help and advice throughout the work.

[3] See this *Journal*, 1911, IV. 89–94.

[4] An illustration and a general description of this instrument are given in Wundt's *Physiologische Psychologie*, 6te Aufl. 1910, II. 91, 92. The Tonmesser, as is well known, produces clangs which are decidedly rich in overtones. For a discussion of the justification for the employment, in fusion experiments, of any but *pure* tones, see the articles by Stumpf and Meyer in *Ztschr. f. Psychol.* 1898, XVII. 401–435, 1898, XVIII. 274–302. The argument which weighs most with the writer is that of Stumpf (*ibid.* XVII. 424), that "individuals who have acquired the little practice in tone-discrimination which they possess, exclusively by means of *clangs*, should, in the fusion-experiments, judge under conditions the same as, or similar to, those under which their discrimination has developed. They will then not be disturbed by the quite unusual soft timbre of the simple tones."

were then asked to judge upon the degrees of fusion possessed by the members of a set of pairs of stimuli presented to them, reporting, after each judgment, the basis upon which the decision was given. It was observed that very soon a considerable decrease occurred in the number of judgments founded upon bases other than that of fusion, and this procedure was continued until the observer reported that he was judging constantly upon the immediate impression of fusion. Prof. Külpe also questioned all the subjects, and satisfied himself that there was a general understanding of the meaning of fusion at the beginning of the preliminary experiments.

Since it will certainly be asked "What was the 'general understanding of the meaning of fusion' arrived at in this manner?" I requested Professor Külpe to write down the main points of his instruction to the several observers. They are as follow. "In this method, the question is not one of giving the number of components, but of a direct comparison of the *Komplexqualitäten* (Krueger's very suitable expression). The comparison of these qualities can be performed, and the judgments 'greater, less, equal,' can be made, just as easily as the comparison of elementary qualities and intensities. This quality cannot be defined, it must be simply experienced. It shows similarity with a rhythmical and spatial *Gestaltqualität.* The observer easily comprehends what is meant, when one calls his attention, on the spot, to the impressions themselves, and he judges with great certainty upon them. In consonance there is something other than this, viz. the *sinnliche Wohlklang*, which is graded, like the fusion-degrees. Between fusion and consonance a distinction must be drawn[1]." It may thus be confidently asserted that the observers began the experiments with an acquaintance with the phenomena of fusion which was derived at first-hand, and was not the outcome of theoretical deduction.

[1] Agreement between different authors concerning the relation between fusion and consonance has not, however, yet been reached. See I. M. Bentley, "A Critique of Fusion," *Amer. Journ. of Psychol.*, Commemorative Number, 1903, 60–72 (containing numerous references). C. Stumpf, *Tonpsychologie*, II. 176, 333; *Ztschr. f. Psychol.* XV. 121, LV. 1–142. *Beitr. z. Akustik. u. Musikwissenschaft*, Heft I. 1898, 34 ff., 1910. W. Wundt, *Grundzüge*, 5 Aufl. II. 111 ff., 421 ff.; *Physiol. Psychologie*, II. 6 Aufl. 123–7 and 434–461. O. Külpe, *op. cit.* 280 ff. F. Krueger, *Psychol. Stud.* 1906, I. 313 ff., II. 206 ff., IV. 201 ff., 1910, V. 294–411; *Arch. f. d. ges. Psychol.* 1903, I. 205 ff., II. 1 ff. Th. Lipps, *Grundtatsachen des Seelenlebens*, 1883, 238 ff.; *Psychol. Stud.* 1885, 92 ff.; *Ztschr. f. Psychol.* 1901, XXVII. 225 ff. F. Jodl, *Lehrbuch d. Psychologie*, 2 Aufl. I. 362. Natorp, *Göttingische Gelehrte Anzeigen*, 1891, 789. R. M. Ogden, *Psychological Bulletin*, 1909, VI. 297–302, 1911, VIII. 93–95, 100 and references on p. 60.

II. PRELIMINARY EXPERIMENTS WITH INTERVALS.

The first series of experiments afforded the practice necessary for the later experiments with chords. In the first experiments, eleven intervals were given on the Tonmesser, and their fusion-grade was determined for each observer. If we assume, for the moment, that the notes given by the Tonmesser are accurately represented by the frequencies marked on the instrument[1], the intervals and their frequencies (theoretically calculated and actually given) are as follow:

Interval	Frequency-ratio	Theoretical frequencies	Nearest frequencies on Tonmesser
Octave	1 : 2	256—512	256—512
Major Seventh	8 : 15	270—506·3	270—506
Natural (Subminor) Seventh[2]	4 : 7	286—500·5	286—500
Major Sixth	3 : 5	296—493·3	296—494
Minor Sixth	5 : 8	266—425·6	266—426
Fifth	2 : 3	280—420	280—420
Fourth	3 : 4	310—413·3	310—414
Major Third	4 : 5	350—437·5	350—438
Minor Third	5 : 6	400—480	400—480
Major Second	8 : 9	270—303·8	270—304
Minor Second	15 : 16	468·7—500	468—500

The accuracy of the Tonmessers. If the Tonmesser tongues be assumed to vibrate with the exact frequencies indicated on the scale above them, it is clear that, since there is a frequency-difference of two vibrations per second between each tongue, the production of any desired note, within fairly narrow limits of accuracy, will be possible. The maximum possible error would, of course, in this case be one vibration per second.

But if the tongues are mistuned to any considerable extent, the chances of error are somewhat great: for, to take an example, if the tongue marked 200 has really a frequency of 202, and if it is chosen, instead of the tongue 198 to represent the theoretically calculated tone 199 (since there is no reason against this choice), the considerable error of three vibrations will be made.

[1] Cf. Külpe, *op. cit.* 286, "The degree of a fusion is not noticeably changed by slight deviations of the component vibration-rates from their strict proportion." For effect of mistuning upon degree of fusion see also Stumpf, *Tonpsychologie*, II. 137, *Ztschr. f. Psychol.* XV. 288; Faist, *Ztschr. f. Psychol.* XV. 129. A note on the accuracy of the instruments is given above.

[2] It was intended to employ here the Acute Minor Seventh (5 : 9) but owing to an error the frequency-ratio 4 : 7 was used instead.

The accuracy of any tongue, compared with an *adjacent* tongue taken as standard, can, of course, be easily tested. It is merely necessary to count the number of beats produced by the tongues vibrating simultaneously. This was done in the present case. The time of 20 beats between every adjacent pair of tongues (mean of five observations) was taken with a stop-watch, and no great deviations from the anticipated time were found[1].

Experimental details. It was found convenient to place the observer in a dark room, and the Tonmesser in a small chamber separated from this room by a thin wooden partition. The wood was so thin that its presence made very little difference to the ease with which conversation could be carried on by two persons, one in each room. During the experiments, an opening, about two-thirds of a metre square, covered only by a thin screen of paper (required by another worker who used the room) allowed free access for the sounds from the Tonmesser to the observer. By the use of the dark room many sources of objective distraction were avoided. The room was always fairly quiet, the only distracting sounds being, usually, the chiming of the clock of the University Church, the times of which could, of course, be anticipated, and the sounds from the quadrangle, one floor below the room.

The Tonmesser was distant about two metres from the observer's head. It was found that the wind-supply from the bellows allowed the tones to begin simultaneously. This is also shown by the fact that, although the observers were asked to report any cases in which one note began perceptibly before the other, only one such report was received in the 968 experiments. By practice, the experimenter found a height to which the bellows could be raised in order to regulate the time during which the instrument sounded to two seconds, and only twice in 968 times was it reported that the notes did not end simultaneously.

From one to two seconds before the presentation of the first interval of any pair, the observer was given a warning "Now!" The interval was then presented for two seconds. After a pause, usually of about four seconds (the exact time depending upon the facility with which the stops could be adjusted, but hardly ever exceeding eight seconds), the second interval was presented for two seconds. The report was then given concerning the degree of fusion of the second interval, as compared with that of the first. The answers "greater," "less," "equal"

[1] The accuracy of the tongues marked 256, 384 and 512 (and, in the later experiments, of those marked 128 and 192) was established by comparison with standard tuning-forks.

and "doubtful" were allowed. In the case of the "equal" answers the distinction was made between a positive experience of equality and an experience of inability to find a difference[1].

The observers were instructed to introspect carefully at the beginning in order to make quite sure that their judgments were based upon the immediate expression of fusion given by the interval. Judgments given upon any other basis were at once to be reported, with the criteria used (presence of beats, pleasantness and unpleasantness, associations, etc.). It was emphasized that, introspection not being the main point of the experiment, it should, when its main object (the assurance of the use of the right criterion) had been attained, appear as little as possible.

Each interval was presented in turn with each of the other intervals, and with itself, *i.e.* some "pairs" were composed of two identical intervals. The order of presentation of the intervals in any pair and the order of presentation of the pairs were decided by lots. Then, in a similar series, the order of presentation of the intervals in the pairs was reversed. This gave 121 presentations. Finally, the series was again worked through in the reverse order (121 to 1) to eliminate, as far as possible, the effects caused by freshness, practice, fatigue, boredom, etc.

Computation of results. In the application of the method of paired comparisons to aesthetics, the number of "preferences" is reckoned. In the present application of the procedure, the word "votes" will perhaps avoid the risk, which might occur, of considering these judgments to be based on aesthetical grounds.

The results can thus be reckoned in the following way. The answer "greater" or "more fusion" (recorded as +) counts as a vote to the second of any pair; the answer "less" (−) counts as a vote against the second, or, more conveniently expressed, for the first. "Equal" and "doubtful" judgments are halved between the pair.

Eliminating those cases in which a pair is made up by presenting the same interval twice, we can tabulate the number of votes given for each interval. By arranging the intervals in the order of the number of their votes, we obtain an indication of the grades of fusion of the intervals as judged by the different observers.

It will be convenient to denote the intervals by numbers, *e.g.* the octave as 8. Distinction between major and minor intervals can be shown by italicising the figure in the case of the major interval, *e.g.* *3*. The interval 4 : 7, the subminor seventh, may be written s. 7.

[1] Occasionally an equivocal judgment, *e.g.* "greater or equal," was given. In this case the observer was asked to give a *final* decision in favour of one or the other.

The following tables of fusion-grade were obtained for the five observers, who may be conveniently designated by the first two letters of their names: Wa, Co, Kö, We and Ha.

Wa			Co			Kö			We			Ha		
Order	Interval	Votes	Order	Interval	Votes	Order	Interval	Votes	Order	Interval	Votes	Order	Interval	Votes
1	8	40	1	8	40	1	8	35	1	8	39·5	1	8	40
2	5	36	2	*3*	33	2	6	28·5	2	5	32	2	5	31·5
3	4	32	3	4	28	3	*3*	28	3	4	24·5	3	6	26·5
4	*3*	25·5	4	*6*	27	4	4	25	4	6	24	4	*6* } æq.	25
5	6	21·5	5	5	26	5	*2*	19·5	5	*6* } æq.	23·5		*3* }	25
6	*6* } æq.	20·5	6	3	21	6	*6*	18·5		*3* }	23·5	6	4	23·5
	3 }	20·5	7	6	20·5	7	s. 7	17	7	s. 7	18·5	7	3	19·5
8	*2*	12·0	8	*2*	10·5	8	*7*	15·5	8	3	13·5	8	s. 7	12·5
9	s. 7	8·0	9	s. 7	8·5	9	5 } æq.	13·5	9	*7*	13	9	*2*	9·5
10	*7*	2·5	10	2	4·0		3 }	13·5	10	*2*	7	10	*7* } æq.	3·5
11	2	1·5	11	*7*	1·5	11	2	6·0	11	2	1		2 }	3·5
		220			220			220			220			220

Consideration of the individual serial-orders of fusion-grade. (1) *Results given by Wa.* The serial-order here is practically in complete agreement with the generally accepted order of the grades of fusion estimated by another method—that of *analysis* of the intervals.

Wa occasionally recognised the intervals. (Each interval was given 44 times.) The octave was recognised twice, the fifth once, the fourth three times, and the major third twice. Three times the minor sixth was "recognised," but as the major sixth. Thus in 484 presentations there were only eleven explicit recognitions, three of which were incorrect. It must be noted, however, that the attention was directed towards the *fusion*, and away from any other features of the experience.

The answers were always given almost immediately (approximately one second) after the second interval had ceased. Wa was asked, like the others, to report any cases of judgment made upon any basis other than that of the immediate impression of fusion, but no such cases appear in the records. At the end of the series he reported that he had always judged in the manner specified.

(2) *Results given by Co.* The answers of Co contain more introspective evidence than those of Wa. At the commencement of the work every judgment given by this observer was supported or supplemented by detailed introspection, but, as the series progressed, the answers became briefer. Co declared that, especially at the beginning of the

experiments, the feeling-tone connected with some of the intervals made the judgment of fusion difficult, but only in a few cases impossible[1].

The main difference between the series given by Co and that given by Wa is that in Co's series the fifth and the major third have practically interchanged the places ascribed to them by Wa.

Regarding the position of the major third, it may be noted that, in six cases in which Co judged the degree of fusion of *3* to be greater than that of the other member of the pair of intervals, she seemed to think it necessary to give an "explanation" of her decision. She did this also in one instance in which she judged *3* to be less fused than 3. Yet these seven introspective reports, when examined in detail, are found to refer to different aspects of the intervals in different answers (tendency to analyse, effect of the unpleasantness of the partner-interval, attempt to separate fusion from consonance, and visual imagery). From this it seems probable that, in the case of *3*, criteria other than that of fusion were used.

Remarks occur which may throw some light upon the factors that give the fifth its relatively low position. On one occasion Co reported the presence of a difference-tone in it. This was also noticed twice by another observer, and yet another heard (very late in the series) beats in this interval. The writer, on testing the interval, could not detect a difference-tone, but on one occasion he noticed beats. As a rule, however, neither difference-tone nor beats were observed. The difference-tone may have contributed towards the diminution of the fusion of the fifth for Co.

Co's judgments were usually given immediately after the conclusion of the second interval.

(3) *We's results.* This observer offered a great deal of introspective evidence to supplement his decisions, and although, like Co, he made fewer introspective remarks as time went on, the amount of introspection is, on the whole, greater than hers. In particular he showed a tendency to *analyse* the intervals, though he reports that only in one case did he employ analysis as a basis for judgment.

[1] It is impracticable, in this account of the investigation, to give more than a few points taken from the introspective reports. These reports, however, were classified under three main heads—I., judgments given upon criteria other than that of fusion; II., judgments accompanied by (unrealised) tendencies to judge upon other criteria; III., cases in which the presence of disturbing or unusual factors was simply noticed, but did not affect judgment. Each introspective report was carefully examined before classification under these main, and other subsidiary, heads, and this procedure threw much light upon the differences between the individual fusion-tables.

Perhaps the most unusual feature in We's serial-order is the low position of the minor third. On one occasion he mentioned its "roughness," but no other clue towards an explanation seems to be forthcoming. It may be interesting to note that the interval above it in the series is the subminor or natural seventh. For there is the possibility of a "seven-group," to which, with the approximate tritone (5:7), this interval belongs, lying between the "imperfect consonances" and the "dissonances[1]," so that it may not be surprising to find its fusion rated as higher than that of the major seventh and the major and minor seconds.

(4) *Ha's results.* The judgments in this case were offered with very little introspective supplementation, and were always given immediately after the end of the second interval.

The position of the fourth appears low in comparison with the usual position which it occupies on the fusion-scale. But it may be noted that three more votes would have raised it to the third position on the scale; 6, *6*, *3* and 4 being separated by very small steps.

The above results, therefore, may be taken as showing a great similarity with the usually accepted fusion-scale of Stumpf and of others[2]. It seems that they may be adduced as evidence to support the use of the method of direct observation in the establishment of fusion-grades. It remains, however, to consider the remaining table before we can inquire if our results are in harmony with each other.

(5) *Kö's results.* Much introspection accompanied these answers. The judgment was often the outcome of consideration, and was not forthcoming directly after the stimuli had ceased. Indeed, Kö, half-way through the series, said, "In order to make a good judgment, I must observe the first interval carefully, and then compare the second interval with it. But this is not an *immediate impression* in the sense in which I have an immediate impression of red or yellow."

The most striking feature of these results is the extremely low position of the fifth. Examining the introspective report, we find that three times Kö declares that this interval possesses *no* fusion. Moreover, at different times the same remark is made concerning the fourth (three times), the minor sixth[3] (four times), the major sixth, and the minor third (twice). But on other days, he reports that the fusion (of the fourth) is "*very high*"; again that the fifth was "a *fairly high* fusion," and yet again "the *contrary* of a fusion."

[1] Titchener, *op. cit.* 333, 334.

[2] *Ibid.* 332 ff. and references on p. 60 of this article.

[3] Its high position (second) in Kö's scale of fusion-degrees should be noted.

These reports suggest strongly that Kö was not employing a fixed criterion for his answers. That the feeling-tone of the intervals had some influence upon his judgment may be surmised from the fact that he speaks of "the beauty, roundness, softness and fullness of the fusion" (of the minor sixth) and of "the emptiness and thinness of the fusion" (of the octave).

But the important point for these experiments is that a single criterion was obviously not adhered to; otherwise the above contradictory introspective reports would not have been given. The serial-order resulting from Kö's judgments therefore needs no further explanation, and does not affect adversely the results indicated by the general agreement of the other tables. It may be surmised that not only do Kö's concepts of fusion differ from those of the other observers, but that they differ from day to day, and therefore render him unsuitable as an observer in these experiments.

It is perhaps interesting to prepare a fusion-table from the combined results of the four other observers. The serial-order then becomes 8, 5, 4, *3*, *6*, 6, 3, 7, *2*, *7*, 2.

The consistency of the results. It will be useful to gain some idea of the "consistency" with which the observers gave their answers. For, although the final order of the fusion-grades may agree in two cases yet the series may in one case be made up of consistent, in the other of haphazard answers. If, for example, an interval a is judged as more highly fused than b, and b as more highly fused than c, then (if the answers are consistent), we shall expect a to be reported as more highly fused than c. Obviously this may not be the case if the difference between the votes for a, b, and c respectively is very small. By securing a definite "coefficient of consistency" from the results of each observer we can obtain valuable evidence concerning the trustworthiness of his answers.

A numerical indication of the consistency of each observer's answers was obtained in the following manner. Each interval was compared with each other interval four times. The results can be conveniently expressed by writing down the several results of the comparison of the first interval, 8, with *7*, 7, *6*, 6, etc., of the second interval, *7*, with 7, *6*, 6, 5, etc., thus obtaining 55 comparisons, each of which has been performed four times. The ordinary mathematical and other signs can be used for the sake of brevity. + means "more fused," – "less fused," = "equally fused," and ? "a doubtful judgment." An extract from one of the tables (illustrating a particularly inconsistent set of judgments), will make this clear.

I. Judgment about	II compared with	I is
5	4	– – ?
	3	+ ? – ?
	3	+ + = ?
	2	– – – –
	2	+ = ? –

We must now compare these fluctuating judgments with the judgments which we should *expect* on the assumption that

(*a*) the serial-order of fusion-grades established for the individual observer is taken as the criterion for his answers;

(*b*) the answers are absolutely consistent, *i.e.* the four mathematical (or other) signs in the same line are similar.

The serial-order established for the observer whose answers are given above, as illustration, is

$$8 > 6 > \mathit{3} > 4 > \mathit{2} > \mathit{6} > 7 > \mathit{7} > \left.\begin{matrix}5\\3\end{matrix}\right\} \text{æq} > 2.$$

Consequently, if we construct a second table, in which column T represents the relative fusion-grades taken from the table of serial-order, and column A gives the four answers, we obtain for the above results—

T	Judgment about	Compared with	A	Error	$\frac{\text{Total actual error}}{\text{Max. total error possible}}$	Total percentage error
(1) $5<4$	5	4	– – ? –	·125		
(2) $5<\mathit{3}$	5	*3*	+ ? – ?	·500		
(3) $5=3$	5	3	+ + = ?	·250	$\frac{1·375}{5}$	27·5
(4) $5<\mathit{2}$	5	*2*	– – – –	0		
(5) $5>2$	5	2	+ = ? –	·500		

(Assuming 'equal' or 'doubtful' to be counted as half-errors.)

Explanation of above Table.

In case (1), if $5 < 4$, and a perfectly consistent judgment is given, the answers should be – – – – instead of – – ? –. Similarly, (2) should be – – – – instead of + ? – ?.

We may reckon the discrepancies numerically in the following way.

The "worst answer possible" to (1) is obviously + + + +, as the best is – – – –. We may then call the maximum error for any four answers, which relate to the same pair of intervals, 1, each answer then having as its maximum error 0·25. To commit this error, the answer "greater" (+) must be returned instead of the correct "less" (–), or *vice versâ*. An answer of "equal" (=) or "doubtful" (?) in such a case may be reckoned as half the maximum error, *i.e.* for each answer 0·125. If, as in (3), the correct answer be = = = =, a + or – will count as

0·125, and "doubtful" answers (since we have no other alternative) must count as correct.

The column "Error" in our table can thus be filled.

In this column the maximum total error possible is here 5, the total actual error 1·375, which gives the total *percentage* error 27·5. This we may call the "*consistency error.*" In the actual results, reckoning (as above) every interval with every other interval (*not*, of course, this time with itself), the total maximum possible error is 55. The "consistency errors" for the various individual observers are Wa 3·64, Co 7·95, Ha 14·77, We 15·23, Kö 27·50. The greater reliability of the judgments of Wa and Co, and the unreliability of Kö's judgments, are well shown by these figures.

III. Experiments with Chords.

Arrangement of apparatus. The addition of a third Tonmesser (similar in detail to the others, but giving the octave 128—256, with two vibrations difference between each note) produced difficulties. It was blown from the bellows of the two Tonmessers which had been used in the preliminary experiments, by means of a rubber tube, which connected a side-tube in the bellows-stand with a similar tube at the bottom of the third instrument. It was then found impossible simultaneously to actuate reeds situated in the new instrument and in those mounted directly over the bellows; hence any chord given was liable to begin and end in a "ragged" manner.

To secure absolute simultaneity of the beginning and ending of the three notes of the chord, the Tonmessers were placed in the lecture-room of the Institute, about seven metres from the position of the observer in the dark-room. Between the lecture-room and dark-room was a corridor, and the walls of both rooms, which separated them from the corridor, were about 75 cm. thick. With all doors closed, and the opening of one door padded with felt, no sound from the instruments could be heard in the dark-room, when a chord was played in the lecture-room.

Through both walls and across the corridor a leaden tube, 2 cm. in cross-section, was fixed. It projected into both rooms. The tube was thoroughly insulated from the walls by wadding, and the parts which projected into the lecture-room and corridor were wrapped round completely with wadding 1·5 cm. thick. Both ends of the tube were curved so that the tube ended in a vertical direction, above the

Tonmessers in the lecture-room, and above the observer's chair in the dark-room.

At the end of each tube was fixed a tin gramophone "horn," 46 cm. high, and measuring 40 cm. across its mouth. Close to the end of the horizontal tube in the lecture-room nearest the Tonmessers, a sliding tap was soldered into the tube. It was composed simply of three rectangular plates of steel, 10·5 cm. long, 5·5 cm. broad, and 0·2 cm. thick. The two outer plates were riveted together, and the middle one (the "slide"), was allowed to move backwards and forwards between them. A slit, 0·35 cm. wide, was cut in the middle of the upper part of the slide. In the centre of each of the outer plates was bored a hole of the size of the conduction-tube, and short pieces of the tube itself were soldered over these holes. When the slide was fully pushed into the space between the outer plates, it blocked the passage between the holes in them, but when it was pulled partly outwards, the slit in it allowed access between the holes. A pin, fixed through the two outer plates and the slit in the slide, prevented the slide from being completely drawn out. Spiral springs, attached to the end of the slide and to one of the outer plates, quickly pulled it back into its original position when it was released.

The tap was soldered in a vertical position into the conducting tube, and could be actuated by pulling downwards upon a string attached to the slide and hanging over the Tonmessers.

After a little adjustment and careful lubrication the tap could be opened and closed without any noise being appreciable to an observer seated underneath the horn in the dark-room. The new Tonmesser, giving the range of tones represented by the frequencies 128 to 256, was placed at right angles to the third one (384—512), with the second (256—384) vertically above the third. By this arrangement a line drawn from the middle of any of the instruments to the centre of the bottom and largest rim of the horn would be about 1½ metres long; in other words, the instruments were practically equidistant from the end of the conducting tube.

It was easy for the experimenter and the observer to converse with each other by simply speaking while standing underneath the horns in the two rooms (the tap, of course, being open). Electric bells were placed in both rooms, and connected with portable "pushes" which could be held in the hand. The bell in the dark-room was "damped" by wadding, and gave, when the "push" in the lecture-room was pressed, only one dull sound which died away at once. The bell in the lecture-room was loud, and rang in the ordinary way, so that any signal from the observer could be heard if necessary above the sounds of the chords.

The necessity of further experiments with intervals. The change of experimental conditions necessitated by the addition of a third Tonmesser demanded the *repetition* of the experiments upon the fusion-grade of intervals, the observer judging upon them as they were heard through the tube. Unfortunately, owing to lack of time, comparatively few experiments could be carried out in this way, but these results are of course used as the *final criteria* in working up the judgments given upon the fusion of chords in the later experiments. The results obtained in the first experiments are reported later for the sake of comparison. Yet the final decision obviously rests upon the results obtained under the same experimental conditions as those which prevailed during the experiments upon chords. Some advantages of these later experiments were that

(1) the actual intervals used in the chords were given; the frequencies of their constituent notes being the same as those in the chords;

(2) the need of further intervals was foreseen, and the following were added: tenth (2 : 5), tritone (32 : 45), eleventh (3 : 8), major ninth (4 : 9), thirteenth (3 : 10)[1], super-second (7 : 8), and the "unmusical" intervals, 16 : 25 (grave superfluous fifth) 18 : 25 (superfluous fourth) and 45 : 128 (diminished twelfth).

Experimental procedure. The main part of the investigation was now begun, viz. the presentation of pairs of chords which had been made up by taking a chord of three notes, and, keeping the terminal notes constant, reversing the position of the intervals in the chord. To the intervals above-mentioned, those made by some of the terminal tones, the twelfth (1 : 3) and the double octave (1 : 4), were added, but it was of course unnecessary to include them in the fusion-experiments with intervals.

Some experimentation was necessary in order to find a convenient number of pairs of chords in which beats or difference-tones were not so powerful as to disturb the judgment concerning the fusion of the chords. The chords which were proposed for use in these experiments were submitted to the examination and criticism of Dr Anschütz of the Würzburg laboratory, an observer musically gifted in a high degree,

[1] For the sake of brevity, these intervals will be written in the tables of results as 10, *T*, 11, *9*, 13 respectively.

and trained in observation. To him the writer wishes here to offer his best thanks for his kind assistance. The chords, as they came through the conduction tube to the dark-room, were observed under the same conditions as those of the actual experiments, and any which were reported as containing noticeable beats or difference-tones were eliminated from the table of stimuli.

No.	Lowest tone	Interval between lowest and middle tones	Middle tone	Interval between middle and highest tones	Highest tone	Interval between terminal tones
1	280	5 : 6, Minor 3rd 18 : 25, Superfluous 4th	*a.* 336 *b.* 388·9	18 : 25, Superfluous 4th 5 : 6, Minor 3rd	466·7	3 : 5, Major 6th
2	240	4 : 5, Major 3rd 3 : 4, Fourth	*a.* 300 *b.* 320	3 : 4, Fourth 4 : 5, Major 3rd	400	" "
3	313·3	3 : 4, Fourth 8 : 9, Major 2nd	*a.* 417·7 *b.* 352·5	8 : 9, Major 2nd 3 : 4, Fourth	470	2 : 3, Fifth
4	300	4 : 5, Major 3rd 5 : 6, Minor 3rd	*a.* 375 *b.* 360	5 : 6, Minor 3rd 4 : 5, Major 3rd	450	" "
5	250	2 : 3, Fifth 3 : 4, Fourth	*a.* 375 *b.* 333·3	3 : 4, Fourth 2 : 3, Fifth	500	1 : 2, Octave
6	200	5 : 6, Minor 3rd 3 : 5, Major 6th	*a.* 240 *b.* 333·3	3 : 5, Major 6th 5 : 6, Minor 3rd	400	" "
7	254	4 : 7, Subminor 7th 7 : 8, Supersecond	*a.* 444·5 *b.* 290·3	7 : 8, Supersecond 4 : 7, Subminor 7th	508	" "
8	246	5 : 8, Minor 6th 4 : 5, Major 3rd	*a.* 393·6 *b.* 307·5	4 : 5, Major 3rd 5 : 8, Minor 6th	492	" "
9	160	5 : 8, Minor 6th 16 : 25, Grave super. 5th	*a.* 256 *b.* 250	16 : 25, Grave super. 5th 5 : 8, Minor 6th	400	2 : 5, Tenth
10	200	4 : 5, Major 3rd 1 : 2, Octave	*a.* 250 *b.* 400	1 : 2, Octave 4 : 5, Major 3rd	500	" "
11	130	2 : 3, Fifth 1 : 2, Octave	*a.* 195 *b.* 260	1 : 2, Octave 2 : 3, Fifth	390	1 : 3, Twelfth
12	162	4 : 9, Major 9th 3 : 4, Fourth	*a.* 364·5 *b.* 216	3 : 4, Fourth 4 : 9, Major 9th	486	" "
13	128	2 : 3, Fifth 3 : 8, Eleventh	*a.* 192 *b.* 341·3	3 : 8, Eleventh 2 : 3, Fifth	512	1 : 4, Double Octave
14	128	2 : 5, Tenth 5 : 8, Minor 6th	*a.* 320 *b.* 204·8	5 : 8, Minor 6th 2 : 5, Tenth	512	" " "
15	128	5 : 6, Minor 3rd 3 : 10, Thirteenth	*a.* 153·6 *b.* 426·6	3 : 10, Thirteenth 5 : 6, Minor 3rd	512	" " "
16	128	45 : 128, Diminished 12th 32 : 45, Tritone	*a.* 364·1 *b.* 180	32 : 45, Tritone 45 : 128, Diminished 12th	512	" " "

The final decision resulted in the formation of the following table which needs little explanation. Taking, for example, chord 11, the terminal tones 130 and 390 (twelfth) are chosen. A fifth (2 : 3) is then reckoned *upwards* from 130 (*i.e.* 195), which makes the remaining interval (195—390) an octave. Thus chord 11*a* is 130,—195,—390. Chord *b* is made by reckoning a fifth *downwards* from 390 (*i.e.* 260), which again makes the remaining interval (this time the lower) an octave (130—260). Chord 11 *b*, 130—260—390, thus presents the same intervals (octave, fifth) as 11 *a*, but in the reverse order.

The sixteen pairs of chords formed an experimental series. They were arranged in the first series in haphazard order, and the order of presentation was *a*—*b*, judgment being given upon the fusion of the second chord. (The same answers—"greater," "less," "equal," and "doubtful"—were required, as in the preliminary experiments, and were reckoned subsequently in the same way.) The second series was made by reversing the first, and the time-order was *b*—*a*. The third series had the same order as the second, and time-order *a*—*b*; the fourth the same order as the first and time-order *b*—*a*.

The first four series will illustrate the procedure.

1. Order of chords: 6, 10, 1, 7, 13, 2, 9, 15, 14, 12, 5, 16, 3, 4, 8, 11. Time-order *a*—*b*, Judgment about *b*.
2. ,, ,, 11, 8, 4, 3, 16, 5, 12, 14, 15, 9, 2, 13, 7, 1, 10, 6. Time-order *b*—*a*, Judgment about *a*.
3. ,, ,, Same as in 2. Time-order *a*—*b*, Judgment about *b*.
4. ,, ,, Same as in 1. Time-order *b*—*a*, Judgment about *a*.

Series 5 was then obtained by means of another haphazard order, with time-order and judgment about the second stimulus, as in Series 1. Series 6, 7, 8, were obtained from Series 5 in the same manner, as in the illustration.

Series 9 was obtained by yet another haphazard order, again with time-order and judgment about the second stimulus, as in Series 1 and 5. Series 10, 11, 12 were similarly obtained from Series 9. Finally the whole procedure of Series 1—12 was reversed, giving Series 13—24, *i.e.* Series 12 and 13 were identical, as were also Series 1 and 24.

The arrangements for the warning of the observer, the duration of the stimuli, and the length of time between the stimuli, were identical with those in the preliminary experiments, except that the damped bell, instead of the warning "Now!" served for a signal. The observer sat with his head directly under the centre of the horn.

The distance from the centre of the circle formed by the lowest rim of the horn to the top of the observer's head, was, on the average, 40 cm. The positions of the chair-legs were marked on the floor, so that the observer always took up the same position with regard to the horn, and the Tonmessers, while in use, were always placed in the positions already described. The observer sat in the dark-room, with the bell-push, connected with flexible wires, in his hand, so that he could at any time, by means of preconcerted signals, communicate with the experimenter if he wished a pair of intervals repeated, if the apparatus was not working properly, or if he wished to speak with the experimenter.

The preliminary experiments were finished early in March 1909. After a two-months' interval, the experiments with chords were commenced with Co, Ha, and We, and, after three months, with Ha. Professor Külpe (who will now be referred to as Kü) also very kindly acted as observer in these later experiments; it being considered unnecessary to give him the practice-series with intervals. In his case, on account of the short time available, twelve series, instead of twenty-four were worked through.

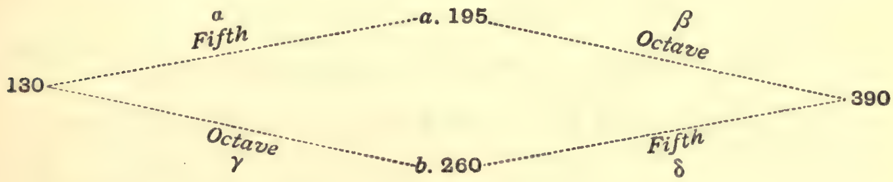
In spite of (or perhaps because of) the rest-interval, most of the observers showed a high degree of practice in the judgment of fusion, if their subjective certainty can be taken as a criterion. Only We found the new conditions (the conduction of the tones through the tube) difficult to work under, but in his case, too, the strangeness soon vanished. All observers were given a practice series (the 16 pairs being each presented once), the results of which are not reckoned in the calculation of results.

After the first few presentations, very little introspection was offered by any of the subjects. The chief point worthy of note is that Co noticed (six times in twenty-four) the presence of beats in 15 *a*, but only twice, she says, was there any possibility of this having disturbed or influenced her judgment. Even in these two cases it is added that the influence, if any, could have been only slight. Kü also noticed (twice in twelve times) the presence of beats in 15*a*, but they did not in this case affect judgment. No other important disturbing influences were reported by any of the observers.

Each pair of chords had 24 votes (except in the case of Kü, where each had 12). These were recorded in favour of either *a* or *b*, "equal" and "doubtful" answers being halved between *a* and *b*. Here again, a distinction was drawn in the introspective report between the positive experience of equality and the experience of "no difference found."

Comparison of the fusion of the intervals contained in the chords. It was now necessary to offer for comparison the *actual intervals* contained in the chords, so that, in attempting to answer the main problem, it would be possible to say which of the two intervals in the chord was judged to be the more highly fused.

But it must be remembered that although in chord *b* we have the same intervals as in chord *a*, yet they are formed by notes at a different pitch, and on this account it may be useful later to distinguish between an interval when it is at the top, and the same interval when it is at the bottom, of a chord. We can use the signs α, β, γ and δ for the four intervals, their use being illustrated by the example of chord 11.



Thus α and δ, or β and γ, are the same interval at different pitches.

It was therefore decided to treat the chords 1*a*, 1*b*, 2*a*, 2*b*, etc., as separate. The whole thirty-two chords, in haphazard order[1], formed an experimental series. Taking any chord, the two intervals composing it were presented successively to the observers, who judged upon their relative fusion in the way described in the account of the preceding experiments.

Owing to the short time available (it was now the end of the summer semester, and most of the observers were about to leave Würzburg), only four such complete series could be worked through. From this it follows that the maximum number of votes obtainable by any interval in any one chord (the votes obtained by the interval in both its upper and its lower positions in the chord being reckoned together) was 8. This comparatively small number of possible votes

[1] The "haphazard" order here, as everywhere else in this investigation, was a genuine random order determined by drawing numbered tickets from a vessel.

reveals, in several cases, a very slight difference in two intervals as regards their judged fusion-degree, and by no means excludes the possibility that a few more votes might have reversed their relative positions in this respect. In other words, the individual differences between the fusion of the intervals as judged by the observers, tend to be diminished by this procedure—an unfortunate fact, but in this case unavoidable.

In the first series (its order determined, as mentioned above, by chance) the order of presentation of the intervals was α—β in a, and δ—γ in b; the judgment being given about the fusion of the second interval, β or γ. In Series 2, both the order of the stimuli and the order of their presentation in pairs were reversed. Series 3 corresponded to 2, and 4 to 1. Thus each pair of chords had eight judgments passed upon the intervals in them, or 128 judgments in all.

IV. "Musical" and "Unmusical" Observers.

It is of interest that, in examining the results of these experiments, we should know something of the musical ability of the observers. An especial reason for determining which of them could be termed "musical" lies in the fact that, up to the present, experiments in which *direct* judgment has been passed upon fusion-degree appear to have been carried out exclusively upon *musical* subjects. This restriction seems unnecessary. The unmusical observer can, after a little practice, pass judgment with great certainty upon *differences* in fusion-degree, as we saw in the account of the preliminary experiments with intervals (pp. 61 ff.). It is perhaps scarcely necessary to point out that this procedure by "paired comparison" is infinitely easier for the observer than the direct arrangement of different intervals in a scale of fusion-degrees by mere casual examination of them, without such methodical comparison.

The tests to determine whether the observers could be called musical or unmusical will be described, with comments, in a separate paper in the present number of this *Journal.* It is convenient here to enumerate them with their results. They were (1) analysis of intervals, (2) difference-limen for pitch, (3) singing a given note, (4) the "consistency" criterion, mentioned on pp. 69 ff.. The results of these tests made possible the classification of the four observers (Professor Külpe's musical ability and training, being well known to us, were not tested

Observer Co.

Number of chord	*a*	*b*	Fusion-grade of intervals, from preliminary experiments	Fusion-grade of intervals, from later experiments	Judgments of greater fusion in *a*	Judgments of greater fusion in *b*	Votes for chord with more highly-fused interval *above* (criterion, preliminary experiments)	Votes for chord with more highly-fused interval *below* (criterion, preliminary experiments)	Votes for chord with more highly-fused interval *above* (criterion, later experiments)	Votes for chord with more highly-fused interval *below* (criterion, later experiments)	Votes for chord with interval possessing greater *frequency-ratio above*	Votes for chord with interval possessing greater *frequency-ratio below*	Votes for chord with interval possessing greater *frequency-difference above*	Votes for chord with interval possessing greater *frequency-difference below*	Votes for chord, the clangs of which are more closely related *directly*	Votes for chord, the clangs of which are more closely related *indirectly*
1	18 : 25/3	3/18 : 25	—	3>18 : 25	9·5	14·5	—	—	14·5	9·5	9·5	14·5	9·5	14·5	14·5	9·5
2	4/3	3/4	3>4	4>3	1	23	23	1	1	23	1	23	1	23	1	23
3	2/4	4/2	4>2	4>2	10·5	13·5	13·5	10·5	13·5	10·5	13·5	10·5	13·5	10·5	13·5	10·5
4	3/3	3/3	3>3	3=3	17	7	7	17	—	—	7	17	7	17	7	17
5	4/5	5/4	4>5	4>5	14·5	9·5	14·5	9·5	14·5	9·5	9·5	14·5	9·5	14·5	9·5	14·5
6	6/3	3/6	6>3	6>3	5	19	5	19	5	19	5	19	5	19	5	19
7	7 : 8/s. 7	s. 7/7 : 8	—	7 : 8>s. 7	12	12	—	—	12	12	12	12	12	12	12	12
8	3/6	6/3	3>6	3>6	17	7	17	7	17	7	7	17	7	17	17	7
9	16 : 25/6	6/16 : 25	—	6>16 : 25	12·5	11·5	—	—	11·5	12·5	11·5	12·5	—	—	11·5	12·5
10	8/3	3/8	8>3	8>3	8·5	15·5	8·5	15·5	8·5	15·5	8·5	15·5	8·5	15·5	8·5	15·5
11	8/5	5/8	8>5	8>5	1	23	1	23	1	23	1	23	1	23	1	23
12	4/9	9/4	—	4>9	17	7	—	—	17	7	7	17	7	17	17	7
13	11/5	5/11	—	5>11	17	7	—	—	7	17	17	7	17	7	7	17
14	6/10	10/6	—	10>6	21	3	—	—	3	21	3	21	3	21	3	21
15	13/3	3/13	—	3>13	1·5	22·5	—	—	22·5	1·5	1·5	22·5	1·5	22·5	1·5	22·5
16	T/45 : 128	45 : 128/T	—	T>45 : 128	18	6	—	—	18	6	6	18	6	18	18	6
Totals...							89·5	102·5	166	194	120	264	108·5	251·5	147	237

in this way) into lists in which the best performance appeared at the head, the worst at the foot. The orders in the various tests were

Order	"Analysis"	"Difference-limen"	"Singing"	"Consistency"
1	Wa	Wa } æq.	Wa	Wa
2	Ha	Ha } æq.	Ha } æq.	Co
3	Co	Co	Co } æq.	Ha
4	We	We	We	We

The relative positions of the observers in this table will be of interest when the individual results are examined.

V. Results.

(i) *The tables of results for each observer.* The table on p. 79 is a sample of the five tables made out for the separate observers, and requires little explanation. The abbreviations are those already mentioned on p. 65. The columns headed "Number of chord," and "a" and "b" show the number of the chord in the table on p 74, and give a graphical indication of its construction. For example, in Chord 1a the symbol 18 : 25/3 indicates that the interval with the frequency-ratio 18 : 25 was uppermost in the chord, while the minor third occupied the lower position. The "preliminary" fusion-experiments are those described on pp. 63—66, the "later" experiments those on pp. 77, 78.

It will be noticed that in this table not only fusion and clang-relationship, but also other criteria, viz. the frequency-ratio and frequency-difference of the intervals, have been taken into account. They will be discussed in the consideration of the tables of combined results.

(ii) *The general results.* The results of the five tables, one of which is shown as a sample on p. 79, may now be thrown into a form in which the effect of the different factors under consideration is very clearly seen.

Tables I and II show the influence exerted upon the total fusion of the chord by the position of the more highly-fused interval.

In Tables I and II the general tendency is to judge the chord as possessing a higher degree of total fusion when the more highly-fused interval occupies the lower position. This tendency is shown by all the observers. Two points here are worthy of notice. In Table I, the greatest value of $\frac{B_{\mathrm{I}}}{A_{\mathrm{I}}}$ is given by the two observers who are *most musical* (see p. 78, and following paper). In Table II the mean tendency of judgment $\left(\text{indicated by the mean value of } \frac{B_{II}}{A_{II}}\right)$ is greater than in Table I, and the ratio of the mean variation to the mean is much less. This is probably because the fusion-experiments which served as criteria for Table II were so much more numerous than those con-

TABLE I. (*criterion—the "later" fusion-experiments upon intervals,* pp. 77, 78).

Judgments of higher degree of fusion of chord.

Observer	A_I, with more highly-fused interval *above*	B_I, with more highly-fused interval *below*	$\frac{B_I}{A_I}$ (if $A_I=1$)
Co	166	194	1·17
We	124·5	139·5	1·12
Ha	129·5	158·5	1·22
Wa	86·5	249·5	2·88
Kü	73	95	1·30

Mean of $\frac{B_I}{A_I}$, 1·54. Mean variation 0·5.

TABLE II. (*criterion—the "preliminary" fusion-experiments upon intervals,* pp. 63—66).

Judgments of higher degree of fusion of chord.

Observer	A_{II}, with more highly-fused interval *above*	B_{II}, with more highly-fused interval *below*	$\frac{B_{II}}{A_{II}}$ (if $A_{II}=1$)
Co	89·5	102·5	1·14
We	53·5	138·5	2·59
Ha	77	115	1·49
Wa	59	109	1·85

Mean of $\frac{B_{II}}{A_{II}}$, 1·77. Mean variation 0·4.

nected with Table I. The smaller number of judgments in the "later" experiments will tend, as mentioned before (p. 77), in cases where the fusion-grade of intervals is very slightly different, to obscure this difference. This fact will be reflected in the smaller values of $\frac{B_I}{A_I}$.

In Tables III and IV the influence of the frequency-ratio and frequency-difference of the intervals is considered.

TABLE III.

Judgments of higher degree of fusion of chord.

Observer	A_{III}, with interval possessing *greater frequency-ratio above*	B_{III}, with interval possessing *greater frequency-ratio below*	$\frac{B_{III}}{A_{III}}$ (if $A_{III}=1$)
Co	120	264	2·20
We	102·5	281·5	2·75
Ha	113	271	2·40
Wa	162	222	1·37
Kü	32	160	5·00

Mean of $\frac{B_{III}}{A_{III}}$, 2·74. Mean variation 0·9.

The general tendency in Table III is unmistakable. It is noteworthy that the *greatest* and *least* values of $\frac{B_{III}}{A_{III}}$ are attributed to the two most musical observers.

TABLE IV.

Judgments of higher degree of fusion of chord.

Observer	A_{IV}, with interval possessing *greater frequency-difference above*	B_{IV}, with interval possessing *greater frequency-difference below*	$\frac{B_{IV}}{A_{IV}}$ (if $A_{IV}=1$)
Co	108·5	251·5	2·32
We	91·5	268·5	2·93
Ha	105	255	2·43
Wa	150·5	209·5	1·39
Kü	28	152	5·43

Mean of $\frac{B_{IV}}{A_{IV}}$, 2·9. Mean variation 1·02.

In comparison with the results of Table III, the fact appears that the value $\frac{B_{IV}}{A_{IV}}$ shows in every case a slight increase upon $\frac{B_{III}}{A_{III}}$. The results of Wa are here interesting, for, in the case of five of the sixteen chords, his tendency of judgment was to make A_{IV} greater than B_{IV}. In other words, in his case the final value of $\frac{B_{IV}}{A_{IV}}$ is the result of the action of two opposite tendencies, the lesser of which was of considerable strength. One might be struck by the fact that the fusion-grade of the intervals seemed to have more effect upon a musical observer, while their frequency-ratio and frequency-difference were more potent factors in the case of the relatively unmusical subjects, were it not for the fact that Kü, the other musical observer, shows the tendencies which are exhibited by the unmusical persons, but to a greater extent than they.

Table V considers the direct and indirect relationship of the clangs in the chords, and the connection of this relationship with the total fusion of the chord.

TABLE V.

Judgments of higher degree of fusion of chord.

Observer	A_V, the clangs of which are more closely related *directly*	B_V, the clangs of which are more closely related *indirectly*	$\frac{B_V}{A_V}$ (if $A_V = 1$)
Co	147	237	1·61
We	158·5	225·5	1·42
Ha	153	231	1·51
Wa	112	272	2·43
Kü	53	139	2·62

Mean of $\frac{B_V}{A_V}$, 1·92. Mean variation 0·48.

We see here two interesting facts. (1) All observers, without exception, judge the total fusion of the chord to be of a higher degree when its clangs are more closely related *indirectly*, and in every case the ratio $\frac{B_V}{A_V}$ is large, so that the tendency is unmistakable. Since the "simpler" chords of each pair (*i.e.* those the frequency-ratios of which, when the chord is taken as a whole, are simpler) are exactly those which have the greater *indirect* clang-relationship, these results may be taken as evidence in support of Meyer's statement concerning the fusion of a chord of three tones[1]. (2) The descending order of the individual values of $\frac{B_V}{A_V}$ is practically identical with the order taken up by the subjects in the tests for their classification as "musical" and "unmusical[2]." The greatest effect of clang-relationship is apparently exercised upon the two most musical, and the least effect upon the least musical observers.

[1] See p. 60 of this article. [2] See p. 80.

All exceptions to the above general tendencies of judgment were carefully studied in order to ascertain whether they were traceable to the peculiarities of construction of any particular chords. But except in the case cited on p. 82, little useful information was obtained in this way. Seldom did several observers agree in making any particular pair of chords an exception to the general tendency, and these chords, upon examination, showed no obvious quality which could account for the exception.

It will be useful, before considering the conclusions to be drawn from the results, to combine the mean results of Tables I—V into a single table, so that comparisons may be drawn between them. Table VI gives these combined results, viz. the various mean values of $\frac{B}{A}$, *i.e.* $\frac{B_{\mathrm{I}}}{A_{\mathrm{I}}}$ to $\frac{B_{\mathrm{V}}}{A_{\mathrm{V}}}$, with their mean variations, and other relations which will be examined later.

TABLE VI.

Short description of contents of table	Table	$\frac{B}{A}$	m.v. of $\frac{B}{A}$	$\frac{\text{m.v.}}{\frac{B}{A}}$	$\frac{B}{A}$ in order of magnitude	$\frac{\text{m.v.}}{\frac{B}{A}}$ in inverse order of magnitude
"Fusion" (criterion, later experiments) ...	I	1·54	0·5	0·32	5	3
"Fusion" (criterion, prelim. experiments) ...	II	1·77	0·4	0·23	4	1
"Frequency-ratio"	III	2·74	0·9	0·33	2	4
"Frequency-difference"	IV	2·9	1	0·34	1	5
"Relationship"	V	1·92	0·5	0·26	3	2

The column $\frac{\text{m.v.}}{\frac{B}{A}}$ and the last column are useful in that they give some idea of the relative deviation of the judgments of the individual observers from the means in the column $\frac{B}{A}$. Since a low value of $\frac{\text{m.v.}}{\frac{B}{A}}$ indicates greater unanimity amongst the observers, and this, combined with a relatively high value of $\frac{B}{A}$, indicates that the tendency which it expresses is emphatically pronounced, the last column has been written so that the least value obtains the first place, and *vice versa*.

On examining Tables I—IV we see that, whether the fusion, frequency-ratio, or frequency-difference of the intervals be examined, the general result is always that that chord, in which the interval possessing these attributes in the greater degree occupies the lower position, is judged as possessing a higher degree of total fusion than the chord in which this interval is placed in the higher position. We also find that in the final results from any *single* observer the value $\frac{B}{A}$ is always greater than unity. In other words, there is in the *averaged* results from any observer, no exception to the general tendency.

Examining the tables in detail, we find that:

(1) In the "fusion" Tables I and II the value $\frac{B}{A}$ is greater in II than in I. This arises from the fact (stated on p. 77) that the later fusion-experiments, which were conducted under the same conditions as those which prevailed during the experiments with chords (and therefore form, from that point of view, the more defensible criteria), were far too few. The preliminary experiments accentuated the differences between the fusion of the intervals. We may say that they "stretched out" the table of fusion-degrees. This accounts for the greater value of $\frac{B}{A}$, and the lesser value of $\frac{\text{m.v.}}{\frac{B}{A}}$ in II, as compared with their corresponding values in I.

Thus Tables I and II confirm the assumption on p. 59 of this article, viz. that in general a chord is judged to possess a higher degree of fusion when the more highly-fused interval is below than when it is above.

(2) The "frequency-ratio" Table III shows a much more decided general tendency. $\frac{B}{A}$ here is much greater than in I or II. Yet the value of $\frac{\text{m.v.}}{\frac{B}{A}}$ is practically the same as in I, indeed, the similarity of this value in I, III and IV is worthy of notice. This stronger general tendency of judgment was shown by all the observers with the exception of the highly musical Wa. His values for $\frac{B}{A}$ are

$$\frac{B_{\text{I}}}{A_{\text{I}}}\ 2{\cdot}9,\quad \frac{B_{\text{II}}}{A_{\text{II}}}\ 1{\cdot}8,\quad \frac{B_{\text{III}}}{A_{\text{III}}}\ 1{\cdot}4.$$

In other words, in his case "fusion" has a greater effect than "frequency-ratio." Yet the other highly musical observer, Kü, whose values for $\frac{B}{A}$ are

$$\frac{B_{\mathrm{I}}}{A_{\mathrm{I}}}\,1{\cdot}3,\ \frac{B_{\mathrm{III}}}{A_{\mathrm{III}}}\,5,$$

strikingly exemplifies an opposite type, and deprives the statement of any general validity.

(3) In Table IV, the "frequency-difference" table, $\frac{B}{A}$ reaches its highest value. Summarising the meaning of Tables III and IV we find that there is, on the average, a very distinct tendency to judge a chord as more highly-fused when the interval possessing the greater *frequency-ratio* is below. But there is a still greater similar tendency when the interval possessing the greater *frequency-difference* is below. Again, in the case of Table IV, the two most musical observers exemplify opposite types, and represent the *greatest difference* met with amongst the five subjects. This fact is important, for were it not for the lack of agreement amongst the musical observers, it might be permissible to argue that, when the interval possessing the smaller frequency-ratio occupies the lower position, the judgment would be affected by the more likely occurrence of beats. Professor Külpe himself said, during the experiments, "The *lower* intervals all seem *less fused* than the higher intervals; it seems to come from the roughness of the single notes."

On examination of the results of the "later" experiments upon the fusion of intervals it appears that Kü, We and Ha all show a tendency to judge higher intervals as more highly-fused than the same intervals in a lower position[1]. But Co and Wa show a slight tendency in the *opposite* direction. Again, it must be noted that the two most musical observers show opposite tendencies.

The greater roughness of the lower notes of the Tonmesser is perhaps unavoidable. But the question of the effect of *beats* upon the judgment receives more light when it is remembered that the beating chords were carefully examined and cast out by a competent observer, who

[1] This was verified in the case of We and Ha (both "unmusical" observers) by special control experiments in which the "pairs" were composed of the *same* intervals but at higher and lower pitches. In both cases the votes for the fusion of the lower intervals were 12·5, for the fusion of the higher intervals 51·5. This experiment was impracticable with the musical Kü.

was on the watch for them and was not, as were the observers in the routine experiments, attending to another attribute of the stimuli.

(4) The meaning of Table V—the "relationship" table—is clear, and the effect of clang-relationship upon the musical and unmusical observers is interesting. The value $\frac{B}{A}$ is large, and the value $\frac{\text{m.v.}}{\frac{B}{A}}$ small. We may therefore conclude that the factor of relationship is here an important one.

VI. CONCLUSIONS.

The main conclusions which may be drawn from the foregoing experiments are as follow.

(1) The method of *direct* judgment upon the fusion-degree of intervals is practicable for *unmusical* as well as for musical observers, provided that (a) the "paired-comparison" procedure be adopted, (b) a sufficiently large number of comparisons be made, (c) the reliability of the individual observer's answers be assured by calculating from the results of preliminary experiments a "consistency coefficient" which satisfies the requirements of the particular investigation.

(2) The above method offers no further difficulties in its extension to the examination of the fusion-degree of *chords*. Here again, unmusical observers may, if they meet the requirements of the provision mentioned above (1 c), be employed in the experiments.

(3) The assumption (p. 59) that "the degree of fusion of a chord varies with the position of its constituent degrees of fusion within the tonal scale, decreasing when the worse degrees are the lower, and increasing when they are the higher," is justified.

(4) The degree of fusion of a chord increases when the interval possessing the greater *frequency-ratio* (*i.e.* the "greater interval" in the musical sense) occupies the lower, and decreases when it occupies the higher, position.

(5) The degree of fusion of a chord increases when the interval possessing the greater *frequency-difference* (*i.e.* the "greater interval" in the physical sense) occupies the lower, and decreases when it occupies the higher, position. This fact does not seem to be attributable, in the present experiments, to the greater possibility of the occurrence of *beats* in the latter case.

(6) The fact that the major chord (whether it be of the kind used in music, or composed by the addition of two intervals chosen at random) possesses a greater degree of *indirect* clang-relationship than the minor chord formed from it, is correlated with the fact that it is judged to possess a higher degree of fusion than the minor chord so formed.

(7) The statement that the greater the simplicity of the ratios of the frequencies of the tones contained in a chord (whether the chord be considered as a whole or the tones be taken in pairs), the greater the fusion of the chord, is supported by these experiments.

(8) The above statements and assumptions were originally made to account for the difference between those major and minor chords which are used in music. These experiments seem to show that they have a *general* significance, for they apply both to musical and to " unmusical " major and minor chords.

That the present work would profitably be supplemented by experiments upon *pure* tones, and upon clangs poorer in overtones than those given by the Appunn tongues, seems obvious. It is possible that the result would be to accentuate the importance, for the total fusion of the chord, of the position of its more highly-fused interval. But this is a matter for further investigation.

THE CLASSIFICATION OF OBSERVERS AS "MUSICAL" AND "UNMUSICAL."

By T. H. PEAR.

Lecturer in Experimental Psychology, University of Manchester.

Test I: analysis of intervals.—Test II: difference-limen for pitch.—Test III: the "singing" test.—General information concerning the observers. Conclusions.

THE reason for applying certain tests for musical ability, together with a brief enumeration of those tests and of the results obtained therefrom, has been given on pages 78, 80 of the preceding paper[1]. By means of them, and by means of questions put to the observers, a great deal of information was obtained. A study of these results and answers throws light upon the performances of the subjects in the main experiments.

The tests were adaptations of those employed by Stumpf[2]. Titchener's suggested additions to these tests[3] were also adopted. They will now be examined in detail.

Test 1. *Analysis of intervals.* The Tonmessers and conduction-tube were used in the same way as before. The observer was told that the stimuli to be given him would consist either of two notes or of one. His answers were to be "two," "one," or "doubtful." The stimuli, which included intervals and single tones, were given for 3 seconds. The frequencies of the intervals given were:

[1] This *Journal*, pp. 56–88.

[2] Stumpf, *Ztschr. f. Psychol.* xv. 299; *Tonpsychologie*, II. 142 ff.

[3] Titchener, *Experimental Psychology*, Instructor's Manual, Qualitative, p. 333.

Interval[1]	Tones theoretically calculated	Tones on Tonmesser
8	180—360	180—360
	140—280	140—280
	248—496	248—496
	208—416	208—416
7	136—255	136—256
	160—300	160—300
	266·6—500	266—500
7 (5 : 9)	142—255·6	142—256
	255·5—460	256—460
6	260—433·3	260—434
6	150—240	150—240
	388·3—466	388—466
5	266·7—400	266—400
	213·3—320	214—320
	326·7—490	326—490
	146—219	146—220
4	132—176	132—176
	360—480	360—480
3	280—350	280—350
3	150—180	150—180
	375—450	374—450
2	435·5—490	436—490
	140—157·5	140—158
2	375—400	374—400
	130—138·7	130—138
	300—320	300—320
1		190
		506
		240
		450

Each interval or single tone was given once, *i.e.* there were, in all, 30 stimuli. Errors in judging the *single* tones were not reckoned finally, so that the maximal number of possible errors was 26. The errors of any individual were expressed as a percentage of this number. The individual errors (in percentages) were for subject Wa, 1·9; Ha, 19·2; Co, 25; We, 40·4.

The most important points in the introspective reports upon this analysis-series are as follow:

Co. "I am quite sure of my answers: at least I think I was absolutely right in nine-tenths of the cases, although it was hard work. I was reminded of the similarity between this and the 'two-point touch' experiment. The analysis in both seems quite similar. (Co had done a great deal of work on the 'two-point limen.') I have visual images in both. In this experiment they are very indefinite ones, not so definite as in the touch experiments. I do not

[1] For meaning of abbreviations in this column see this *Journal*, p. 65.

use visual imagery in order to make my judgment. The 'two-ness' is a 'side-by-side' two-ness."

We. "I thought I should see (*i.e.* visualize) two tones, when I judged 'two' in this experiment, but this was not the case. I do not think that I really *analysed* in these analysis-experiments, but only *knew* if there were two or one."

Test 2. *Difference-limen for pitch* (approximate estimation). A standard 256 tuning-fork and a variable fork, furnished with riders (both forks made by Appunn), were used. The "method of limits" was employed, the procedure being by "complete descent and ascent" (twice, once with standard presented before variable, the other time in the reverse order). A space-error did not occur. The standard and the variable forks, when presented to the observer, were always put in a constant position behind him, which was so chosen that both ears heard the tone with the same intensity.

No great significance is attached to the results obtained by such a brief experiment, yet they afford another clue upon which to base a judgment of the musical capacity of the observers. The rank-order resulting from this test shows a similarity with that found in other tests in this investigation, viz. the "consistency" test (pp. 69—71 of the preceding article), the above "analysis" test and the "singing" test (see below). The approximate difference thresholds obtained for the several observers were—Wa, 1·4 vibrations per second; Ha, 1·4; Co, 1·8; We, 3·1.

Test 3. *The "singing" test.* The subjects were required to sing notes, given at random and definitely within the compass of their voices. As in this test the division into grades is arbitrary, it was decided to group the subjects merely into *three* very distinctly-different grades (see p. 80 of the preceding article). For the particular observers here employed it may be confidently affirmed that this mode of classification is free from objection.

General information concerning the observers.

To supplement the foregoing tests, each observer was questioned concerning his interest and training in music, his possession of and use of auditory imagery, his capability of imaging a complex of auditory stimuli (*e.g.* an orchestra of 100 performers playing the March in "Tannhäuser"), the importance of music in his general life, etc.

The answers are given in full.

Wa. Very interested in music, very fond of it as a child. From his thirteenth year onwards had a musical education; can play the piano, organ, violin and viola. Has sung much, and with great pleasure, in choirs. For several years was director of a "Gesang-verein," and was a teacher of singing for five years. Has had instruction in choir-singing and in systematic singing.

States he can image a tone, especially in the once-accented octave, best with a' (about 440 vibrations per second). Can easily image the "Tannhäuser" March played by 100 performers. Can very well image a Bach fugue, better as played by an orchestra than by an organ.

When auditory images are present, has no images of movements in throat, tongue, etc., but experiences *tendencies* to sing. Visual images are unnecessary in this connection. Images of musical perceptions may be unpleasantly or pleasantly toned, or neutral as regards affective tone.

When alone, sings frequently to himself. Whistles, with very great pleasure, but badly. Can hear that he whistles badly, and criticizes it constantly.

Co. "Intellectually" interested in music, does not pretend to understand it. Uses it in general life, primarily as something which arouses different subjective moods, secondarily in the more objective sense. Enjoys it in the same way that she enjoys a pleasing combination of colours which is merely "objectively" pleasant or beautiful.

Trained (more or less) in music in a public school (U.S.A.). Had piano lessons for two years when aged 9 or 10, but gave it up, hated it. Could not "keep a tune in her head" when a child, but was laughed at, so seldom tried.

Has rather erratic auditory images, occasionally very distinct, but always much clearer when they are of people's voices. Can image a pure tone: the image probably accompanied by a kinaesthetic image (throat and tongue). Can image the orchestra of 100 performers while playing the March in "Tannhäuser," but know it "isn't right." Can call up an auditory image of any tune only by the help of kinaesthetic imagery. Seldom has auditory images of melodies, and they do not seem to be under the control of the will.

Power of recognising a tune is poor. Tunes are "carried in the head" in kinaesthetic terms, controlled by auditory images. Thinks that when she images a voice it is always the voice of some particular person. Can tell if a piano is mistuned only when it is badly mistuned, as badly as the one in ... (instancing a particularly horrible example in a piano known to the members of the Institute). Can image the clangs of well-known instruments, in connection with visual images of the instruments, but the timbre in the image possesses at most two or three tones.

Laboratory tests show the auditory acuity to be a little subnormal. She knows, from the result of experiment, that her lower limen for pitch is quite high and the upper limen rather unusually high.

Ha. Enjoys theatre-music. At 10 years of age learnt violin for one year, but abandoned playing on account of defective eyesight. At 19 years learnt flute and continued it for some time, but has now quite given it up.

Has auditory images, but states that they are not so clear as visual images. Can image the voice of a friend quite well. Images, if auditory, are not tinged with pleasantness or unpleasantness unless the feeling-tone is attributable to other events. Can remember Wagner's music, but not that of Brahms.

Usually does not sing, seldom whistles.

(N.B. Ha frequently, in the fusion experiments, called the major and the minor chords "the same.")

We. Interested in music for its own sake. At 10 or 11 years of age had violin lessons. Made no progress, so gave it up. Had singing lessons in school, but was not encouraged to proceed further.

Doubts if, when in a normal condition of mind, he ever experiences auditory images. They come only during a condition which strongly resembles "*Ideenflucht.*" Has a primary memory image after anyone has been speaking to him, but suspects that it is not purely auditory. Recollects sounds of instruments in an orchestra by means of a tendency to imitate, *e.g.* in the case of the cymbals he "sees" the metal, bangs the cymbals together, feeling the kinaesthetic sensations in the arms. Seems to remember a complicated melody, *e.g.*, the March in "Tannhäuser," by means of rhythm and the help of motor imagery.

When alone, he never sings; he sometimes whistles (but never a tune), always with movements of the hands.

The rank-orders in the various tests are given on p. 80 of the main paper. As stated there (p. 78), the tests were adopted with a special aim in view, viz. the classification of the observers as "musical" and "unmusical" respectively upon the basis of tests used by *other* workers in the field of tone-psychology. For in this investigation the *direct* judgment upon fusion-degree was accomplished by "unmusical" as well as by "musical" subjects—the terms "musical" and "unmusical" being here used in the same sense as that tacitly adopted by others who have used the above tests. This seems to amount to the assumption, or hope, that these tests cover the range of proficiency indicated by the term "musical."

This assumption is open to serious doubt. The peculiarities of the mental constitution possessed by a person whom we, in ordinary life, term "musical," are assuredly beyond the grasp of a mere handful of tests such as these, even if they can be carried out with the utmost care and precision. We may find (as indeed we found in this investigation) that the person who gives the poorest performance in the tests greatly enjoys music, and of his own accord takes every possible opportunity of hearing it. When we can assure ourselves (as we could in this particular case) that a frequent attendance at musical performances is spontaneous, and not enforced by social obligations and tradition, we must inevitably assume the presence of musical appreciation which has withstood the search of our tests. We may agree with Meyer, when he says[1], "In the question of the establishment of laws, it is best that the vague concepts 'musical' and 'unmusical' be completely banished from tone-psychology, and replaced by those conditions which, in a special case, cause us to term an individual musical or unmusical."..."The difference between musical and unmusical is certainly not fundamentally essential (*grundwesentlich*), but only gradual. This does not by any means exclude the fact that certain individuals under special conditions are incapable of certain (even elementary) musical performances, of which others under the same conditions are quite capable."

There yet remains the possibility of applying numerous better-devised and more searching tests, and of treating the results by means of the mathematical theory of correlation. It would be of great interest to find the degree of correlation between the results of the various tests, and to discover whether any central common factor exists. Were this the case, we might have a more scientific basis for the concepts "musical" and "unmusical."

[1] *Ztschr. f. Psychol.* 1899, xx. 20, 21 (footnote).

SOME RELATIONS BETWEEN SUBSTANCE MEMORY AND PRODUCTIVE IMAGINATION IN SCHOOL CHILDREN.

By W. H. WINCH.

I. The Problem stated.—II. General plan of the experiments.—III. First series of experiments. School "O. K.": (i) A brief chronology of the experiment; (ii) Specimens of the tests and method of marking; (iii) Results.—IV. Second series of experiments. School "S.": (i) A brief chronology of the experiment; (ii) Specimens of the tests and exercises, and method of marking; (iii) Results.—V. Third series of experiments. School "N.": (i) Chronology of the experiment; (ii) Tests and methods of marking; (iii) Results.—VI. Fourth series of experiments. School "N.": (i) Chronology of the experiment; (ii) Tests and methods of marking; (iii) Results.—VII. Fifth series of experiments. School "W.": (i) Chronology, tests, and methods of marking; (ii) Results.—VIII. Summarized conclusions.

I. The Problem Stated.

An attempt is made in the following investigation to discover some of the relationships which exist between substance memory and productive imagination in school children.

It is still very common for such questions to be argued largely on extreme cases in adult life. A. is a man of copious and accurate memory, but is weak imaginatively: B. on the contrary, is a man of fertile imagination, but his memory is weak. So the argument runs. My reader will easily supply further examples for himself from the biographical or other records of men of genius and talent. Hence it is inferred that it is likely that a negative correlation exists between the two functions or groups of functions called by the generic names, imagination and memory. That is, if measurements were made of the two functions or parts of them in a number of different individuals, it would be expected, generally speaking, that those who are most proficient in one way would be found to be the least proficient in the other, and *vice versa*. A further cause for this belief, at least among English educationists, is the dissatisfaction that has been felt, and rightly felt, at the results of a set of educational methods which have employed the continuous memorization of material, often imperfectly understood, as the leading educational device. We may remember the dictum of Professor Bain, who was, in his day, a great educational reformer as well as a philosopher and psychologist. He said, "The retentive faculty is the faculty that most of all concerns us in the work of education[1]."

Our pedagogical writers of to-day would certainly and rightly, I think, decline to accept any such dictum. But further, there is a general notion, more or less vaguely formulated, that all exact acquisition of knowledge is likely to interfere with the spontaneity, vigour, and freshness of the child's mind. Imagination, it is thought, must be exercised in the child from the very beginning of his school life. He must be encouraged to throw the knowledge he already possesses, for he knows a great deal before he begins school, into new combinations, which shall be, psychologically considered, true inventions. If one may be allowed to express a personal bias in a scientific journal, I confess to a strong belief in this last proposition. But, without more evidence than is usually adduced, I doubt how far productive imagination, at any rate in the young, need be considered as a function which is always, or even generally, in inverse relationship to memory. That, under some conditions, it may be, I have little doubt. That it is generally so, under the ordinary school conditions of to-day, I seriously question.

II. General Plan of the Experiments.

The term "substance memory" probably requires little explanation. It is taken to mean the memory for ideas rather than for words. It is opposed to rote memory, either of things with or without meaning, for rote memory requires the exact reproduction of symbols (meaningless or otherwise) in a very definite order. Words, phrases or sentences which mean the same as the content to be remembered are not admissible. In substance memory exercises, on the contrary, it is the meaning alone which counts; a very large latitude is allowed in the

[1] Bain's *Education as a Science*, 7th edition, p. 20.

verbal expression of it. And, need I say, the tests are of such a length verbally and given in a time so short that rote memorizing, except of the first few words, is not possible.

The term "imagination" is used without reference to the production or non-production of images. Nor is it used here to mean reproductive imagination. It will be remembered that text-book definitions of memory sometimes give it as equivalent to reproductive imagination; in which sense imagination is made to consist of images, and memory is regarded as the imaged reproduction of percepts. With imagination in that sense this paper has nothing to do. The term imagination is used in this research as equivalent to invention or productive imagination. It is used with this connotation in the list of mental characters which was drawn up by the Anthropometric Committee of the British Association for the Advancement of Science in 1908 to guide investigations into the relations between the mental characters of school children.

The experiments were made in a number of different schools in neighbourhoods of a somewhat different character, with varying syllabuses of work and varying methods of teaching. In each case a whole class did the work. I thus avoided the dangers of selecting children for the purpose, except in so far as selection is implied by the fact that all the children whose work is compared are members of the same standard or class, and are approximately of the same age.

I gave preliminary exercises so that the children should understand what was expected of them, and then several tests in each of the functions to be compared. I did this so as to avoid the instability of result which is inevitable in new exercises if only one test is taken. I was careful to allow adequate intervals between the tests, and they were given usually on the same day or days of the week, and after the same school lessons. The results of the two kinds of tests were then correlated and were found to be positively related. What may we infer from that? Perhaps we may conclude that the normal development of the one function had not apparently injured the development of the other. But are we entitled to go further and to infer that the development of the one had assisted that of the other? When we remember that correlation does not necessarily imply causation, we shall be prepared to admit the need of further experiment to determine that issue.

To deal with that question, I divided some of my classes into two equal groups, groups "objectively" found to be equal by a division dependent on the results of several tests in imagination. Then, for a period, one of the groups was trained in exercises in substance memory, the other was not. Afterwards the two groups worked further tests in invention. The two sets of tests in imagination will be referred to as the preliminary and final tests respectively. It appeared from the first table of completed results that the group trained in substance memory did better work in invention, not only as a whole, but section by section, than the group not so trained. Improved memory resulted in improved work in imagination. The element of natural growth in imagination or the influence of the usual subjects of the school curriculum upon it is ruled out by this method of equal groups, since it is only the excess improvement of the trained over the untrained group which may be regarded as improvement due to training. A considerable part of the total improvement from the preliminary to the final tests would have occurred without intermediate training in substance memory, but the improvement is common to both groups.

It appeared therefore that improvement in memory implied improvement in imagination. It previously appeared from the correlation between the results of the tests with different individuals that the two functions grew in strength side by side in school children.

May we therefore conclude that the pedagogic opinion that memory training obstructs imaginative work is wrong?

It certainly is, if by that is meant that all mnemonic functioning affects invention inversely. Under the conditions of my experiment, it clearly had a beneficial influence. But I still felt that memory work could easily be pushed too far. If the correlation between the two functions implies a real connection, an excess of memory work would produce, not an improvement, but a decline in the corresponding inventive work.

To test this issue, I worked in another school. I used the method of equal groups as before, but, with the group trained in substance memory, I increased the difficulty of the exercises after a time and somewhat shortened the intervals between them, so that I obtained a series of successive results which showed no improvement. The children were clearly at fatigue point for this particular sort of exercise.

If the two functions or sets of functions are really connected as well as positively correlated, we ought, when the trained and untrained groups work together again, to get a different result from the previous one. We ought not to find so much improvement on the part of the trained group over the untrained group as in the previous case. But I was hardly prepared for what I actually found. The group, both as a whole and section by section, which had been trained up to fatigue point in substance memory, appeared to do *worse* work in imagination than the group which had not received the special training.

Such a result seems to me fertile with important consequences. But just now I pause to remark only that the element of time is so important a factor in all these questions of human evolution that, unless dates and hours of tests and intervals between tests are given in such experiments as these, there is no check on the adequate validity of the results.

Different experimenters, by merely using different intervals between the tests and exercises may, in all good faith, quite well produce results which, apparently, are diametrically opposed the one to the other. But the negative results, when the conditions which led up to them are known, are often not contrary to the positive results, but support them. My apparently contrary result emphasizes the reality of the connection between the two powers or functions whose relationships we are investigating. For the connected fatigue in two functions A and B is a good indication of the real connection between them, if the fatigue has been produced in one of them by overtraining the other.

III. First Series of Experiments. School "O. K."

A first series of experiments was carried out in a municipal boys' school situated in a rather poor neighbourhood in London. The work was done with the whole of a Standard VII class whose average age would be 13 years 4 months at the end of the school year, seven or eight months later. The tone of the school was such as belongs to a strongly-disciplined and hard-working one. The teacher of the class was a young man, able, vigorous, and enthusiastic; his boys were probably fairly near their possible limits in school work for their ages, and they might be relied on to give full attention to the work, even after the novelty of the tests and exercises had somewhat worn off. All the tests and exercises were administered by the teacher, who had had considerable experience of work of this kind.

(i) *A brief chronology of the experiments.*

The tests and practice exercises were worked from 11.5 to 11.55 in the morning. They were given on the following days:

Wednesday,	Nov. 6th,	1907,	a first test in substance memory (test 1),
Friday,	Nov. 8th,	,,	a first test in imagination (test 1),
Wednesday,	Nov. 13th,	,,	a second test in substance memory (test 2),
Friday,	Nov. 15th,	,,	a second test in imagination (test 2),
Wednesday,	Nov. 20th,	,,	a third test in substance memory (test 3),
Friday,	Nov. 22nd,	,,	a third test in imagination (test 3).

It became evident at this point that, so far as the memory exercises were concerned, the boys were taking up fairly steady positions relatively to one another; they were reaching what, in matters of sport, we should call their "true form." For correlation purposes, therefore, we felt that we had enough tests to go upon. The reliability coefficients, so called, between the various tests in imagination were not so high. I incline to attribute this partly to the fact that the memory work (though estimated by a much more rigorous method than that which could be employed in ordinary school work) was similar to some school work already done, whilst the imagination tests were quite new. We therefore decided to give some more imagination tests, though we did not think it necessary to give more memory tests, and the work proceeded.

On Friday, Dec. 6th, a fourth imagination test was given (test 4),
and on Wednesday, Dec. 11th, a fifth imagination test (test 5).

I now had the material to enable a coefficient of correlation to be found with tolerable accuracy between the total results in memory work and the total results in imaginative work. I also had the material to enable me to divide the class into two groups "objectively" found to be equal in imaginative work of the kind given.

At this juncture the work was interrupted by the terminal school examinations and the Christmas holidays. And, as I was compelled to forego the tests and exercises for a few weeks, I thought it would strengthen the conclusion (were a definite one obtained) if I allowed a longer interval between the division into "equal" groups and the special training than we had at first thought of doing; so I delayed until February, 1908, before resuming the work.

On Friday, February 7th, one of the two equal groups, called hereafter the practised group, worked an exercise in substance memory, whilst the other group worked algebraic exercises.

On Wednesday, February 12th, Friday, February 14th, and Wednesday, February 19th, the practised group worked a second, third, and fourth exercise in substance memory, whilst the other group, as before, worked algebraic exercises.

Both the "practised" and "non-practised" groups worked in the same room under the same teacher. On the day following each exercise and at the same hour both groups were informed as to the marks they had obtained in the practice exercises, the one in substance memory, the other in algebra.

The two groups then worked together three final tests in imagination. These were given on Friday, February 21st, Wednesday, February 26th, and Friday, February 28th.

(ii) *Specimens of the tests and methods of marking.*

The memory tests were stories. The imagination tests were the invention of stories by the boys, certain words being given. There is, in this experiment, no endeavour to discover the relation between memory and imagination when the subject-matter for each is quite different. But it must not be supposed that the excess improvement for inventive work shown by the group practised in substance memory as compared with the group practised in algebra was due to a transfer of the definite content of the remembered stories to the invented ones. That is by no means the case. The invented stories are, almost invariably, on a much lower plane.

A similar relationship is found between the capacity to solve given problems in arithmetic and the power to invent arithmetical problems; there is also a high positive correlation, but the invented problems are almost invariably of a much lower type than those which the children can solve when the problems are given to them.

It was an astonishing thing to the teacher who marked the imaginative exercises that the boys did not appear to fit into their invented stories pieces of the actual content of the stories which they had memorized. The improved functioning is a much less "atomistic" thing than a mere transfer of certain portions of the content from one set of exercises to another.

The stories, moreover, which were given for memorizing had a unity and a point and a linguistic expression of a fairly high order. The invented stories fell much below in all these characteristics. They were really stories made up by the children.

What were the children instructed to do in these tests in invention? It would hardly be sufficient to tell the boys to make up a story, and just leave the instructions there. Some of them, at least, would present us with remembered ones. So we determined to give a number of words whose meanings were well known, and require the story to be so constructed as to include within it all these words.

(*a*) *Invention test.*

The instructions were in writing as follow:

"Write a story containing the following words: thief, landlord, crab, shake, hotel, basket, cries, provisions, escape, custody.

"You are to write the longest story you can; because the longer the story is the more marks you will get, provided that everything you write has something to do with the story. You will get no marks at all for them and only be wasting your time if you write sentences which have no connection with the rest. Try and think out the story you are going to write before you start, and see that the progress of the story will enable you to fit all the words in properly."

(*b*) *Substance memory test.*

Five minutes were allowed for studying visually, audible articulation not being permitted.

"One evening, early in the last century, a young waiter at the Three Tuns Hotel, Penzance, was asked to fetch a penny roll from the nearest bakery. Always prompt and obliging, the young fellow started on his errand without even taking his hat. Annoyance, astonishment and alarm succeeded each other as time passed by without his return. The country was scoured in every direction, but in vain. After purchasing the roll, he had vanished as utterly as if the earth had opened and swallowed him alive. That day twenty years, which happened to be on the same date and day of the week, a man entered the hotel at precisely the same hour and minute. Making his way to the bar parlour, he laid down a penny roll before the landlady and said, 'I've been a long time, ma'am, but it wasn't my fault.' He had met a press-gang, been seized, gagged, bound, carried to the quay, and borne off to sea so quickly and quietly that he had no power to utter a cry. Singularly enough, he found the same proprietors at both hotel and bakery on his return."

Toward the end of the practice exercises, it was found necessary to increase the length of the tests; but the time for memorizing and for writing the stories remained the same.

(*c*) *Method of marking the substance memory tests.*

These were marked on a system of mnemonic units which depend upon the mental level of the class with which we are experimenting.

With these children (it would not be the case with very young children), subjects, predicates, and objects, or purposive extensions of the predicate, are remembered together or forgotten together. Adjectives, adverbs and adjectival or adverbial phrases are remembered or forgotten separately, while connective ideas other than mere copulation (expressed by "and") are also separate units. Conjoined nouns in subjects and objects and conjoined verbs in predicates also receive separate marks. The system is by no means perfect, since all simple sentences (predicate-subject-object) are not equally easy to remember, and the connecting ideas, expressed by connective words which give the "tone," so to speak, to subsequent sentences and paragraphs, are usually more difficult to remember; at least, the expression of them is more frequently omitted than that of the other memory "units." Nor could we assert that adverbial expressions of manner, time, and place are equally easy to remember or forget. Experiments on a very large scale with children taught in various ways would enable a scale of marking to be made out which would be more accurate than the one I have adopted. But the theoretical imperfection of the system does not prevent us from obtaining sufficiently regular results in any particular class to enable us to deal with problems such as that which I am now undertaking. Always the actual worked papers help us to our units. If any sentence or phrase which, in the preliminary analysis, we set down as one unit is found to be divided (that is, part of the notion is remembered and part forgotten) by any pupil in the class, we add one to our units and revise the marking of the papers already dealt with. A piece of analysis will probably add to the comprehension of the method.

A waiter was asked to fetch a roll

young penny

at an Hotel in the last century at evening from a bakery

Tuns at Penzance early the nearest

Three

Then at once arises the problem of what we are to allow verbally as a correct expression of the various notions. Latitude rather than rigorous exactitude controls our decisions. Where the meanings given are such as are felt by the children to be equivalent, we accept them as equivalent. "A waiter was sent to bring some bread" is taken as equivalent to the first unit; and "a man who worked at a public house" would be taken as equivalent to "a waiter at an hotel." "A hundred years ago" would be accepted in the place of "early in the last century." "A baker's shop close by" would be accepted for "the nearest bakery"; and so on.

(*d*) *Method of marking the imagination tests.*

With very young children a difficulty exists which is not felt with these big boys. The former sometimes leave out some of the words which are required to be included in the story. And it is a very difficult problem to know if any marks should be subtracted for that failure, and, if so, how many. But so far as children of this mental level are concerned, all the words are included somehow. The story invented is marked as if it were a substance memory exercise. This does not mean that the longest story receives the most marks; it certainly will, other things being equal; but a shorter story will often contain more units. Qualifying phrases will be attached to the nouns and verbs, and connectives will be introduced showing the bearing of one sentence on another. Many units of meaning will thus be condensed within one sentence.

The boys have been instructed that no marks would be allowed for any sentences which do not belong to the story. The continuity and connectedness of the "units" must therefore be taken into account. But it is obvious that we cannot apply to the work of these schoolboys any difficult criterion of aesthetic unity. Yet we must apply some criterion, and I decided on the following. No marks were allowed for any sentence or parts of it which did not arise connectedly from preceding sentences. In his desire to get one of the given words in, a boy might very well insert a sentence, *vi et armis*, which was without connection with the others; such insertions received no marks. The aesthetic unity which belongs to the various elements of a good story in their relation to the *dénoûment* was not allowed for; it was unimportant, because these children, whilst quite capable of making up stories showing continuity of construction, were incapable of making every element in the story converge to a point; their stories lacked point in that sense. I mention this, because the method of marking does not allow for this restrictive and highly critical use of inventive power; it provides only for an estimate of that fertility of imagination upon which, after all, consciously or subconsciously, the winnowing process of selecting only those ideas which seem to lead directly to the *dénoûment*, and, as it were, render it inevitable, must depend. It is claimed only that the method of marking is practically satisfactory for estimating in schoolboys of this mental level the fertility of continuous and connected imagination. I have said nothing of imagery, because I am of opinion that, whether imagination is aided by abundance of imagery or not, inventive power, at least of a literary kind, can quite well subsist without it.

(iii) *Results.*

First let me find the correlation between the results of the tests in imagination and substance memory which were worked at the commencement of the experiment for this purpose. We started the tests with 30 boys, the whole of the class. We were very fortunate in the school attendance of these pupils; for only two of them were absent at all throughout these tests. These two were excluded, so that the correlation found is that subsisting between 28 pupils, in the two functions of substance memory and productive imagination.

As I have explained above, we were fortunate in the steadiness of the results from our substance memory exercises from the first. The results of test 1 correlated positively with the results of test 2 with a coefficient of ·654; and those of test 2 correlated with those of test 3 with a positive coefficient of ·682; the coefficients being in each case worked out directly from the individual results by the formula,

$$r = \frac{\Sigma xy}{n\sigma_1 \sigma_2}.$$

It seemed that no serious error would arise by taking the total of these three tests as fairly representing each boy's position in substance memory.

With the preliminary tests in imagination we were not so fortunate. The coefficients of reliability were as follow:

Tests 1 and 2............·567
,, 2 and 3............·475
,, 3 and 4............·509
,, 4 and 5............·548

We decided to accept the totals of these five tests, for the purposes of this experiment, as satisfactorily estimating the relative work of each boy in imagination.

The totals of the two series were now arranged in order showing the comparative value of each boy's work in the two functions or groups of functions. I propose to show the correlation between them in one or two ways.

First, I will show it on a system of marking recommended by the Anthropometric Committee of the British Association in their report on the estimation of mental characters of school children. They propose to assign the letter C to some 50 °/₀ of the pupils taking the tests in any given function; to the 20 °/₀ above and below this 50 °/₀, the letters B and D respectively; and, to the 5 °/₀ at the top and bottom, the letters A and E respectively. In order to get my table most nearly approximating to this arrangement, I was compelled to take 7 °/₀ at the top and bottom and 18 °/₀ for classes B and D. This classification, in classes A, B, C, D, E, was made for both lists, the one for substance memory and the one for imagination, and the results compared.

TABLE I.

Showing the correlation between substance memory and imagination, the results being arranged in classes.

Memory work		Imaginative work				
2 A's	correspond to	1 A	1 B	0 C	0 D	0 E
5 B's	,, ,,	1 A	2 B	1 C	1 D	0 E
14 C's	,, ,,	0 A	2 B	9 C	3 D	0 E
5 D's	,, ,,	0 A	0 B	4 C	1 D	0 E
2 E's	,, ,,	0 A	0 B	0 C	0 D	2 E

This table means that of the two boys who scored an A for memory, one scored A for imagination and the other B, and so on.

It is quite obvious that a high positive correlation exists; but I will show it in a form which those accustomed to deal with averages will appreciate more clearly than by means of the above grouping into classes.

TABLE II.

Showing the correlation between substance memory and imagination, the sections being arranged according to the marks obtained in the memory tests.

Marks in memory tests	No. of boys	Av. memory mark	Av. imagination mark
Over 85	2	87·5	154·5
75 to 85	7	80·7	137·7
65 to 75	14	70·3	117·3
55 to 65	4	60·7	103·7
Below 55	1	51·0	82·0

The fact of high positive correlation is probably more apparent from this sectionized grouping than from the preceding table.

Finally, the coefficient of correlation was worked out by means of the Pearson formula from the individual results, and was found to be + ·749, with a probable error of ·05.

The next work with the results of the preliminary tests was to find two groups equal in the preliminary tests for imagination. They were obtained and arranged as follows. Four boys unfortunately had left school, being over age before commencing the practice exercises; their names are not included.

TABLE III.

Showing the two equal groups arranged from the results of the preliminary tests in imagination.

Group A		Group B	
Name (initials only)	Marks for imagination in 5 preliminary tests	Name (initials only)	Marks for imagination in 5 preliminary tests
F. G.	147	A. M.	177
E. N.	146	W. B.	145
W. P.	138	W. F.	143
M. M.	137	J. A.	132
H. M.	125	P. M.	129
A. W.	123	W. S.	122
W. S.	119	G. H.	120
A. McK.	118	A. F.	115
W. H.	112	H. A.	113
A. R.	98	T. S.	98
A. H.	96	A. S.	97
H. J.	86	J. B.	83
	Total 1445		Total 1474

A. M. in the second group is much the strongest; his work throws the group slightly out of balance; otherwise, I was successful in getting not only equal groups but a fair equality in the corresponding pupils of Groups A and B.

I will next show the improvement of the practised group in the memory exercises themselves. The names are arranged in the order of proficiency shown in the five preliminary tests in imagination.

TABLE IV.

Showing the improvement in substance memory of the practised group.

Name (initials only)	Marks for 3 preliminary tests		Marks for 4 practice exercises	
	Totals	Av. per test	Totals	Av. per exercise
A. M.	84	28·0	137	34·7
W. B.	81	27·0	135	33·7
W. F.	79	26·3	141	35·2
J. A.	83	27·7	152	38·0
P. M.	68	22·7	119	29·7
W. S.	78	26·0	138	34·5
G. H.	74	24·7	142	35·5
A. F.	69	23·0	130	32·5
H. A.	63	21·0	116	29·0
T. S.	80	26·7	143	35·7
A. S.	71	23·7	135	33·7
J. B.	56	18·7	131	32·8

Some of this improvement is due to growth and perhaps also to the influence of other subjects of the school curriculum; but these factors are also operative on the unpractised group. So that, when the two groups are put together again, the only improvement that we can rely on (if there is any transfer of improvement in memory to improvement in imagination) to differentiate the two groups in the final tests is the excess improvement (if any) of Group B due to the set of practice exercises in substance memory. It will be remembered that, whilst Group B were doing these exercises, Group A were working algebraic exercises. This group showed decided improvement in algebra, but the exercises were neither chosen nor marked in a way to render the figures of scientific service, so that no further notice will be taken of the results. In all other respects the curriculum for the two groups throughout the whole experiment, which lasted several months, was the same, and their time table of work was the same.

I next present the results of the work done in the final imagination tests when the two groups worked together again. The classification will be based on the work done in the preliminary tests in imagination, from the results of which the two equal groups were formed.

TABLE V.

Showing the comparison between the preliminary and final tests in imagination of Groups A and B.

Marks in 5 prel. tests in imagination	Unpractised group A			Practised group B		
	No. of boys	Av. mark in 5 prel. tests	Av. mark in 3 final tests	No. of boys	Av. mark in 5 prel. tests	Av. mark in 3 final tests
Over 140	2	29·3	46·3	3	31·0	50·6
120 to 140	4	26·1	41·0	3	25·5	45·4
110 to 120	3	23·3	38·7	3	23·2	42·3
80 to 110	3	18·6	32·4	3	18·5	36·7

The averages given in Table V are average marks per boy, per test.

There seems no doubt that the group trained in substance memory, not only as a whole, but section by section, has done better work in the final imaginative tests than the group not so trained. Both groups have much improved; it is the excess improvement only of Group B over Group A which we can attribute to the practice work in substance memory. Let me attempt a more precise measure of this improvement. I will show for both groups the percentage improvement of the final on the preliminary work, and the excess percentage of that of Group B over Group A.

TABLE VI.

Showing the percentage improvements of Groups A and B in imagination, and the excess improvement of Group B.

Marks in 5 prel. tests in imagination	Group A Percentage of improvement	Group B Percentage of improvement	Excess of B over A (per cent.)
Over 140	58	63	5
120 to 140	57	78	21
110 to 120	66	82	16
80 to 110	74	98	24

The general result, namely, a considerable excess improvement of Group B over Group A seems clear. But it is a matter of further interest to see how far this appears to be related in a more detailed way with the percentage improvement in memory shown by the sections of the practised group.

TABLE VII.

Showing percentage improvement in memory and excess improvement in imagination, section by section, of Group B over Group A.

Marks in 5 prel. tests in substance memory	No. of boys	Percentage improvement in memory	Excess percentage improvement in imagination
Over 140	3	27	5
120 to 140	3	34	21
110 to 120	3	41	16
80 to 110	3	48	24

This table has been inserted mainly because I wished to emphasize what it does not mean. It does not mean that a practice improvement in the highest section of 27 °/₀ in substance memory transfers an improvement of 5 °/₀ to the imaginative work, and so on. Unless we could subtract from the 27 °/₀ the improvement due to natural growth, shared, be it remembered, by the unpractised group also, we cannot say how much of the improvement in memory due to practice alone has been transferred to the work in invention. That transfer exists seems quite clear from a comparison between the results of the practised and unpractised groups in the final tests of imagination; but, without a knowledge of the growth factor in memory apart from the improvement by practice in the memory exercises, it is impossible to work out a percentage of transfer on the amount of improvement due to practice in the practice medium itself.

We know then that an improvement in imagination has resulted from an improvement in memory due to practice; but we do not know what percentage of the improvement in memory due to practice has been transferred.

I now present the results shortly for the two groups as wholes. Group A has an average mark per boy per test in the preliminary imagination exercises of 24·1 with a mean variation of 3·1; Group B has an average mark per boy per test in the preliminary exercises in imagination of 24·6 with a mean variation of 3·7. In the final tests for imagination the corresponding figures are 39·2 (mean variation 4·9) and 43·8 (mean variation 5·9) for Groups A and B respectively. The higher variations in Group B are largely due to the presence of A. M., who, as I pointed out before, was so good that he was difficult to classify in this grouping. A's percentage improvement in imagination is 63 °/₀, B's is 78 °/₀. The excess percentage of improvement, namely 15 °/₀, is regarded as mainly due to the influence of the practice exercises in substance memory, as all other school conditions for the two groups remained the same throughout the period of the experiment. The practised group improved in substance memory from an average per boy per test of 24·6 (mean variation 2·3) to 33·7 (mean variation 1·8), an improvement of 37 °/₀. As I have pointed out above, some of this 37 °/₀ is due to natural growth and perhaps also to the influence of other school subjects, so that it would not be fair to say that a practice improvement of 37 °/₀ in substance memory transfers an improvement of 15 °/₀ to imaginative work; to obtain such a numerical relationship of transfer we should need to allow for natural growth and subtract that from the 37 °/₀. I have no experimental material at present to enable me to do this.

IV. Second Series of Experiments. School "S."

A second series of experiments was carried out in a municipal girls' school situated in a poor neighbourhood in London. The work was done with 55 children, the whole of Standards VI and VII, of an average age of 12 years 11 months at the commencement of the experiment. These children formed one class under one teacher, and worked on the same syllabuses, except for arithmetic. Three tests were given in the invention of stories, and on the results of these tests the class was divided into two equal groups. One of these groups was then practised in substance memory. The exercises were increased in difficulty until improvement appeared to cease and the children were fatigued by the work (of course I am using the term fatigue "objectively"). After this the two groups again worked together some further exercises in invention. The question to be solved was whether the group fatigued for substance memory would do better work in the final imagination tests than the group not practised at all in the substance memory exercises. The tests and exercises for both groups were administered by the teacher of the class, who had had considerable experience in experimental work.

(i) *A brief chronology of the experiment.*

All the tests and exercises were worked from 11 a.m. to noon. On Friday, December 6th, 1907, the whole class worked a test in the invention of stories; on Tuesday, December 10th, a second test; and on Wednesday, December 11th, a third test. It was not wise to allow one day only between the second and third tests; I adopted this interval because I wished to get all the practice exercises in substance memory finished before Christmas, which, however, I eventually failed to do.

The class was divided on the results of these three tests, which will be referred to as the preliminary tests in invention.

On Friday, December 13th, one of the two groups—hereafter called the practised group—worked a practice exercise in substance memory, and on Wednesday, December 18th, a second exercise.

The Christmas holidays now intervened, and the work was discontinued until Tuesday, January 7th, when a third exercise in substance memory was done. A fourth, fifth, and sixth were worked on Friday, January 10th, on Tuesday, January 14th, and on Friday, January 17th, respectively.

Whilst the practised group were working the practice exercises in substance memory, the other group worked sums. In all other respects the school work for the two groups during the whole period of the experiment was the same. On the day following each exercise the pupils of both groups were informed as to the results of their previous day's work.

On Tuesday, January 21st, the two groups worked together again and did the first of the final invention tests. On Friday, January 24th, and on Tuesday, January 28th, a second and third test were given.

Monday was in all cases avoided. Monday's results are always low and tend to reduce the regularity in the sequence of the figures for the successive tests of a series. This is not due to fatigue but to lack of adaptation to work at the commencement of the week.

(ii) *Specimens of the tests and exercises and methods of marking.*

(*a*) *Invention tests.*

The instructions were similar to those given in the preceding school, but the words given to be woven into a continuous story were selected with a view to being more suitable to girls than the words given in the preceding boys' school. One such set consisted of the following words: orphan, garden, hungry, station, parents, clothing, visitor, cottage, train, country.

The tests were marked in a similar manner to those of the preceding school.

(*b*) *Memory exercises.*

As before, the memory exercises were stories. They were studied visually, audible articulation not being permitted. Five minutes were allowed for the study of the first one, a time which was subsequently increased to seven minutes and then to eight minutes as the exercises increased in length. It will be remembered that it was intended to increase the difficulty so that a temporary fatigue for this work and its allied functions should be produced. The exercises were marked on the system of mnemonic units already explained. The stories given were simpler in character than those which were suitable to the boys' class in the school in which the previous experiments had been made. It is, of course, of extreme importance that a memory exercise should not contain ideas which are incomprehensible to the children or which they only understand with difficulty. The following portion of one of the exercises will show the level of difficulty of the stories used.

"One day an Eagle and an Owl swore to be friends for ever. The Eagle promised the Owl he would never hurt her children. 'But will you know them when you see them?' she asked. 'No, but if you will tell me what they are like, I will be sure to remember'."

The analysis of this passage into memory units is as follows:

An Eagle }
an Owl } swore to be friends
one day for ever
The Eagle promised the Owl
he would hurt her children
never
But
she asked
Will you know them
(when) you see them
No,
but
if you will tell me what they are like
I will remember
surely

These are the separate units of meaning which give a satisfactory system of marking for the exercises of these children. Expressions of any meanings which were fairly equivalent to the above were accepted. "Said they were always going to be good friends" would be accepted for "swore to be friends for ever." "Said to the other bird he would not injure her little ones" would be accepted for "promised the Owl he would never hurt her children." "How will you tell them if you meet them?" would be accepted for "Will you know them when you see them?" "I shall never forget" would be taken in place of "I will be sure to remember." "Once," or "once upon a time" would be accepted for "one day." The exercises were made intentionally so long that mere verbal memorizing had no chance of success; the expression of the meanings in other language was inevitable.

(iii) *Results.*

(*a*) *The influence of over-training memory on imaginative work.*

There was no great difficulty in making out two groups approximately equal in the preliminary invention tests. One girl only, I. B., was so much the best that it was difficult to place her. When the two equal groups had been obtained, the group which should practise in substance memory was decided by chance. I propose, on this occasion, to present first the table of final results in relation to the preliminary results, leaving subsidiary considerations until later.

TABLE VIII.

Showing a comparison between the work of Groups A and B in
(a) The preliminary tests in invention of stories,
(b) The final tests in invention of stories.

	Unpractised group			Practised group		
Marks for 3 prelim. tests in invention*	No. of children	Av. mark 3 prelim. tests in invention	Av. mark 3 final tests in invention	No. of children	Av. mark 3 prelim. tests in invention	Av. mark 3 final tests in invention
Over 125	6	147·1	221·5	6	157·3	217·2
100 to 125	7	110·3	150·8	7	111·7	139·7
90 to 100	5	94·4	127·4	5	96·6	128·8
80 to 90	6	84·6	143·1	6	86·5	125·3
Below 80	3	66·6	105·0	4	69·0	105·0

* I should have liked to have presented the numbers in this column in a more regularly sequent way; but it would have been impossible, in that case, to have obtained corresponding sections in Groups A and B equal in number.

It will be seen that, whereas, section by section, the unpractised group are lower in the preliminary tests than the corresponding sections of the practised group, after the practice exercises the positions are reversed. Three sections of the unpractised group are better absolutely in the final tests than the corresponding sections of the practised group, and the third and last sections are better relatively. Each of the groups, indeed, every individual child, has done much better work in the final than in the preliminary tests: the children are older and there is the practice effect of the tests themselves. But the group which worked the special practice exercises in substance memory carried beyond the limit of improvability, has done *worse* work in the final tests than the group not so practised. This is shown perhaps more clearly by a comparison of the percentages of improvement, section by section, of the practised and unpractised groups.

TABLE IX.

Showing the percentage improvements of Groups A and B from the preliminary to the final tests in invention.

	Unpractised group		Practised group	
Marks for 3 prelim. tests in invention	No. of children	Percentage improvement	No. of children	Percentage improvement
Over 125	6	51	6	38
100 to 125	7	37	7	25
90 to 100	5	35	5	33
80 to 90	6	69	6	45
Below 80	3	58	4	52

The teacher who administered the tests and exercises during the experiment and marked the papers was a strong believer in the cultivation of memory functions, and was not aware that I intended to endeavour to produce a decline in the inventive function (if the mnemonic and inventive functions are really connected) by over-cultivation of memory. She was expecting to find the group practised in substance memory for stories much superior to the unpractised group in the final tests in invention. Nor was I myself prepared for such an apparent reversal of the results of the previous school. The view that memory can be over-trained and thus have prejudicial results on imaginative work is confirmed. The practical issue seems clear. As long as the mnemonic functioning itself is improving from exercise to exercise (I speak always and only of school children), the corresponding imaginative function improves too. But when the memory work is pushed beyond the limits of improvability the imaginative work suffers also.

(*b*) *Correlation between memory and imagination.*

In the course of the second series of experiments, though both groups of children worked the tests in invention, only the practised group worked exercises in memory; I cannot, therefore, show the correlation between memory and imagination for all the members of the class, but for Group B only. Further, since the memory exercises were worked at a time subsequent to the preliminary imagination tests, and prior to the final imagination tests, there are no tests or exercises of imagination or memory worked *pari passu*. Hence the two sets of results are not strictly comparable from the point of view of correlation. Since, however, I have six separate tests in imagination and six separate memory exercises for the same child, I have thought it worth while to correlate the results for the two functions. Three children who had been absent on several occasions whilst the work was being done were excluded from the correlation list. The results are classified according to the marks obtained in the six memory exercises.

TABLE X.

Showing the correlation between memory and imagination among the members of the practised group.

Marks for 6 memory exercises	No. of girls	Av. mark for memory	Av. mark for imagination
Over 270	3	276·7	429·0
250 to 270	5	259·2	272·6
230 to 250	8	241·2	236·6
210 to 230	5	220·6	194·4
Below 210	4	174·5	204·7

It is obvious from the above table that considerable positive correlation exists. If the coefficient of correlation is obtained from the individual cases by means of the Pearson "r" formula from the actual marks, it is found to be + ·55, with a probable error of ·09.

(*c*) *Lack of improvement in the final memory exercises.*

I propose to present the marks for the 4th, 5th, and 6th practice exercises in memory. Eight minutes were allowed for the visual study of each of these exercises; the memory units worked out so as to be suitable for this class were 70, 83, and 75. It is fairly obvious, from the subjoined table, that we were dealing with a function at fatigue point. The classification in the table depends on the marks obtained in the preliminary tests in imagination, and is thus comparable to that in Table IX.

TABLE XI.

Showing the marks in memory of the practised group for the 4th, 5th and 6th exercises.

Marks for prelim. tests in imagination	No. of girls	Av. mark for 4th exercise	Av. mark for 5th exercise	Av. mark for 6th exercise
Over 125	6	59·5	57·5	55·2
100 to 125	7	54·5	53·9	55·2
90 to 100	5	53·4	54·4	53·6
80 to 90	6	40·8	46·2	40·8
Below 80	4	40·5	41·2	39·2

With children, improvement in any practised mental function is the rule, provided, of course, they know and understand what they have to do, and are under sufficient incentive. Consequently, a sequence of figures like the above tells us that we are going too fast: the exercises are succeeding each other too rapidly. If measured at the time when this fatigued condition exists, allied functions will show a falling off from what they might otherwise have been, as we have seen in Table VIII.

(*d*) *How far are these fatigue effects permanent?*

Are we then to suppose that the practice effects of the exercises tabulated above have been wholly prejudicial and without value? To test this point a month was allowed to elapse in which no memory exercises were given, and, under the same conditions as before, a 7th exercise, containing 93 units, was given on February 18th. The rise was considerable, beyond what would have been due merely to the increase of a month in age.

TABLE XII.

Showing the memory marks of the practised group after a month's interval.

Marks for prelim. tests in imagination	No. of girls	Marks for memory exercise after the interval
Over 125	6	74·7
100 to 125	7	66·9
90 to 100	5	63·5
80 to 90	6	53·0
Below 80	4	45·5

The table appears to show a considerable advance on the preceding work, and seems to indicate the result of a practice effect which the abstention from practice had allowed to become apparent. Once again I should have put my two groups together and given further tests in imaginative work. Now that the fatigue effects had passed away, my practised group should easily have beaten my non-practised group. But alas! I did not think of it until too late. And if I had, I doubt whether I should have considered myself justified in making further experiments at the time with the same class, not because I think this work is other than highly educative, but because the school syllabuses must, after all, be got through.

(*e*) *How far is the improvement due to the memory practice permanent?*

I have asked the question in its most generic form. It is the most important question that can be asked relatively to any question of improvement through educational influences. It is probable that much, if not most, of the improvement which we attribute to practice and training is due to natural growth and would occur without our machinery of training, and that much of the improvement really due to practice rapidly fades away. I do not draw the conclusion, however, that we may as well abandon our attempt to influence mental functions by consciously directed education. I incline rather to urge that, of the little we are sure we can do, nothing should be left undone. But I fully realize the need for distinguishing the results of natural growth from those due to special practice; indeed, I am, I believe, the first to have arranged experiments on the mental functions of school children so that these two factors of growth may be considered separately. And I fully realize the need for determining the degree of permanence of all practice results.

If the factor of natural growth were the only factor, the unpractised group should likewise show great improvement in memory function during the progress of the experiment. There should be no considerable difference at the close of the experiment, or, perhaps more fairly, there should be no considerable difference between the two groups after an interval had elapsed after the experiment. Unfortunately, however, these arguments rest on an assumption, which may be true, but which I cannot justify. I am supposing that the two groups were equal in memory power at the commencement of the period of special practice by the "B" group. This is supposition merely, for their equality was based upon imagination tests, not upon memory tests. And though the two functions are highly correlated, we cannot be sure that equal division for imagination will bring with it an approximately equal division for memory.

To test the permanence of the practice effect, on February 25th, under the same conditions as in the preceding exercises, a substance memory test was given to both groups, called "The Abolition of Slavery."

"In Saxon times there had been slaves in England, men who as prisoners of war, or on account of their crimes, or for debt, had lost their liberty," and so on.

It was marked on a system used for marking historical exercises, a system which considered each historical fact as one unit. It is not very satisfactory and I do not recommend it. But as every paper was marked on the same system it may be worth while to compare the results. The test was done, be it remembered, more than five weeks after the completion of the memory practice series.

TABLE XIII.

Showing marks for memory of historical facts of Groups A and B after interval.

Marks for prelim. tests in imagination	Unpractised groups		Practised groups	
	No. of girls	Av. history mark	No. of girls	Av. history mark
Over 125	6	33·7	6	36·8
100 to 125	7	20·7	7	24·7
90 to 100	5	22·2	5	19·0
80 to 90	6	16·0	6	25·3
Below 80	3	12·0	4	21·5

There is some irregularity, but, since the division into groups and their corresponding sections is dependent upon work in imagination tests and not in memory, that is hardly to be wondered at. It is also unsatisfactory in such exercises as these to present a table of averages compiled from one test only. Also we have involved, in addition, the transfer of improvement in memory by changing the exercise from a story to generalized historical fact. Notwithstanding all these causes of irregularity, I think there are indications that the effect of the practice exercises in memory has been evident in duration, and, with respect more particularly to the lower sections, considerable in quantity.

V. Third Series of Experiments. School "N."

A third series of experiments was carried out in a municipal girls' school in a very poor neighbourhood, a school classed by the Local Education Authority as one of "special difficulty." The teaching was of a very high order but of a somewhat different type from that in either of the two preceding schools. On this occasion I limited myself to the establishment of correlations. I was anxious to see how far a divergence of result would appear in a somewhat different educational *milieu* and with children of an inferior social class. The work was done with the upper section of Standard V and the whole of Standards VI and VII. Standard V was taught by one teacher, and Standards VI and VII by another. The tests were administered by the teachers of the classes, of whom one had had experience of experimental work, and the other had not.

(i) *Chronology of the experiment.*

This was the same for both classes.

10.55 to 11.50 a.m.	Tuesday, Feb. 18th,	1908,	a first test in substance memory.
,, ,,	Thursday, Feb. 20th,	,,	a first test in imagination.
,, ,,	Tuesday, Feb. 25th,	,,	a second test in substance memory.
,, ,,	Thursday, Feb. 27th,	,,	a second test in imagination.
,, ,,	Tuesday, Mar. 3rd,	,,	a third test in substance memory.
,, ,,	Thursday, Mar. 5th,	,,	a third test in imagination.

(ii) *Tests and methods of marking.*

The tests and methods of marking were similar to those given in the preceding schools, except that the tests were easier. Four minutes were allowed for the visual study of the substance memory exercises in Standards VI and VII. This class would reach an average age of 13 years 6 months at the end of the educational year then some five months distant. The Standard V class was allowed five minutes to study the substance memory tests. The average age of this class at the end of the educational year was 12 years 8 months.

(*a*) *Specimen imagination test.*

The following words were given and the children were required to write a story containing them:

Snowstorm, children, ticket, clock, dog, screams, church, basket, river, ice.

(*b*) *Specimen memory test.*

The following extract will show the level of difficulty of the substance memory tests:

"Some time ago, a gentleman who owned a large business in Manchester advertised in the newspaper for a boy to assist him in the office...."

(iii) *Results.*

It will probably be sufficient in this case if I present the final tables.

TABLE XIV.

Showing the general correlation between memory and imagination.

	Standard V			Standards VI and VII		
Marks for 3 tests in substance memory	No. of girls	Average memory mark	Average imagination mark	No. of girls	Average memory mark	Average imagination mark
Over 120	—	—	—	3	127·3	239·3
110 to 120	6	116·8	165·0	9	114·9	218·5
100 to 110	5	107·4	140·6	3	105·7	183·3
90 to 100	12	95·3	129·0	9	95·8	177·5
80 to 90	6	84·7	114·3	2	88·5	133·0
Below 80	3	67·3	135·0	4	74·7	183·7

It is evident from the above table that high positive correlation exists in both classes between memory and imagination. Only in the case of the lowest section in each class is the imagination mark *against* high correlation. The lowest section of Standard V contained J. S., whose memory marks were 19, 25, 29, and whose imagination marks were 56, 68, 60. She was 30th in memory order, and 4th in order of imagination. Such cases are very rare, but their existence is evidence, if evidence be required, that, even among school children of this age, there may be a wide divergence between the powers of an individual in these two functions. Three of the four children comprising the lowest section of Standards VI and VII, whilst having very low marks for memory, have marks above the average for imagination; but in this class we have none of those extreme differences in rank for the two functions which characterized the work of J. S. in Standard V.

The coefficients of correlation have been worked out from the individual cases for both classes on the Pearson "r" formula. In Standard V, $r = + \cdot 48$, with a probable error of $\cdot 09$, and in Standards VI and VII $r = + \cdot 43$, with a probable error of $\cdot 1$.

VI. Fourth Series of Experiments. School "N."

A fourth series of experiments was carried out with much younger children in the girls' school previously mentioned. The tests were given by an excellent teacher, who was, however, unaccustomed to experimental work.

The work was done with a Standard II class of very various ages, containing many old and backward children. These latter children would almost certainly never reach the highest standards of the school at all. We should therefore be making no comparison useful for our present purpose by including them. But all the children in the class who would be between eight and nine years of age at the end of the educational year (then five months distant) would, unless they left the neighbourhood, subsequently reach the highest classes. The results that follow are limited to this particular age group in Standard II.

(i) *Chronology of the experiment.*

All the tests began at 11 o'clock in the morning and each lasted some twenty minutes; but no child was stopped before she had written all she could.

Four tests were given in substance memory on the 14th, 20th, and 27th of February and on the 5th of March, 1908.

Four tests were given in the invention of stories on the 12th, 18th, and 25th of February and on the 3rd of March, 1908.

(ii) *Tests and methods of marking.*

The stories were read to the children once, after which they wrote down what they could remember. A specimen story follows:

"One day the door of a bird's cage was left open, and the bird flew out. A cat which was in the room ran after the bird and caught it in its mouth. It did not hurt the little creature, but carried it gently to its mistress and placed it at her feet."

The test was marked on the same system of units as would have been adopted for the work of the older children in the school. I do not regard this as satisfactory; I ought to have adopted a scale of marking which would have contained more units. Perhaps, however, the comparisons may be clearer if the same sort of units are used for the three classes.

As in previous tests for inventive work, the children were required to write a story containing certain words: one set of such words was the following:

Dog, clock, basket, man, children.

The tests were marked as in previous cases. The lack of continuity in the stories was one principal cause of low marks; it was necessary to discard entirely the first set of papers (though there had been a previous preliminary practice experiment), as these young children either could not easily grasp the fact that continuity was required, or, more probably, could not easily fulfil the two conditions, namely, to get the given word rightly used and include that usage within their story. Some of the given words were omitted. Two marks were subtracted for each such omission. But this subtraction I do not consider satisfactory and, in subsequent work with young children, have refrained from it.

(iii) *Results.*

The correlations are calculated on the totals of marks for four substance memory tests and three imagination tests.

TABLE XV.

Showing the general correlation between memory and imagination.

Marks for 4 memory tests	No. of girls	Av. memory mark	Av. imagination mark
Over 45	2	48·5	48·5
40 to 45	8	42·1	42·4
35 to 40	11	37·7	39·0
30 to 35	7	34·1	37·4
25 to 30	5	29·4	42·0
Below 25	3	18·7	32·3

Again we find that, with the exception of one section, there appears a general positive correlation between the marks for memory and those for imagination. Worked out from the individual results by means of the Pearson "r" formula, the coefficient of correlation is found to be +·276 with a probable error of ·1.

VII. FIFTH SERIES OF EXPERIMENTS. SCHOOL "W."

A fifth series of experiments was carried out in a municipal boys' school in a fairly good neighbourhood. The work was done with the whole of the Standard VII class, of an average age of 12 years 11 months at the commencement of the experiment. Three tests were given in substance memory and three in invention. All these tests were administered by myself, and I was present with the boys during the whole time that the exercises were being worked. The investigation was limited to the establishment of the correlation between the memory work and the work in imagination. A very brief summarized account of the experiment follows:

(i) *Chronology, tests and methods of marking.*

On Thursday, November 19th, 1908, at 10.50 a.m., a newspaper passage about Winter was read aloud three times, one minute being given to each reading; and, immediately after, the boys wrote down what they could remember. Every boy had finished writing in fifteen minutes, and, after an interval in the playground (ten minutes), returned to the class-room, and wrote an imaginary account of Summer, which was required to contain the following words: poppies, sea, insects, hot, blue, ships, dead, flowers, newspaper. Fifteen minutes were allowed.

On Thursday, December 3rd, at corresponding times, a substance memory test was given in a similar way. The test was a passage from a geographical text-book. The invention exercise was geographical also. The boys were required to describe an imaginary island, which they were to call "My Island," and their account was required to contain the following words: island, mountain, shores, springs, plains, hot, cape, river.

On Thursday, December 17th, at corresponding times and in similar ways, a third memory test and a third invention test were given. The memory test was chosen from a history book, the first paragraph of which ran as follows:

"At Waterloo, Wellington halted while Blucher, whose army was in excellent condition, notwithstanding the defeat at Ligny, marched with all possible speed to join Wellington. But before the junction could be effected, Napoleon attacked the British (June 18th) and for five hours strove to break their line. The last desperate attack he made was repulsed, but perhaps he might have succeeded afterwards, had not the Prussians come up just then."

After the usual interval, the boys were required to write an account of an imaginary battle in which the following words were to be used: army, hill, artillery, victory, cavalry, fight, captured, brave.

The tests were marked by precisely the same methods as those previously described.

(ii) *Results.*

I do not propose to try to show separate correlations for geographical memory and geographical imagination, nor for historical memory and historical imagination. One test in each department of knowledge would not be sufficient to base such a correlation upon. But it is worth while to correlate the general results of memory work in three different fields with the general result of imaginative work in the same three fields. The results of the work of seven boys who had been absent on one or more occasions were excluded.

TABLE XVI.

Showing the general correlation between memory and imagination.

Total marks for substance memory	No. of boys	Av. memory mark for 3 tests	Av. imagination mark for 3 tests
Over 55	4	58·0	80·2
45 to 55	7	50·7	64·1
35 to 45	11	40·6	59·6
25 to 35	7	31·0	52·9
Below 25	1	24·0	33·0

High positive correlation is evident, even from a cursory inspection of the table. Worked out from the individual cases on the Pearson "r" formula, the coefficient is found to be + ·623 with a probable error of ·07.

VIII. Summarized Conclusions.

1. There appears to be considerable positive correlation in school children between the two functions or sets of functions employed in memorizing the substance of stories and in inventing stories under given conditions. This correlation appears to be higher in the case of the more proficient classes and lower in the less proficient classes, so far as this evidence goes. In the case of School "N." the correlation

appears higher in Standard V than in Standards VI and VII (one class). This is possibly explained by the fact that the Standard V class is an upper section and does not contain the whole of the Standard V children.

2. It appears from experiments in School "O. K." that children practised in substance memory for stories become thereby more proficient in the invention of stories. The improvement is not due to the insertion of parts of the content of the memorized stories within the invented stories, but to some community of function less atomistic.

3. It appears from experiments in School "S." that children practised in substance memory up to fatigue point, which is taken here to mean the point at which consecutive exercises cease to produce improvement, are thereby prejudicially affected so far as their power to invent stories is concerned.

4. The fatigue effects in School "S." appear to be temporary, whilst the practice effect (improvement through practice) appears to have considerable duration.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

Jan. 21, 1911.	Mental Tests applied to the Insane, by B. HART and C. SPEARMAN.
	Is Memory a Faculty? by W. G. SLEIGHT (introduced by C. SPEARMAN).
	The Function of Generalisation, by F. AVELING (introduced by C. SPEARMAN).
	New Apparatus for the Localisation of Temperature Spots, by A. WOHLGEMUTH.
March 11, 1911.	Emotions and Morals, by W. BROWN.
May 6, 1911.	The Psychology of Genius, by CARVETH READ.
	Some Observations concerning Colour Vision in Dogs, by Miss E. M. SMITH (introduced by C. S. MYERS).
	Some New Exercises in Reasoning suitable for the Mental Diagnosis of School Children, by W. H. WINCH.

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VOLUME IV SEPTEMBER, 1911 PART 2

THE ELEMENTS OF EXPERIENCE AND THEIR INTEGRATION: OR MODALISM.

BY HENRY J. WATT.

§ 1. Introduction.

A considerable time has now elapsed since psychology in its turn awoke to the new vigour of life that the experimental method brings to every science. Inquiry has been pushed into every part of the field and multitudes of new facts have been made known. These have been arranged as well as possible to show how one is dependent on the other or what is the joint effect of several. But nothing like a coherent body of knowledge has emerged therefrom. It is a common complaint that psychology is a medley of all sorts of curious and commonplace facts, which can hardly make a show of the coherence that is expected of a science. In many ways there seems to be no difference between it and physiology. Psychology stands at a certain disadvantage in this respect. For although the elements of its subject-matter are for it pure data, given without the possibility of further question, yet from the point of view of the biological sciences and perhaps philosophy in general, experience is an effect conditioned by physical and physiological circumstances. It is a curious fact that many prominent workers of the sister science of physiology have recently claimed the right to expel consciousness entirely from the scope of their subject-matter and from their list of conditions and results. However foolish and impossible this may be for the physiologist as inquirer, much may be said for it when the systematic ideal of causal explanation is the goal in view. For a mere knowledge of condition and effect is never quite satisfactory. It is this same spirit which has led some psychologists to banish any mention of body or brain from their treatises and to aim at a pure science of experience. Thus far, perhaps, the burden has lain more lightly on the physiologist, upon whom the stringently closed system of causes of the physical sciences acts with great compulsion. For the psychologist, however, it is no easy matter to set himself free. Only in respect of the intellectual and emotional parts of experience has it been attempted with any success, for we are, in any case, almost quite ignorant of any detailed connexion between these and physiological conditions. The psychology of the senses, on the other hand, cannot be loosed from physiology in any high-handed manner. By themselves, sensory experiences seem very erratic and peculiar. They seem too much the product of other influences and too independent of one another to form a closed field in themselves. And yet, as it stands, psychology cannot but be ashamed of its feeble command of the senses. Its knowledge of them is hardly more than a mere bundle of clippings from physiology. And when the attempt is made to treat sensory experiences purely as such, the chapter on the attributes of sensation which results is so dry and barren, that it is condemned and omitted altogether by not a few writers.

A fresh attempt must be made to secure the independence of psychology. This will have little value unless the province of sensation is first attacked and freed from the domination of physiology. No general demand or principle will meet the case. It must be shown that physiology can make no positive contribution to the special work of a psychology of the senses and that the seemingly scattered and incoherent material of sensory experience is capable of self-complete and satisfactory systematisation. Only thus may independence properly be claimed. For we shall then have proved that psychology not only must, but can stand alone. It is, indeed, not to be forgotten that psychology and physiology are, in certain respects, closely attached to one another. Physiology provides a basis for experimental interference with experience which is invaluable to psychology, while psychology acts to some extent as a feeder to physiology. But however complete may be the parallelism between the two regions which general considerations lead us to expect, the promise of a comprehensive science of psychophysics seems to lie rather in the independent development of the two contributory sciences than in their narrow companionship.

It has been claimed sufficiently that introspective observation is the primary method of psychology. But every advance in the science sends us back to a more minute and observant pursuit of the method. If we are to convert our scattered sensory experiences into a coherent system, we must re-examine the whole field. We must note with all possible accuracy every variety of experience simple or complex. Every complex experience must be analysed into its simplest parts. But in doing so, we must not fail to observe whether the process of analysis destroys any feature of the complex experience, whose origin in the elements of our analysis we are unable to explain completely by synthesis of these elements. Just in this respect has psychology been in grave fault. We have been satisfied to know upon what conditions our complex experiences and their modifications rested, but we have not tried sufficiently to show how the elements of our experience combined to give our complex experiences. This must be attempted again and there can be no rest for our science till it is accomplished.

In those cases in which our experiences show variation in respect of any characteristic, the strict method of introspection seems to suffice.

Thus we know that some sensations have certain attributes in common, intensity, extensity and the like. But a number of cases are in dispute. Some sensations do not seem to share these attributes and some others seem to have peculiar ones of their own. Introspection is thus obviously insufficient to meet all cases. We must find another method whereby the properties of the elements of our experience can be determined. Like the chemist and the physicist we can find this only in a detailed study of the compounds into which these elements enter and of the manner in which they join to form complex experiences. We must know not only what are the elements of our experience and what are their essential properties or characteristics, as far as is possible by direct inspection of them, but we must also know the manner and method of their integration and other elements, so that by this knowledge we may be enabled to complete and perfect our knowledge of these elements.

But that is not all. Our knowledge of these elements and their properties must enable us to understand completely how, when a certain complex of elements is given, a certain complex experience results therefrom. No characteristic of the latter must remain unexplained. We must be able to give assent to a statement in the form of an equation, that this or that arrangement of the elements of experience, as we find them given, is wholly and completely identical with a complex experience. The notion of transcendence must finally be banished from any self-respecting science of psychology. In a word, we must be able to show the presence of causality in experience. That we have not been able to do so, has undoubtedly been due to the fact that the complexities of experience were taken too much for granted, that analysis was the prevalent method of study and that no attempt was made to show the connexion between the attributes of the elements and the integrative complexes of experiences and their modifications. No use was made of the attributes except that of unprofitable definition. It must speedily become clear that in psychology, as in the natural sciences, the problem of the elements and their properties is second to none other. Far from being dry and useless, the problem of the attributes must become a centre of the most vivid psychological interest. There can be no doubt of its difficulty. We must follow the example of the sister sciences of nature and converge the efforts of all pure mental science upon the problems of the constitution of experience and its fundamental laws.

§ 2. PRELIMINARY DEFINITION OF SENSATION.

A definition of sensation may be attempted by reference to its psychological peculiarities or to its psychophysical basis or to both of these. But as sensation is generally considered to form only a part of experience, it is hardly possible to begin study of the varieties of experience by enumeration of its purely psychological characteristics. For we do not yet know what these are and we must be able to point out sensation to one another ere we reach any sort of unanimity regarding its psychological definition. Besides, even although we may finally ascertain that certain psychological features are common to all sensations, there are difficulties in the way, which would become the greater, if we could not delimit the matter of sensation by other means. We must therefore turn to psychophysical means to fix our subject. We have sufficient security, if we find a means of pointing out to each other which of the whole mass of experiences we call sensation and intend to study. That means we find in the sense-organ and in its stimulation. We may accordingly define sensation as the simplest parts or elements of those experiences that are immediately and regularly dependent upon the stimulation of a sense-organ. Such a definition as this accords well with the practice of an experimental science. By means of a periodic recurrence of the stimulation and consequently of the sensation, it is easy to direct the attention of the observer towards the experience to be observed, while all possibility of confusion with other experiences which might be evoked along with sensation can be avoided by the observation of the parts of the total complex which recur regularly and without the mediation of any other experiences. Sensations are not attached to any other experiences as if they depended upon them. They form, at least in part, the groundwork or foundations of experience. As sensations are dependent upon the stimulation of sense-organs, they are clearly largely independent of such influences as attention and abstraction, so that we run less risk of error in starting our study with them. Having obtained a definite reference within the whole, we have provided ourselves with a means towards definite study of the other varieties of experience, as well as with a gauge for such variable influences as attention.

It is often said that there is no such thing as pure sensation to be found in experience and that sensation, therefore, exists only as a psychological abstraction. Without knowledge to seize hold of it and convert it at least into perception, it is declared, pure sensation would be pure nothing, as unintelligible as is to the idealist an object independent of the mind which knows it. In the face of such an extreme view, any attempt to study sensation would be futile and objectless. But whilst we may admit that sensation never does in us occur as an object of study unless it evokes other mental processes than itself, whatever they may be, we may yet maintain that sensation is a real object of study. In the introspection of sensation, our observation is directed upon sensation as defined, in whatever setting it occurs. Sensation may often be observed to remain constant in character under differences in the introspective processes directed upon it. Besides, our definition of sensation does not call for the isolation of sensation in experience, but only for its isolated study.

While defining sensation in the first place by reference to the stimulation of a sense-organ, we do not forget that we are by no means sure of the position and nature of the sense-organs of all well-accepted sensations. Yet we are justified in regarding them as sensations, because we can verify their immediate and regular dependence on external stimulation. We know from obvious examples of sense-organs, how specific in quality and point of action the stimulation must be that is to affect a sense-organ and how dependent its success is on the integrity of afferent nerve-fibres.

The regulative simplicity of the definition refers in the first place to the experimental procedure implied therein. Simplicity of some kind will, of course, also be a characteristic of the psychological nature of the sensational element. But we cannot presume upon the ultimate psychological definition. We must just carry experimental analysis as far as we can, although we cannot hope to find therein any means of judging whether our analysis is complete. For a mere *ne plus ultra* cannot form a systematic criterion. Only in the psychological characteristics of sensations can we expect to find some such standard, the formulation of which will then constitute the psychological definition of sensation. Our only guide will therefore be the typical uniformity of sensation.

§ 3. The Independence of Psychological Investigation.

The chief classes of sensations are (1) those of cutaneous origin, touch or pressure, warmth, cold and pain; (2) those of taste, of which there are four chief varieties; those of (3) smell; (4) sound; (5) vision; of each of which there are an indefinite number of varieties, which differ only slightly from one another and, except in the case of smell, can easily be arranged in order of resemblance; those of (6) articular, and (7) labyrinthine origin, each generally recognised to consist of two groups, sensations of position and sensations of movement; (8) those of muscular origin; and finally, (9) a crowd of less varied and obscure sensations.

All these varieties of sensation are said to differ from one another in quality. If the difference in quality is so radical that it is impossible to pass from the one sensation by gradual or imperceptible steps to the other, as *e.g.* that between tones and colours or between warmth and cold, it is sometimes called a difference in modality. It is generally considered to be of great systematic importance to determine all the possible varieties of quality and to arrange them as such and in relation to sense-organs and stimuli. Distinctions of quality are, of course, in all cases based primarily upon true introspective differences between sensations, but there is as yet no clear test of quality. Consequently a number of cases have been long disputed. Is pitch, for example, the quality of tonal sensations, and in what respect do the articular sensations from various joints differ? We must not forget that sensations differ from one another in other ways than quality and that, unless we are guided by some defensible criterion, we may mistake these other aspects for quality. This is most to be feared in those cases in which no variation in quality is really present. Any apparent variation is so readily ascribed to quality. The conclusions we shall reach later will show that the present classification by quality is not sufficiently critical and does not lead to useful systematic results.

In spite of the primacy of the introspective basis of quality, the relation to the sense-organ still exercises a fairly strong influence upon the distinction of qualities. It is generally admitted (1) that from one and the same sense-organ only one single quality or a group of closely allied qualities can be evoked. Conversely it is held (2) that each marked difference of quality or each difference of quality, no matter how slight, so long as it cannot be obtained from a mixture of other qualities, *e.g.* those of tonal pitch, implies a different sense-organ. It might therefore readily be supposed that the existence of different systems of sense-organs implied some qualitative difference between the corresponding sensations. Such is not the case. The above statements are still true even if the same quality of sensation may be evoked from different systems of sense-organs. Experimental research of recent years has, in fact, distinguished a superficial from a deep system for touch and pain, and a protopathic from an epicritic system for touch, warmth and cold. It is, conversely, a familiar fact that similar series of visual sensations—all tones from white to black of fair intensity—may be evoked by the medium of different sense-organs, namely the rods and cones of the retina. There are no marked differences of quality between the sensations from these different systems of organs, but there is a tendency to interpret any obscure difference as qualitative (23, p. 36). In the case of labyrinthine sensation a qualitative distinction is usually made between sensations of position and sensations of movement and is justified by reference to the anatomical and functional independence of the two systems of sense-organs and the resulting independence of the two groups of sensations. The reasons generally given for the distinction of two groups of articular sensation (difference of threshold, different relation to galvanic interference) also imply a reference to sense-organs, although no duplicity of sense-organs has yet been established. We shall consider these arguments later in more detail. It is sufficient to point out here, that even if they were valid, they would not effect their purpose. The sensations in question, even though they were evoked from perfectly distinct systems of sense-organs, might still be of identical quality. Any difference in quality must be decided purely and solely upon the basis of an examination of the psychological characteristics of the sensations concerned.

The same conclusion is applicable to the sensitive areas of the eyes, ears and nostrils and to the multiplicity of individual sense-organs found in all senses. Each of these repetitions, of course, has its own special use. We may also expect them to be represented psychically, but we can only determine the nature of this psychical differentiation by psychical methods of examination and comparison.

Sense-organs may be reduplicated for other reasons than distinction of quality. A differentiation in respect of the intensity of stimulus may sometimes be necessary, as in the eye, where the rods respond efficiently to a stimulus which evokes no reaction from the cones, while the cones respond comfortably to stimuli which overwhelm the rods and necessitate their instant withdrawal from the full severity of the stimulation.

In the same way, we find in the sense of temperature a double form of apparatus, of which one, as judged by the sensations evoked from it, seems to respond vigorously and diffusely to all effective stimuli, while the other has a wide range of response and adaptation. There is no reason in this fact why the two series of sensations should not be closely related psychically.

We are therefore compelled to make our study of experience to a large extent independent of the physical and physiological study of the sense-organs. There is certainly a close relation of dependence between sensation and sense-organ, but we must beware of expecting a continuous parallelism between the arrangements of each. The very complexity of the more central arrangements of afferent fibres should warn us of this. We must therefore conclude that a reduplication of sense-organs of allied function may determine a variation of sensation by quality or by intensity, by extensity or by localisation or by some other aspect. What the variation in any case will be, we cannot tell by mere examination of the sense-organ. We must examine the experience itself. Not even in cases of doubt can we safely allow ourselves to be guided by consideration of local or functional separation in the sense-organ. We can expect to settle the classification of a disputed aspect of experience only by a direct study of it or by comparing it in form and function with other similar experiences. Our psychological interest lies only in the forms of variation of our experiences and in their functions as experiences.

Still more distant, therefore, must our interest be in those physiological processes inherent in the sense-organ which produce no new variation of sensational experience, *e.g.* adaptation, positive and negative after-effects, and contrast, while theories of the adequate or proximate stimulus to the sense-organ have no psychological significance at all. Our only interest in the sense-organ lies in the fact that it somehow makes a certain form of experience possible at a certain place and time.

§ 4. THE TYPICAL CHARACTERISTICS OF SENSATION.

Typical characteristics are often distinguished and are commonly known as attributes, which are said to be inseparable from sensations and to be variable independently of one another. These attributes have hitherto been determined solely by mere direct inspection of the elementary sensations themselves and, as commonly accepted, include intensity, extensity, duration and perhaps order or localisation. Feeling-tone, which may be pleasant or unpleasant, has often been included amongst the attributes, but is now generally treated as an elementary kind of experience, qualitatively different from all sensations (14, p. 227). It is recognised that sensations often occur which are indifferent in respect of feeling-tone, which, in other words, are devoid of it. Besides, even when one and the same stimulus is used to evoke sensations, feeling-tone varies so much in different people, that it might well be considered to be another kind of experience not directly and immediately dependent upon the stimulation of a sense-organ. Of course, a state cannot at one time occur with, and at another time without, one of its attributes, if this word attribute is to have its usual meaning. It is possible, however, that the separability of feeling-tone from sensation is only one of many indications that the various modifications of experience, of which the attributes form one group, are capable of much more freedom and complexity than has commonly been supposed.

(*a*) *Intensity.*

If attributes are inalienable accompaniments of sensation, we may expect to find them in the most elementary sensations—in those evoked by the stimulation of the simplest elements of sense-organs that can be functionally distinguished. Although it is by no means easy to determine the elements of even the comparatively simple sensory apparatus we find in the skin, yet it may safely be maintained that, as far as we know, a variation in the sensation from the simplest parts yet distinguished of accessible sense-organs like those of the skin, tongue and eye, is possible. This direction of variation or attribute is that of intensity, which is produced typically by an increase in the amount of the physical stimulus acting on the sense-organ. It is a peculiar fact that the lowest degree of intensity of similar sensations is not always so comparable as we should expect. The stimulation of a protopathic area of skin, for example, always evokes, when effective at all, a more vivid sensation than does the stimulation of normal parts. Weber's law, in fact, seems to hold only for the epicritic system; the variation of intensity found in the protopathic system is much more limited and rigid (23, pp. 50, 106 f.). The minimal degree of intensity produced by certain sense-organs cannot therefore possibly be considered to be the absolute psychical minimum for that sensation; hence it is illusory to say, as many do, that when the intensity of a sensation is reduced to zero, the sensation disappears, for we have no conceptual means of determining the degree to which any given minimal intensity approaches zero.

We also find that certain sensations vary in intensity very little or not at all. Such are the labyrinthine and the articular sensations of position in particular, and also some of the less frequent, miscellaneous sensations. Yet we could hardly maintain that these sensations offer to introspection no aspect of intensity. Only it is particularly hard for introspection to seize hold of any aspect of experience that cannot be varied; for it is just by variation, especially in definite relation to changes in the evoking stimulus, that an experience offers itself best to introspection. We may therefore admit the presence of intensity in all sensations, except perhaps those of vision, where, though apparently present, it seems to certain psychologists to be merged in quality.

Extensity.

Besides intensity there is no other obvious variation in the sensation dependent upon one and the same sense-organ. But another attribute can be made clear if we evoke the same quality from neighbouring sensitive elements of the same kind. Sensations of the same quality from neighbouring sense-organs stimulated simultaneously fuse with one another and give rise to a more extended sensation of that quality. In this form the aspect of extensity is easily observed, so that we can now detect its presence in the sensation from the elementary sense-organ, although it is practically devoid of all variability there. Differences in extensity can be traced between the correlated sensations of cutaneous origin. The sensations from skin-spots are undoubtedly extended; but that of warmth is certainly more extensive than that of cold and cold than that of touch or superficial pain. All protopathic seems to be much more extensive than epicritic sensation. Possibly extensity is now, for the most part, in that rigid undifferentiated condition in which we find intensity in the protopathic cutaneous, and in some other senses. It is hardly variable and therefore difficult to observe in labyrinthine and articular sensations of position and in the less frequent sensations. Some of these are, however, more or less massive or diffuse, so that we need not doubt its real presence in them. In smell it seems also to be latent. In sound it takes the form of voluminosity (*v.* later, p. 143).

In one group of cases only do we find a variation of extensity comparable to that of intensity. If *e.g.* the two eyes are directed in varying degrees of convergence upon two pictures which together give a clear binocular picture, it will be seen that the combined figure seems much smaller and nearer when the convergence is great and progressively larger when it is reduced. It is not possible to measure this variation of extensity, as we measure lines, by laying a graduated measure against it, for the measure itself changes in apparent size with the change of extensity of what we measure. We can only compare these variations in extensity as we compare different intensities. This form of variation, like intensity, does not involve any change in the number or identity of the sensitive elements stimulated; for there is no change in the visual stimulus corresponding to the change of extensity. The changing stimulus lies elsewhere, probably in the kinaesthetic sensations connected with convergence and divergence.

Between extensity and intensity we find very often that there is less independent variability than the usual definition of attributes requires. An increase of extensity often leads to an increase in intensity and *vice versa*, so that in the judgment referring to the stimulus and based on sensations, an increase of the one in the sensation may lead to the judgment of the increase of the other in the stimulus. Explanations of this reciprocity of intensity and extensity suggest that neighbouring stimulations overlap to some extent and so become intenser, or that an intense stimulation radiates and so becomes more extensive. These explanations are, of course, physiological and not psychological. But the slight correlation of intensity and extensity thus given does not seem to call for any psychological explanation.

There are certain exceptions to the rule for extensity just given. The two ears and the two eyes are not two neighbouring sense-organs of the same kind; they are rather two sensitive areas or masses of sense-organs. When they are combined to certain special uses, other modifications of sensation than that of extensity appear. Extensity is not obviously given in sound; the same quality of sound does not appear in different extents, although tones of different pitch vary from one another in voluminosity. Probably the two nostrils act in ways analogous to that of the two eyes and ears, but our sense of smell is so degenerate and our knowledge of it so limited, that we may even suppose we make little use of the powers we have. Further consideration of these cases must be postponed.

Order.

But even when two sensations are of the same quality, intensity and extensity, they can easily be distinguished from one another. Let, *e.g.*, two spots on either hand be isolated and stimulated in the same way. We can tell at once that they are two and from what parts they came, so to speak. We know "where they are." It is a familiar fact that the primacy of this local aspect of sensations has long been the subject of debate. And it may safely be said that the nativistic theory is in so far correct, as some sort of inalienable aspect, responsible directly or indirectly for localisation, must be attributed at least to some sensations. Otherwise it is not evident how any sensations should ever come to be located. For differences of quality are not introspectively identical with those of localisation, or they would not be so easily distinguished from one another. If they are the same, there must be two kinds of quality—true quality and localisation-quality—which is the same thing as before. If it is meant that slight variations of quality combine to form localisation, it is not at all clear why just localisation and not some other form of experience should result from the combination. On the other hand, if only some sensations possess a local sign, it is not evident how the significance of that sign is to be transferred to any other sensation, even if the latter happens to accompany it regularly. It is not even evident that such a transference could take place without any mechanism, on the basis of mere contiguity in experience. For how could we expect experiences to attach themselves to one another, not to speak of interchanging characteristics, merely because they occur together? The only way in which they might do so is by mere mechanical association. We could not then expect to find that one would be for our experience attached and referred to the other as belonging to it, or that out of the connexion some new modification of experience should arise. For how could we claim to understand or to explain such occurrences? A science of psychology would here be faced with the unintelligible and irrational. The problem is merely a case of the general problem which is the object of our study: if a modification of experience is not common to the primary elements of it but arises with their combination, how does it so arise, and upon what aspect of these elements is it based? The formulation of such a problem calls for a vigorous protest against the admittance of irrational sequences in experience. The natural consequence of admitting such possibilities is the abandonment of every attempt to resolve them for psychological science. Salvation from such hopelessness could only be brought by some happy accident of experimental research. But if the mind is the instrument of rationality, we may at least expect it to be itself thoroughly amenable to rational, scientific treatment. And science cannot stop at the determination of mere dependencies; that would be a blind science, a science without the light of causal statement and conviction.

For the present, therefore, we shall accept naïvely the presence of a distinguishable aspect in sensations localised in different parts of a sensitive area. There is no doubt that they can be distinguished primarily and in isolation, without the help of any sort of special association or inference. But it may be said at once, that this aspect of elementary sensations, though it undoubtedly distinguishes each element from others of the same kind, is capable of development along its own line, like any other attribute. There are complex, separable forms of localisation-consciousness, just as there are of quality, intensity and extensity. The success of our view will depend largely, if not wholly, upon what we can do with such a starting point. Its sufficiency and correctness may be questioned at present, if only for the reason that the single method of introspection, as we have already pointed out, is liable to error in dealing with the less variable attributes of sensory experience. But the results which our starting point leads to will ultimately justify it.

This third aspect of sensations we will call order. It is to some extent a form of individuation, by which sensations are differentiated, in the first place in relation to others of the same quality, but also ultimately to those of a different quality. But order is essentially an aspect of sensation, perfectly comparable to intensity and extensity. We might call it place in the mind, if it were not that place tends to imply that the mind has some real spatial extension, at certain points of which the sensations are to be found, whereas we have to remember that order is a place-aspect of sensations, which it qualifies, without any relation to real *locus*. Order is therefore the better name, as it involves only the idea of distinction, relatively to others of the same quality, intensity and extensity, by means of an aspect of arrangement inherent like these attributes in the sensation itself. The order of every sensation is fixed relatively to all others present, but does not depend upon the number and kind of these. Two sensations are not as such of neighbouring order because they are alone together in consciousness. They may still be of very different order. This attribute of order is much more important than any of the others for the development of experience and especially of the higher reaches thereof. It is the basis of all kinds of localisation and of many other complex modifications of experience.

Order is present in the form of localisation relatively to one another in all sensations, except those of sound and smell. In the case of articular and labyrinthine sensation, it seems much more advisable to treat their differences as differences of order rather than of quality. How should we otherwise be able to treat them as a system of interrelated positions? Sounds are, of course, localised in space, but this localisation is known to be not a primary peculiarity of these sensations, but an integration based on the simultaneous use of the two ears in ordinary cases and perhaps also often on differences of timbre or harmonics. Sounds are not localised at a certain point of a sensitive area; indeed it is improbable that each ear contains neighbouring sense-organs of the same kind. Sounds, however, assume an order relatively to one another in the form of pitch. In smell we are unable to suggest any primitive aspect of order. Smells are localised by a secondary indirect process, similar to that of sound and most usually by variation of intensity, consequent upon turning or approaching the nose towards the source of smell. Have we now so little versatility in smell that we cannot pick up the lines of their order? It is impossible to say. Our very ignorance regarding smell constitutes by itself one of the most difficult of psychological problems. The two ears, the two nostrils and sometimes the two eyes do not afford us sensations which differ in order, so that sensations of the same quality from each of these pairs of parts do not give rise to the same modifications of sensory experience, as do sensations of different order from a single sensitive area.

Other aspects.

To the above three attributes a fourth—order in time, duration, or protensity (24)—is sometimes added. There are many good reasons why such an aspect should be expected. But many difficulties lie in the way of its study. It is for one thing very hard to decide the simple introspective problem whether the order in time of an experience is a true attributive aspect, or is merely position in real time, identical with the temporal succession of events in the material world. Two sensations may be of the same quality, intensity, extensity and order, and yet be distinguishable. But is this distinction not a purely conceptual one? On the other hand it may rightly be asked whether any such conceptual distinction could be made, unless it had first a basis in sensational experience itself. The answer to this very important question is one which will be decided largely by the results of a study of those attributes which are clearer, as well as of the complex modifications of experience which result from them. For the present, we may without inconsistence decide in favour of an attribute of time-order or protensity of some kind, without attempting to give it a precise characterisation. Indeed, further study may give us reason to look for and find still other attributes of sensation than those enumerated. The problems raised in this paper can only be worked out in detail for a few cases, but it will be evident that they are very general. The study of the one will act and react upon that of the other.

(*b*) *Difficulties.*

Study of the attributes of sensation allows us to institute a comparison between such senses as those of cutaneous origin and that of hearing, which gives results of great advantage to the further study of the modifications of sensation. This comparison is based in the first instance upon introspective evidence, and finds its further justification in the psychological results which emerge from it.

Cutaneous sensations are varied by the attributes of intensity, extensity and order, but they show little variation in quality. Hearing, on the other hand, has a great variety of distinguishable "qualities," which are undoubtedly capable of variation in intensity. But these qualities do not seem to be extended after the manner of cutaneous sensation; and in so far as only simple forms, poor in harmonics, are presented to one ear, they seem to be devoid of any sort of localisation or order. Such is the result of what might claim to be a simple unreflecting comparison of cutaneous and auditory sensation. Sound seems to diverge more from the probable "type" than any other kind of sensation.

Closer consideration, however, leads to a very different conclusion. Instead of having given a purely unreflective, unbiassed judgment, we may possibly have been influenced very strongly by a comparison of results. Have we not really been comparing the outcome of integration in cutaneous with that in auditory sensation? Have we not, in fact, argued that, if sound had extensity, it ought to give us the sort of spatial extensiveness that we find in touch and vision, and that, if the elements of sound from one ear had order, they should arrange themselves over our skin or in space around us, unaided by the other ear or by differences in timbre? That we have indeed done so we shall best and easily find out, if we ask the question, whether the aspects inherent in sound, both in their primitive and in their complex forms, are closely comparable with those of touch and vision. If they are really comparable, we must, of course, look for some reason for the differences between touch and vision which influenced us in the first instance. If we can find it, we shall have clear proof that we were influenced in our argument by a principle we failed explicitly to state or to adopt: that like attributes should lead to like integrative results.

Sound.

Are the attributes of sound and touch, then, exactly comparable? There can be little doubt but they are. The manifold "qualities" of hearing are capable of the most definite arrangement in one continuous series of tones from the lowest to the highest pitch. No other "qualities" fall of themselves into so precise and unmistakable an arrangement. What evidence has introspection to offer against the classification of these differences as differences of order? On the contrary, introspection can justify such a treatment now, as it did long ago. "Till the time of Aristotle tonal qualities were considered essentially as a *ποσόν*, not as a *ποιόν*" (25, vol. I. p. 136, note). In fact, in spite of definite rejection of this view, it is hard for the adherents of the qualitative view of pitch to suppress the tendency to treat it as order. The qualitative order of tones is said to be "analogous" to the spatial (*ibid.* vol. II. p. 55). Mach, indeed, traces an analogy between the fixation of spatial points and the fixation of tones (18, pp. 182 ff.). Let us, therefore, frankly treat pitch as order and see what the result will be.

Tones are generally recognised to vary in voluminosity progressively, the deepest having the greatest, the highest the least volume. Pitch and voluminosity cannot be identified with one another, for we are able to discriminate differences of pitch much more keenly than differences of voluminosity. Several tones of the same or of different pitch sounded together do not give an increase in voluminosity, as we should expect. They fuse in extensity as little as do the extensities of tactual sensations from the two hands. But for the former there may be forthcoming as good an explanation as we can give for the latter. The facts, therefore, should not prevent us calling voluminosity the extensive aspect of tone, if we are justified by introspection in doing so.

If we recognise, then, that, just as all tactual sensations have the touch-quality in common, so all tones share the same sound-quality, we shall have our full complement of attributes: quality as such or mere sound, order or pitch-place, and extensity or voluminosity. Beyond these three aspects, tone does not seem to have any other characteristics than intensity and those that are the result of the integration of different tones. Even timbre is shown to consist of, or to be actually analysable into, separate tones, each provided with the aspects we have enumerated.

The peculiarity of tone is that of these attributes two—pitch (order) and voluminosity (extensity)—are mutually dependent variables. It is impossible to run through the variations of pitch without at the same time varying voluminosity and *vice versa.* As tone is elementary sensation and as pitch and voluminosity are its primitive attributes, it is impossible to look for an explanation of this interdependence of attributes elsewhere than in a physiological theory of the sense-organ. Such an explanation, if it is not actually there already, may be said to be in sight at the present time. But it cannot concern us here. We can do no more than acknowledge the introspective fact of interdependence of pitch and voluminosity.

For this very reason it is evident that extensity in tone cannot, as in the other senses, be a variable dependent upon the occurrence of many tone-sensations of neighbouring or different order. For as each pitch or order has a different voluminosity inseparably attached, though easily distinguishable from it, the aspect of pitch will always suffice to segregate its fellow voluminosity from others, even when there might be a tendency for them to fuse in some manner, and *vice versa.* Many tones together, therefore, will not fuse, as sensations of the other senses do, in any way except intensity[1]; and even that will occur, of course, only when the tones are the same in quality in its threefold aspect, *i.e.* in pitch. In other words, tones of different pitch sounded together will always be distinguishable, even if they are not always distinguished from one another. It need hardly be added that sounds fuse together as mere sounds apart from all aspects or attributes.

Many high tones of nearly the same pitch, even when they are consonant, would not therefore give a voluminosity equal to that of a tone of great depth. The voluminosity of all together may very well differ from that of any of them alone, in a way peculiar to such combination, but it will never approach towards that of a deep tone. For this and other reasons it will often occur that many tones together are not distinguished from one another, but they can always be distinguished as soon as the attention is directed upon their order or pitch, their voluminosity, or the integration of these. For if homologous aspects of two tones are distinguishable from one another in isolation, they cannot be completely fused with one another when they occur simultaneously.

We have thus brought the introspective nature of auditory sensation into line with that of all the others except the visual and olfactory. And we have explained the first apparent discrepancy between tone

[1] Or timbre, which we may neglect for the moment, as it is obviously not a characteristic of the elementary sound. Timbre, as an integrated character of tone-complexes, forms an interesting problem for psychological treatment.

and the general type of sensation. But it is generally recognised to be very difficult to decide whether noise is a unique quality of sound or not. Our revision of sound will here also give us a definite point from which to proceed. A noise may be said to be a simple sound whose pitch is not yet audible, because it has lasted less than the time of two vibrations, or a complex sound of many pitches which make each other indistinguishable to the unaided attention. We have good reason to let this definition pass, as we know similarly from the sense of pressure that it takes less time to be aware that we have been touched than to be aware where we have been touched. This is Külpe's law, that "general denominations are more easily reproduced than special" (14, p. 172).

If our analysis is so far correct, we shall expect to find it justified by the nature of the modifications which result from the further integrations of tone-sensations. These integrations should be parallel in mechanism and effect to the integrations of similar attributes in touch and vision. On the other hand, the treatment of pitch as quality defers indefinitely all hope of explaining the facts regarding melody, interval and tonality, besides those of discrimination already mentioned. There is also evidence of a genetic nature to show that the sense-organs of hearing have in all probability developed out of those of a sense with the full number of attributes, viz. pressure. But our argument can hardly lead us to suppose that sensations of hearing have actually developed out of those of pressure. For the skin sensations, whose sense-organs might also be connected genetically with those of pressure, all show differences of quality without any obvious loss or integration of attributes. If any theory of psychical development is suggested by the analysis of the attributes of hearing and their identification with those of the other groups of sensations, it must be one which traces all varieties of sensation back to a common origin or at least to a common type.

Vision.

We have already noticed that visual sensations are characterised by the attributes of order and extensity. Certain observers, however, hold that they are devoid of all intensity. The intensity which is apparently present, it is urged, is really a form of quality[1]. It is clearly impossible to settle this question on its introspective basis alone.

[1] For references, *v.* 28, pp. 21, 324.

We must look for some other ground of argument. It is a further peculiarity of vision that it offers the vast range of progressively different qualities indicated in the colour-body. All other groups of sensations than those of vision, hearing and smell, occur only in one, or perhaps sometimes a few discrete qualities and do not seem to lack any of the usual attributes except perhaps in virtue of their obscurity to introspection. We should hardly venture to urge a plea of obscurity to excuse the apparent absence of certain attributes in vision and hearing, although the plea might hold for smell. It is of interest to recall that the great variety of sound-qualities can be explained by the variation and integration of extensity concomitantly with that of order. Only one form of quality similar to the unique qualities of pain, pressure and most other sensations except taste need be postulated. This integration, further, has to be accounted for by reason of physiological determination not of any special psychological integration of pitch and extensity; for, though correlated differently in sound, these are attached to one another in the same way in all sensations. In pitch we have still obviously a difference of psychical order, now inseparably attached to quality. There seems to be a great variety of sound qualities, though there is clearly only one.

We might therefore surmise that the typical form of elementary sensory experience is such that, when a difference of quality occurs, it is a radical difference and that these elements of experience could not be expected to fall into different classes of very similar sensations, such as those we find grouped together as visual, auditory and olfactory. We should rather expect discrete forms, such as touch, pain and more especially cold and warmth, which, though they are both concerned physically with temperature, have nothing in common as sensations. Primitively we should have one sound experience, one or at most a few unconnected and dissimilar visual experiences and a few for smell[1]. For a sensation which has a number of variable aspects must have some unchangeable aspect. Why should it otherwise be called one? If there are any primitive visual experiences, it is certainly difficult to locate them purely psychologically amidst the flux of qualities. The great variety of visual experiences, therefore, calls for some explanation. This explanation must, however, be left for the

[1] It is interesting to notice that it is in these complex senses only that we find most evidence of physiological integration and in particular the processes of compensation and rivalry. This would suggest that even the four qualities of taste are too many for one sense and that without integration only a single quality is found.

future to bring. The need for a psychological theory of vision is great, for we have none as yet. Our theories of vision run out into pure physics or physiology and leave all purely psychological problems entirely alone.

Smell.

Of all the sensations, those of smell offer the greatest resistance to any form of investigation. The mere difficulty of manipulating the smell stimulus is overwhelming. We have practically no kind of a theory of smell at all, physical or physiological. The slight clues given by partial congenital anosmia and olfactory fatigue have lead to no tangible results. In psychological theory, where we might reasonably expect to be less hindered, we are quite as badly situated. For we have only a tentative and imperfect classification of smell qualities at the best. These seem to be of indefinite number and devoid of all extensity or order. This ignorance constitutes of itself an important puzzle. For if the rich and progressively differentiated varieties of our experience, including those of the senses, are derived from simpler, more abruptly differentiated elements, as we must suppose, it is difficult to understand how this process of integration can be completely hidden as it is in vision and smell[1]. For vision we might suppose that we just do not yet see what is there to be seen. For it is a postulate of our whole treatment that the elements of experience are not lost to view when they integrate to form some new modification of experience, but that they may be seen in the integrative modification once we can read this rightly. But we have to remember, on the other hand, that these elementary integrations are always physiologically conditioned, although their form must follow psychological lines; special physiological conditions may make the process of integration very complex indeed, especially in highly developed senses, such as those of smell and vision. Thus we may expect that the unravelling of such difficulties may come rather by means of physiological experiment, than by unaided psychological analysis. Whatever happens, there can be no doubt that the psychological result must consist of a reduction of the complex progressively variable qualities of vision and smell, characterised by peculiar attributes and wanting in some of the usual forms, to a few simple abruptly different elementary sensations, characterised by the typical attributes.

[1] But cp. feeling, later, p. 193.

(c) *Conclusion.*

The typical characteristics or attributes of sensation may therefore be put down as (1) quality, in virtue of which sensations fall into separate species, abruptly differentiated from one another thereby, (2) order, which constitutes the individuality of single sensations of the same quality and gives them a definite place in the total experience of any one moment, (3) intensity, by which a variation of each individual sensation is made possible, and (4) extensity, by virtue of which each individual sensation is capable of continuous fusion with others of the same quality, whatever be their intensity, so long as they are of neighbouring order. It is a peculiarity of extensity that it is not bounded by precise limits; and for this and other reasons it cannot be argued that the distinction of elements of experience is fallacious and destructive on the ground that we should never be able to understand how such discrete elements fuse and combine with one another. No real psychical limits are presupposed by the distinction of elements of experience and their typical characteristics. In spite of the difficulties of vision and smell, so many different kinds of sensation do actually show all these characteristics, that we may expect every elementary sensation to be characterised by them. We have the more reason to assume this for sensations of certain kinds which, as we have remarked, show little or no actual variation by way of these attributes, since we have good cause to believe that the occurrence of many of the possible variations of cutaneous sensations, such as those of temperature, is dependent upon the range of function of the sense-organs which subserve them. There can be no doubt that the most highly developed senses are those of sound, vision and smell. Sound, which is still clearly in course of development, as the peculiarly rapid advancement of the musical art indicates, we have already reduced to the type. Vision is even more complex, but it still stands close to the type, except for the alleged absence of the attribute of intensity. As the linear series of progressively different tone-qualities is explained by integration of a single unchanging quality with other attributes of the type, so we may hope to explain the tridimensional variation of visual qualities by a similar process of integration. The vast and probably multi-dimensional variation of smell qualities would suggest perhaps a still more elaborate process of integration. One attribute of smell which might account for some part of this, at least, is missing, viz. order. It is uncertain whether extensity is also missing there or is only difficult to observe, because it is not integrated to varying a real extensity, as in touch and vision.

It is important to emphasise that the problem of the elements of sensory experience and their typical characteristics forms the central and essential problem of any psychology of sensory, if not of all experience whatsoever. For, as we shall endeavour to show in the following pages, it is by means of the fusion of variations in these attributes, that elementary sensory data are linked and integrated into complex experiences, which contain these differences in them subsequently in the form of new modifications of sensory experience attached to the whole unity of integrated data. An architectonic of experience is as unthinkable without the attributes, as is an architectonic of matter without the physical and chemical properties peculiar to its elementary constituents. Far from being the outcome of meaningless psychological abstraction, the problem of the attributes is vital to the existence of any pure science of psychology; and its progress is dependent not only on the means of observation peculiar to psychical subject-matter—introspection, but it is assisted enormously by a study of the forms of compounds which experience shows. It must be our next task to analyse as many as possible of these compound experiences, and to ratify, extend or correct our knowledge of the attributes of elementary sensation by means of the knowledge of the mechanism of combination we thus gather. A means or basis of combination is always necessary; for we must remember that experiences, whether elementary or compound, cannot be expected to arrange themselves by any means external to the mind or not operative in the mind. They must arrange themselves entirely by themselves, purely in virtue of their inherent psychical powers. We expect, of course, some sort of parallelism between the psychical and the physiological, so that we may trace the dependence of the one upon the other. But we have, as yet, no hope of explaining the characteristics of the former in terms of those of the latter. It is still more vain to suggest that physiological arrangements explain a form of psychical arrangement which is not grounded in characteristics inherent in the psychical elements themselves. The physiological arrangement, doubtless, determines the latter and is, of course, a valuable item of knowledge. But an explanation of psychical arrangement must be full and satisfactory and must carry conviction in itself. Experiences hardly ever come singly and successively, or in pairs and simultaneously, so that they might be connected or arranged by mere isolation; they come always in crowds. Why, then, should one of them link with another and not with a third, if not by virtue of the intrinsic affinities of their characteristics? No external power of body or of will could rule them. This reflection is often ignored by those whose interest in the study of experience is partly or wholly physiological, and by all who take the orderliness of experience, as it stands, for granted.

And it is just in the attributes that this means or basis of combination of the elements of experience is to be found. What could be more likely? Where else should we look for any means of combination? The states which result from the combination of the elements of experience show an introspective character which stamps them at once as elaborations or secondary forms of the primary modifications of experience, the attributes. Nothing could be more plausible than the theory that all secondary modifications are derived from the primary attributes by the integration of differences of the elements of experience given in respect of one of these attributes. We have every reason to maintain this for all secondary modifications until we meet with some pure datum of experience other than sensory. Integration must result in a modification of the integrated attribute. We cannot expect to find a modification of extensity resulting from differences in the attribute of order of the sensory elements given, or a modification of order consequent upon differences of intensity. For we should not be able to give final assent to any such equation and should thereby fall short of our ideal explanation. By experimental investigation we may exhaust all the discoverable conditions which affect an event, but experimental exploration can never be enough. Our knowledge can never be complete, till we can supply a convincing causal identification which contains evidence in itself that it is complete. We must be able to show that, in respect of some one aspect, conditions and event are identical. This is clearly impossible, if on the one side stands intensity, on the other order, no matter how clearly we may have shown a correlation between the two sides. To uphold this position, however justifiable it be, calls for some courage. For we find in the psychology of the day quite a number of these irrational sequences. Only one need be mentioned; it is commonly held that our localisation of sounds is dependent upon the difference in intensity of a sound as it reaches the two ears. As it stands this may be true. But that psychically realised differences of intensity of sound turn into or produce of themselves a localisation of that sound is a proposition no one can assent to. Either the facts, as stated, are wrong, which does not seem to be the case, so careful and repeated has been the experimentation on this question; or differences of intensity in physical sound evoke some hitherto undetected attribute of sensation, which, along with the sounds given, suffices by integration to yield psychically localisation of sound. If we can discover this integrating attribute of sensation, we must then be able to assent to the identification and we shall be justified in considering our statement as final, unless experimental exploration shows us that we have omitted one or more stages. In any case, the final statement must be convincing as such. Nothing less than this can be our ideal, if we are ever to have a causal science of pure experience.

§ 5. THE MEASUREMENT OF EXPERIENCE.

It is a familiar fact that the attempt has been made to measure the variability of the simple sensation. And as intensity is the only attribute of the elementary sensation from the unit sense-organ that is capable of variation, it is natural that the effort to measure should have been concentrated upon this attribute rather than upon the others. Yet one might have thought that the idea of measurement was more applicable to the attribute of extensity; for the simple sensation provides a natural unit of extensity, whose multiplication seems to lead to an increase of that attribute. But it will be remembered that this increase of extensity, which can be measured by the conformity of a unit-standard with parts of the amount measured, is not at all a variation of extensity comparable to the psychical variation of intensity. The extensity of one and the same elementary sensation is never variable, and in sensations of any one class it is usually found in a rather rigid, undifferentiated state; it seems to find true variation only in vision and sound. In vision, its variation is dependent upon change in convergence; in sound, we find it attached to pitch in the form of voluminosity, which is variable, but does not grow by the accretion of sounds. In regard to order, it was hardly to be expected that the attempt to measure should be extended thereto, for no elementary sensation differs by itself in order and each elementary sensation has a different form of order from every other. Only in sound has order, in the form of pitch, been made the object of measurement and there it is notable that the usual results are not obtained.

But even though intensity seemed to invite a quantitative study, it is obviously impossible to apply the quantitative concept to that attribute. For there is as little hint of any distinguishable unit in a given intensity, as there is in a given order or in extensity in the strict sense. Nor does the fixation of an arbitrary scale of just perceptible differences of intensities attributive to different sensations lead to any other reality underlying or conditioning intensity than those of a physical or physiological nature, which are already sufficiently measurable. More decisive than all else is the fact that we cannot manipulate our arbitrary unit, however chosen, so as to add it to or take it away from a given sensation.

It is hardly possible to bring further argument to bear against the possibility of measurement. We cannot hope to make one aspect of experience the basis of standardisation of any other. We should expect with as much reason to succeed in applying the notion of sensational intensity or extensity to the quantitative concept as to succeed in applying the terms of conceptual quantity to sensational intensity. Just as great is the world of difference between the order-differences of sensation and the conceptual orders of a mathematical or of any other system. These and indeed any other psychical characteristics are utterly incomparable and incommensurate. We can, therefore, only demand that the lines of variation of experience be carefully observed and compared. It will then become evident that experience varies along certain lines in ways peculiar to itself. A multiplication of units would not constitute variation at all. Nor is anything to be gained by the supposition that these variations are really quantitative; for the actual variations in any modification of experience serve us well enough to indicate the physical stimuli which evoke them and to enter as such indications into the work of the mind. When we find, as we do, that these stimuli can be treated and manipulated as consisting of unit-amounts, the variations of our experience will serve to indicate their presence and action and will stand conceptually as indices of quantities. It is not our concern, nor is it possible to show at this point how this takes place.

The question whether experiences may differ from one another without being recognised as different, does not arise here. For a slight difference by way of variation may just as well pass unrecognised as a slight difference by way of quantity. There is also a great difference between the mere presence of differences and the distinction of differences. The integration of differences and the process of distinction of differences have each their special conditions, which are not necessarily the same (cp. below, p. 176). Outside of these limits we cannot expect differences to lead either to any form of integration or to their own distinction. Indirect proof of their presence therefore creates no problem. Turn the matter round as we may, we never do more than recognise differences by variations of certain modifications of experience directly or indirectly, as far as is possible.

In this connexion it is important to notice that, besides the primitive attributes already treated, we find in sensory experience a number of secondary modifications, each of which has its own peculiar manner of variation. Examples of these modifications are motion with its variation by speed, distance with its much less marked variation by extent of distance, and depth. Motion and distance we shall study in some detail immediately. These modifications have not usually been held clearly before the attention in the treatment of the problem of measurement, although quantitative experiments have been carried out upon them. The reason for this neglect is that they have not been treated properly as modifications of experience. We may say generally, however, that the problem of mental measurement and any formula such as Weber's law are applicable only to variable modifications of the same nature as that of intensity. We may also with much safety assume that where a threshold and a just perceptible difference are determined, we are there dealing with one of these variable modifications of experience. So many quantitative determinations have been made of distance in the form of discrimination of points that it is surprising that explicit reference is not always made to the fact that distance is a modification of the same peculiar kind as intensity, with a line of variation of its own. For that and other reasons the work on the discrimination of points looks awkward and out of place in any systematic treatment of psychology, unless it is recognised for what it is: the investigation of the discriminability of orders (primary attribute) and of distances (secondary modification, *v.* § 7).

The attempts that have been made to measure sensation have sometimes been characterised as the determination of sense-distances or of distances between the different points of variation of any modification of sensational experience, as fixed with reference to the evoking stimuli (27). We may, for example, judge that one degree of intensity is as far above another as the latter is below a third, and the like. If there is any such distance which may be presumed to be objectively fixed and constant, *e.g.* the just noticeable difference, it may be adopted as the basis of measurement. Our measurement with this unit will be as real as is the measurement of height, time and weight, for what is measured is in these cases never magnitude, but merely the distance between limiting points ("magnitudes"). "The prototype of all measurement is linear measurement in space" (27, pt. 1, p. xx).

We must be careful to see that we know how much is involved in this statement. Spatial points have certainly no magnitude. But they are equally devoid of any inherent qualitative character. For conceptual science they can be fixed only by their relations of distance to some fixed point. But neither this fixed point nor the unit of distance has any inherent qualitative fixation in science. Hence the necessity for science of finding some natural unit of distance which is independent of the immanent qualifications of our experience; hence also the impossibility of finding a naturally fixed position. For natural distances, *e.g.* wave-lengths, are repeated and therefore lend themselves to conceptual treatment, in so far as they may be presumed to remain constant in repetition, in spite of the inconstancy of their bounding positions. It is therefore enough, if these can be fixed in attention for a short time. But position cannot in turn find its ultimate fixation by reference to distance. Being in itself nothing, it can be fixed only by reference to the inherent specifications of experiences.

It is the peculiarity of experience that each part of it contains its own qualitative characteristics apart from all relation to other parts. These characteristics we have found to be quality, intensity, extensity, order and, perhaps, protensity. Even the point or "spot" of sensation is qualitatively fixated in a way that is independent of all real positions and of time. These immanent characteristics cannot be taken over by science into its conceptual schemes, so that they must be converted into conceptual indices, based upon processes as independent of experience and its intentions as possible. But it would be a mistake to suppose that science is interested only in the fixation of points by conceptual distance-references. It wishes, wherever possible, to state the actual composition of these "points" themselves. This weight, it says, is 1 cwt., or one hundred weights. When it says a hill is 1000 feet high, it does not mean that the highest point of it is itself 1000 feet. It has, in each case, to consider its own intention and the license of the real facts. The inherent indications of experience cannot be treated conceptually. Any success in doing so would destroy them utterly. But they could be arranged conceptually by reference to one another. One must, however, remember that this reference (sense-distance), as a scientific instrument, must itself be purely conceptual. It has for its unit a process whose constancy is presumed, but whose nature is hardly understood—distinction of difference. It cannot properly be compared with the distance between points in tactual or visual space or purely sensory distance. Distance is as direct as feeling and as *anschaulich* as intensity or depth. What the distinction of differences is, we do not yet know, but it can, at least, never be identified with sensory distance. For sensory distance is given only by sensations which differ in respect of order. It does not result from differences in respect of intensity, extensity or quality.

In a word, the whole work of measurement from the purely psychological side ends just where it began, in the determination of relations to effective stimuli, to favourable and unfavourable circumstances, of the just noticeable presence or difference of experiences or of their modifications. As experiences and more especially their modifications can be made to vary regularly in most cases, although some are practically unvaried, just noticeable differences between these variations are thereby implicitly arranged. That the stimuli corresponding to these just noticeable differences stand to one another in average cases approximately in a certain relation, is an important fact, but it tells us nothing about the experiences that was not already revealed by the changes of these experiences themselves. As the distinction between two variations of any modification of sensational experience is not itself a modification of sensational experience or a variation of such a modification, but a different, later and probably highly complex mental process, it follows that the determination of just noticeable differences will be subject to a number of influences of a purely psychological nature, which we cannot at the present moment understand or systematise. They may therefore be put aside as belonging to another part of our study, although they may serve there as an important basis of research. It will also be clear that we may pass by all detailed questions regarding the stimulus-values of thresholds and just noticeable differences. The value of these, as evidence of the existence of a relation of dependence between one mental state or modification and another, has probably been very much overestimated. For these values depend, as we have already noticed, very largely upon physiological conditions in the sense-organs and do not seem to be due to purely psychical restrictions. We may therefore expect them to fluctuate so much from type to type and from case to case, that their values for psychological theory can only be the slightest. We are therefore free to proceed with our study of the varieties of experience.

As no true measurement of experience is possible, we cannot expect the mind's evolution to be based upon its measurement of itself, or to show quantitative laws. Nothing is given but a number of experiences qualified by certain variable or unvariable aspects. The mind's evolution must therefore rest upon these differences. We must expect to find that the widest use is made of these differences. Far from being a hindrance to unification and progress, they are just what makes these things possible.

§ 6. Secondary Modifications of Sensation.

It need hardly be said that all secondary modifications of sensation must be observable directly; their presence may not be inferred. Changes of a peculiar indescribable kind, evident only after direct experience, supervene under certain circumstances, and though seeming to add something to the complex of sensations to which they are attached, nevertheless do not so radically change their sensational foundations that the identity of these before and after their appearance is ever in doubt. As modifications of sensation, they are distinguished from other modifications of experience in that they are dependent upon the stimulation of sense-organs for their first occurrence at least, and that, in their full variety and distinction, they attach only to sensations. They can be distinguished from the attributes of sensation by the fact that the latter are hardly separable from sensation at all, as far as we know; whereas secondary modifications never accompany the single sensation derived from a single sensitive element. On the contrary, they are always evoked by the action of stimuli on two or more sensitive elements, unless successive stimulations of one sensitive element suffice. From the psychological side they presuppose the simultaneous or successive conjunction of two or more sensations. While these necessary conditions are always complex, they are not always of the same nature. Sometimes the stimuli or the sensations refer to one and the same sense, sometimes to different senses, while the modification which results forms an extension of that attribute whose differences are integrated. The study of the secondary modifications of sensations will therefore be rather complex, and will in any case involve consideration both of their introspective nature and of their sufficient conditions, in so far as these are of a purely psychological nature. It will be necessary in each case to find for each secondary modification of sensory experience and its variations not only an unambiguous complex of sensory data, but to show how certain variable aspects of these can be identified with the modification which results from them.

For the present, however, we shall study only two of these modifications—motion and distance—and the simplest and most primitive forms of these.

I. *Motion.*

When we cast around for further differences in sensations than those already mentioned, we cannot fail to have our attention drawn early to one of the simplest and biologically most important of all further warnings from the environment of an organism, viz. motion. In its generic form, *motion is obtained when successive sensations from neighbouring, or, within certain limits, separate sense-organs of the same kind, differing at least in respect of the attribute of order, fuse with one another.* We shall refer later to the limits of difference of order within which the integration of motion can occur. For the present we shall neglect them and consider only the case of continuous motion produced by a moving stimulus. Motion is found developed upon every group of sensations which show distinct variations from one another in order, viz. the cutaneous sensations, especially touch, articular sensations of position, visual sensations and also auditory sensations, where it is known as melody.

Cutaneous. On the skin it is found that every nerve-ending and every touch-spot can be distinguished from every other, with the exception, perhaps, of those that lie too close together to allow of isolated stimulation. If this result is to be obtained, certain precautions must be taken. The stimulation must be confined to the two touch-spots to be examined, a sufficient pressure must be used, as nearly equal in the two cases as possible, and a certain interval must be allowed to elapse between the two stimulations. If two points are stimulated in this way, we have the impression that the stimulus has moved on the skin (10, pp. 721 f.). Motion is thus found in its simplest and clearest form in passive cutaneous touch. As a secondary modification it rests in this case solely upon the difference in order of the sensations from two neighbouring pressure-points.

Articular. Motion is developed upon the sensations of position of the limbs and appears, as such, in the form of what is known as sensations of the movement of the limbs. These two groups of experiences are usually carefully separated from one another, as if there were even a qualitative difference between them. For this reason they are both known as kinds of sensation, whose differences presuppose the existence of different kinds of sense-organs. In favour of their separation, it is argued that sensitivity to movement varies from part to part of the body, but does not run parallel to the sensitivity of these parts to their position. Thus the movements of fingers and toes seem to be felt equally well, although we are hardly conscious of the position of the latter (10, p. 751). In favour of their identification through the medium of the modification of motion, the following considerations have to be urged. (1) It is a familiar fact that in the sense of vision and more especially in that of touch, the discrimination of simultaneous points is very much less acute than is the sensitivity to a moving stimulus. The sense of position, in touch and in vision, or the sensitivity to the mere presence of a sensation may also be said to be much blunter than the sensitivity to movement, especially if the stimulus whose position has to be observed has been acting steadily for some time and is accompanied by others. Let it move even very slightly and it will be noticed at once. (2) Both the sensitivity to position and the sensitivity to movement vary in different parts, but not concomitantly[1]. It is evident, therefore, that the objective disparity between sensations of movement and those of position is not greater than that between a moving touch[2] or sight and a simple sensation of these kinds. (3) From the subjective side, it may also be said that there is quite as great a difference between a steady visual sensation and a moving one as there is between sensations of position and sensations of movement. It is clearly an easy matter to show that both visual sensations and visual motion are dependent upon the same sense-organ, but there are obvious difficulties in the way of the accurate physiological identification of articular sensations of position and movement. We are therefore thrown back upon psychological comparison and analysis and there can, surely, be no doubt that in the light of the considerations just put forward the physiological

[1] Cp. 10, p. 366, "Die Wahrnehmung von Bewegungen an der Netzhautperipherie ist nach Exner und Aubert viel feiner als das Distinktionsvermögen daselbst, und Exner schreibt den peripheren Netzhautpartien geradezu die Rolle zu, Wahrnehmungen von Bewegungen zu vermitteln." It is therefore evident that any difference of effect produced by faradisation of a joint upon the thresholds for articular position and for movement cannot be brought forward as an argument in favour of the qualitative distinction of articular position and movement. In fact, the greater blunting to position is quite natural.

[2] Such expressions are used deliberately. Seen from the level of perceptual integration, they are of course insufficient. They would then become "a moving tactual stimulus," etc. From the sensational level, with which we are here concerned, "a moving touch" is correct and unambiguous. In strict psychological sense, there never can be any confusion of stimulus with sensation or the like, but only of one level of integration with another.

and psychological independence of these two classes of sensation would constitute a gross extravagance of sensory mechanism[1].

We are therefore confirmed in our previous opinion (p. 140) that the sensations of position from one joint, or from various joints for that matter, are to be considered as differing in order. The derivative nature of the sensation of position is sometimes supported by reference to the fact that we gradually lose a clear sense of the position of the arm if the attention is distracted and every movement and contact of the arm with other parts of the body is prevented (cp. 30, p. 155); the sensation of position, it is held, is only an after-effect of that of movement. But such an argument is worthless. The facts can be explained by a theory of adaptation similar to that commonly accepted for touch, that pressure is only felt where there is a quick change of pressure over a given area (8). The facts, therefore, support the primacy of the articular movement as little as that of tactual movement, as against the simple sensation from the "spot." Psychologically the facts may indicate the presence of the aspect of intensity in articular sensation. A semblance of extensity seems to be given in the different voluminosity of the sensation of movement from the thigh compared with that from the little finger. We should then have the full complement of attributes in this sensation, all of which, however, owing to the peculiar physiological conditions of the case, are much clearer and more easily observed in the complex of movement than in the single elementary sensation of position.

Labyrinthine. Our awareness of the motion of the body as a whole may also legitimately be conceived as a form of motion and as based upon sensations of position of the body as a whole. This view is also opposed to current theory, which treats the two kinds of experiences as different kinds of sensation. Physiological investigation supports the latter in so far as two separate sets of sense-organs are found, one for each group of sensations. But this is only apparently a difficulty. For it is well known that the various parts of the skin and of the retina, which contain very frequent repetition of the same sense-organ, are not

[1] The physiological problem of the sensory mechanism, of which at the present time we know next to nothing [cp. 30, p. 25], is in this case, as in all others, quite irrelevant, for it is quite possible that it consists of a very complicated form of physiological integration. This is unimportant to psychology, so long as the sensation evoked possesses the full number of attributes, including order. It would, on the other hand, be a highly important fact for psychology, if it suggested to us the lines of psychological integration. We find a physiological integration, for example, in the labyrinthine organs of position and movement.

equally sensitive. Again, we find different systems of sense-organs in the skin, which provide us with very similar kinds of sensation whose peculiarities show variation; the sense of temperature, for example, is based on a protopathic and an epicritic system, of which only the latter shows the process of adaptation. So too in vision do we find different kinds of sense-organs procuring very similar sensations, which differ, however, again in regard to the process of adaptation or special sensitivity to certain degrees of light. It may be agreed, then, that a reduplication of sense-organs giving the same primary experience, whose actually realised complications vary somewhat in character, is quite a usual occurrence.

It may be taken for granted that the sense-organ connected with motion of the body as a whole is a special device for obtaining sensitivity to all acceleration of movement, so that the organism may adjust itself to the change. This sensitivity to acceleration of motion can only be obtained if the change brought about by any acceleration is removed as quickly as possible, so that the organ may be highly receptive to any new acceleration. The organ of position, on the other hand, must be specially sensitive to position as against movement. An organ which has to be stimulated continuously by the fact of its having taken up a certain position could hardly at the same time be one which responds delicately to even an incipient change of speed of motion. For the readjustment of the organ to motion might very well be taken for a readjustment to position and *vice versa*. Their separation, therefore, becomes a matter of necessity. The provision of a large sensitive area, such as the skin, in part of which a motion-complex could be produced and set in order-relations to sensations from other parts, would not obviate the necessity for separation. For the stimulus to sensations of position and movement of the body as a whole must surely affect the whole body and, therefore, the whole specially sensitive area at once. If the whole skin at once were always affected either by constant or by moving pressures, our tactual would closely resemble our present articular sensitivity. For we should then be keenly sensitive to waves of motion passing over the skin, but we should quickly lose our sense for them, when they came to rest and acted continuously on the same elements of the sensitive area. Creatures endowed with our sense of touch, who lived in a fluid medium which never moved over them except in continuous waves passing from head to tail and which never exerted steady punctate pressure stimulation upon them, would never experience anything but touch-motion. There must, therefore, be specialisation as well as separation of sense-organs for position and movement of the body as a whole.

What, then, are the attributes of these sensations? Sensations of position do not seem to be capable of variation in intensity or extensity. We can therefore have only the vaguest, if any, introspective appreciation of the actual degree in which these aspects are given, and we can make no use of them in experience, if they do not vary. For integration with an unvarying element could not render an ambiguous complex of sensory data unambiguous. But of variations of position we are definitely, although not often in isolation, aware. The question therefore arises whether these variations are variations of quality or of order. For several reasons it would be more acceptable to call them variations of order. For our sensations of position do not seem to differ in quality. How should we be able to treat them all as sensations of position, if they differed in quality? Or how should we come to arrange them for our use into a system of interrelated positions? If they differ in order, however, the basis of their arrangement and of their use is at once given. Mere introspection can hardly lead us further than this.

When introspection fails, we must have recourse to a comparative study of the forms of integration in which labyrinthine experience occurs. An examination of these must show us how it enters into combination with other sensory experiences and what new feature or modification of experience results therefrom. If even then we are not quite clear of our difficulties, we must resort to general principles of integration, derived from an examination of the manner of integration of sensations whose elementary characteristics are familiar to us. Now we do find cases of the integration of the order-aspects of sensations, whether these be qualitatively the same or different, while we have no good example for the occurrence of an integration of the order-aspect of one sensation with the quality of another of the same or of a different kind. We shall, therefore, assume, for the present, that labyrinthine sensations vary in order.

It must, however, be noticed at this point that the psycho-physiology of the labyrinth is entering upon a critical stage of its existence. It is on the one hand, a matter of doubt whether the vestibular nerve has any direct connexion with the cortex (2, pp. 78, 91), and it is asserted that the existence of vestibular sensations proper is not proved (*ibid.* p. 91); on the other hand, there is evidence that voluntary inhibition of nystagmus does away with the sense of bodily rotation, not merely after the rotation has stopped, but also during the actual rotation

(5, 13). It may be shown in time that our labyrinthine motion is a modification, resulting from a more or less complex process of integration of visual or, in the broadest sense, pressure-sensations or both. It must, however, in any case remain the modification of motion it is and be amenable to the line of treatment here advocated. Its difficulties and problems offer no particular obstruction to our theory, which will, on any showing, probably be right in the main principle.

Olfactory. We can point to nothing resembling motion in the sense of smell. The attribute of order is not patent in olfactory sensation. Presumably there are in this sense no neighbouring sense-organs of the same kind. If there are, the attribute of order has been so integrated with others that it is at present unrecognisable. Probably the very slow rate of change possible with olfactory stimulations precludes the realisation of the integration of these hidden differences of order into a motion-like modification, which would, as such, be readily noticeable. For in all other senses the rate of change of order which constitutes motion must not fall below a certain minimum. We are here faced with problems, not with radical exceptions or difficulties.

Visual. Motion is visual, *par excellence.* If the primary visual sensations are well marked and in sharp contrast with one another, as are *e.g.* those from a broad black strip upon a white ground, motion of the black strip can be detected at some 50—60 sec. of arc per sec. of time displacement. The limit of distinction of visual points from one another is found when these subtend an angle of about one minute. On the retina this angle would allow one unstimulated visual element to intervene between the two excited by the points seen. Higher visual acuity than this is rather exceptional and is very difficult to explain visually without the help of eye-movements, whereby the increase of sensitivity may possibly be obtained by a movement of the eye allowing one and the same visual element to be stimulated by the two points successively or, less probably, by the kinaesthetic sensations afforded by the eye-movements as such (10, pp. 346 f.). As the minimum angle for the detection of motion is smaller than that for the detection of distance, where only one intervening point is presupposed, we may at least assume that visual motion supervenes upon the successive stimulation of two sensitive elements of neighbouring order.

Time-limits of motion. Change of order must take place at a certain minimum rate, if motion is to appear. This is most familiar in vision, for which the limiting value has just been given. With slower speeds the motion only appears after some seconds or not at all. In this respect motion behaves quite like the attributes, *e.g.* intensity. In the simplest form of pressure-motion, when the stimulated surface is quite at rest, there is also a minimal rate of displacement, which has not yet been determined exactly (10, p. 722). For articular sensation the minimum rate of displacement varies from 0·25° in the hip-joint or 0·3° in the shoulder-joint to 1·4° in the ankle-joint per second (10, p. 753). The range of speed of displacement throughout which motion is appreciated is very great; in vision the highest limit is some 24,000 times the lowest limit (10, p. 368). The threshold for the perception of motion is the higher, the farther towards the periphery of the field of vision the stimulation takes place.

Speed. The rate of change of order appears in motion in the form of speed. Speed is measured by reference to the distance traversed by the moving body in the unit of time. In experience, however, we notice differences of speed without any conscious reference to distance or time and without any medium of comparison. Motion as a modification is not more truly motion with a fast speed than with a slow one. It is always just motion. Its form of variation is speed, which, however, we can measure only in the way we can measure other variable modifications of experience, by relating stimuli to just perceptible differences. Judgments of speed are, therefore, based upon a direct criterion, present in experience (17, p. 374).

Order-difference limits of motion. No real motion of an object is necessarily presupposed by a moving sensation. Change of order, as defined, is alone requisite. But this change need not progress strictly from one order to the next neighbouring. A considerable change of order is compatible with the effect of motion. Certain stages of the motion may be omitted without spoiling the effect. Upon this fact the familiar apparatus of the wheel of life or the stroboscope and of the cinematograph is based. A succession of pictures of an event, each of which, of course, is entirely devoid of any movement or displacement, is projected upon the eye and is seen as a perfect representation of continuous motion. A series of small electric lamps set at a certain distance from one another, which can be lit and extinguished successively, serves to demonstrate this fact in its simplest form (19, pp. 60 ff.). The continuity of the motion is broken if the time or space intervals between the lights exceed certain amounts, which are to some extent interdependent; but the effect of motion is not suppressed unless these intervals are much larger. If the time-interval between the lights is decreased beyond the value for continuity, several of the lights become visible at once, each one being in apparent motion. A full psychological definition of motion states, therefore, that motion is the unification of successive differences in order of sensations which follow one another within a certain range of time-intervals. This range is determined by the degree of the difference of order of the sensations, which may not exceed a certain amount. The introduction of intervals without the omission of phases means a slowing of the motion which results; the omission of phases is followed by no marked effects, until the interval reaches a certain amount, when the motion becomes jerky and interrupted. Although the effect of motion is still distinctly present, the single sensation or picture can be distinguished more and more as the time-interval increases. The modification of experience which results from this integration of order-differences may be described as unitary and progressive change of order.

How do the primary sensations integrate to form the modification? There seems to be no valid reason why we should not say that, when two or more sensations of position of neighbouring order are evoked successively at a certain interval, they unite to form the experience of motion. Conversely, we may assert without fear of serious opposition that two or more sensations of position are given psychically, when a corresponding experience of motion is evoked by the successive stimulation of two neighbouring sensitive elements. We cannot object that no sensations are distinguishable in the integrated states. For we could not expect these sensations to be distinguishable, so that we might discriminate them one from another. It is just because they integrate to form a unity, that we have any such state as motion at all. To prove a fusion of particulars to unity we do not need to show a temporal process whereby discrete particulars have come together into unity. And we do have a multiplicity of sensations in this immediate unity, in so far as we realise it in its own inherent character—change of order.

It is important to mention a number of ways of stating or explaining the connexion between the integrating sensations and the resulting modification, which have been put forward for one or the other modification of experience. The consideration of these statements may seem very pedantic and forced in relation to motion. But it is just because of this that we would repeat them here; for if they are inapplicable in the case of motion, we shall learn to dismiss them here and shall understand their invalidity in cases parallel to motion, where they have seemed to be of genuine worth.

Our past experience is often considered to have an important influence. It might be said that, having often experienced two tactual or visual experiences successively in a complex of circumstances which otherwise led us to know that a stimulating object was moving over against us, we have come to know that these successive sensations mean motion; so that, when they afterwards occur without the complex interpretative circumstances, we yet know from past experience that they mean motion. Or it may be maintained that some inner power of thought operates upon the data of sense and extracts from them certain meanings, previously implicit in them, or unites with them to form a state of meaningful perception. Or it may be claimed simply that our experience grows from within and blossoms out into these modifications. It may even be said in abandonment of all problems that the mere juxtaposition of the data of sense is all that we ever seem to experience or do experience; there is no new, nameable modification of sensory experience at all; from beginning to end we have only sensations in juxtaposition.

Most of these "explanations" are empty, because they do not state how the result actually obtained is brought about by the means alleged. The mechanism of the operation is left in entire obscurity. How, for example, should past experience be capable of all it is supposed to do? If we only mean to state the fact that our present experience is dependent upon experiences we have had, we must be at pains to state that we do not know the mechanism of this dependence. We must also attempt to discover its specific nature[1]. Not only motion, but all secondary sensory modifications—melody, distance, interval, depth, apparent size, position, distance and depth, tonality, and all the *nuances* of perception—present the same problem: by what means does it come about that the presented appearance of sensory data changes with circumstances? These many and various changes cannot be adequately explained by a reference to the knowledge we have gained of the approximate real nature of the objects which evoke them, or the like. For it does not appear how the significance of any knowledge we may have gained should actually change the appearance of our sensory experiences as they present themselves to us. It can be

[1] As Stumpf says (25, Vol. II. p. 195): "Wenn die Kraft, welche allein Verschmelzung bewirkt, wegfällt, wird der Effect ebensowenig eintreten, als die Locomotive aus Gewohnheit läuft, wenn sie einmal nicht geheizt ist oder...dem Kurzsichtigen, der sich eine Brille anschafft, nun etwa gewohnheitsmässig immer noch alle Umrisse ineinanderlaufen." So too, of course, for any secondary modification of experience and any extra-mental influence.

easily recognised that knowledge has not this penetrating influence in every case. If it is shown for any case that cognitive states are the effective influence, we must also be able to show how they produce the change in question. If we can show that the sensory data themselves suffice as an explanation, then we can dispense with remoter influences, whether these accompany any changes regularly or not. For it is not at all unlikely that knowledge in many cases is dependent upon sensory changes and not *vice versa*. In any case, it is impossible to work with the conception of transcendence, whereby a complex state of mind derives its appearance in part from influences which are not given psychically at the moment. We have already noticed that we cannot carry our demand for a causal explanation further back than the elementary data of experience; but we must be able to reduce our whole experience to these and to explain it fully without appeal to any other data. For, as it is clear that all our knowledge has been gained from our experience, it is not intelligible how our experience should reveal what it is not yet affected by. For experience can only reveal that which modifies it. If experience shows any change, there must be some new datum present responsible for it. We cannot expect to explain the simpler in terms of the more complex, but the contrary. We must therefore find all the elements of experience and attempt at least to explain all experience in terms of these.

Our only possible conclusion, therefore, is that a moving sensation consists of at least two primary sensations as such and in so far as they are not the same in respect of order; so that the two together present a change of order, that is motion. The further difference of increased extensity which they also present, we are not at present concerned with. It might, however, be urged that there is no apparent way in which two pressure-sensations could come into such close union that their differences of order might form a new unitary modification of sensation-complex whose elements do not seem to be individually segregated. But our definition of sensation sets no limits to the boundaries or affinities of sensations to one another. The presence of extension as an attribute may logically, but does not psychologically, presuppose the existence of limits to that extension, which, as we see in vision, are only got by virtue of a quick change in quality, *i.e.* by contrast. In touch a boundary is given by a special emphasis on quality, where the change in pressure is rapid; no sharp limit is given thereby, but only a certain amount of extension (22). We may therefore confine ourselves to saying upon what occasions sensations do actually fuse their differences in respect of any one attribute into some new unitary modification.

Motion is not based upon any conscious comparison of the order-aspects of the first and last or of these and the intervening sensations. Nor is there any unconscious inference from these. It is simply the integration of the differences in respect of order of the given sensations. Nor can we analyse the experience of motion into a series of sensations of position. We know the positions a flying arrow has occupied, but we cannot separate out in sensory experience the unit-sensation of any one position. For where motion is in experience, there never is merely a number of different positions, but a series of positions which unify to form progressive change of position. Motion is not merely a way of speaking of or a name for a number of positions. It is a new modification, which though based on sensations of different order, is more than these, because it is a unity of them. It is a difference of position[1], based on given orders and integrated from them immediately in the way characteristic of experience. Our point of view, therefore, cannot be called sensationalism. Nor is it that view which looks upon every new unity of experience as a unique, irreducible element. It contains both of these positions in itself and finds a partial truth in each.

The sensations upon which motion in any particular case is based are not lost in the resulting experience of motion. We do not propound a kind of mental chemistry, as that was understood by early British psychologists. For the experience of motion, though new and unique, supervenes upon the quality of the sensations given as an integration of their order without thereby changing their quality so as to make them in any way unrecognisable. Nor are the extensity and intensity of the integrating sensations necessarily changed in the least, although they may be so slightly according to circumstances, when these operate upon them. This is a point of view which must be maintained throughout the whole treatment of mental modifications.

Motion and the Attention. Motion is said to exert a strong attractive power over the attention. But we need not yet appeal to remoter powers such as that of the attention. In an otherwise resting field of cutaneous, visual or auditory sensations, a moving sensation is not merely one among others. It is one like the others, of course, but it

[1] Here is the inset for one of the central problems of philosophy, how the mind knows differences together. This is first of all a problem for descriptive psychology. It must not, however, be confused with our issue, which is concerned with what results when differences are given together. The problem of knowledge is quite another.

is characterised by a peculiar modification which the others lack, and having this feature of motion it behaves in our mind as would the sight of a single red rose in a bunch of white ones, a single light in the darkness or a single sound in the silence. That our attention is drawn to each of these things, means simply that only one of a peculiar class of experiences is presented in each case. The separation for the attention is given without the help of the attention at all. To this peculiar isolation, which the presence of a mental modification may give to a sensation, we have, of course, to add the peculiar sensitivity which is represented by the much lower threshold for successive than for simultaneous discrimination.

The attention may be directed upon any of the phases of a motion generally. But in particular instances, it is very much easier to isolate certain phases than others. This fact accounts for the conventional representations of men and animals in motion and especially in rapid motion. The most prominent phases are, of course, those at the beginning and end of any motion or at a change of motion, where vision obtains the advantage of the slightly longer duration of these phases. These positions, once made familiar in art and illustration, help to fix the attention of those who study them, so that they are seen regularly and are used to suggest or symbolise motion. The strange positions which men and animals occupy when in motion, as revealed by modern photography, are observed for the first time by everyone with great surprise. Naturally they seem very ludicrous, because we never do see animals or men in these positions unless they are in motion. To see them in these positions at rest has the same queer effect as the sight of a person suspended in mid-air, as if comfortably at rest upon a couch, would have. We see them without that conscious modification which alone supplies the key to their interpretation. The difficulty we experience in isolating these phases of motion in the attention really shows us the attitude of attention towards motion. When many motions are given together, the attention behaves in the same way as when many sensations of any kind are given together. No one would suggest that when many motions are given, the attention to them all is raised consistently to a higher level. Attention to motion, therefore, is rapid when only one motion or unitary complex of motions is given. Then the attention behaves as it does to any peculiar and unique object. When there are many motions, the attention acts towards them as towards any group of similar experiences, sights, sounds or thoughts. It even finds it particularly hard and embarrassing to follow one among many motions, until it is trained to it, and it will overlook one movement of importance among many others of a similar kind as readily as it will overlook one of many motionless objects.

In attending to motion, the attention must in the first place be directed towards the moving sensations. We may express this better in accordance with actual speech by saying that we attend more to the things that move than to their actual motion and that we cannot abstract their motion from them entirely, so as to separate the one from the other. For the present, however, we must attempt as far as possible to avoid the phraseology of knowledge, for such modifications as motion do undoubtedly occur before there is any clear evidence of the occurrence of knowledge. The matter may, perhaps, be best stated by saying that the modification of motion cannot be separated from the basis of sensation upon which it rests. No motion, we may assert, ever occurs without the simultaneous occurrence of primary sensations. The connexion between these two things is, however, psychically much more obvious than this. Motion is psychically always *attached* to primary sensations. This fact it is which has led, as we may now say, to the hypostatisation of a class of sensations of movement of the limbs. Obvious though it be, it is important to emphasise here, that a modification such as motion cannot be experienced alone or attended to alone in separation from its basis in primary sensations. Apart from such separation, it may be attended to for any length of time allowed by the continuous operation of the sensory stimuli to the primary sensations which carry it. For, as has already been indicated, these sensations, and with them motion, are adequately conditioned by the stimulatory complex and the ensuing integration, apart from all higher processes of integration which may be implied by attention. No modification of experience can be separated or detached from its integrative basis, so that the observation of the former is dependent upon the continuance of the latter. If the integrative basis of a modification is itself dependent upon the attention, the resulting modification will of course be destroyed if the attention is directed upon it.

Melody. Melody is based upon tone-sensations which differ progressively or within certain rather indefinite limits in order or pitch[1].

[1] There is, in sound, another form of motion that stands for change in the place of origin of the sound-stimulus. But that is obviously a derivative of the localisation of sound, which again is dependent upon intensive differences. The nearest relative of visual motion is, therefore, melody. The spatial motion of sound resembles the integration discussed in the text in many ways, but it cannot be dealt with here.

There can be no doubt whatsoever about the introspective similarity of the two modifications; which seem different only because one is change of localisation and the other change of pitch. Pitch *moves* in a melody. A succession of tones of different pitch which does not move is no melody and can be realised only under certain circumstances of time- and pitch-interval. A melody is not merely change of brightness, nor is it merely meaning or emotionality, although it may also be these at any time. It is essentially a unity and progression of pitch.

All the psychological characteristics of motion may be transferred to melody. The minimal order-difference which will constitute melody, as all the physiological theories of hearing suppose, is the passage of a stimulation from one sensitive element of the ear to the next neighbouring. As in vision, so here also change of order must take place within certain rate-limits, if melody is to be appreciated. An upper limit of melody, as of motion, is only given by the possibilities of the physiological process of damping of the resonators of the ear, or of the equilibrium of forces at the sensitive element. Melody therefore also varies in a characteristic way by its speed, or by the interval which it compasses in a given time, although this is very much affected by the simultaneous change of voluminosity, which adds to the quickness of change a certain difference of brightness and lightness or sombreness and weight, or, it may be, also an emotional sense of gloom or gaiety. Melody, like motion again, is restrained within certain limits of successive order or pitch-differences. The continuity of melodic progress is not markedly affected by the introduction of a pitch-interval between two immediately successive tones, as in *legato*-playing on an instrument with fixed tones. With certain rates of succession of tones it seems to be perfectly continuous in its progression[1]. With slower rates it seems to rest at each tone for an instant and then to spring to the next following, while with higher rates we hear several tones together. Beyond a certain, not very definitely fixed, interval our

[1] Certain pathological conditions may very much increase the maximum pitch-interval that may separate successive tones, which, played at a certain rate of succession, seem to form a perfectly continuous progression of pitch. In the case described by Grant Allen (3) this interval even in the middle octaves was as great as a third. These pitch and time intervals and the whole introspective problem of melody have not been investigated experimentally, as far as I am aware. The statements of the text are based only upon general observation, but are easily verified. It is significant that Stumpf (Vol. I. p. 185), against the view of Grant Allen, who compares this increase of the critical pitch-interval "properly" to loss of quality in vision (*i.e.* colour-blindness), finds in the facts a greater resemblance to pathological cessation of function of *parts* of the field of vision, *i.e.* disappearance of certain "orders."

sense of melody is not aroused. This limit we reach approximately with the octave. Nor is melody affected by the introduction of a time-interval between the successive tones, as in *staccato* playing, provided that it is not too large. Here again the limits have not been precisely ascertained.

It is therefore evident that, on the whole, the musician's use of the words motion, line, curve, wave and the like in relation to melody is, from a psychological point of view, perfectly justified. Obviously it is no mere analogy with vision or with the arts of vision which prompts the use of these terms, but rather introspective familiarity with the motion-like nature of melody, its smooth continuity or jerky abruptness and its evenness or variation of speed. In this connexion, the usual means adopted to increase the motion-like progression in melody are interesting. The player often dwells very slightly on one or more tones to the disadvantage of a few following, which have then to be got into a slightly less time, so that in them the speed and therefore the continuity of progression is increased. In exaggerated form this is the familiar *tempo rubato*. A proper grading of intensity will also often accentuate motion. The composer has the obvious means of multiplying the number of intervals of a unit-size passed in the bar, which heightens speed, the introduction of continuous or chromatic passages, which increases the smoothness and continuity of motion or line, *legato* indication, and in *legato* passages the variation of the number of tones passed in each beat, which, by varying the motion, makes it more prominent.

Melody also offers itself with the same ease and difficulty to the attention, as does motion. If pitches can be distinguished at all, it is impossible to overlook a melody upon a background of consonance which does not physically overwhelm it. It is difficult to follow one melody amongst several, unless the tones are marked out by some constant feature, *e.g.* highest pitch of tones sounded simultaneously, a certain timbre, as when melody is played on one instrument amongst others, a certain intensity or the like, as when several voices are played on one instrument at once. It needs practice to follow several melodies together. An isolated part of a melody is as bizarre and meaningless as is part of a motion. If anything, it is the beginning of each which is most typical and representative for imagination and recall.

Melody is inseparable from tones, to which it is always attached. It cannot be recalled apart from them and is therefore ever experienced anew. Properly speaking, we should say that a series of sensations is revived which integrate to melody. Of course the melody may be the real object or aim of recall, but nevertheless the integrating tones are the mechanism of this recall. If we make the continuance of the integrating tones dependent upon the attention, it is impossible to attend to a melody without destroying it. If melodies are not separately reproducible, neither do they leave an image behind, nor can they associate with one another or with images. Melodies have no intensity, voluminosity or localisation, apart from the tones upon which they are based. The variation of their constituent tones in voluminosity gives them, as already noticed, a varying character of brightness, besides that of "speed" native to them. Their other qualities come from other forms of integration.

It is often said that melody presupposes one or other thing, such as rhythm, consonance, interval or tonality. But after our consideration of motion as a modification, we may conclude that melody presupposes nothing not included in its definition. It is possible without tonality or consonance, as in the birds. Its intervals may be most indefinitely fixed, as in the first cooing of a child. It may or may not always be psychically concomitant with rhythm; it is at least in no way dependent upon it.

§ 7. II. *Distance.*

The next modification of experience in order of simplicity and the nearest allied to motion is distance. When a motion of some extent occurs, we do not recall at the end of it where its beginning was and infer the amount of its course therefrom; we have rather a direct experience of the amount of the distance. This direct experience, like motion, is independent of any conscious comparison of the order-aspects of the first and last or of the intervening sensations. The experience of distance is not composed of sensations of position; nor is it the imagination of the extended pressure of an object stimulating the extensity intervening between the two points touched (*v.* 29). It is based on the differences of order of certain sensations of the same qualitative class. Nor is there any conscious or unconscious inference from the two end-positions. As we have already urged for motion, so we would argue for distance, that the difference in order of two or more sensations of the same quality constitutes a distance. No one assumes the existence of a class of sensations of distance. Distance is generally recognised as a perceptual result, but such a classification clearly raises it much too far above its real sensational basis.

Distance must be carefully distinguished from motion. An approach to progressive difference of order is essential to motion. Beyond certain limits of difference the motion tends to disappear. Distance is not confined by these limits, so long as the two or more constitutive sensations are not restrained by one or other circumstance from free integration. Even two points at the limit of order-differences may constitute a distance. Successive occurrence of sensations is presupposed by motion, but not by distance, which is only restrained by too great a time-interval between them. Though the limits of this interval have not been fixed, it is clear from experiments already done that the time-limit for distance is much greater than for motion. Motion is within its limits the integration of successive and progressively continuous order-differences. Distance is within its limits the integration of any simultaneous order-differences. The limits of motion are set by degree of order-difference and by time-interval. The limits of distance are set only by time-interval. Distance may therefore occur apart from motion when the integrating sensations are given simultaneously. It is naturally more distinct in this form, since observation may be directed upon it as long as it continues or for any length of time. It is, on the other hand, often more urgent and clearer when it accompanies motion, for being clearly delimited by the progression of motion, its objective basis is thereby already unified and therefore always unifies to distance as well. Besides, two modifications are more effective than one. If there is any rivalry of distances, that characterised by motion will be more effective.

We find distance in all those senses which show order and are capable of the modification of motion. In the sense of pressure it has been treated experimentally in an exhaustive manner in the discrimination of points touched on the skin. This is the very familiar aesthesiometrical work. In vision extensive research has also been carried out involving the comparison of lengths of lines or of distances between points. Distances traversed by moving limbs have also been carefully studied. Only in hearing is the modification of distance less familiar under this name. There it forms the familiar phenomenon of interval. It can hardly be disputed that as a matter of fact we are in some way aware of the extent of movement or of translation of the body on the basis of labyrinthine sensation. We cannot expect to have a fine sense of distance in this particular quality of sensation, for, as we have seen, positions are here not given in isolation. It is therefore as impossible to separate single positions from the continuous motion here, as it is in continuous visual motion. Labyrinthine distances are thus appreciable and comparable, but they cannot be accurately fixed or subjected to conceptual treatment.

That distance, like motion, is constituted not by orders or by sensations of position, but by difference of order, is borne out by many facts. The chief of these is the ease and accuracy with which extents of movement and distances can be noted and compared, even when the end-positions of the distances compared are different. We may easily remember a distance or motion without remembering the position of its limits[1]. All the facts concerning the variation of apparent distance under certain circumstances also bear this out. "When a movement is freer and easier than an other, and so produces a less sensation, it is underestimated with regard to this other and tends to be prolonged" (30, p. 109). The apparent distance of a movement is also affected by fatigue, slowness of motion and attention, which make a movement appear longer than it otherwise would appear to be (30, p. 109). Each of these influences has the effect of making differences of order seem greater than they really are, because the difference of orders of the end-positions is distorted and not these orders themselves, as they are psychically given. This distortion of differences is doubtless great where distances are given by means of motion, for then the temporal lapse of the first sensations leaves nothing to guide the judgment except the modification of distance; but the same kind of distortion is possible in distances given in simultaneous stimulation, especially when distances proper and not end-positions are compared, as in the comparison of short lines as such. Distances seem greater at one time than another, merely because a variable modification of experience like distance is a direct psychical datum which arises under the same conditions as *e.g.* intensity. The distortions of all modifications of experience by various influences seem to have a common nature. They would have to be considered systematically in connexion with the illusions.

Threshold of distance. For the discrimination of successive stimulations this is nearly always somewhat, and sometimes very much, lower than for simultaneous stimulations. Two touch-spots stimulated

[1] **Cp. 30, p. 155. The systematisation of the facts suggested by Woodworth makes the path of research seem infinitely long and completely excludes any gleam of daylight from it. On the other hand, the one here proposed has all the merits of a system. The facts arrange themselves in it willingly and form profitable knowledge. The whither and where of surrounding facts also become clear and violence is done to none.**

successively are distinguishable from one another when they constitute neighbouring sense-organs, while simultaneous stimulations need to be many times as far apart from one another to be distinguished as two. The origin of this peculiar fact is to be found rather in physiological than in psychological conditions. It is usually explained by supposing that the stimulation for each sensitive point radiates over a certain area round its most intense effect upon the cortex, so that the edges of two areas excited simultaneously from two neighbouring points often overlap and produce either one maximum or a level, until they are so far away that the sum of the edges where they overlap is not equal to the maximal part of either area, and these therefore form two points of maximal excitation. This explanation is supported by introspection, which shows that when the distance between the two points is increased, the stimulation is felt first as one point, then as an increasing oval or small line and then as two separate points. The distance in the oval or between the two points increases rather markedly as soon as the points stimulated begin to be differentiated as two or as an oval. It is therefore evident that if we are to attribute any systematic psychological importance to the fact of thresholds and their variations, we must, in the case of touch at least, hold rather to that of successive than to that of simultaneous stimulation.

The facts are much the same for vision; one unstimulated sensory element must lie between two that are stimulated, if these are to be distinguished. Otherwise the two points are felt as a short line or oval. With the help of successive stimulation or eye-movements, the threshold for the psychical realisation of order-differences and therefore of motion and distance may be reduced to the lowest possible limit, to that of neighbouring sensitive elements. So in articular sensation, we are able to distinguish short movements, before we are able to discriminate from one another the two end-positions occupied by the limbs. We may therefore maintain generally that the modification of distance is present as soon as a difference in the order-aspects of two successive sensations is given at the proper interval of time. These intervals are not known to be different from those indicated for motion.

Direction. Short distances are therefore perceived before the points bounding them can be distinguished from one another. Only when the points stimulated are some little distance from one another can they be distinguished as discrete. The same holds for the appreciation of the direction in which two points lie to one another; for this involves a quite clear discrimination of the order-values in at least two points of the line formed by the end-points. Awareness of direction seems to be an experience which involves higher forms of integration than motion and distance. Appreciation of distance, on the other hand, is based on the psychical presence of order-differences and involves no discrimination of positions, as in cutaneous, visual and articular sensations. In labyrinthine sensation direction is said to be distinguishable as soon as motion is felt at all (10, p. 750). We have already noticed that we have here no means of distinguishing end-positions. Possibly the peculiar composite nature of the sense-organ has some determining effect here.

The apparent distance separating two points varies with the threshold for their discrimination; the higher the threshold, the smaller will the distance seem. This relation is doubtless based upon the unequal number of touch-spots at various parts and the consequent unequal representation of various areas of the skin upon the cortex. There is no reason to suppose that the physiological separation of two areas of excitation on the cortex should vary much from part to part.

The variation of distance. A form of this is given in the greater or less distance that may be integrated from the differences in psychical order of the constituent sensations. Judgments of distance, or, as they are often called, of extent, are therefore, like those of speed, based upon a direct criterion present in experience. So *e.g.* movements of the arm may be and will usually be judged as to their extent directly by mere reference to the modification of distance which ensues. That this should have been denied in favour of duration as the basis of judgment[1], can only be accounted for by the fact that opinion generally separates sensations of position and of movement into two different classes. But if sensations of movement are supposed to be elementary, it is, to say the least, unusual to suggest that they are primarily qualified by an aspect of extent of this unique kind, so different from the usual extensity. On the other hand, these sensations can hardly be supposed to have an aspect of extent, for their supposed derivatives—sensations of position—do not show much of it. In place of extent, therefore, duration is the only obvious sort of attribute these sensations have to show, and even that can hardly be called obtrusive in sensations

[1] Cp. 17. "The comparison of the length of arm-movements is made through the comparison of the duration of one or several of the sensations arising from the movements (preferably the joint-sensations) and of a particular value of the joint-sensation, called here the rate-value." For experimental data against this view, cp. 12 and 30, chap. IV.

of position. Duration and extent may, of course, be distinguished in sensations of movement after a fashion; where extent is the distance in our sense and duration the time taken to move the arm through that distance (21). But if extent can vary, surely order, or perhaps even quality, should also be variable, which does not seem to be assumed in this case. The only convenient and at the same time the obvious way out of these difficulties is to connect the two groups of sensation as we have done, and to see that sensations of movement constitute the modification of motion and distance for sensations of position, which then have the full complement of attributes necessary for the judgments based upon them, viz. quality, order, extensity, duration and intensity. We can then readily allow[1] that motion, the speed of motion and distance are all specifically perceived, while the duration of motion is as directly given in experience as is any duration. The same is true of the order of any sensation or of the general character or change of character of any motion, *i.e.* of the position of a movement.

There is a very great difference between the true comparison of distances in introspection and the comparison of lengths of line by laying one alongside the other. In the former we compare with one another the differences between two pairs of orders; in the latter we compare single orders with one another and infer from the result the comparison of the intervening distances. These two processes are both possible, because distance, like extent, is based upon the order-aspects of more than one sensation. It is therefore possible to turn both the primary and resulting secondary modifications into amounts or quantities by the identification of the orders of the elementary unit-sensations. Motion can also be treated in this way and is actually measured for physical purposes for the identification of points passed in a unit of time. But such measurement is not usually possible to the unequipped eye, except in the case of the modifications based on simultaneous data, such as distance and extent. It is so easy and advantageous to measure in this way that we have constantly to be on our guard against it in experimental work. No one relies solely upon the comparison of distances as such, where comparison by identification of orders is at all possible. If we wish to obtain comparison of distances, we have to use a method which will prevent the identification of orders. Under these circumstances we find that results conforming closely to Weber's

[1] With Woodworth, 30, pp. 150, 169 f. Woodworth, however, gives no clear indication of the basis of these different perceptions. With him too we may readily allow direct "judgments" of the force used in, and the resistance opposed to, any movement.

law are obtained for short lengths of line. The law does not hold, however, throughout a large range of distances, because of the ready applicability of the quantitative, conceptual form of identification. In the simple form in which distance and extent occur within the data of any one sense, it cannot be said to vary truly as a modification. True variation may, however, occur within narrow limits, or by the integration of data of heterogeneous senses, *e.g.* touch and articular sense, to a large degree.

The attempt has been made to express the results of the measurement of sensations in Weber's law in terms of sensory distance instead of in terms of component units of sensation. But the least recognition of the nature of the modification of distance, as discussed above, shows that it has nothing to do with the conceptual or numerary order of just perceptible differences of any kind, even of distance itself. Distance is not integrated from any other attribute than order (cp. above, § 5, p. 153).

Distance and the attention. We have discussed the relation of the modification of motion to the attention, and have suggested that the attention is apparently attracted by motion, because motion in an otherwise resting field forms a single one of a class of experiences not represented and therefore seems to attract the attention as does a single light, a single sound or any other unique experience. Now it can hardly be said that distance exerts a strong attraction upon the attention. There is no doubt about its presence in the case of distance as given by the aesthesiometer, by separate points in an empty visual field, or as an accompaniment to any motion. In the last case its presence is as evident as is that of motion. In the first two cases and more especially when the two points are rather far apart and are not the only points excited, its presence is not so evident and unmistakable. For in this case not merely one, but all our visual experiences are modified by distance. There is therefore just as much rivalry in reference to the attention as when any group of similar experiences is given. So long as our sensory experiences are taken collectively or the attention is in any way helped to grasp a number of points as a unity, the integration of distance will be complete and exhaustive. We are all familiar with the effect of symmetry and balance of distances in this respect. If very many points are given and if the attention for any reason is directed closely towards one point, *e.g.* by its motion, there may be an imperfect psychical realisation of its distance. We very often notice a tendency to emphasise and heighten the effect of distance by the conversion of a simultaneous distance into a successive one, as for instance, when we more accurately measure the distance between two points by looking from the one to the other. In this way one distance is separated from others by means of motion and reduced to the form in which sensitivity, at least to the threshold sensation, is greatest. It need hardly be added that distance is not realisable apart from its constituent sensations, whether actual or revived.

Interval. In sound distance appears as interval. The characteristics of the modification of distance are found in that of interval. Interval results from the integration of either successive or simultaneous tones. It is directly experienced and is not the result of judgment or of the conceptual comparison of the pitches given. It presupposes no knowledge or realisation of the absolute pitch, but only the psychical presence of tones of different pitch. Appreciation and comparison of interval can therefore occur in a perfect form without "absolute ear," as for example in the case of Wagner. Interval has no limits in respect of the pitch-differences of the constituent tones, although its successive form is limited by time-interval. Melody, as we saw, has both "space" and time-limits. Interval may occur without melody, but it is more urgent and clearer with it than without it. In fact many people can recognise interval only in its successive form.

The threshold of interval is peculiarly affected by the physiological peculiarities of hearing, which give rise to beats and intertones when tones of neighbouring pitch are given simultaneously. The difference between simultaneous and successive intervals is therefore marked. Small successive intervals are not disturbed by physiological excrescences as are small simultaneous intervals. But for these disturbances we might expect to find that the threshold for the simultaneous form is higher than that for the successive form[1]. For if the physiological theory of cortical representation used to explain touch-discrimination be adopted here, we should have a fusion of excitations corresponding to order-differences and with it a fusion of differences of voluminosity, which are much rougher. The result would be a rather more intense tone of voluminosity equal to that of the greater of the two tones and of slightly

[1] Cp. the facts detailed by Stumpf (25, Vol. II. p. 397). A tone, under certain circumstances, seems to be slightly lowered in pitch when another, considerably deeper, is sounded, and to be raised slightly, when a much higher one is given. This probably has a physiological foundation, as well as the psychological one that is exemplified in some of the visual illusions.

indefinite pitch. For all we know, this may be actually realised[1] in those cases in which the pitch of the two ears is different. But it cannot become a prominent peculiarity of the discrimination of tones. As in other forms, the distinction of direction in melody and interval has a higher threshold than has that of motion. Interval shows the same relation to the attention as does distance.

The appreciation of interval, as of melody, is independent of consonance, tonality and rhythm. It arises, as we must suppose, simultaneously with melody, and both are there as soon as the constituent differences of pitch are given. The origins of consonance, tonality and rhythm are quite separate problems. In talking of interval in the primitive sense, we cannot mean consonant, dissonant or "tonal" intervals. There can be no doubt that, whatever may be the actual state of human hearing now, interval is psychically conceivable and possible without any consonance, tonality or rhythm. It seems best to refer consonance to a physiological basis, whereby, owing to the partial identity of stimulation of a tone and its octave and the like, a partial fusion similar to that of simultaneous touches, too near to be distinguished or from neighbouring sides of two adjacent fingers, takes place. The recurrence and mutual compatibility of pitches seems to be quite another phenomenon[2], which is known as tonality. For it a special explanation suitable to its peculiarities has to be sought.

§ 8. Retrospect.

These two, motion and distance, are the only modifications of sensory experience which result from the integration of the elementary sensations of one and the same sense. We have selected them for study in order to show clearly the peculiar modification of experience inherent in each, its derivation from a common attribute and the similarity of the phenomena peculiar to the same generic modifications, motion and distance, in the various senses. A number of other peculiarities of these modifications were mentioned and will be referred to again. For having thus established the general type of a derived modification, we shall now use our knowledge to classify certain experiences, hitherto supposed to

[1] Even without this, we must allow that the discrimination of simultaneous tones is not more wonderful than the discrimination of touches on the skin. In fact, our whole treatment shows that these processes are parallel. The arguments of page 143 are only special pleas. The extensities of sound cannot be supposed to overlap just because they are neighbouring or because one is greater than another.

[2] Cp. Stumpf (25, Vol. II. esp. p. 197).

be elements or aggregates, as modifications resulting from integrations as yet undiscovered. We shall thereby justify our starting point and by it advance to new knowledge.

There are many well-known modifications of sensory experience besides motion and distance. There is no need to attempt an exhaustive enumeration of them. Some, like the localisation of sounds and tonality, belong apparently to the products of a single sense. Others seem to result from the integration of sensations of different senses. Examples of these are depth and apparent size, the vertical direction in vision, and many complex forms of apparent motion and distance. Each of these will call for careful study. But that cannot be attempted here, for it is the purpose of this paper not to cover the whole field, but by a study of the simplest cases to draw attention to these new and highly important problems.

The phenomenological study of these other modifications of sensory experience presents no new difficulties. We can readily classify them in reference to the primitive attributes of sensation. We can explain their introspective barrenness and elusiveness, their attachment to sensation, their incapacity for isolated existence or recurrence, and so forth. Only the actual analysis of these modifications into their constituent elements, and the discovery of the whole mechanism of their integration, physiological and psychological, now present any difficulty. And that rests ultimately in our ignorance regarding essential facts involved in these complex integrations. But having succeeded in dealing with the phenomenological problems of our subject-matter, we may feel assured that we are on the path towards a solution of the new integrative problems which will arise.

§ 9. CONCERNING THE SUFFICIENCY OF SENSATIONS AS ELEMENTS OF EXPERIENCE.

The efforts of the earlier psychologists of the associationist period seemed to lead to a clear conclusion. The only elements of mind appeared to be impressions or sensations and their counterparts in indirect revival, while the only bond of connexion between them was association. But although this result was eminently satisfactory and efficient in the first rush of study, on closer examination it was soon found to break down in many subtle cases. A subsidiary principle was therefore needed to account for the fact that the elements of mind do not always seem to survive in the complex state; for where no further elements were forthcoming, psychologists were justified in seeking to explain, as well as they could, how the given elements could be thought to account for all known varieties of experience. Thus we get the conception of mental chemistry, which we can, of course, now easily recognise to be even in its origin mistaken. But at the time, reasoning by analogy suggested it as a likely manner of realistic interpretation of the mind. Such a conception breaks down, because we cannot apply those indirect tests that are pre-supposed by a realistic interpretation of the mind. Physiological tests of its correctness, even if they were unambiguous, are often practically beyond our reach; and we have had no success in indirect psychological tests, which might have proved the unconscious presence of sensory elements in states that could not be reduced directly to these elements. Whether these tests were ever actually carried out is a matter of indifference to our present interests. The chief objection to the view was based on its greatest difficulty. It did not explain how out of the elements given something arose which appeared to be essentially different from these elements. Only one explanation lay to hand—association; and as, in ordinary cases, no such radical change of appearance was produced by the action of association, its presence could not explain these mysterious transformations in certain cases. Instead of seeking an outlet by the postulation of new forms of connexion between the elements of mind, later psychologists allowed their minds to be impressed with the apparent qualitative difference between the elements and the alleged compound states. Thus we next find a growing conviction that at least feeling is an elementary state of mind, other than any of the known sensations. It might have to be classified as a peculiar kind of element with characteristics fewer or other than those of any sensation, but it must in any case stand apart. When this point was reached, the influence of the prevailing Kantian attitude towards knowledge and the needs of the experimental extension of psychology which had just come into vogue, checked any further advance for a number of years. Now that experimental observation has greatly extended the basis of psychology and a temporary exhaustion of the more obvious problems of the senses has encouraged the attack upon the less tangible states of mind, we find a rapid extension of this attitude towards feeling. Any mental state which is not clearly reducible to more elementary states is to be itself an elementary unit. So we find thoughts, conscious relations, attitudes, recognition and the like added to our lists of elements (6, 7, and others[1]).

[1] Cp. also 29: "Wiedererkennen ist als Bewusstseinsinhalt ebenso primär und unerklärbar, wie Rot oder Lust."

We have now to turn to the other side of the process and ask how the study of the modes of combination of elementary states of mind has progressed. Unfortunately, we find practically no advance whatsoever. The experimental investigation of memory and reaction has worked so successfully with the notion of association, that, in spite of all sorts of restrictions and parentheses applied to any suggestion of its universality, no other form of combination has been sought. Even the earlier attempts to vary the form of association by adding to mere contiguity the bond of similarity, contrast and the like, have been very often abandoned by experimental research. Whatever may be our final conclusion regarding association, it is clear that even in its primitive form it has been a most useful conception. Whether its statement is complete and adequate is another question, which need not be touched upon here. The only really satisfactory chapters of psychology of the present time deal with association. But the scope of this force is rapidly being traversed and its limits will soon become rudely apparent.

Psychology can hardly remain satisfied with such elements as thoughts, relations, recognition and feeling. All sorts of difficulties have already been raised regarding the last of these. What are its adequate conditions? What are the organs which subserve it? Why is it individually so very variable? It is not at all easy to construct a physiological theory to answer these questions. Much more must this hold for thought and the like. From the psychological side also many questions demand an answer. We want to know what characteristics these new elements have, so that we may be able to distinguish them as experiences from our sensational elements. And if their characteristics are other, fewer or more than those of sensations, we have to ask how they contrive to exist without attributes which are generally considered to be essential to the existence of sensations. A satisfactory answer to these questions will not be readily forthcoming.

Amidst the ruins of the old associationist theory in its various forms two parts remain intact and firm: the elements of sensation and the bond of association. We have seen how the distinction of new elements attempts to fill out the deficiencies and raise a new scheme of mind. But it is possible that the elements of sensation are, after all, sufficient in themselves and that it is our binding material that is insufficient and unstable. Considering the difficulties involved in the postulation of elements other than those of sensation, it is surely the more correct method to see how far we can carry our elements of sensation by the postulation or demonstration of a variety of forms of combination.

Only when we fail to progress on these lines need we recur to the differentiation of new elements. Their justification, in any case, will not be easy.

§ 10. Feeling.

As sensation. It is certain that feeling is a peculiar modification of experience, extremely unlike sensation. To try to reduce feeling to aggregations of organic, or, more especially, visceral sensations is a hopeless task (4). For, however decisively it may be shown that feelings are always accompanied by or are dependent for their occurrence upon some or certain sensations, no means has yet been established of proving that feelings consist of sensations. Feelings do not appear to introspection to be composite; and they do not show those sensational characteristics which we should expect to find in aggregates of sensations. Any decisive differentiation between feeling and sensation, therefore, precludes the theory that feelings are aggregates of sensations. For no matter how many accompanying sensations are tabulated, the feeling itself will always constitute an irreducible remainder. It need hardly be added that other peculiarities of feeling, especially its inherent reference to all kinds of processes, whether they be sensational, intellectual or conative, are not adequately explained by this theory.

On the other hand, the mere classification of feeling as sensation (26) is undoubtedly a weaker method of dealing with the problem. It is hardly possible, if a strict psychological definition (28) of each is sought. Only if we emphasise the discrepancy between different kinds of sensation, so that we treat them not as a type, but as a heterogeneous collection, can we sufficiently apologise for the inclusion of feeling amongst them. But to do so is to discount the value of what we thereby gain. If the value of classifying feeling with sensation does not lie in the introspective identification achieved, it must be found in the consequences for physiological and genetic theory. For the former a parallelism of relation between sensation and feeling on the one hand, and their sense organs on the other, is the weightiest proof. But here the difficulties are greater still. For an independent feeling, isolated from all reference to other experiences, must be of the rarest occurrence[1]. Any attempt to determine the sense-organ of a feeling-

[1] The occasional independence of feeling is witnessed by Külpe (14, pp. 227 f.); Ladd, whom Titchener quotes (28, p. 42), retains the reference of feeling, but denies any necessary time-relation of feeling to "the sensations and ideas by which we classify them." The

sensation is idle speculation, while various peculiarities of feeling to be mentioned later remain unexplained. Such a theory of feeling would be useless, even if it were possible. It makes no positive contribution to the explanation of any of its peculiarities. If it be said that the theory explains the rapid evolution of such an art as music, in which things formerly unpleasant are now very pleasant, it may be pointed out that, on the basis of analogy, more could be said against the rapid evolutionary adaptation of a sense-organ than for it.

There remain, therefore, only two psychological theories of feeling for our consideration. None other seems possible. Both maintain the unique peculiarity of the experience; but, while the one considers feeling as an irreducible element of experience, the other holds that feeling is the result of the integration of other experiences. To the former most psychologists of the present day adhere, while the latter has been advocated by Herbart and Lipps.

As element. Objections have already been raised to the view that elements exist heterogeneous to the sensational type. If the occurrence of extensity in some sensations makes it hard to admit its total absence in other sensations, we must find the case of feeling equally embarrassing. In dealing with sensation, we had the advantage of starting from a psychophysical definition which definitely grouped our material for us before we attempted psychological definition. Feeling was not included therein. For not only is its sense-organ purely hypothetical, but it has as experience none of that local precision and dependence upon stimulation which is sure evidence of dependence upon a sense-organ. Even if we could let that deficiency pass, we can hardly turn to a study of the compound experiences into which feeling enters in the hope of discovering thereby any latent attributes, not observable by introspection. There is no integrative modification of feeling to be thought of, unless it be the reference of feeling to other experiences, which it thereby qualifies. But that would probably necessitate the postulation of an attribute of order inherent in feeling, a clear, definite localisation or basis of psychical arrangement in the independent, isolated feelings. Since feeling can be excited by practically any kind of experience, we should then be able to arrange and realise a whole system of feelings, a feeling-world similar to our visual world, or a feeling-world which would really

extreme position held by Külpe is now modified to refer only to Gemeingefühle (*v.* 16, p. 185): "Die Einzelgefühle sind an bestimmte Einzelinhalte (Empfindungen, Vorstellungen, Gedanken und deren Komplexionen) gebundene Gefühle. Die Gemeingefühle sind umfassende, allgemeine, das ganze Bewusstsein färbende Gefühlszustände."

constitute a psychical universe. But, as a matter of fact, it is feeling which is placed by reference to other experiences. These do not constitute two separate systems, mutually coordinated like vision and touch.

Integrative theories of feeling. These have taken various forms. For Herbart feeling is the relation to one another of ideas which support or inhibit one another. Much the same is maintained by Lipps, with the addition of a direct reference to the relation of ideas to the ego. A recognition of this feature of feeling is also given in the earlier statements of Plotinus, Descartes, Leibniz and Wolff, that it portrays the momentary perfection or imperfection of the soul. We may neglect, for the moment, their use of the word knowledge, which for the sake of systematic statement we must here read as awareness, for there can be no suggestion that feeling is a state of knowledge. If we leave out of account the old superstitious craving for mystical unity in the greatest things, which led to the connexion of unity with perfection and therefore to the assertion of the unity of the soul, in spite of its many-coloured experiences, we may claim the view for this class. For the perfection of the soul was doubtless based upon the harmony of the soul and its experiences or of these amongst themselves. Why should the state of perfection of the soul otherwise change? Lastly, we find a similar view in the reference of feeling to the form of reaction of apperception to sensations (Wundt), or perhaps in the classification of feeling as a "Gestaltqualität" or formal, qualitative modification of experience. But none of these theories has explained why these relations should emerge as feeling and why feeling should have its many peculiarities. It is unnecessary to discuss the validity of these views now. It will be sufficient to point out, after the development of our own theory, in what their validity consists.

Varieties and characteristics. It is now commonly recognised that there are only two kinds of feeling—pleasure and displeasure. By some, *e.g.* Wundt, a multiplicity of qualities is advocated. The position we take up does not, however, require a preliminary discussion of this question. No better statement of the arguments against a multiplicity of qualities could be given than that of Külpe (16). These are: (1) the general comparability of pleasures and displeasures in reference to one another, whereby a methodical view of the value of experience can be obtained, no matter what its underlying qualitative differences may be; (2) the possibility of an unlimited interchange of feelings; (3) the indifference of feelings in reference to comparisons of sensations, images or concepts, whereby a purely unbiassed, objective comparison of these things is rendered possible; (4) the fact of a general transference or irradiation of feeling, whereby a feeling dependent upon an experience (*a*) can be transferred to an experience (*b*), if there be a regular bond between (*a*) and (*b*); (5) the fact of a very extended analogy among feelings and the resultant possibility of a replacement of one impression by another or of the characteristics of one by another, whereby we can talk of a bitter sorrow, a sweet happiness, a tender regret, a rude misfortune, a cool feeling, an ardent sympathy; (6) the absence of direct influence of feelings upon memory and (7) the improbability of a great variety of pleasures and displeasures; for if we had this, it would be easy to arrange feelings on their own merits into a vast scheme, whereas, as we have already suggested, there is no such vast variety, but only the merest distinction of pleasant and unpleasant feelings in independence of the objects or states they qualify. As Külpe says, there is no need for feelings to express over again the variety sufficiently expressed by impressions, but only to show their nature, attractive or otherwise. This whole statement is of the greatest interest to our position, for, as we have maintained that the properties of sensation can be determined, not merely by direct inspection of them, but also by examination of their modes of combination with other experiences, so it shows that a broad survey of the forms of connexion of feeling with other states will help to settle the nature and forms of variation of feeling, even when introspection may leave these still in dispute.

These two kinds of feeling—pleasure and displeasure—(1) do not depend for their occurrence upon the stimulation of any one particular kind of sense-organ. It is remarkable that they (2) seem to leave no image, (3) are not reproducible, and (4) are not associated with one another or with images. They are (5) also very frequently, if not always, consciously referred to or attached to other experiences[1]. Feelings are (6) amenable to introspection only to a limited degree. Anything that tends to weaken or dispel the experiences upon which they are based, thereby weakens or dispels them. Feelings (7) vary in intensity, but do not seem to have any extensity or localisation, except in so far as they are attached to experiences which are localised and extended. Finally it may be maintained that feelings (8) arise only when two or more experiences are given, or that no single elementary sensation is of itself capable of evoking feeling necessarily or regularly,

[1] Cp. on these points, 16, pp. 183 f.

but that, if the feeling seems to be aroused regularly by some elementary sensations, as *e.g.* by those of the lower senses, taste, smell and the like, the regularity of occurrence is not absolute and is therefore dependent upon some other element of experience which is usually present, but may be absent. Whatever detailed casuistic may be brought against these statements, they have all a large amount of probability and may therefore be presumed in favour of any position which can use them.

Comparison of feeling and motion. Our theory of integration demands that we refer a modification of experience such as feeling to an experiential basis in more primary experiences and make a statement of identity between these two which shall be self-evident. We have every right to seek our primary basis of feeling in the experiences to which it refers or is attached. We saw in the case of sensory integrations how an integrated modification of experience is attached to its primary basis. Motion, *e.g.*, is inseparable from a sensory basis; it (1)[1] need not always be attached to one particular sense, but it cannot be experienced in isolation from all sensory forms. Even the recollection of a motion never implies the isolation of the experience from its sensory basis; for to dream of a movement is to dream of the successive sensations progressively different in order and so to realise afresh the experience of motion. In other words, (2) motion of itself leaves no image behind. This does not, of course, mean that we rarely think of motion that is not given by stimulation. Such a statement would be just as absurd as the declaration that we never think of feelings unless they are actually elicited, whether for the first time or afresh by the recall of the experience liked. We can think of motion or of depth[2] or of any experience we may have had, whether we can have it now again or not. But we usually recall events in single successive stages of projection and motion (3) is not reproducible in isolation by itself, but is re-created afresh in our experience when our memory of successive phases is sufficient to re-establish it. There is no evidence for the existence of a memory image of motion which differs from sensational motion, as the imagery of the usual sensations differs from these. The same is true of feeling. But as our theory provides an adequate basis for the re-creation of motion, so also may an integrative theory do for

[1] The numbers in this paragraph refer to those of the previous one.

[2] It has been said that we never recall depth in representation. Probably we seldom do so. The most vivid memory of depth I have noticed occurred when I was engaged in a special study of the experience of depth. I dreamt I saw a picture of a bunch of flowers in perfect depth-effect. In my dream I shut one eye to observe disparity of images and the depth-effect immediately vanished.

feeling. In the same way it would be easy to show (4) that motions do not associate with one another. So one process of recognition does not recall another. Not only is motion psychically inseparable from, but it is always (5) attached to, or more or less embodied in, the experiences which form its sensory basis. It is obvious, therefore, that (6) motion cannot as an experience be studied in isolation from its sensory basis. It is even impossible to lay hold of motion by itself and describe it. Motion is indescribable except in terms of the sensations upon which it is based. Of course, sensory data may be steadily maintained by the action of external influences, and we may exert our introspective attention to the utmost without disturbing the experience of motion, so long as the effort does nothing to dispel these sensory data. Motion is a purely mechanical sensory integration. Feeling, on the other hand, even when its primary basis consists of elementary sensory data, *e.g.* those of vision and hearing, seems to involve attention or some vague attitude in a subtle way. This accounts, however, not for the elusiveness of the pleasure experience for introspection, but for the speedy collapse of the sensory basis of a feeling as soon as attention is directed to other experiences than that sensory basis itself. Hence the rapid disappearance of feeling when introspection is turned upon it. We can bring the observation of motion into the same state, if we try to observe it in isolation from its sensory basis by diverting the eyes from the moving thing. This argument is, of course, not at all prejudiced by the fact that we have experiences of motion which, in the view of some, are really primary and irreducible, *e.g.* those of articular and labyrinthine origin. On the contrary, we must conclude that the very difficulty of these experiences for introspection is due to the fact that they are modifications of motion, which are not usually correctly analysed and whose primary elements are very weak when isolated, or resist isolation altogether. It is clear, finally (7), that a derived state like motion need not share all the characteristics of the sensations upon which it is based. Indeed it cannot. It is a secondary form of that attribute whose differences it integrates to unity, and it may show forms of variation owing to the influence of factors which affect the primary attribute integrated, as does motion in speed, in so far as the rate of change of order in time varies. A motion cannot, as such, be intense, or spread out or saturated. Feelings are of two "qualities," pleasure and displeasure, and they also vary in intensity. But they are not extended, localised, ordered, or saturated.

There is therefore a very close resemblance between the two, experience of feeling and of motion, which would certainly justify their classification together. We have every reason to expect that the same kind of explanation is valid for the two states; and as we have found the theory of integration adequate to explain all the characteristics of motion, we may apply the same principle of explanation to feeling with much hope of success. However different feeling may be, as experience, from any others, it is clear, at least, that its characteristics are not unique and that it is probably the product of integration.

Is the integrative basis for feeling sufficient? Although in the previous pages we have shown the similarity between feeling and other products of integration, we have not yet verified for feeling one of the essential conditions of integration, viz. (8) the presence of a multiplicity of primary experiences in every case of feeling. There can be no question, however, that such a plurality of data is present in the vast majority of cases. It is the harmony of experiences, of colours, sounds, tastes, smells, motions, distances, objects and thoughts which is the object of feeling, *par excellence*. In many cases a single one of these is distinctly pleasing only because it is in some way a change from some other. The only formidable exceptions are found in the so-called lower senses of taste and smell. There we seem to find single, isolated, elementary sensations which evoke very pronounced feelings. All children and most adults find sweet tastes pleasant and sour or bitter ones unpleasant. At least there seem to be clear cases in which a merely sweet taste, as such, is liked, while strong, bitter tastes are disliked. In considering cases like these, we have to remember that the same sweet or bitter taste does not always evoke the same feeling, although it seems to act solely by itself. To the one person it may be pleasant, and to another or to the same person at another time it may be indifferent or unpleasant. In the face of the apparent isolation of any taste and its feeling, this has often been expressed by saying that the feeling evoked by a single experience is not due to psychical, but to physiological necessity, which again is to be referred to physical and chemical conditions or to the vagaries of biological selection. But such an explanation has already been shown to be untenable. For it either implies the existence of a sense-organ for pleasure or it denies altogether, or rather ignores, the possibility of a psychical causation, and it fails in any case to explain the peculiarities of feeling. We can really do nothing at all with the assumption that our experiences are merely hitched to one another, we know not how. Unless we can show convincingly that they are pure and primary datum, we must at least endeavour to show some form of systematic and regular connexion between them. In so far as they are primary datum, we must be able to show that our experiences are immediately and regularly dependent upon some form of objective condition which is not experience, as we know it. In so far as they are not mere, primary datum, we must endeavour in principle to show that they are wholly and solely the result of the interaction of those experiences which are mere primary datum. If a primary element of experience is found in apparent isolation with what is obviously a derived modification of experience and then again is found without it, we may, on our hypothesis, fairly look for some undetected variable element of experience, present in the first case and absent in the second.

Fortunately there is evidence to show that some such variable element exists. An extreme case like that described by d'Allonnes (4) shows that the integration of feeling is impossible without internal visceral sensations. There is therefore no difficulty in assenting to the statement that a multiplicity of sensory data must always be present with feeling. In the case referred to, no distinction of feeling was made between a glass of water and a glass of castor oil; a choice was made in favour of the water only by help of conceptual remembrance. It would certainly be wrong to maintain upon the basis of this and similar cases that feeling consists of visceral sensations. For we should again fail completely to explain the peculiarities of feeling. If feeling were an integration or aggregation of such sensations, we should experience it as such, it should bear all the characteristics of sensation as such or of an integration of sensation, and it should be referred or attached to visceral sensation. In the case of the simple feelings, however, we find an attachment or reference, not to visceral sensation, but to all or any kinds of sensations or experiences which evoke them. That the visceral factor is not a direct constituent of the feeling itself is shown by the considerable unlikeliness and unexpectedness of the existence of a visceral factor at all. Feeling, therefore, does not consist of visceral sensations, nor are these the only essential element in feeling. If the parallelism of feeling and motion is of any value, it shows that one, at least, of the essential elements of an integration is that to which the modification which results refers or is attached. The pleasant sight or sound, the nasty taste or smell must each contribute one of the differing elements which constitute the integration. In the case of tastes and smells the visceral factor indicated by these abnormal subjects may be highly probable and acceptable as the integrative complement to the exteroceptive sensation. For the sensations of the lower senses bear a clear reference to the internal, digestive apparatus. Our appetite is stimulated by them, they suggest inhalation or embodiment or they at least draw us nearer. But it is hardly so with sights or sounds. Lovely pictures and music do not often consciously stir our bowels or rouse our bodily appetites; nor does their unpleasantness bring to our minds the dispeace of our organs. The pleasantness of pictorial art or of music seems usually to reside wholly in itself. It is pleasing or ugly solely on its own merits, or at least largely so, and hardly at all because of its effect upon the viscera of the connoisseur. At the most we say we do not care for a work, because it does not appeal to us, does not arouse in us, perhaps, the emotions and sentiments to which we are most prone. These latter experiences may be dependent upon and may carry a conscious reference to the internal organs of the body. But the peripheral, primary pleasantness of the sensations of the higher senses can hardly be thought to do so. This view is supported by the fact that it is so rare to find a single sensation of these senses which is pleasing, purely by itself. But there is, of course, nothing to prevent visceral sensations from being aroused by and integrating with these higher sensations upon occasion.

Of what attribute of experience is feeling the integration? Far from being peculiar to visceral sensation, it must be one which is universal in experience. We know that two or more different qualities of almost any group of sensations may form a pleasant or unpleasant combination, although differing only in order or place in time. We shall, of course, look for examples of this in those senses in which we do find a variety of qualities, *i.e.* in vision, sound, smell and taste. Senses like touch or those of the joints or muscles, which have little or no qualitative variations, seem to be more or less indifferent. Even a variety of orders, as in visual, pictorial arrangements, of durations, as in rhythm, and possibly also of intensities and extensities, as in the arts of vision and sound, may be pleasant or unpleasant, without any other accompanying differences. We do not usually find that qualities of different senses combine to give pleasure or displeasure, unless we except those single sensations of the lower senses of cold, warmth, pain, taste and smell in their conjunction with sensations of visceral origin. Differences in a secondary modification of experience are also often the object of feeling. A complex of motions, of distances or of depths may be liked or disliked, while this can hardly be asserted of any single one of these. In view of this fact and of the high frequency of feeling in reference to the senses of vision, sound, smell and taste, which already show direct or indirect evidence of integration, it might be thought that feeling is the index or result of the mutual harmony of integrations. It is certainly not a regular accompaniment of integration as such. It would have no *raison d'être* and could not explain itself as such. If feeling means the harmony or smooth working of the mind, this can only commence with a second level of integration, if at all. It would be rash to attempt to go beyond these conclusions at the present time to a specific theory of the attributive basis of the modification of pleasure. The results of experimental investigation are too scanty and contradictory to give any clear leading (8). We need not be surprised that we are meanwhile unable to point to the integrative basis of feeling, although we have made the demand that psychological explanations must be causal and self-convincing. In the articular and labyrinthine "sensations" of movement, we have examples of experiences the reduction of which baffles many psychologists, although the integrative basis is in these cases moderately obvious. We shall hope to be able to explain feeling completely, when our attention has been drawn experimentally or by analysis to its attributive basis. For this purpose, we must have more details of an introspective nature concerning the moment of realisation of feeling. There need be no particular difficulty in applying introspection fully, for we do not need to introspect feeling itself, but only its sensory or other accompaniments. We have the further guidance of the variation of intensity peculiar to feeling, which, after the analogy of motion, would suggest that the attribute of which feeling is the integration is capable of a variation by degree, similar to that of intensity in the sensations. The duplicity of quality of feeling also suggests that we have to deal here, not with a simple, primary attribute, but with an integrative activity of some kind, which is capable of reversal. The activity theories of feeling seem of all to be nearest the mark. It is quite unnecessary to point out what theories of feeling of a metaphysical or other nature are completely discounted by the integrative theory here advanced.

§ 11. RECOGNITION.

It is generally acknowledged that recognition is a peculiar experience which calls for some explanation from any theory of the constitution of mind. It has hardly been claimed as a sensation; the prevailing tendency has been to treat it as a complex experience or as a unique, non-sensational element.

As sensation. On sensationalistic principles, recognition is easily accounted for. It consists simply in the revival of those sensations which were previously given simultaneously with the complex of sensations now recognised. On general principles, it is explained in the same way, without the restriction to sensation. Experimentally this has been verified in so far as it has been shown that such revival does accompany or follow recognition in the vast majority of cases (9). Cases of revival are, however, possible without accompaniment of recognition. We seem unable to say what kind of recall constitutes recognition. Although experimental analysis gives an almost general rule, the synthetic statement of it seems very unsatisfactory. Put a number of actual and of revived sensations together and does recognition supervene? Surely not! We miss some proof that the elements or experiences given are actually such as can be shown convincingly to give the state to which they are equated. The difficulty of such a synthesis is only increased when we find that the state of recognition can supervene before the sensations upon which it might be based are revived at all[1]. An explanation of this is sometimes attempted by supposing that recognition can occur when associated experiences are merely excited and not yet actually revived; as if a tendency to reproduction, in some physiological or real psychical sense, could produce an effect upon the experience which is to be recognised, without reviving in experience the states which it serves to reproduce. For the present, we refuse to discuss a theory which thus begs the question and does nothing to explain the psychical peculiarities of the state of recognition. It can be sufficient only where everything else fails, where, as in the case of the sensational elements of experience, there is left nothing psychical by the use of which we might attempt to explain their peculiarities. But it is not claimed in this case that recognition is an isolated, elementary state; for it is firmly attached to the state recognised and does not occur alone. These and other peculiarities call for some explanation, which such a view cannot give. For, while its theory may be sufficient physiologically, it is insufficient psychologically, in that it cannot explain how the state of recognition comes to be hitched to one out of, possibly, many experiences of the same kind. There can be no doubt that the state of recognition is, at least, a modification of experience which is not identical with any one

[1] Cp. 1, "Dans l'acte de la reconnaissance le souvenir se joint à l'impression avant qu'il se développe en image."

sensational element or mere aggregate of these on the lines of associative synthesis.

As element. But it has been claimed that recognition is a peculiar non-sensational element of experience. Against this we have to urge, as before, that nothing has been done and probably nothing can be done to explain the psychical peculiarities of these elements. The acceptance of such a view bars the possibility of any closed science of experience, at all complete and self-coherent. We are left with a psychical pluralism which does not even invite reduction. It must be said, however, that the acceptance of non-sensational elements of experience is not really an independent view, but is only an expression of the recognition that certain states of mind are unique in character and very unlike sensation and have, so far, defied any satisfactory reduction to sensation.

It may be noticed that recognition does not seem to occur entirely in isolation. I am not aware that the contrary has ever been maintained in this case, as has been done for feeling by Ladd. It would seem absurd to think that recognition should occur as a state without any experiences which might constitute an object for it. It may seem equally absurd to make a similar statement for feeling, unless for a general state of feeling, in distinction to the special, detailed state not referred to the self as a whole. Except that it may be established that other experiences of some kind always do form one of the conditions of the appearance of recognition, there is no apparent reason why recognition, if it is an element, should not occur in isolation. The view which considers recognition elemental might, however, point to false recognition as a case of the partially isolated occurrence of the state, apart from its correct and realistic implications. But here, again, we find no explanation of the reference of recognition to an object, correct or false.

As secondary modification of order. We must hence revert to an integrative theory of recognition. Clearly this modification of experience bears a strong resemblance to the attribute of order, as we find it in the sensations, in the form of localisation or especially in sound as pitch and secondarily as localisation, or in the modifications of motion, distance, depth and perhaps direction. Recognition is "qualitatively" the assignment of a place or order of a special kind to an experience which, so far as its elements, their attributes and integrations are concerned, is in recurrence[1]. It may, therefore, be classed as a

[1] The notion of mental causality cannot demand a recurrence of the real material of mind, as if our experiences, once had by us, went off on a round by themselves and then

secondary modification of order. It seems, however, to be an unvaried modification and, in this respect, unlike motion and feeling. We either recognise or we do not. Our certainty and clearness of recognition vary somewhat, but these variations can hardly be said to be necessary and characteristic, as are those of feeling and motion. They are more probably based upon the process of recall involved in recognition than upon the integration which constitutes recognition. The falsity or correctness of recognition is also no true variation of it, but is rather another modification of experience which may occur in many other connexions than that of recognition. The order given to each experience by the recognition of it is, of course, different, as is the order of different sensations of the same quality, intensity, extensity and duration, but the recognition-order of one and the same experience does not vary. One and the same experience may be recognised by different contexts, but in this case a radical change has been made in the complex which integrates to recognition, similar to that which occurs when an object that has aroused pleasure in one mental setting, arouses displeasure in another mental setting. This is no true variation of recognition, but a change in the states revived and thereafter integrated to recognition. Its explanation, therefore, belongs to that of reproduction in general.

Comparison with feeling and motion. Recognition (1)[1] does not depend for its occurrence upon the stimulation of any one particular kind of sense-organ. It seems (2) to leave no image of itself behind. This does not, of course, mean that we never think or remember of having recognised anything that is not now presented for recognition. It means, that if the state of recognition is ever "revived," it is recreated afresh by the recall of the experience which was recognised, the subsequent recall of the context of recognition and the integration of these experiences anew to the unity of recognition. But if this occurs outside the efforts of introspective experiment, it must be of very rare occurrence. Obviously, then, recognition (3) is not reproducible in isolation, nor (4) do states of recognition associate with one another. Recognition is (5) always attached to or, more or less, embodied in the experiences which constitute its integrative basis and cannot, therefore, (6) be studied introspectively apart from that

as *esse ipsissima* returned. We cannot expect to go further than identity of quality and all attributes of each element. Anything more than this would lead us out of psychology into metaphysics.

[1] These numbers refer to those on pp. 187 and 188.

basis. It is of the most fleeting nature and vanishes as soon as the attention is turned exclusively upon it. It resists description except in conceptual terms referring to the experiences which form its integrative basis. Recognition, finally (7), need not share the characteristics of the experiences upon which it is based. It has and can have no intensity or extensity or localisation in space or the like. It shares all the peculiarities of an integrative modification of experience and has in special those peculiar to integrations of order[1].

Is the integrative basis sufficient? An integrative theory of recognition will demand a reduction of the state to the integration of differences in order of the states upon which it is based. We must, therefore, be able to show (8) the presence of a multiplicity of more simple experiences in every case of recognition. But it may be pointed out that such a multiplicity cannot exist in every case, for the state of recognition has been admitted to occur before the revival of the circumstances of the first occurrence or without the recall of the first experience, in so far as it was identical with the data now recognised. We here face an important problem which can only be settled by a comparative study of the integrations of sensations of different senses, such as constitute our full and complex space-perception. For the present, we suggest that an integration seems possible that leaves the quality and other attributes of the objectively less interesting elements very much in the background, while making use of their aspect of order for the purposes of integration. A lengthy search may be necessary to reveal the presence of the elements to which one of the differing orders belong. For unless the orders integrated are very little different from one another, why should we expect the qualities, extensities or intensities, whose orders are integrated, to be near one another in the focus of attention? Even in the case of next neighbouring orders, there is no sufficient reason why we should expect this, except in the case of the primary aspect of order, attributive to the

[1] It is an obvious conclusion from the whole work of this paper, that states like feeling and recognition can be attended to just as well as can sensory states. We should find no great difficulty in stating the attributive relationships of any secondary modifications of experience. But attention cannot find in any mental state what is not there to find; and we can have no desire to make in reference to it a needless assertion of incapability. The argument of the text, especially that of point (6), is, therefore, justified only by reference to the usual similar statements made for feeling. Recognition disappears as soon as the attention is turned upon it exclusively, because to do so is to divert the attention from the integrative basis of recognition and to destroy it.

elementary sensation[1]. It is just the aspect of order whose integration takes place in this particular case, and not that of quality or intensity. It is, of course, impossible to specify *a priori* the time that may elapse between the occurrence of a state with recognition and the presence of the revived state. It would even be difficult to show that the latter is not present simultaneously with recognition and that its apparent absence is not due merely to the fact that it has not yet passed before the objective of introspection. But it would be unworthy to base a position upon such a possibility. We know from experiments on abstraction (15), that the orders of certain elements may be present and admitted introspectively without their qualities being distinctly introspectable, even if present; and we know that qualities can be given and distinguished without their localisation, right or left of one another, being introspectively distinguishable. The possibility of the separation of the attributes for introspection, even if only for a moment, must be admitted. Where, therefore, integration refers to one aspect among several and unites it to a similar aspect in another state, round which as a unit the interest of the moment concentrates, there we may expect a still greater separation of the remaining aspects for the introspection. We may maintain that recognition is based upon the psychical integration of the order-aspects of percepts, although it is often present before any associated percepts can be identified introspectively.

The integration of recognition and full revival may properly be considered to be different processes[2]. It is one thing for a state to be revived and joined to another to form an integration, and another thing for that revived state to be considered by itself and identified. We should expect with as little reason always to be able to state the direction or to distinguish from one another the constituent elements

[1] The focus of the attention and other similar terms are misleading, in so far as they suggest that our experiences make their way to the centre of a circle, as it were, dragging all connected experiences with them, more or less. On the contrary, it may be claimed that only the order-aspects of our experiences change without any movement of attention, unless we use that term to indicate the direction of the integrative and associative processes now taking place. If we recognise an object, therefore, our 'attention' must necessarily be directed to the process of recognition and the object recognised, but it need not be directed to the associated states which form the integrative basis of recognition, unless these take up the work of integration and recall, as they often do.

[2] Such a psychological account agrees well with the physiological theory which postulates the partial or incipient excitation of reproduction-tendencies to explain the occurrence of recognition without actual revival.

of a motion or distance, as soon as we appreciate the presence of these, as always to be able to refer a recognised state conceptually or otherwise to its first occurrence. We found this difficulty already in the case of feeling, where the apparent isolation of the integrated state with one side of its integrative basis is in some cases even more pronounced. Recognition, therefore, may be accompanied by the assurance of its correctness or by the knowledge of its point of reference in experience, or it may occur without these (20, pp. 39 ff.). Experimental investigation reveals the presence of a direct unmediated experience of recognition, which can occur alone and must be considered to be the only true form of recognition (11) in no way to be confused with the conceptual inference of "previous presence in experience" from certain "criteria" (20). It is impossible that recognition should always be based upon such criteria, formal or otherwise, as Meumann (*ibid.*) suggests, for there is in primitive experience no means whereby any formal or other characteristics of experience should be made to stand for previous presence, unless an experience which conveys the fact of recognition to the mind is first given. If this state is once given, all sorts of criteria or means of certainty may be found for it. This truth must be recognised once and for all, if psychology is to be freed from hitherto incessant fallacy of argument. Integrative processes which contain their full determination within themselves are absolutely necessary, if experience is to be explained; for experience is not the product of mere incoherent chances. Recognition and assurance are both integrative processes of a function and character different from the process of conceptual reference within experience. All three may take up different time-relations to one another.

Recognition is not a modification of the time-aspect of experience. To recognise may often mean to refer a present percept to a previous occasion, but it does not do so as a modification of the time-aspect. Recognition, as experienced, does not vary in the sense of "a little time" or "a long time." Although it may undoubtedly lead to a fixation in time, this time and the process of fixation are both conceptual, and not experienced modifications of the peculiar non-conceptual kind of recognition and feeling. Recognition conveys in immediate experience the fact that a given percept falls into a certain perceptual place or order. How that place is made explicit or conceptual, whether by circumstance of time, of name, or of thought, is quite immaterial in the present connexion.

It is important to notice, in the next place, that recognition presupposes the modification of order that is characteristic of the percept and its integration. It is unnecessary to go into the problem of the conceptual or general percept at the present. We may confine our attention to the particular percept, "that here and now." That the particular object of perception is a modification of order will hardly be put in question. Its full explanation presents peculiar difficulties, which, however, are not our present concern. Integration of the elements of experience to perceptual units must precede recognition. For the revival of order that might be evoked by the bare elements of experience, would be that of other forms of primitive, attributive place-order, and not that of secondary, recognitional order. The former, however, would give only some illusive increase of extensity or the like, and not recognition. If a bare element of experience is ever recognised, it must first become a percept and acquire the perceptual modification of order. The integration to the effect "that (tone, colour, object, face, word) here" must precede the integration to the effect "that here has been" or "that here is that there," both now, when recognition takes place, and then, when the experiences included in the recognition were first given.

Recognition implies simple revival, as we have seen. Once set up, revival may proceed, beyond the amount presupposed by recognition, along any lines open to it. Generally speaking, it will follow the lines of least resistance. In the case of the first recurrence of a percept which has not entered into any other integrative processes than perception, the freest line will be that of a revival of the circumstances of its first occurrence and it will therefore lead to recognition. But a percept which has been extensively manipulated in experience on the occasion of its first occurrence, may upon recurrence excite other lines of revival and may, therefore, be illuminated by the light of other modifications than that of recognition. The more these other lines of revival are strengthened, the more these other forms of integration are produced, the more should we expect to find that recognition recedes on the average, until it disappears entirely. This is confirmed by an introspective examination of the course of our experience. It would, of course, be absurd to extract from this statement the implication that I am, as a psychical actuality, unfamiliar with my most usual surroundings; for that would suggest that the statement made implies the presence of unfamiliarity in the absence of familiarity. There can be no such real implication. Really habitual surroundings are generally modified in many ways other than that of recognition. The flush of familiarity is experienced not by the hearth-bound native, but by the returning wanderer, whose first concern is to recognise.

§ 12. CONCLUSION.

The classification of experiences. Our study of the two integrative modifications of experience, feeling and recognition, serves to demonstrate the method to be followed in the study of all those forms of experience which are not conclusively elementary. What is to be considered elementary will be decided in the first place by the standard of the sensational type. Forms of experience that show most or all of the characteristics of those modifications whose manner of integration has already been traced, will be considered to be the result of integration. But if they are not reducible to elements already catalogued, their integrative basis must be sought in hitherto undetected elements. Any states of mind which may remain thereafter may be elementary or integrated. Their exact classification will have to form the object of special inquiry.

Feeling and recognition represent for our study the two great fields of affective and intellectual states. Whatever limits may ultimately have to be set to our procedure, we have, at least, shown that it is applicable over a large range of experience. No effort has been made to conceal the difficulties which faced the complete explanation of feeling and recognition. On the contrary, the hope may be expressed that something has been done to make them more distinct and assailable. Several other difficult investigations will probably have to be made before the theory of feeling and recognition can be completed. For these reasons, it is, for the present, quite unnecessary to apply our method to forms of experience similar to recognition and feeling, however interesting the task would be. Having shown how the study of such experiences is to be approached, we may leave it to the reader to make further applications himself. If the value of our method has been appreciated, that will readily be done. Each integrative form of experience will call for special study. There can be little doubt that there are a vast number of these. Research has already begun to discriminate some of those which group themselves round the term "thought."

The gain for the experimental study of thought. In this connexion it is interesting to consider briefly what positive gain accrues to psychological study from our method. Two items may be put to its account. The first concerns the experimental study of thinking. Although this has made much progress in the last ten years, it has, so far, been impossible to show conclusively upon what experiences certain special states of mind are founded. Thought itself, for example, without prejudice to any theory, may be called a peculiar form of experience, not obviously of the same type as sensation. It may, nevertheless, be held that it is reducible to sensation, and to this end all accompanying sensations will be carefully recorded. But, as we have already pointed out for feeling (§ 10), if any sort of discrimination is made between thought and sensation, it will always be impossible to reduce thought without a remainder to sensations or to other elements. Thought is either clearly an aggregate of sensations, in which case it will, at least, show all the characteristics of sensation; or there is obviously no such thing as thought, except in common parlance; or thought is a peculiar form of experience, irreducible on the basis of exhaustive experimental introspection alone. Our service to experimental research consists in showing how a connexion is to be traced between thought or any other peculiar experience and the distinguishable contents of the mind that accompany it. There is then some hope of mediation between an extreme sensationalism (28 a), which either reduces thought to a mere name for groups of experiences or utterly fails to explain those characteristics of it that are not evident in elementary sensation, and an extreme elementism, which discards all sensational accompaniments as irrelevant and builds solely upon the unity and peculiarity of thought (6, 7). We combine these two views by recognising both the relevance of the accompanying experiences and the peculiarity and unity of the thought.

The gain for genetic study. In the second place, our theory makes a positive contribution to genetic psychology. In showing how any complex modification of experience is integrated out of simpler forms, we are able to delimit that particular state very much more carefully from others closely related to it. Knowing fully its adequate conditions and its nature, we shall have more success in determining at what point of development it can arise. There can be little doubt, for example, that a large number of animals have particular percepts and recognise them. They need not, however, necessarily be able to locate these percepts in their past experience conceptually or have the assurance of the correctness of their memories and the like. We are, therefore, freed from much of the restraint that is put upon comparative psychology by the fear of imputing some form of conceptual thought to the animal mind.

On the whole, finally, it may seem probable that sensations are the only elements of experience and that all apparently different states of mind are modifications which result from the integration of these sensations in respect of some common attribute. But we put no special value upon this conjecture at present. For the moment we would only claim careful attention to the method we have followed. Our work may be incomplete at every point. We have pointed out that our enumeration of the attributes is imperfect. Our enumeration of the characteristics of integrated modifications of experience may need amplification. And we have only selected the two most obvious and easy examples of these for study. There may be dozens of others. It is only in order to characterise and name our theory over against sensationalism on the one hand, and elementism on the other, that the word "modalism" has been appended to the title of this paper.

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THE FUSION OF SENSATIONS OF ROTATION.

By W. MULDER.

From the Physiological Laboratory, Utrecht.

1. *Introductory.*
2. *Experimental methods.*
3. *Results of Experiments.*
4. *Theoretical.*

1. In 1873 Mach found[1] that one sensation of rotation can be annulled by a second rotation opposite in direction to the former. For example, the after-sensation of turning obtained by stopping a rotation (which, when of uniform speed, itself gives rise to no sensation) may be destroyed by a new rotation.

This well-known fact induced me to find out what would happen when a person was subjected to a periodically interrupted rotation, the period of rotation and interruption being always of equal length.

2. The sensations with which this paper deals were obtained by rotating the subject on a circular turn table, a modified form of that devised by Mach. The table was so made as to run with very little friction. It was driven by an electromotor. The room in which it was placed was darkened and all noise was excluded. The subject sat on a bicycle saddle, his head being supported by a frame which was inclined forwards at an angle of 25 degrees above the horizontal plane in order to limit stimulation solely to the two horizontal semi-circular canals.

The rotation of the turn table was arrested by means of a lever, covered with felt, which could be pressed against the margin of the table, at the same time interrupting the current that drove the electromotor. This lever was hinged to the floor, and was actuated at the outset by the hand, in later experiments by means of a second electromotor.

[1] Described in his *Grundlinien der Lehre von den Bewegungsempfindungen*, 1875.

The current of the first motor was interrupted by leading it through a wire attached to the lever. This wire dipped into mercury so long as the lever did not touch the table, but it was lifted out of the mercury when the lever was pressed against the edge of the table, and thus the current was broken.

3. When the table was turned round with a speed of 24 degrees per second and the lengths of the periods of interruption and rotation were gradually reduced, the following results were obtained:

(*a*) If the interruption periods be long enough, the person on the table experiences just what occurs: he feels that he is being alternately turned and stopped. More accurately, his sensations may be described as follows:

(i)	During rotation:	increasing sensation of rotation,
		maximal ,, ,, ,,
		decreasing ,, ,, ,,
(ii)	During rest:	increasing after-sensation of reversal,
		maximal ,, ,, ,, ,,
		decreasing ,, ,, ,, ,,
		no ,, ,, ,, ,,

(*b*) By shortening the periods, his experience is changed into one of going to and fro: the subject thinks he is oscillating about a point. In other words the sensations of rotation produced by the turning and the stopping immediately alternate with each other.

(*c*) By employing the lever still more quickly, all experience of rotation completely vanishes. The subject thinks that he is at rest, whereas he is actually being turned round interruptedly in a uniform direction.

The following table gives the lengths of the periods of turning and stopping, necessary to produce the phenomena:

TABLE I.

Average angular velocity of the turn table during the turning, 24° *per sec.*

Length of periods of rotation and interruption	Experience of
0·9″	Rotation and rest
0·7″	Oscillation
0·44″	Rest

It is possible to find for each speed of the electromotor a corresponding length of periods of rotation and interruption, at which the complete fusion of the sensations (*i.e.* the illusion of rest) may be brought about:

TABLE II.

Average angular velocity of turn table	Interruption time
1° per sec.	0·73 sec.
2° ,,	0·53 ,,
5° ,,	0·45 ,,
12° ,,	0·44 ,,
24° ,,	0·44 ,,

Throughout these experiments the acceleration was approximately uniform, owing to the high power of the motor and the small amount of friction[1].

By calculating the acceleration from the average velocity[2] and taking the number of interruptions per sec. instead of the length of the periods we obtain the following table:

TABLE III.

Acceleration	Number of interruptions per sec.
3° per sec. per sec.	1·4
7° ,, ,,	1·81
22° ,, ,,	2·22
55° ,, ,,	2·27

Inasmuch as the acceleration represents the stimulus, we see that doubling or trebling the stimulus strength necessitates only a very small increase in the number of interruptions.

There can be no doubt that with increased acceleration of the table more interruptions per second are needed to obtain fusion. Of course there are individual differences. I had the opportunity, however, of submitting twenty members of the Dutch Oto-laryngological Society to the experiment, and I failed to find any among them in whom the sensations did not fuse as they did in my own case. The differences in the required combinations were only slight.

[1] If the turn table were moved and stopped in absolutely the same way, there ought to be a period also during rotation in which the turning was imperceptible. But it was always stopped more suddenly than it was set in motion, the period of rotation gave rise to a more prolonged sensation. *The after-sensation therefore increased more acutely than the rotation-experience* proper and overtook it so that a moment of experience of rest ensued.

[2] Calling the path p we have: $p=\frac{1}{2}at^2$ and $p=vt$; whence $\frac{1}{2}at^2=vt$, $a=\frac{2v}{t}$; thus in our first instance: $a=\frac{2\times 1^\circ}{0{\cdot}73}=3^\circ$ nearly, and so on.

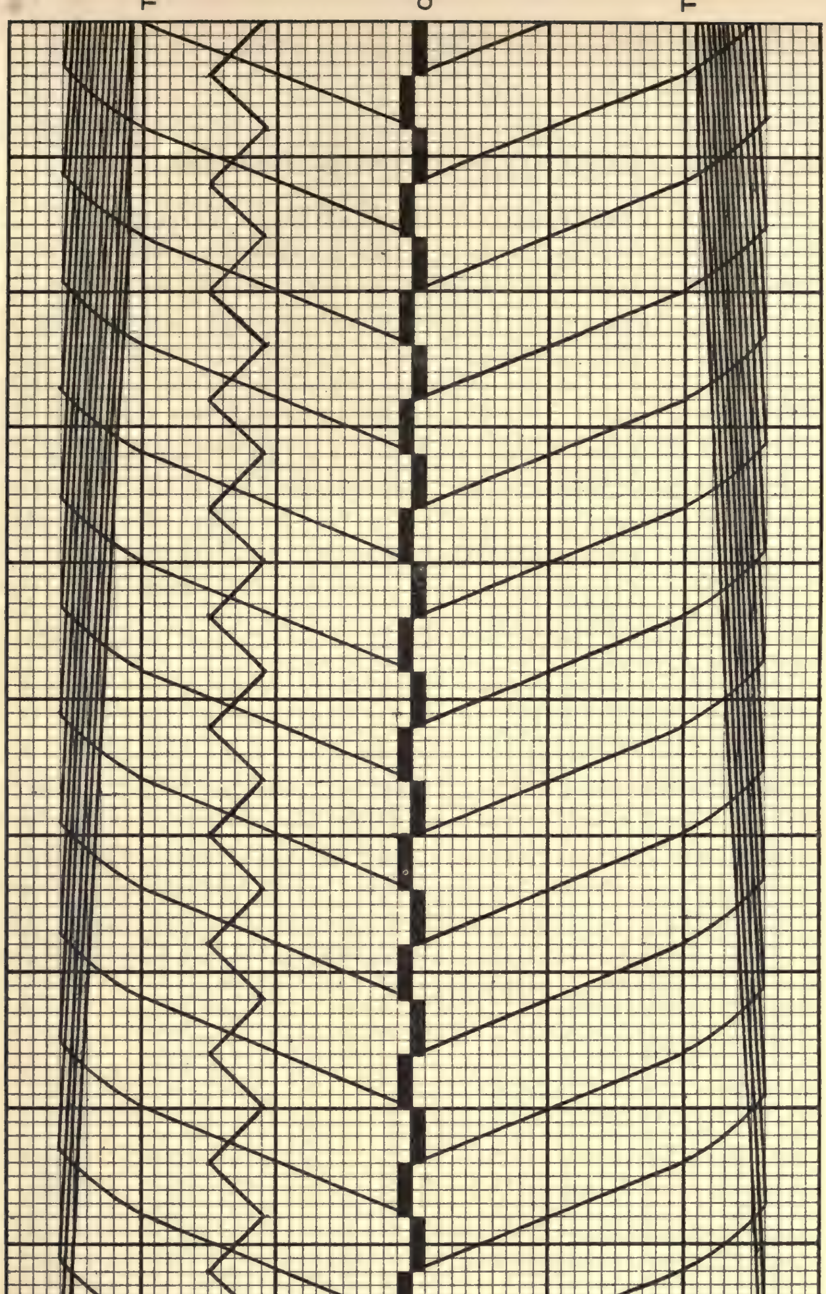
4. I have already quoted Mach's statement concerning the mutual interference of sensations derived from the labyrinth. The phenomena brought to light in my own experiments may be explained in a similar way. According to Mach the labyrinth is susceptible to stimulation only by accelerations, positive or negative; there is no possibility of uniform rotatory motion being felt, as there must be an internal push or pull, acting on the cells of the end organ. Hence it follows that cessation of rotation must give rise to the same sensation as turning in the opposite direction. In other words, when a person is stopped during a rotation he will feel as if he is being turned the other way. As the accelerations can only be experienced through pushes or pulls administered to the end organ, this experience must vanish upon the removal of a pressure by a simultaneous counter-pressure, as occurs in presenting successively contrary stimuli. The only point which requires consideration is the protracted character of the after-sensation when rotation has ceased; a feature which enables the experimenter to present two contrary stimuli influencing each other.

One striking characteristic of this interference of sensations must be borne in mind, and that is *the suppression of the sensation before it has come to consciousness*, or—to avoid the contradiction in terms—*the annulling of the effect of the stimulus before it has had the opportunity of giving a sensation.*

In the accompanying diagram of the sensations which arise during the experiment I have endeavoured to make clear how this interference takes place. The stimuli are represented by the thick black lines along the smaller squares which represent tenths of seconds. In the example taken the conditions are supposed to be an angular acceleration of 22 degrees per sec. per sec. and interruption and rotation periods each of 0·45 sec. The following data are assumed correct: (a) action time = 0·8 sec. (as determined by Rossem in his *Dissertation*, Utrecht, 1907); (b) moment of maximal sensation = 0·5 sec. after the sensation has passed the threshold (as ascertained by me in a series of experiments which will be published elsewhere)[1].

The positive stimuli and sensations (corresponding to periods of rotation) are represented by the lines above the horizontal base line, OO, the negative ones (occurring during stopping) are represented by the lines beneath it. Upon the application of the stimulus we see the curved line, which represents the sensory effect, rising from the base

[1] For the acceleration of 22° per sec. per sec. the difference between 20 and 22 was neglected, and the period 0·45 sec. was taken as 0·4 sec., being just half the action time.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line, and crossing the threshold, *TT*, after 0·8 sec. From this point it represents the sensation which reaches its maximum 0·5 sec. later. Half-way however between the origin of this curve and its passing the threshold, the contrary effect has developed, caused by the stopping of the turn-table. This is expressed by a similar curve below the base line. The cycle of positive and negative effects is repeated. After reaching their maxima, the curves gradually descend; the shape of this part of the curve I have analysed in my paper mentioned above.

By subtracting the distances from the base line of the two sets of curves, those above and those below the base line (an operation corresponding to the interference between the contrary sensations), we get the zig-zag line between the base line and the threshold. We can now see quite clearly what happens. Each stimulus is on its way to produce a sensation, but before the threshold of consciousness is reached, it fuses with its opponent and is annulled. The relation which we found between the speed of the motor and interruption number also becomes quite clear. As the speed becomes greater the curve must rise quicker, and will the sooner pass above the threshold. Hence it follows that the counter-stimulus must be administered sooner, otherwise a sensation will be felt. In other words the quicker the motion of the motor, the more interruptions are needed to keep the effect below the threshold. When the upper and lower apices of the zig-zag line pass respectively beyond the two thresholds, we have an illusion of the sensation of oscillation (page 206).

THE RELATION OF THOUGHT-PROCESS AND PERCEPT IN PERCEPTION.

By FRANCIS AVELING.

I. Introductory.

II. Methods; discussion; criticism; statement of problem; description of experiments.

III. Results.

A. Series I; compilation of statistics; effect of instruction; 'inversions'; order of consciousness for part contents of percept, colour content in I perceptions; causes of inversions; varying degree of consciousness for sensorial elements in a unitary percept.

B. Series II; corroboration of observations; comparison of percentages.

IV. Conclusions; classification of perceptions as colours, pictures or symbols; type and individual perceptions; correspondence of differences in sensorial-structure of percepts to differences in logical value; symbolic and asymbolic types.

I. Introductory.

The following notes of a research made in the *Laboratoire de Psychologie expérimentale* of the University of Louvain are offered as a small contribution to the psychology of perception. This field, one of the most important and fruitful in psychological research, has already been much investigated. The work done in the past embraces all the problems of psycho-physics, of the relations between the intensity or quality of stimuli and the corresponding psychical phenomena, of space and time, of degrees of consciousness, of extent of field of apperception and the relative degrees of consciousness of the various contents, of the effect of concentrated and dispersed attention, etc. The effects of previous phenomena upon subsequent perception have been studied—

abstraction by similarity (Grünbaum[1]), Recognition (Reuther[2]), Determined apperception and feelings arising upon perception (Ach[3]); while much work has been done upon the subject of the effect of perception upon other psycho-physical and physiological processes. Though it might at first sight appear that little remained to be worked over in so well explored a field, there still remains considerable scope for discovery and verification on the part of the student. A promising corner was pointed to by Professor Külpe in 1904, when he showed that the processes of abstraction are determined by a previously given (and accepted) instruction. Külpe demonstrated that the internal constitution of the perception varies with the variations of the instruction. Thus the form of a picture shown to an observer is reproduced with greater accuracy when the corresponding instruction is given than when the observer is told to pay attention to the colour, and *vice versa*. He showed that the other part-contents of the percept can be arranged in a graduated scale in respect of the accuracy of reproduction; and that these facts not only were due to differences in memory, but were given in the perception itself[4]. Mittenzwey, using an entirely different method, was led to similar conclusions[5]. Since it has thus been shown that differences of consciousness-value exist, and, by appropriate instructions, can be produced, in the internal structure of the percept, the problem that was proposed for solution in the research from which these notes are made is the following:

Are there any forms of the internal structure of percepts (*i.e.* of the sensorial complex) constantly correlative to the differences in the thought-processes[6] which are involved in the perception?

II. Methods.

The first method employed was an attempt to determine beforehand the thought-processes desired in the subsequent perception—to determine

[1] "Ueber die Abstraktion der Gleichheit," *Archiv f. die ges. Psychol.* 1908, XII. 340.

[2] "Beiträge zur Gedächtnisforschung," *Psychol. Stud.* 1905, I. 4.

[3] *Ueber die Willenstätigkeit und das Denken*, Göttingen, 1905; *Ueber den Willensakt und das Temperament*, Leipzig, 1910.

[4] "Versuche über Abstraktion," *Bericht über den I. Kongress f. exp. Psychol.*, Leipzig, 1904, 56.

[5] "Ueber abstrahierende Apperzeption," *Psychol. Stud.* 1907, II. 358.

[6] By thought-process as distinguished from percept is here meant the manner in which the stimulus (picture or object) is perceived; as, *e.g.*, individual, type, universal, etc. The distinction is made more clearly later in the text.

the manner in which a given stimulus would be perceived—by an ideational preparation of the observer, corresponding to different kinds of perception, and secured by the giving and acceptance of the instruction. The thought-processes being thus predetermined, it was possible to discover, in the introspective accounts of the observers, whether there were any constant differences in the sensorial contents of the percepts, corresponding to the differences in the thought-processes. Such were found to exist. But the method was open to *a priori* criticism: (1) Does the instruction really produce the effect desired? (2) Are the differences observed in the sensorial contents due exclusively to the differences in the predetermined thought-processes; or are they to be attributed to disturbing causes (trouble, inhibitions, etc.) arising from the greater difficulty of one or other of the tasks set?

(1) The doubt raised in the first criticism cannot be maintained against the evidence of the introspective protocols. These show that there was real correspondence between instructions and thought-processes, except in a few cases, and for particular reasons noted below. (2) The only way to meet the second criticism was to make a second set of experiments to control the first. Such control was indicated as possible by the fact that, in the first set of experiments, some of the thought-processes involved in the perceptions were observed to depend, not on the instruction given, but on the sensorial content itself. (These are the 'inversions,' of which below.) The method employed in the control experiments was the production of perceptions without previous instructions (*i.e.* without ideational preparation). In these experiments differences in the thought-processes were found to appear spontaneously; and it was thus possible to ascertain whether there was any correlation between the sensorial contents of the percepts and the thought-processes. This second method gave results identical with those of the first as to the fact of correlation, and confirms their accuracy. It proves that the ideational preparation has an influence on the perception, since the differences observed in the first series corresponded in fact to the instructions given. It also enabled us to discover some of the properties of the sensorial contents which exert an influence on the thought-processes.

The original problem may thus conveniently be separated into two: (1) What is the influence of the thought-processes involved in perception upon the sensorial content of the percept? (2) What is the influence of the sensorial content of the percept upon the thought character of the perception? The further problem may also be raised.

(3) What is the influence of antecedent facts of consciousness on subsequent perceptions, as well in their sensorial content as in their intellectual character? These problems are evidently exceedingly elementary in the psychology of the relations between thought and perception. They would, however, seem to indicate that the field of research with regard to perception is by no means worked out; and in the light of their solution fresh lines of investigation may be made plain to the student.

The thought-processes which the instructions were devised to determine were of a very simple character. Two kinds of perception were envisaged, which we have respectively called 'type perception' (T) and 'individual perception' (I)[1]. By I is meant the perception in which an *individuum*—this concrete thing, here and now—is placed before us; as, *e.g.*, when a man is perceived as "this particular" man. By T is understood the perception in which we have before us something that may represent any of and all the *individua* of a given class; as, *e.g.*, the indefinite perception of 'a man.' The difference between these two kinds of perception is a striking one in ordinary life.

Following this distinction the instructions were given to the observers that they were to perceive the stimuli-coloured pictures of well-known objects, exhibited for 80^σ in the tachistoscope—as individuals, or as types of a class of similar things[2]. One of the instructions, which were given in irregular order, was repeated before each exposition. The observer in each case signified that he had clearly understood it, generally by repeating the word 'type,' or 'individual[3].' A signal—

[1] The experiments, however, showed that a third class of perceptions was recognised by the observers—perception of symbols.

[2] "You are to perceive what will appear as an individual"; "You are to perceive what will appear as the type of a class of similar things."

[3] The following are the statements of the observers as to how they understood the instructions. M. (T) "I must know *what it is*, and be able to describe it as accurately as possible." (I) "I must see *what appears*, and describe it as well as possible." C. (T) "I must have consciousness *of general value*, and be able to describe what I see." (I) "I must have consciousness *of that thing*, and describe it." G. (T) "Mi fu dato l' ordine di vedere come tipo di classe. Ho capito." (I) "Mi fu dato l' ordine di vedere come individuo. Ho capito. Io non ho fatto nessuno sforzo determinato per rappresentarmi la cosa sotto l' uno o l' altro aspetto. Lascio che l' istruzione avuta eserciti il suo influsso liberamente da se." A. (T) "I am *to have the meaning clearly before me*, and try to hold all details of what will appear." (I) "I must perceive *this definite thing*, and hold the details as well as possible." An. (T) "For me T perception means that the picture is perceived as representative of a class." (I) "I perception means that I have had a thing like it, or been interested in a similar thing." B. had a strong imaginative tendency to complete, or deform, the pictures by assimilations. He made no statement

'Attention'—was then given, and, after an interval of 1·5 seconds, the tachistoscope was set in motion by an electric key. Immediately after the exposition the observer dictated an introspective analysis of the phenomena of the perception to the experimenter. He was required to give an account of the psychic processes intervening in the period between the giving of the instruction and the exposition; (2) to describe, not only the content of the percept in detail—determining, as far as possible, the degrees of consciousness to be assigned respectively to form, colour, localisation, size, contour, interior detail, etc.—but also (3) declaring the manner in which the perception had taken place (*T* or *I*). (4) He also stated whether what he had seen was seen as a picture, a complex of colours, or a symbol. (5) He then drew, as accurately as he was able, what he had seen. (6) Finally the picture was handed to him, so that he might call attention to any omissions or falsifications in the previous perception (Recognition). The pictures exhibited were taken from children's toy-books. They were mounted upon white cards three inches square, and divided into sets of ten pictures each. At each sitting, one set was shown to the observer.

The first series of experiments was made with one hundred pictures, fifty being shown with *T*, and fifty with *I* instruction.

The second (control) series consisted, for two of the observers, of four sets of ten pictures each; for the other two, of ten sets: exhibited without any other instruction than that which was clearly understood in all the experiments—that the observer was to do his best to see what was shown as well as possible.

A third series was also made with small objects of various kinds (screws, rubber tubing, cork, wire, worsted, etc., about 4 cm. × 5—10 mm.) mounted on cards, and shown tachistoscopically in the same manner as the pictures.

The observers were six in number: Professor A. Michotte (M.), A. Centner (C.), A. Galli (G.), H. Angel (An.), E. Boyd-Barrett (B.), and F. Aveling (A.). Of these M., C., G. and A. served as observers for the first series; C., G., An., and B. for the second. M. and A. observed a sufficient number of expositions without instruction (Second Series) to verify the general conclusions reached; but limitations of time

as to the distinction he drew between the *T* and *I* perceptions: but it is clear from his introspections that the *I* cases were invariably of distinct and definite things often unlike what was exhibited, but not the less answering to the instruction: while the reasons he advanced for the *T* perceptions were of a nature to show conclusively that by *T* he meant precisely what was meant by the other observers.

prevented a complete series of experiments on the part of these observers. The experiments were made during the first and second semesters of 1909–10.

III. Results.

A. As samples of the protocols which were marked for purposes of statistics, the following may be given:

(i) "Type. I saw a smallish man in blue on a heavy hunter. The reins are hanging down loose. The horse looks too yellowish—it looks like a picture. The man wears long blue trousers. I'm not sure if he wears a hat. He has whiskers. The horse is very slightly shaded; the tail is reddish and hangs down. The horse is standing still." [The drawing was then made, showing size, position, etc.; and showing also reins in man's hands, and coat.] "Recognition: in general very good. It seems now much more natural and better proportioned. The man seemed smaller and the horse bigger. The general outline, size and colour are good. The details are good, too: but the colours of some are wrong."

This was classified, by reference to the original picture and to the drawing made, as follows: manner of appearance, picture; general form, good; size, fair; localisation, good; outline detail, good; interior detail, fair (more than 50 °/₀ of these were omitted); localisation of interior detail, fair; general colour, good; colour of detail, fair; recognition, good.

(ii) "Little girl, with *I* signification: golden hair, blue eyes expressing surprise, eyebrows raised, one finger in her mouth; pigtail in air tied with bluish ribbon. Saw it as a real object. Recognition, good: very good: all good." [The drawing, allowing for the lack of skill of the observer, was correct throughout.] Good in all entries.

(iii) "I heard the words 'Opera glass,' all alone, instantly. It was yellow, with brown streaks somewhere, blue lenses (these I interpret, not very clear about them). I saw a binding link, I don't know where or of what colour." [The drawing was in detail very unlike the picture.] "Recognition: Oh! I really had a most schematic view of it. The two tubes were all I perceived. After perceiving them, I paid no more attention to the picture. I saw what is dark blue as brown." The only good entries here were for general form, localisation and general colour.

In compiling the analyses from the protocols for statistical purposes, each of the eight part-contents (form, colour, etc.) was classified as "good" (G), "bad" (B), "fair" (F), or "omitted" (O). The subsequent "recognition" was treated in the same way. The statistics were, however, made without any value being allowed for fair or bad entries, or for omissions; so that, while each entry could have had some fraction of the maximal value 1, only the good were counted, and the percentages made on the total possible value thus attainable[1]. This gives the relative value of the various heads of classification for each form of perception (T and I—as well as for the Inversions) in the case of each observer. These percentages are then combined to form a single percentage value for the total percept, in each of the four cases, and for each observer. Finally the average percentages are calculated for the four cases, viz., the average value of (1) T perception with T instruction, (2) I perception with I instruction; (3) T perception with I instruction (*Inversion*); and (4) I perception with T instruction (*Inversion*). The appended Table I shows (1) the percentage values of the part-contents

[1] As a sample of the way in which the analyses were made from the protocols, with the aid of the drawings and recognitions, and the values calculated for the part contents, the following imaginary table may be given:

N (with no instructions), T result.

Series	Manner of Appearance	General Form	Size	Localisation	Outline Detail	Interior Detail	Localisation of Interior Detail	General Colour	Colour of Detail	Recognition
XI. 1	Picture	G	G	G	G	O	O	G	B (o)	G
XI. 4	Real *	G	G	G	B	G	G	G	B (oa)	G
XII. 8	Spots of Colour	G	G	F	F	F	F (o)	G	O	F
XIII. 6	Symbol †.........	F	G	G	F (a)	F	O	F	B	F
XIV. 7	Real...............	G	G	G	G	B	F	G	G	G
XV. 10	Picture	F	G	G	F	G	B (oa)	G	O	F
XVI. 2	Picture	G	G	G	B (o)	F	O	F	B	F
XVI. 4	Real ‡	G	G	F	F	O	O	G	F	F
XVI. 9	Real...............	G	G	F	G	F	F	G	O	F
XVII. 5	Real...............	G	G	G	F	B (o)	G	G	G	G
No. 10	Real 5 Symbol 1 Colours 1 Picture 3 Omissions 0	G 8 B 0 F 2 O 0	G 10 B 0 F 0 O 0	G 7 B 0 F 3 O 0	G 4 B 1 F 5 O 0	G 2 B 2 F 4 O 2	G 2 B 1 F 3 O 3	G 8 B 0 F 2 O 0	G 2 B 4 F 1 O 3	G 4 B 0 F 6 O 0
		80 %	100 %	70 %	40 %	20 %	20 %	80 %	20 %	40 %

(*o*) inserted at the side of an entry means omissions, (*oa*) omissions and additions, (*a*) additions.

Remarks— * Very schematic picture. † Assimilations. ‡ Very complex picture.

TABLE I. (SERIES I. *with instruction.*)

	"Type" Instruction										"Individual" Instruction									
	"Type" Result					"Individual" Result (*inversion*)					"Individual" Result					"Type" Result (*inversion*)				
Subject	M.	C.	G.	A.	Combined	M.	C.	G.	A.	Combined	M.	C.	G.	A.	Combined	M.	C.	G.	A.	Combined
No. of Cases...	35	36	35	43	=149	10	11	15	9	=45	28	42	35	46	=151	15	7	14	0	=36
General Form	71	77·5	65·7	51·1	=66·32	70	100	80	66·6	=79·15	67	90·5	88·5	82·6	=82·15	60	100	57·1	—	=72·36
Size	70	36	65·7	44·1	=53·95	100	100	93·3	55·5	=87·2	71	78	91·4	80·4	=80·2	53	43	57·1	—	=51·03
Localisation ...	80	83	80	72	=78·75	100	82	73·3	66·6	=80·47	96	98	82·8	86·5	=90·82	66	86	85·7	—	=79·23
Outline Detail	29	14	20	2·3	=16·37	40	36	53·3	11·1	=35·1	50	48	48·8	41·3	=47·25	6	43	21·4	—	=23·46
Interior Detail	8	14	20	2·3	=11·07	40	9	0	11·1	=15·02	46	33	34·2	15·2	=32·1	13	14	14·2	—	=13·73
Localisation of Detail ...	17	22	20	4·6	=15·9	50	36	6·6	22·2	=28·7	42	43	40	26	=37·75	13	29	21·4	—	=21·13
General Colour	62	61	71·4	62·7	=64·27	50	73	93·3	55·5	=67·95	82	93	91·4	69·5	=83·97	66	57	38·7	—	=53·9
Colours of Detail	29	19	31·4	30·2	=27·4	50	27	20	11·1	=27·02	60	45	28·5	39·1	=43·15	20	29	14·2	—	=21·06
Total (Percept Value)	45·75	40·81	46·8	33·66	=41·75	62·5	57·87	52·47	37·43	=52·52	64·25	66·06	63·2	55·07	=62·14	37·12	50·12	38·72	—	=41·98
Recognition ...	31	33·3	57	37·2	=39·62	50	54·5	53·3	33·3	=47·77	75	78·5	71·4	65·2	=72·52	40	43	42·8	—	=41·93

for each observer, (2) the combined percentage values according to the realisation of the instructions, (3) the percentage values of the percepts (the average percentage of the eight part-contents recorded), as well as (4) the percentage value of the recognitions. The last two rows of figures in columns 5, 10, 15 and 20 of this table show the average aggregate of these results for the four observers.

As to the effect of the predetermined thought-processes upon the perception it is to be noticed that, in 149 cases out of a possible 200, with *T* instruction, the *T* perception was realised. In 45 cases there was an inversion, with realisation of the *I* perception. Six experiments were failures, or unclassified. In the cases in which the instruction was followed, the total aggregate value of the percept (calculated in the manner explained) was 41·75 °/ₒ: recognition, in the same cases, had the value of 39·62 °/ₒ. In 151 cases, of a possible 200 with the *I* instruction, the *I* perception was realised. There were 36 inversions of this instruction, and 13 failures or unclassified cases. In the cases in which the instruction was realised, the total aggregate value of the percept was 62·14 °/ₒ. Recognition here had a value of 72·52 °/ₒ.

Comparing these figures, it will appear that (i) in the case of the realised instruction, the sensorial contents of a perception as type of a class (indefinite) have 20·39 °/ₒ less value than as an individual; (ii) the recognition differs likewise by 32·90 °/ₒ.

As to the inversions, the value of the percept when the *T* result followed on an *I* instruction was 41·98 °/ₒ; the recognition, in this case, being 41·93 °/ₒ. With *I* perception following on *T* instruction, the value of the percept was 52·52 °/ₒ, and that of the recognition was 47·77 °/ₒ. In the inversions, therefore, the percept of the individual was 10·54 °/ₒ, and the recognition was 5·84 °/ₒ, better than that of the type.

The results of the realisation of the *T* instruction, compared with those of the inversion (*I* instruction, *T* result) are as 41·75 °/ₒ to 41·98 °/ₒ; while recognition is as 39·62 °/ₒ to 41·93 °/ₒ. With *T* instruction, the sensorial content value of the perception as type was thus ·23 °/ₒ worse, and recognition was 2·31 °/ₒ worse than in the inversion of *I* instruction.

It is worthy of note that, when the various part-contents are classed in their order of consciousness for *T* and *I* realisations of the perceptions, although localisation (position, *situs*), form, and size have nearly always the highest values, colour comes always higher in the lists in *I* than in *T* perception.

Further, when the proportion of "good" entries for the various part-contents is studied in relation to the manner of perception (*T* or *I*), it is noteworthy that there are more "good" entries, under *all* the heads, in the case of *I* perception than in that of *T*.

Four causes were assigned for the inversions—one subjective and three objective. But further investigation is required to corroborate this, and other influences than those mentioned would probably be found to be involved. The subjective cause would seem to be assimilation, at the moment of perception, of memory images which fuse with the perception,—or an associative process by which the representative content is immediately modified by the reproduced elements. The observer as a rule 'recognises' what he sees, and, though not invariably, perceives it as an individual. This was a frequent cause of the inversions *T* to *I* in the first series of experiments; and in the second series (without instructions) it provoked introspective remarks such as the following: "I saw a guitar I once had" (the picture exhibited was of a violin)—"a penknife I had"; "an aunt of mine had one"—"associated *immediately* with A.'s black bag." (The picture was of a brown valise.)—"Same as last picture, only turned round"—"I saw *that* picture but my introspection was made on another"—"Lamp we had at home, but different colour." These remarks are taken from the protocols of our observers. Clearly, such assimilations and recognitions are favourable to the individual result.

The objective causes found were (1) the complexity of the picture exhibited, (2) the schematic nature of the picture: (both these were observed to be very favourable to the *T* result): (3) the presence of striking or realistic details, especially of colour, in the picture. (This is favourable to the *I* result.) These inversions, as well as the results of the experiments in which no instruction was given to see the picture in a particular manner, show clearly that the sensorial content of the perception has an influence upon its thought character which, in some cases at least, disturbs or nullifies the ideational preparation.

As samples of these inversions the following may be given:

1. Subjective cause—assimilations, recognition, previous experience.

(VI. 3, Dec. 9, 1909: Subject, M.; Instruction, *I*; Perceived as *T*.)

Introspection: "It appeared as a type of a special kind of lantern used by servants: and as a real object. What I had before me was that kind of lantern (specific type). It appeared distinctly. The word 'lanterne' came afterwards. It is brown; and seems to be varnished. I do not know the shape of the top. It has a bevelled glass in front—and perhaps also on the right. Two wires and a cross-piece of wood form the handle. This is orange-brown or very light red."

Recognition: "Pretty bad: but I saw it *slightly* as it is. I had no idea of its having *four* glass windows."

Remark: "The description I have given is more like that of a lantern I had when a child: but mine had no handle. In this case I think that there were assimilations at the first moment of perception."

Again (v. 6, Dec. 3, 1909: Subject, C.; Instruction, *T*; Perceived as *I*).

Introspection: "I did not see that as the type of a class. It was a letter-scale. The bars were yellow, the support black. I cannot draw it; for the meaning of the instruction came to me and troubled me. There was an inhibition. No word came to consciousness. I at once thought of Nardi's machine. He has one."

Recognition: "I did not see the brown outlines of the yellow. All else is quite correct."

Remark: The assimilation here ("Nardi's machine") seems to be clear.

2. Objective causes.

(i) Complexity of picture[1].

(IX. 4, Dec. 8, 1909: Subject, G.; Instruction, *I*; Perceived as *T*.)

Introspection: "I perceived it as a picture (which brought to consciousness a great number of words) with simultaneous signification of type. *This is certain.* I saw a mixture of colours, and am certain that there were many. A part of the clothes was green or yellowish-green: but I cannot localise it. I do not know if there were stockings. I perceived blue somewhere, but cannot localise it. There is also something on the head—I do not know whether cap or hair. The two arms are extended to right and left. *She* is turned with the back towards me. I did not see the face."

Recognition: "Oh, no! I recognise nothing except the green."

Remark: The picture was here very complicated—at least eight colours and much detail. In his introspection G. was almost entirely incorrect—but he was aware that it was "a girl" (type).

Again (VI. 2, Dec. 9, 1909: Subject, M.; Instruction, *I*; Perceived as *T*).

Introspection: "It appeared as a type. At the first instant of the exposition I knew what it was. It seemed very complicated. The words 'c'est un chardonneret' came to consciousness. But this I knew before and without the words. The introspection is troubled by the fact that I had not satisfied the instruction. I am not sure of the position. The wings are above and slightly open. There is a red spot somewhere, but I cannot localise it. Also brown and black, and a yellow spot—perhaps on the wings? It may be on a branch, but I do not know."

Recognition: "It is incredibly bad! I would never have recognised it."

(ii) Schematic nature of picture[2].

(VII. 4, Dec. 7, 1909: Subject, C.: Instruction *I*; Perceived as *T*.)

Introspection: "It was a pair of bluish nippers. No word came to consciousness. They were closed. I think I saw it as a picture. And as type."

[1] Pictures with great abundance and variety of detail, more than four or five different colours, etc. The latter seem to inhibit each other and produce the effect noted.

[2] Pictures in outline, monochrome, etc., lacking detail and simple in design.

Recognition: "In general, good: as to general form, colour and position. I saw none of the dark outlines—black—on it, nor the other lines marked."

Remark: The picture was exceedingly simple and schematic; and the few details quite plain.

The same image was thus seen by M. (Dec. 15, 1909).

Introspection: "I saw it as a type. I had before me the thing—forceps; and heard auditively 'c'est un pince.' It is of iron, light blue, with form as drawing."

Recognition: "The general impression is good. I had no idea that it was three-dimensional nor that it had any black lines on it, nor that it was closed."

Again (VII. 8, Dec. 6, 1909: Subject, G.; Instruction *I*; Perceived as *T*).

Introspection: "I saw the picture, and heard the word 'tino.' I had consciousness of type. The form is that of a barrel. The colour is a medium brown. There are two bluish hoops: and, perhaps, a bunghole. But this may be an assimilation. I am not sure of it."

Recognition: "Yes, perfect in all details."

(iii) Striking details.

The following is a very interesting case of this inversion, showing the effect of a process of abstraction by difference.

(X. 8, Dec. 20, 1909: Subject, M.; Instruction *T*; Perceived as *I*.)

Introspection: "I saw it as an individual real thing. I was struck by the details: viz. the small squares of colour around the drum, either on the rims or in the central part. I know those drums. They are generally painted with red and white triangles on the rims. *This one has red and blue triangles.* I saw the drum from above, etc."

Recognition: "General form, good. Position of cords wrong. I did not see the squares."

Another case, with M. as subject, was IX. 7, *T*, which was perceived as an individual. The subject "was struck by the colour of the hair, and by several other details that had a higher degree of consciousness (*e.g.* knot of ribbon on hair, etc.)."

Again (XIII. 4, Dec. 12, 1909: Subject, A.; Instruction *T*; Perceived as *I*).

Introspection: I saw it, I think, as a picture. The word 'face' came immediately into consciousness; then 'eyes.' I believe that I saw it as an individual. There were two large, open eyes looking at me. The mouth was covered with something red: and the upper part of the head with something from which hang yellow sequins."

Recognition: "Good. I did not see the star between the eyes, localised the cheeks wrongly, and did not notice the small jewel."

Remark: The picture was both incomplete and unusual.

Again (VIII. 2, Dec. 10, 1909: Subject, G.; Instruction *T*; Perceived as *I*).

Introspection: "I saw a bluish-yellow skirt, and below it, stockings and 'sabots.' The sabots were dark, and the stockings red. *The skirt was blown out by the wind.* The picture is cut across. There was no generalisation. I saw it as an individual."

Remark: Note the significance of the italicised statement.

Again (x. 8, Dec. 10, 1909: Subject, C.; Instruction *T*; Perceived as *I*).

Introspection: "I did not see it as a type. 'Drum' came into consciousness auditively. The front part is yellow, with little red ornaments on top, and a sort of crosses over it. I saw the top. It is yellowish-gray."

Recognition: "On the whole, save for some details, good."

Remark: A drum with such details would not be familiar to the subject. Query: Abstraction by difference?

A further interesting, though in no sense novel, conclusion that may be drawn from these experiments is that the part-elements entering into the complex of the percept vary in degree of consciousness. The abstracted unit in consciousness is the thing, or picture, as perceived. The sensorial elements that go to make it up range from values of 2·3 %, 4·6 %, 6 %, etc., to 100 %, as may be seen from Table I. In envisaging this or that content-unit, we are able to (and do) isolate it as a whole from other contents of consciousness, as a group of elements, varying relatively in value, which is held together and isolated as a unitary content by the active or passive direction of intention.

B. In the control series of experiments similar mounted pictures were exhibited to the observers in the tachistoscope without previous instruction (ideational preparation). Forty of such pictures were shown to C. and G., and the resultant perceptions, divided into *T* and *I*, analysed in Table II in precisely the same way as those of Series I. Of the eighty pictures thus exhibited 41 were perceived as *T* and 38 as *I*. One experiment failed, or could not be classified. The combined percentage value of the *T*-perceived percept of these two observers was here 43·1 %, recognition 25·1 %; while that of the *I*-perceived percepts was 63·75 %, recognition 72·95 %. It will be noticed that the percept value for the *I* realisation here is higher than that for the *T* realisation by 20·65 %—a figure strikingly like the 20·39 % of the experiments of Series I: and it is worthy of remark that the number of *T* and *I* cases realised in this series almost balances—a fact not observable in Series I, in which the inversions are comparatively few. This would seem to be a conclusive indication of the fact that the antecedent instruction exerts a real influence upon the manner of realisation of the perception.

It is further noteworthy that the actual percentage totals of all the *T* and of all the *I* perceptions, with and without instruction, are exceedingly close. The former are—with instruction, 41·75 %; without instruction, 43·1 %; and the latter are—with instruction, 62·14 %;

TABLE II. (SERIES II. *without instruction.*)

	"Type" Result			"Individual" Result			"Type" Result			"Individual" Result			"Type" Result	"Individual" Result
Observer	C.	G.	Com-bined	C.	G.	Com-bined	An.	B.	Com-bined	An.	B.	Com-bined	(C., G., An. and B. combined)	
No. of Cases...............	17	24	=41	22	16	=38	45	26	=71	53	68	=121	112	159
General Form	64·7	54	=59·35	81·8	75	=78·4								
Size	94	79	=86·5	95·5	87·5	=91·5								
Localisation	58·8	62·5	=60·65	81·8	87·5	=84·65								
Outline Detail	11·7	12·5	=12·1	45·4	50	=47·7								
Interior Detail	11·7	4	=7·85	54·5	31	=42·75								
Localisation of Detail...	29·4	12·5	=20·95	40·9	25	=32·95								
General Colour	58·8	58	=58·4	81·8	87·5	=84·65								
Colours of Detail.........	17·6	20·8	=19·2	40·9	45	=42·95								
Total (Percept Value)...	43·3	42·9	=43·1	65·3	62·2	=63·75	80	30	=56	90	48	=69	49·55	66·37
Recognition	29·4	20·8	=25·1	77·2	68·7	=72·95	84·4	42·3	=63·35	88·7	54·4	=71·55	44·27	72·25

without instruction, 63·75 %. There is only 1·35 % and 1·61 % difference respectively between them.

100 pictures were also exhibited to the observers An. and B. in similar conditions. Their introspections were classed according to the *T* or *I* realisation of the perception; and were further analysed into 'good,' 'fair,' 'poor' or 'bad' perceptions, and controlled by a recognition. Of the 200 experiments 71 had *T* realisation, and 121 *I*; 8 failed, or were not classed. The combined percentage of 'good' and 'fair' perceptions for the *T* realisation was 56 %, recognition 63·35 %; for *I* realisation 69 %, recognition 71·55 %. The percepts were not analysed into eight part-contents as were those of C. and G.; but the relative superiority of the *I* over the *T* percept is marked. Were it allowable to combine the results of the four observers, we should have: *T*, 49·55 %; recognition, 44·27 %. *I*, 66·37 %; recognition, 72·25 %: though such combination lessens the contrast observed in the somewhat more carefully analysed results of C. and G.

IV. CONCLUSIONS.

1. The pictures were perceived by the observers in three different fashions: (*a*) as spots of colour on the white background; (*b*) as pictures representing things (*i.e.* the object of thought is the picture, *plus* the thought that it pictures a thing); (*c*) as symbols. This third is the most important case. In it the object of the thought of the observer is the *thing* which the picture represents. Of the 652 experiments tabulated in Table III, 37 belong to *a*, 282 to *b*, and 295 to *c*. 38 could not be classed.

2. In all three cases, however, the perception may be *T* or *I*. The object in the *I* perception, according to the introspections of the observers, is "this individual, concrete, particular thing, which has this or that special character." In the *T* perception it appears as "a thing of this kind." The perception places the observer before "a bird," etc., more or less generalised. It is certainly not, as in the previous case, individualised. Its individuality, as a numerical unit, disappears from the perception. Even the representative content of the picture has the character of indefiniteness. In this case, the object is evidently not universal, but abstracted, in so far as the individual character is lacking.

It has already been pointed out that a close connexion is to be found between the thought-process involved in the perception and the sensorial contents of the percept. Indefiniteness of the percept is

TABLE III.

	Series I.				Series II.				Total
Observer	M.	C.	G.	A.	C.	G.	An.	B.	
No. of Pictures......	88	96	99	98	39	40	98	94	652
Seen as Pictures ...	13	35	56	54	8	37	47	32	282
,, Symbols ...	58	49	30	19	29	3	51	56	295
,, Colours ...	9	7	3	11	1	0	0	6	37
Omissions............	8	5	10	14	1	0	0	0	38

generally, though not exclusively, a concomitant of the type perception; and, *vice versa*, the individual perception generally, though again not exclusively, has as its concomitant a definite percept. It will be remembered that the statistics on p. 219 showed that the percept in the type perception is 20·39 °/ₒ less in value than that in the individual perception, in the cases in which the several instructions were realised; while the percept in the type perception following on individual instruction was 10·54 °/ₒ less in value than in the individual perception following on the type instruction.

It is important to note also that the representative elements entering into psychical life, far from being fixed and always identical with themselves, vary enormously, even when the sensorial elements of which they are made up are the same. These facts, insufficient as they are to support an intellectualistic or a sensualistic theory of thought, throw some light upon this problem; showing, on the one hand, that any intellectualistic theory must keep in sight the close connexion between the thought and the representative elements; and, on the other, that a sensualism which attempts to explain thought merely by the laws of succession and association of fixed and inalterable elements, is leaving out of account observed and observable facts that vitiate its theory.

3. At this point an objection might be raised, viz. that in the *T* perception the object *must* be individual as in the *I* perception; but that in the former there may be something more, as, *e.g.*, the thought that this particular individual belongs to a class of similar individuals. Reasoning would lead to some such conclusion; but reasoning is an affair of reflexion, not of observation, and in the present case there is ample introspective evidence to prove the contrary. In the *T* perception the observers do not perceive an individual, but an indefinite or indeterminate; and the particular properties that are given in the

percept are as indefinite as is that of which they are recognised as the properties. This is, of course, only true with respect to the actual moment of perception. When, afterwards, the observer reproduces what he has perceived in order to describe it, the image necessarily becomes this particular individual picture that he has just seen. But, since we have ascertained that differences in the internal sensorial structure of the percepts correspond to differences in the logical value of the two kinds of perception, we may consider the different internal structures as correlative to the logical values. The fact of this correlation is the only point to which attention is here drawn.

Several other questions arise in this connexion, for a reply to which the experiments carried out are not adequate. A complementary investigation is needed in this matter, to which the present notes are merely offered as an introduction.

4. An interesting observation, from another point of view, is that the same picture may be symbolic for one observer and asymbolic for another. This fact is certainly due to individual characteristics of the observers who could thus be roughly classed into 'types'; in one of these types the tachistoscopically exhibited pictures tend to place the observer before the thing pictured, in the other, before a picture only. From Table III it will be seen that for M., C., An., and B. the symbolic perception predominates; for G. and A., the asymbolic. The explanation of this phenomenon would seem to lie, if the introspections are trustworthy, in the facility with which a 'symbolic' subject assimilates, in the perception, previous experiences, frequently of many (10, 15, 20) years anterior date[1]. But it may be noted also that C., An., and especially M. and B. are strongly-marked visual reproductive types, while G. and A. are not,—A. in particular almost entirely lacking the power of visual representation.

The cordial acknowledgement of the writer of these notes is due to the gentlemen who served as observers in his experiments, and especially to Professor Michotte, under whose direction he carried out the research on which they bear, and who has been good enough to read and criticise the proofs of the present paper.

[1] In confirmation, see the remarks quoted on p. 221, extracted from protocols of the observers, as well as the first two introspections printed in small type in the text. Considerable evidence, of the same kind, might be extracted from the protocols.

A CASE OF SYNAESTHESIA.

By CHARLES S. MYERS.

1. THE subject of this note, whom for convenience we shall call *A*, is about 30 years of age. It was quite by accident that I was led to investigate his condition of synaesthesia. He happened to be one of a number of persons who were the subjects of some experiments which I was conducting upon the aesthetics of music. One part of this investigation consisted in sounding certain single tones and chords by means of tuning forks (struck with uniform loudness by an arrangement which it is unnecessary to describe here). The subject was asked what came into his mind while he was listening to individual single tones or chords; he had to describe his attitude towards it, to state if it gave him pleasure or displeasure, and, if so, to explain why.

2. The following are specimens of *A*'s answers when single tones were sounded. The figures indicate the number of vibrations per second; the letters in brackets give the approximate musical notes.

800 ($a''\flat$). Gives me the idea of a bell. It is of a light-blue colour. I get the idea of a fairy, then of a fairy bell. I have no visual image. It is a pleasant sound.

1200 ($e'''\flat$). This is not so pleasant. It suggests the high note of a fiddle. It is whiter,—a silvery grey. It is too thin. I have no image of colour.

500 (c''). It suggests a gong. It is rosy to brown in colour. An idea of a halo comes into my mind. It is indifferent, neither pleasant nor unpleasant.

300 ($e'\flat$). This recalls the chapel bell of my College. It is a deep rich brown, passing to vermilion. The sound is very pleasant, owing to its richness.

900 ($a''\sharp$). A blue sound,—light blue. Moderately pleasant.

700 (f''). It at once made me think of a pink glass finger bowl on table. I could not see the table.

(The same fork sounded again.) I call it lilac,—perhaps from association with finger bowl. It rather suggests the taste of thick soup.

Next I sounded pairs of tones simultaneously, and obtained the following answers among others:—

1000 : 1200. Unpleasant. Grey-blue. No associations. Unpleasant sensations in ear.

500 : 800. Much more pleasant, a mixed colour,—blue and pink—like the Emmanuel College blazer, only the pink is deeper. The blazer suggests the river. The mixed colour is, I think, the result of the knock[1], the blue belonging rather to the knock.

400 : 700. A definite mixture of two tones. *I had not heard two tones in the previous sounds*[2]. This is a combination of two colours, brown and some lighter colour, perhaps blue. It is a muddy mixture; there is confusion of the colours.

600 : 1000. Distinctly blue, with an undercurrent of dull red. It is rather pleasant. I get the idea of an undercurrent.

700 : 800. This is like two notes, like a light and dark blue clashing, not matching. The general effect is displeasing.

[1] The forks were struck by a hammer, but later experiments showed pretty conclusively that *A* did not attribute any colour to the knock.

[2] *A*'s failure to notice two sounds in the two previous pairs of tones is especially interesting. On another occasion I played $f'c''$ simultaneously on the pianoforte. *A* only observed, in addition to the lower tone, a sound of such high pitch as to be "colourless; not a musical note." On the pianoforte, on other occasions, *A* only failed to recognize two notes in the case of the octave.

600 : 1200. I only hear one sound. It is a bright pink, and quite pleasant. A salmon or mullet suggests itself, probably owing to colour.

800 : 1000. Again there is an attempt to separate a blue from grey. But I am not sure the grey is there. I think the colour is an electric blue with a sheen-like effect, which suggests the hood of a gown, the B.A. hood of Birmingham University.

600 : 800. This gives a burnt-sienna effect, with an overtinge of blue. It is indifferent or unpleasant. It suggests a French artist, whose name I forget, who paints with an overtinge of blue.

800 : 900. *Only one sound.* A good thorough mixture of colours, a steel blue colour,—as if the components were being rotated on a colour wheel.

From these answers it will be observed that a complete mixture of the colours of the individual tones only rarely occurs when they are sounded simultaneously. To test this on another occasion, I presented the "blue" tuning-fork tone 512 with the "orange" Tonmesser tone 256. But the colours would not combine; *A* obtained both blue and orange. At the same sitting I also presented to him a few single tuning-fork tones, both those I had given him singly before and those which I had already sounded simultaneously with others.

600 ($e''\flat$). A rich dark Prussian blue, suggesting pictures, such as Burne-Jones painted.

500 (c''). Soft brown. It suggests a lady's dress.

1200 ($e'''\flat$). Half-way between blue and white. It suggests a clear sky after sunset.

500 (c''). Brown to pink, finally shading off to blue. It appeared to change as the tone was dying away.

300 ($e'\flat$). Brown with definite pinkish tinge, finally shading off to pink.

1300 (e'''). Thinnish blue.

1200 ($e'''\flat$). Fainter, thinner. Blue, shading off to grey.

700 (f''). This seems like a mixture of pink and blue. It reminds me again of the College blazer; only it is bluer than before.

In all these cases the colour came first, and later suggested the object or scene (when it occurred)[1]. On another occasion, I sounded three or more tones, simultaneously. The chord 300, 500, 600 was described as "a medley of colours," finally distinguishable as purple. The chord 400, 600, 800 gave rise to a "brown and a colourless blue." The chord 600, 800, 1000, 1200 produced "a distinct pink and a blue, possibly two blues." The chord 300, 800, 1000, 1200, caused "a very definite red tinge, pink to red predominant, with other colours playing about."

Several other sittings were obtained from *A*, during which other tones and other instruments were employed.

The following table gives the series of colours obtained for the tuning-fork tones of different pitch:

256	Brown.
300	Brown to vermilion or pink.
400	Brownish pink.
500	Rosy brown, brown or pink, becoming blue.
600	Rich dark blue.
700	Mixed pink and blue, lilac.
800	Light blue.
900	Light blue.
1000	Very light blue.
1100	Very light blue.
1200	Blue, shading off to grey.
1300	Thinnish blue.

For higher tones, a Galton whistle was used. At 3000 vibrations *A* began to note a greenish tinge in the blue. Between 4000 and 12,000 it was definitely green, but becoming more and more colourless, passing in the case of tones above 12,000 into a colourless grey.

For tones below 256, I employed an Appun's *Tonmesser*, obtaining from *A* an orange or reddish orange colour for the tones in the region of 150 and 200. I also tested *A* with the lowest tones of the pianoforte. C_0 (about 65 vibrations per second) and even F_0 were dark brown, c^0 was dark chestnut. To C_1 *A* found it difficult to give a colour. No sound ever appears black to him; he said, "I don't know what a black sound would be like."

We have thus obtained from *A* a scale of colours, each of which is in some way related to a tone of a given pitch. Brown and orange

[1] It is true that in some of the earliest experiments *A* reported the colour after he had mentioned some associated object, but he is quite certain that the colour always came to him quite independently.

colours characterise tones of a pitch below 600 vibrations per second; blue is the predominant colour of tones between 600 and 4000 vibrations, changing into green, which remains the colour of tones up to 12,000 vibrations, beyond which the tones are a colourless grey.

3. *A* has had this synaesthesia as long as he can remember. He does not think it has ever undergone any change. The colours are especially apt to appear when he is at a concert, but he often actively suppresses them. Words and numbers evoke no colour and no spatial schemes. Voices, on the other hand, are coloured; Madame Clara Butt's, for example, being violet, and male voices (definite instances given by *A*) being pink, red or brown, according to their depth and timbre. Similarly, the tones of the violoncello are brown to pink, of the bassoon brown to yellow, and of the horn brown to rose. The tones of the trombone are still redder, while the tones of the violin are pink to blue, and those of the fife are light blue verging towards green[1]. Green, says *A*, occurs very rarely among the sounds of nature, the notes of certain birds "shading from blue through peacock colour to green."

4. But the colours which *A* attaches to tones of different pitch are not absolute. They depend—

(*a*) On the timbre of the sound, as the following table shows:

Pitch	Tuning-fork	Tone variator	*Tonmesser*
200	—	Dull brown	Reddish orange
250	—	—	Reddish pink
256	Brown	Lilac, pink	Light orange to pink
300	Brownish pink	Lilac	—
400	Brownish pink	Blue	—
500	Blue	Blue	—
1000	Very light blue	Blue with trace of green	—
1200	Steel blue	Peacock blue	—

The richer tones take on brighter, 'higher' colours, the browns, pinks and blues of the forks becoming tinged respectively with red, blue, and green in less pure tones of the Tone variator and the *Tonmesser*.

(*b*) On the loudness of the tone. As the fork tones 'ring off,' they acquire 'higher' colours. Thus,

300 passes from brown to pink or vermilion,
500 „ „ {brown or pink} to blue,
1200 „ „ blue to grey.

This is in accordance with the well-known apparent rise of pitch in

[1] Further examples are given in Table I, page 237.

tuning-forks as their sound dies away. But A, it is hardly necessary to remark, was wholly unaware of such change of pitch.

(c) On the pitch of the immediately preceding tone. Thus 500, when sounded after 1200, appeared as rose-brown, and 500, when sounded after 600, appeared as soft brown. Yet 500, given on two occasions in the absence of preceding tones, was called blue, and (upon my suggesting brown) A insisted "certainly not brown or rose."

Similarly 256, when sounded after 128, appeared as bluish brown, but when given alone was called brown. 150, when sounded after 250, appeared as deep brown, but when given alone was called orange.

In each case, a previous lower or higher tone 'raises' or 'lowers' the colour of the following tone. This contrast effect appears to occur (but less markedly) with simultaneous as well as with successive tones.

5. It will be observed that in the introspective data given on pages 228–9 A several times asserts the absence of visual imagery. On many other occasions he insisted, "I don't *see* the colours in my mind, I have no imagery of them." Several simple association experiments, subsequently conducted, yielded a similar result. In these experiments, such words were chosen (*e.g.* signal, theatre, whistle, star, band, strain, crocus, paper) as might have reasonably been expected to evoke some form of imagery. But A insisted that his imagery was verbal or more often that his thoughts were entirely imageless. He had no visual, auditory or kinaesthetic imagery of the external objects thought of.

Nor were the colours of tones at all affected by the presence of actual colour before his mind. Some experiments were tried in which A was instructed to think of a definite colour when the sound was produced. In other experiments he was asked to regard a sheet of given colour. But in neither case was the colour of the sound affected. As A said, "the two colours remain separate."

The degree of A's musical ability can be at once estimated by those of his introspective remarks, given on pages 229–30, which I have italicised. He states, however, that he is very fond of music, although it produces little emotional effect on him. He cannot reproduce airs he has heard, but occasionally he is worried by "tunes running in the head." He obtains more pleasure from pure melody than from the effects of sound colour, and finds least pleasure in the rhythmical element of music.

6. A's case is unusual in two respects. In the first place, colour synaesthesiae more usually occur in connexion with vowels, words, persons, days of the week, months of the year, hours of the day, or languages. It is exceedingly rare to find cases where they are confined

solely to musical sounds. Moreover, very many of those cases of synaesthesia which at first sight resembles *A*'s, are fundamentally different. In one class of such synaesthesiae, the notes of an octave have each a different colour, and these colours are repeated in the corresponding notes of all other octaves. The colour, in fact, is attached not so much to the absolute pitch of the tone as to its name or its position in the octave. In many of these cases it appears that the synaesthesia is really derived from that of the vowel in the name of the tone. Thus in a case reported by J. Breton[1], we have the following correspondence between the colour of the tone and that of the vowel contained in the name of the tone:

Name of note	Colour of tone	Vowel sound	Colour of vowel
Do	Black and white	O	Grey-black
Re'	Brown	E''	Colour of undyed wool (*beige*)
Mi, Si	Red	I	Red
Fa, La	Grey	A	Black and white

But even in this case there is one colour which cannot be thus explained; the note *Sol* was red, yet we have no report that *Ŏ* had this colour. And in another case[2] though the note *a* and the vowel *a* and the note *e* and the vowel *e* were of the same colour respectively, yet other notes had colours independent of their letters and preserved their colours even when the experimenter succeeded in puzzling the subject so that he was uncertain of the name of the note. Obviously subjects of this class have, as a rule, very considerable musical ability. Their colours are usually distinctly affected by sharpening or lowering the pitch of a given note. They insist that the major chords have a brighter colour than the minor, that the colour of a piece of music is determined by the key or by the composer[3], and so on.

From this type of tone synaesthesia, *A*'s case is clearly distinct. Nor does it coincide with another type in which association with the colour of the instrument plays a part[4]. There is no reason why the colour of the tones of a violin should lie between pink and blue or those of a bassoon be between brown and yellow, if the colour of the tones of different pitch had been derived from the instruments producing them.

[1] Cited by Henri Laures, *Les Synesthésies*, Paris, 1908.

[2] R. Lach, "Ueber einen interessaaten Spezialfall von *Audition Colorée*," *Sammelbd. d. Internat. Musikgesell.* 1902, IV. 589.

[3] Cf. Th. Flournoy, *Des phenomènes de Synopsie*, Paris, 1893, p. 100.

[4] *Ibid.* p. 606.

A's case is also unusual in the complete absence of visual imagery. The colours come to him as mere thoughts. They have no form, no position in space. He knows quite well what images are, but he gets them very rarely. This condition may be contrasted with the description given by Binet[1] of the general characteristics of persons who have coloured hearing. They display, he says, a marvellous wealth of imagery in describing their mental state, they are characterised by their love for colour and for nature, by *their fondness for thinking in visual imagery*, by their general culture, or their literary or artistic profession. "One point," he says (p. 607), "is certain, namely that the impressions of colour suggested by definite auditory sensations are mental images." So Sokolov[2] writes of these people as having a very lively imagination, deep sensibility and a visual type of memory; he states that they are incapable of working with pure abstractions, they having "always the absolutely concrete mind." Much of this is quite inapplicable to *A*'s mental build. On the contrary, his memory is certainly not of the visual type; he is occupied constantly with abstract problems, being a very able mathematician and a person of exceptionally wide range of interests.

In many cases of synaesthesia the imagery is certainly most vivid. In one subject[3], for example, its vividness almost approached that of a hallucination. He localised the colour sometimes on the retina, sometimes before the lens or cornea, sometimes between the eyes! The tone gave rise to a field of colour upon which raised points, flickering, glittering and lustrous, appeared in relief. But psychology has passed beyond the stage when imagery was considered an essential element in conceptual experience. No one now questions the occurrence and the importance of imageless thought nor, among some individuals, the even complete absence of sensory imagery. We are coming to regard imagery no longer as the master, but as the servant, the bearer, of thought or meaning,—essential no doubt for mental development and persistent in concrete types of mind, but gradually becoming discarded as experience is centred more and more in the abstract.

7. I pass now to points of more theoretical interest. For all of us, different pitches have what I have elsewhere[4] ventured to term different 'tone characters.' The lowest tones appear heavy, massive, thick, rounded and dull; the highest are light, fine, thin, pointed and bright;

[1] "La problème de l'audition colorée," *Rev. des deux mondes*, 1894, CXIII. 586.

[2] "L'individuation colorée," *Rev. philos.* 1910, LI. 41.

[3] Lach, *loc. cit.*

[4] *Textbook of Experimental Psychology*, 2nd edition, Cambridge, 1911, I. 32, 33.

and intermediate pitches have intermediate characters. The colours which *A* ascribes to different pitches appear to be of a similar nature. For him the deep tones are brown to pink, the highest are green to colourless, while tones of medium pitch are lilac or blue. And just as we may change at will the massiveness, the roundedness or the brightness of a pure tone by combining with it certain other pure tones (overtones) in suitable number and intensity, so *A*'s colours may be changed by interfering with the purity of the tone stimulus. He ascribes lower colours to the tones of a tuning-fork than to the more metallic tones of the *Tonmesser* or the richer bottle tones of the Tone variator, of the same pitch. This accords with the well-known fact that a tone poor in overtones appears lower in pitch than the same tone accompanied with overtones.

Indeed *A* may be said to some extent to hear in terms of colour. He ascribes 'higher' colours to higher sounds without being aware of an actual change in pitch (cf. page 232). He may even recognize the presence of two colours, without being aware that two tones are present (cf. page 230).

For his own part, *A* regards his synaesthesia as the result of some 'sympathy' existing in him between auditory and visual experiences. A similar sympathy, less developed, appears to me responsible for the tone characters we ascribe to different pitches. It is some sympathy between experiences of pitch and those of movement, touch and luminosity that makes us describe a tone as heavy, rounded or dull. The physiological and psychological bases for this sympathy are quite unknown to us; but, inasmuch as synaesthesiae seem to be commoner among children than among adults, and to arise generally (though, it appears, by no means invariably) during childhood, their origin may perhaps be ascribed to the persistence of a primitive stage in the differentiation and elaboration of sensations and in the development of their functional inter-relation.

8. That it is individual experience that determines just the colours of sounds of different timbre and pitch is indicated by the following tables[1], which give the colours ascribed by various subjects endowed with colour synaesthesia.

[1] The letters at the head of the columns have the following references. A is the case described in this paper. H is Hoffman's case described by him in 1786, and by Goethe in his *Theory of Colours* in 1810. Rf is Joachim Raff. Rs is a case related by A. de Roches. L is a case of Lauret's. B and L one of Bleuler and Lehmann's. K is a case (Miss B.) described by W. O. Krohn from whose paper (*Amer. Journ. of Psychol.* 1892–3, v. 20–41) the other cases have been taken.

TABLE I.

Instrument	A	H	Rf	Rs	L
Violoncello	Brown to pink	Indigo blue	—	—	Chestnut to carmine
Horn	Brown to rose	—	Purple	—	—
Clarinet	Yellowish to bright blue	Yellow	—	—	Yellow
Oboe	Yellow	Rose red	Yellow	Blue	Chrome yellow
Violin	Pink to blue	Very bright blue	—	Deep violet or glossy black	Garnet, orange, yellow to white
Flute	Bright blue	Dark red	Intense azure blue	Blue	Yellow, blue to white
Flageolet or Fife	Light blue to green	Violet	Grey	Deep violet or glossy black	—
Trumpet	Scarlet to bluish pink	Bright red	Scarlet	Brilliant yellow	—

TABLE II.

Pitch	A	B and L	Rs	K	L
Low	Brown to pink	Black, reddish brown	Deep rose	Dark	Chestnut to red
Medium	Lilac to blue	Yellow	Yellow	Red	Wavy yellow
High	Green to white	White	Deep violet or black	Yellow	Blue to white

It is impossible to find more than a very broad resemblance between the colours recorded by these various observers for one and the same kind of sound. The resemblance is in the main attributable to a common experience of what we have called tone-character. The deep tones, being sombre, have dark or highly-saturated hues, the high tones, being bright, are palely coloured,—with the exception of the subject *Rs*, whose colours appear to be dictated solely by the order of those in the spectrum. This brings us to another point of general resemblance, namely the progression of tones and colours *pari passu* according to the wave lengths of the corresponding stimuli. Colours of long wave length correspond, on the whole, to tones of long wave length ; and, with few exceptions, tones of higher vibration frequency give rise to colours of higher vibration frequency.

But when we come to resemblances in detail, they are characteristically absent. To one the notes of the oboe appear rose, to another yellow, to a third blue. One subject sees a green in higher notes, another sees blue, yet another yellow. It is difficult to explain these individual variations otherwise than by accidental personal associations. As is well-known, two members of the same family, both of whom are endowed with synaesthesia, may in some instances agree, while in others they will hotly dispute the colours of certain words or vowels. But although accidental associations may thus play a part in determining the precise colour that is to be ascribed, be it to a vowel, to a person, to the day of the week or month of the year or to a tone, the primary *conditio sine quâ non* of synaesthesia is that nexus, or 'sympathy' as I have called it, between auditory and visual experience.

9. From these considerations it is probable that for the *full* development of synaesthesia a strong tendency to a certain kind of association is requisite,—a tendency to form associations between corresponding members of two homologous series. Clearly our own subject *A* affords a striking example of this tendency. He associates numbers with letters. Any letter (*e.g.* *Y*) immediately tends to call up the number (25) expressing its position in the alphabet. *A* states that he has certainly had this habit since boyhood; he believes that he developed it at about twelve years of age when, in isolation owing to an infectious illness, he amused himself by "playing about with figures." At all events he is certain that the connexion did not originate as early as that between tone and colour. If at any time now, but especially when he has nothing to think of or is fatigued, or when the letters have no meaning, he stares at a group of letters, the corresponding numbers at once come to his mind. Tendencies to associations of this type[1], combined with the sympathy (whatever be its basis) between sound and colour, and (as a rule) with a vivid visual imagery, appear to condition the phenomena of coloured hearing.

[1] Occasionally *A* has colour experiences with tastes; these appear generally to be suggested by the colour of the tasting object. *A* observes that while the thought of a sound may provoke its colour, the thought of a taste fails to do so.

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FOUNDATIONS AND SKETCH-PLAN OF A CONATIONAL PSYCHOLOGY.

BY S. ALEXANDER.

1. *Introductory. Separation of the mental and the non-mental.* In any so-called experience we may distinguish, on analysis of the experience, two things which are present together and are experienced in different ways. In the perception of a tree we can distinguish the act of experiencing, or perceiving, from the thing experienced, or perceived. Both the act of perceiving and the tree perceived may be said in different senses to be experienced. But the first is an experiencing, the second is the object experienced of which the act is said to be the perception. The act of perception does not occur in isolation, but is

continuous with other acts of the same or allied kind, and, since this continuum is the mind, the act of perception may be called an act of mind. The perceived object (or *perceptum*) is continuous with other such objects of experience, and since this continuum is what is called a thing, the object of perception may without risk of confusion be spoken of as a thing (that is a thing in so far as perceived). The thing of which the act of perception is the perception is experienced as something not mental. In the act of perception there are accordingly these two things, the mind engaged in a certain act, and the thing called the tree which is not mental. That these two things, the act of consciousness and the object of which it is conscious, are present together and distinct from one another is not a theory or a philosophical postulate, but a description of the event which is the perception of the tree in its simplest terms. What is the meaning of the togetherness of the perceiving mind, in that peculiar modification of perceiving which makes it perceive not a star but a tree, and the tree itself, is a problem for philosophy. So far as concerns the act of perception, a little consideration shews that the mind and the tree are two things so together that the tree incites the response of mind in the form of this act of perception, in the same way as a wall repels a stone thrown at it; the difference being that the wall is mindless while here the responding thing is a mind founded as we know upon a body. The perceptive act is a reaction of the mind upon the object of which it is the perception.

In a perception, then, two things are present together, the perception and the *perceptum*. Extending this analysis, we come to distinguish two things in every experience: in sensation, the sensing and the *sensum*; in an image of memory or fancy, the imaging and the image itself; in an idea, the ideation and the idea or *ideatum*; in a conception, the conceiving and the *conceptum*,—in general an awareness[1] and an object: in each case the object of which the mind is aware is a non-mental thing, external, distinct from mind, and revealed to it now as a *sensum*, now as an image and the like. What it is that makes the tree as a *perceptum*, and the same tree as an image or a *conceptum*, is again a problem for philosophy. For us it is enough to note the initial fact, that whenever

[1] The word awareness is used to cover any conscious mental process whatever; it is not used in the limited sense in which the word *Bewusstheit* is used by a school of German writers as merely a consciousness to which no definite single image or perception corresponds. I fear that I did not make it plain in the paper on "Mental Activity in Willing and Ideas" in *Proceedings of the Aristotelian Society*, 1908–9, that I was using awareness in this general sense.

our minds act there is present along with the act some non-mental thing of which we are said to be aware in that act, or, as I prefer to say, which is revealed to us in the act. In perceiving, the mind responds to stimulation from the object perceived. This is not the case in memory or imagination. But the same distinction of the mental and the non-mental still is found. When, owing to some internal physical stimulus or some mental process, the mind imagines or remembers, it experiences, in the two different senses, first itself and second a thing which is not mental, which is present along with the mind, only in the form not of a *perceptum* but of an image[1].

It is convenient to distinguish the two kinds of experience which have thus been described, the experienc-ing and the experienc-ed, by technical words. The mental act is 'enjoyed,' the thing sensed or ideated is 'contemplated.' I can find at present no better words. 'Enjoy' is taken to cover any experience which is undergone or lived through. It has no reference to pleasure; the enjoyment may be a suffering. Still less is the word used to hint a contrast with what can be understood, as in the words in which Wordsworth speaks of the poet as "contented if he but enjoy the things which others understand." On the contrary, enjoyments can be understood and analysed, and it is the business of psychology to analyse enjoyments. The description and analysis of enjoyments constitute what is known as introspection. Psychology is the science of the act of experiencing, and deals with the whole system of such acts as they make up the mental life. The difference between the subject matter of psychology and that of the physical (under which are included the mathematical) sciences is that the second consists of things which are 'contemplated' and the first of things which are 'enjoyed,' or lived through. These enjoyments are what Locke termed ideas of reflection. But unfortunately Locke treated ideas of reflection as if they were another class of objects of contemplation beside ideas of sensation. It may be added, to prevent misunderstanding, that when I speak of contemplated objects in this last phrase as objects of contemplation, the act of contemplation itself is of course an enjoyment.

2. *Ultimate analysis of mind.* It is useful to consider the ultimate

[1] See on the non-mental character of objects contemplated, *Proceedings of the Aristotelian Society*, 1909–10, "On Sensations and Images," especially p. 15, on images. I speak above of contemplated objects as non-mental, or external, in order to avoid the difficulties raised for some readers by calling them physical. When the object is a physical one, its image is physical also, is in fact the same thing as the percept, appearing in a different form.

characters of mind with special reference to Prof. Ward's exposition[1]. Of the three elements which he recognises as entering into any psychosis: attention, feeling and presentation, the element of presentation must, if the above analysis of experience is correct, be excluded, because it is non-mental and does not form a part of the *psychosis*. True, there can be no psychosis without an object,—mind only works in the presence of an outside thing; though we cannot convert the proposition and say that there can be no object or outside thing without a psychosis: for there may be things which exist in all their characters before there are minds to which they are revealed; and there may be characters of things which the mind is not a suitable instrument to apprehend at all. True, also, the psychosis is a different one according as the object is a *sensum*, an *ideatum*, etc.; or according to the various sensory qualities of the object; or according to the various categories under which the thing presents itself. All that this means is that various psychoses are related to various aspects of the non-mental objects; from which it cannot be inferred either that the psychosis itself contains as part of its own constitution an idea in the Lockeian sense or that it is in some way qualified by a presentation in much the same manner as it is qualified by feeling. We cannot therefore say that mental acts contain a cognitive as well as a conative element. The alleged cognitive element in an experience is purely non-mental. A mental act is cognitive only in the sense that it takes place in reference to some object, which is said to be known. But the variations in the conation with the character of the object are conational variations and not cognitional. The difference in the perceiving of a star and a tree is a variation in some intrinsic character which belongs to conation as such. Psychology is directly concerned with the various intrinsic characters of the enjoyment itself, and the so-called presentation, in any of the senses of that term, does not belong to it directly, but only indirectly as an indirect means of discovering the intrinsic character of the enjoyment.

There remain, then, for the nature of mental acts nothing but the elements of conation and feeling. Of the position of feeling in the mental act I propose to say nothing, recognising at the same time that to do so is a grave defect in this paper. I am content, as at present advised, to regard it as not independent of conation, but as a qualification of conation. The attempt to treat it as sensory does not appear to me successful. At any rate the connexion of feeling with conation is so

[1] Art. 'Psychology,' *Encyclopædia Britannica*, 11th ed. XXII. 554 *a* (9th ed. XX. 44 *a*).

intimate that I am justified in following Prof. Stout in regarding it as a modality of (a variation in) the conative process.

In his latest analysis[1] Mr Stout has reverted to the ancient bipartite analysis of mind, grouping the affective and conational elements together under the name of interest, over against the element of cognition. The proposed exclusion of cognition as a separate element amounts to the proposition that there is only one mental process, namely conation with its connected feeling. The tripartite classification of mental elements, which has been replaced by a bipartite one, it is proposed here to reduce to the proposition that there is but one ultimate mental process, namely conation. If the name 'attention' be preferred to conation, I have no objection, and shall freely use the expression. Further it should be added that in thus excluding presentation from Mr Ward's triad, I do not recognise as given in enjoyment any further so-called subject such as he feels called upon to postulate. The subject, as given in enjoyment and therefore in the only form in which it enters into psychology, is nothing but the continuous tissue of its acts of conation or attention. Finally it may be stated (what has been implied) that these conations or enjoyments are what are called 'consciousness'—consciousness is the general form of such enjoyment.

It is the intrinsic characters of the conational act, in their variation and combination, which constitute for psychology what might be called the 'content' of the mental process, were not that word so ambiguous that its avoidance seems desirable, as far as that is possible. There are two main senses of the term. The content of a mental process may mean that with which the process is concerned, or it may mean that of which the mental process is made. We may say of a glass of water that its contents are the water. But the water is entirely independent of the glass, except so far as the shape and size of the glass, whether cylindrical or barrel-shaped, determine the volume and form of the water. But the contents of the glass, in a different sense, are the material of which it is composed and its shape and size. Now the object experienced is to the act of experience as the water is to the glass. It is a different and independent thing, and the character of the mental act only determines how much of the object is apprehended and in what form. But the psychological contents of the mental process are comparable to the composition and form and size of the glass. And it is such contents which it is the primary business of psychology to describe. In other words, the distinctive characters of mental processes

[1] *Groundwork of Psychology*, London, 1903, ch. III.

must be variations of the process itself, not characters of the object. An equally homely example is perhaps still more relevant. The direction of a bicycle varies according as the rider rides with a clear road in front, or a stone lies in the way, or the road bends round a corner. But though we may for convenience describe its motion as that of avoiding a stone or in the slang phrase 'negotiating' a corner, these phrases do not describe the variations of the motion of the bicycle as motions. The content of the motion in the legitimate sense is the varying direction of the motion, and the varying inclination of the machine which the rider has to adopt in order to adapt it to the varying character of the earth as the earth presents itself to the bicycle[1].

3. *Practical and theoretical or speculative conation.* But though cognition is not an element of mental action, nor even in any real sense of the word an aspect of it, the distinction of cognition and conation has if properly defined a definite value. They are not distinguishable elements in every psychosis, but every species of conation assumes two different forms, theoretical or practical, according to the different interest

[1] Some writers mean by the content that partial aspect which the real thing or the real world presents according to the particular mode of apprehending it. This corresponds to the water in the above illustration according as it is contained in a tumbler or a barrel or a tank. I prefer always to call that which we apprehend the object, distinguishing these real but partial objects from the fully developed object or thing of which they are side-views. What we contemplate is something or other, which is now a *sensum*, now a thought. It is experience itself which shews that these objects are not discontinuous but belong together. The complete thing or connexion of things is the same thing which reveals itself incompletely when the instrument is incomplete. To be aware of a 'thing,' something permanent with attributes, is only to be aware of one kind of object. Now the above protest against the use of the term 'content' is directed against the attempt to assign content to the mental act except in the sense defined. It is not directed against the use of the term 'content' to describe partial aspects of reality. But I think the usage inconvenient. For after all, the reality or the 'object' or what you 'mean' must itself be the content of the apprehension in which you think it; and so you are with no advantage dealing with contents of which one is the completion of the rest, and you have to explain that an object is the complete content; it is surely better to say that the thing is the complete object. Secondly, to speak of contents misleads one into supposing that these contents are not real, are in some way the work of the mind; and accordingly the *caveat* has to be reiterated that these contents are real. If you call them objects, no prejudice stands in the way of believing them to be real in their own right. Finally, I believe that the name 'content' blurs the relation of act and that which we apprehend, which it is the object of this paper to make clear. It must be admitted that the choice is one of convenience. The inconvenience seems to me to be on the side of content, which has a tainted history behind it. I am not sure that it is not responsible in part for the position maintained by Miss H. Wodehouse in her *Presentation of Reality* (Cambridge, 1910; *passim*) that acts of apprehension have no differences from one another, but only their contents are different.

which the conation possesses. Mental life is indeed practical through and through. It begins in practice and it ends in practice. But in a more specific sense it is sometimes practical and sometimes theoretical or speculative. Theoretical acts of mind are such as subserve the continuance of the object before the mind without alteration of it. Practical acts are such as, through the medium of our bodily movements, alter the object or its relation to ourselves or to other objects. The practical act which alters the object is the primordial one. All sensory life is in the first instance practical and means pursuit and avoidance. We only have pure cognition or 'theory' when the ulterior act of aggression or embrace or flight is inhibited. Curiosity begins as an act of tearing to pieces or analysis. It ends with the delight of eye and ear, the contemplative senses: instead of pulling the works of the watch to pieces, the child is content to watch the wheels go round and listen to their whirr. Hence in the life of the 'theoretical' animal, man, as compared with the more exclusively 'practical' lower animals, the relatively greater importance of sight and hearing, the organs that do not themselves affect the object but only give the signal for those which do.

The same conation is theoretical or speculative (I use the words interchangeably) when the practical attempt to alter the object is suspended and the conation, instead of issuing in movements which alter the object, terminates within the body or leads on to fresh mental processes or issues in speech. The presence of a bone to the eyes and nose of a dog excites a conation which is the instinctive act of prehension. This is practical perception or instinct. The same object may to a man mean not prehension but the shape of a thigh. This is a speculative perception. The great usefulness of speculation for mental life lies in its thus suspending practice and introducing consideration. It will be one of the main objects of this paper to exhibit in detail the identity of each theoretical activity or process of knowing or cognition with some practical activity.

But two qualifications of this position seem to be needed. In the first place, it is plain that theoretical and practical conation cannot be divided sharply. For every act of theory has some result in bodily movement; and again every piece of practice involves a cognised object, that is it occurs in relation to an external object which is present along with it and is said to be cognised in so far as and in the form in which it is revealed.

Secondly, the distinction of theory and practice emerges more clearly in the later stages of mental life than in the earlier ones, just because the lower forms of mental process are more directly concerned with embrace and avoidance of objects. The distinction is, however, still present. In the sphere of mere sensation, sight and hearing are more theoretical than taste or touch. Perception is eminently practical, but its speculative as compared with its practical form may be illustrated by a more obvious example than the one used above, by comparing, for instance, the meaning which a cricket ball has to a person who is trying to catch it as it comes from a particular part of the field (a practical perception) with its meaning to a person who is interested in its round shape or its being leather-bound (a theoretical perception). This latter perception might be practical in its turn if the percipient were a tanner and supplied leather for the manufacture of cricket balls. Thus the same object may supply a practical perception to one person and a speculative one to another, or the same person may perceive it partly practically and partly speculatively. The speculative part of a perception is that part which does not enter into the present practical interest, for instance, the leather binding to the cricket player. When we come to images or memories or thoughts, speculation, while always closely related to practice, is more explicit, and it is in fact not immediately obvious that such processes can be described in any sense as practical.

For psychology, then, the important distinction is not between cognition and conation as mental elements, for there is no element of cognition in the mental process itself; but between the practical and speculative varieties of conation. If we choose to replace the terms cognition and conation by knowing and doing respectively, these are not the elements of mental acts, but different classes or varieties of psychoses, distinguished by their different interest. And both of them alike involve on the one hand the object known which is not in the mind at all, and on the other the conation which is appropriate to that object, or which is the instrument by which that object is revealed[1].

There might still be made in defence of the position that there is a cognitive as well as a conative element in the psychosis, an appeal to physiology. Cognition corresponds, it is said, to the afferent and conation to the efferent side of the process. But this appears to depend on the lingering belief that somehow in a centre there is an affection from the incoming current, which is followed by a motor discharge. This overlooks the essential continuity of the physiological process and divides

[1] I do not inquire how far the distinction of practical and theoretical conation as expounded in this paper is identical with that distinction as stated by Mr Stout in his chapter on "Cognition and Conative Synthesis," *Analytical Psychology*, II. ch. 7.

it into arbitrary portions. And the whole conception of conscious centres is in the last degree questionable. It is far simpler and, I understand, more harmonious with physiological fact to regard the whole process as one. At what particular point of the nervous path the process becomes a conscious one it is not possible to say at present. But at least the process is a continuous one, and difficulty disappears, at least to a great extent, if we regard afferent and efferent paths as merely parts of the arrangement for converting a peripheral excitement into a bodily discharge of movement, and treat the consciousness as a character of the whole physiological transition.

4. *Difference of this distinction from others.* It is clear that the distinction of practical and theoretical varieties of each kind of conation is not the same as the distinction drawn by Mr Ward between two classes of psychoses which he characterises as respectively receptive or sensory and active or motor. For practical and speculative conations are not two classes of conations, but are, as already explained and as will be made clearer later, nothing but different varieties of each kind of psychosis. To enter into detail: the distinction is not that of active and passive or receptive attention. For both speculative and practical conations may be either receptive or active. Thus sensation, as theoretical, is receptive and judgment active; while practical sensory conations are receptive and willing is active. Nor, again, is the distinction that of sensory and motor. For theory and practice are alike both sensory and motor. They are both sensory in so far as both imply objects present in the experience, upon which the conation or attention is directed. They are both motor in so far as both issue in movements, which may themselves become in turn apprehended or attended to. It is clear that the distinction drawn here is unlike Mr Ward's and founded on different considerations.

But without further direct comment on this difference of treatment, it is important to indicate what the relation of the conation is to the motor discharge. All practice is effected by movements. But all theoretical actions also issue in movement. But the motor experience, that is the consciousness of the non-mental motor objects, is not itself part of the conation, but superadded to it. One possible misapprehension is to be guarded against in particular, namely that practical actions mean the direction of attention to movements. Even the highly developed practical acts of desire or volition cannot be represented as defined by the direction of attention to the movements[1]. Rather they are

[1] This matter is returned to later, p. 264.

complicated movements of attention or complexes of conation which, being directed upon certain objects which are non-mental, issue in bodily movements which do in turn become observed and may even, as in considering the means to an end, become part of the initial object of attention. But these movements are not the primary determinants of the process of volition or desire. Thus the sight of a purse or an apple may excite desire; but though the consciousness of the resulting bodily movements (some forward towards embracing the object, some resisting possession) sustains the process of attention to the object desired, it is distinguishable from the movements of the attention to the object desired which are the essential ingredients of the desire. So again in willing, the primary object of the conation is 'the remote cue' or end. Willing is the conative process by which this end is transformed from a merely ideal or represented condition into a real or presented condition, and this is effected in the ordinary case through bodily movements. The consciousness of these movements is an additional consciousness (I should say an additional conation) which sustains the primary one. The necessary movements may even be thought of, and the attention in such cases is then partially directed initially upon these means. But this is not the simplest case of willing. The same thing is true, and it is perhaps clearer in the simpler circumstances, of perception, considered in its connexion with instinct. In watching a ball which one is trying to catch, the perceptive conation whose object is the approaching ball is that complex of visual and anticipatory tactual conations which issue in movements of the eyes and more particularly of the hands. But these movements as apprehended in consciousness are an addendum to the instinctive movement proper. In the next place, these sensory experiences reinforce the conation itself, because the consciousness of the external movements is itself a fresh conation which travels along the same line of motor discharge as the primary conation. Once more however it is not the case that the attention to the external movements constitutes the instinctive reaction, but rather the instinctive reaction which produces an external bodily change, and this bodily change excites a kinaesthetic 'sensory conation' of a kind which sustains the parent mental process.

5. *Plan of psychological details.* Such being the nature of mental life, the business of psychology is primarily to describe in detail the various forms which attention or conation assumes upon the different levels of that life. It will describe how differently it *feels* or *is enjoyed* when we sense or perceive or will or the like. From what has preceded,

it is clear that any such process of conation will need to be distinguished both from the object attended to, or cognised, and from the motor discharge the effects of which in turn may be attended to. For both the object cognised and the organic or kinaesthetic *sensa* in which attention results are objects contemplated and are not enjoyed. At the same time the exposition should exhibit side by side with the process enjoyed both the object as cognised and the bodily expression.

It must be distinguished, too, from the physiological process which is commonly said to underlie it, or which is more properly described as exhibiting the character of consciousness in the particular form enjoyed. For this, too, is an object of contemplation, and, moreover, is not even directly contemplated by the person who attends or has the enjoyment. But, again, although the mental act is to be described for itself, it stands in an intimate relation with the physiological process, and therefore any physiological knowledge which may throw light on the nature of the consciousness is a proper instrument of psychological inquiry. For the same reason, any legitimate knowledge derived from what is called the unconscious, however that is understood, is available for the study of consciousness. There is no discontinuity on the view adopted here between the conscious on the one hand, and the physiological and the unconscious on the other. But further discussion of these matters is explicitly excluded in order not to break the main thread of the statement.

It might be supposed that to confine psychology to an account of acts of conation or attention, together with whatever further data might be utilised for more fully elucidating and explaining them, would mean the abandonment of the greater part of present psychology. It means, on the contrary, nothing more than a rearrangement of existing material or future material of the same sort. Not a single fact which now enters into psychology would disappear from its purview. It would only assume a different place and value, and be exhibited in a different relation. The object of the following paragraphs is to attempt in the broadest and most tentative fashion to sketch the plan of such an enterprise. Fortunately the problem has been already solved in the case of perception, considered in its connexion with instinct. Upon that topic the best exposition (that of Mr Stout in his *Manual*) begins by describing the feel or enjoyment of the acts of conation which constitute instinct. Such an exposition allows us to understand therefrom how in the *perceptum* the sensory elements can be regarded as qualified by non-sensory or ideal elements. Upon the plan, then, here proposed, in each case the account of the conation itself must precede. Its cognised object is indicated in correspondence with it, and the bodily expression should follow. Above all, it must be insisted that each so-called act of cognition is some form of conation or attention and consequently the intellectual development will appear as stages in the growth of conation or, to use more pointedly the name of its final development, of volition.

The principle of the inquiry is therefore simple. Since in every experience we have, side by side with each other, two things, one the mind in a mental act and one the non-mental object, in each case there is a conation, and co-ordinately some form which the non-mental world assumes in correspondence with it. It is never true that we have first an object cognised and that then the cognition of the object determines the appropriate conative response. On the contrary, according to the character of the conative act, however brought about, the object is revealed under a certain aspect. Thus, to take the case of instinct and perception, the object is not first perceived and then reacted upon instinctively, but in being reacted on instinctively it is perceived, that is it is before us in the form of a *perceptum*, not of a mere *sensum*, nor as a memory or thought. An object is not first imagined or thought about and then expected or willed, but in being actively expected it is imagined as future and in being willed it is thought. The expression was used, "the conative act, however brought about." For in some cases, as in perception or sensation, the conation is initiated by the physical action of an object upon the organs of sense. In other cases, as in memory or thinking, it is initiated by some internal physical excitement or by some other mental process. Important as this distinction is, it leaves the principle unaltered. In all cases there is a conation brought about, either by the actual object through physical means or in some other way, not by any antecedent or independent cognition of the object.

For psychological purposes the most important differences in conation are those in virtue of which the object is revealed as sensed or perceived or imaged or remembered or thought. In each case the nature of the conation is the clue to understanding the character of the object. Because certain parts of the perceptual act are not present and actual, but only anticipated or prepared, the object is revealed partially in the form of tied ideas, *i.e.* it appears as something sensory, qualified by something 'ideal.' The difficulty consists in describing accurately and fully what are the modifications of the conative process in virtue of which the real object assumes its familiar forms of percept or memory or the like, with which psychology is more specially concerned. But there are other differences in conation, such as the differences in the sensations, where the character of the conation does not afford the reason of the character of the object. We cannot tell why one sensory process should make us see green and another make us see blue and another make us smell scent. And there are other conations, to which correspond the ordinary so-called categories, which call for description from the psychologist. Both these last classes of conative differences stand on a different footing from the differences which reveal an object as sensed or perceived and the like, and in an appropriate sense they may be called material[1]. But this is not to be taken to mean that the characters of objects in virtue of which we style them *percepta* or thoughts or images do not belong to the things themselves. Finally, it must be observed that the *cognita* which are the objects to which our conations correspond, though always non-mental, are not necessarily present to us as they truly are in reality. They may be imaginary or erroneous and not real, but the distinction of the real from the imaginary, is not one which concerns psychology, or, if it does, only so far as there is a difference in the conation according as the object is before us as real in contradistinction from imaginary.

6. A. *Sensory conation.* The greatest difficulty meets us at the outset. The mental act of sensation which issues in reflex movement is so simple as to defy analysis. Its corresponding non-mental object is the *sensum.* But though it does not admit analysis, it has describable characters. We can note at any rate its characters of intensity and duration. In what cases and in what sense it possesses extensive character (to be carefully distinguished from the extensive character of the *sensum*) is a debateable point. But when we ask ourselves how we are to discriminate as conations a sensation of green from one of blue or of sweet, we find that there is at any rate no difference of quality in the sensory conations themselves, but only in the *sensa.* Nor is there, indeed, any difference of quality between conations of any order. All consciousness has the single quality of consciousness. Accordingly we must hold that sensing as such, *i.e.* as sensory act, possesses some conative character which varies with the quality of the *sensum* but is not itself a difference of quality. This difference I have ventured to call a difference of direction. But so vague and ill-defined is this character that in order to distinguish sensations into their sorts we commonly adopt, or at least we may adopt, either of two indirect methods.

[1] See for this A. Messer, *Empfindung und Denken*, Leipzig, 1908, 50.

The first is to distinguish them by their corresponding *sensa*. This method is that which the psychologist in general pursues. It is a perfectly legitimate method. The conative differences apparently elude description. The sensory acts are accordingly distinguished by their objects. These *sensa* are non-mental, and strictly speaking they are the proper inquiry of physics and biology. They are studied by the psychologist with a specific interest. The physicist studies them with more particular reference to the spatial and temporal characters and the other primary qualities of their stimuli. The physiologist studies them in connexion with the neural mechanism which subserves the consciousness of them. The psychologist studies them as a clue to classifying and distinguishing the different sorts of sensory conations. Just as we are unable to discriminate the qualities of smells in themselves, but describe them as the smell of violets or of asafoetida or of roses ; so, our discrimination of the acts of sensory attention in themselves being insufficiently delicate, we fall back upon their objects. A conational psychology may accordingly with perfect correctness employ this resource on the same principle as we infer from a man's energetic language the strength of his sentiments.

The second method of distinguishing the various sensory processes would be to describe as far as possible the underlying physiological process indicating the course of the movement in the parts of the brain more particularly concerned, and this is habitually done by the physiologist with help from the anatomist. This, too, is indirect. And since it is only by experience of the different *sensa* that we can be sure that such and such a physiological process corresponds with a given sensory process, the reference to the *sensum* is still indispensable, and the sorting of sensations by their objects seems the directer of the two methods.

That the differences in conation corresponding to the quality of the *sensum* should admit only of this indirect description is an improbable and at least an unsatisfying solution. There is good reason for believing that these differences are really of a spatial character, really are differences of the locality and direction of the physiological processes, and that that locality and direction are actually enjoyed[1]. But to defend this statement, with its implication that mental process is in space as well as in time, raises so many debateable points as to interfere unnecessarily with the course of the exposition. If the suggestion is valid,

[1] See *Proceedings of the Aristotelian Society*, 1910–11, "The Self as Subject and as Person," sections 4, 5.

the place in which the physiological process occurs is really far more intimately concerned with the distinction of sensory processes than the mere indication of the quality of a *sensum*.

7. B. *Instinctive or impulsive conation, or perception.* This has been fully described by Mr Stout as a conative complex, consisting of a train of conations where each actual or sensory conation is qualified by preparation for the next step in the series, and the whole runs down to a close with the attainment of the end. So far as this complex is preformed in the organisation, we have instinct proper. But besides these organic preformations, there are acquired ones, and these come under the same description. This is the mental act of instinct. The corresponding object is the percept or thing perceived. To the preparatory or qualifying tendency corresponds the tied idea or ideal element which enters into every perceived object. The simplest case of such a *perceptum* is the simple quality (blue or sweet) distinguished from the *sensum* by its suggestion of the past; not, of course, a quality in the abstract—blue—but in the concrete this blue or this sweet[1], that is to say, this thing so far as exhausted by blue or sweet. Or, to speak more accurately, the simple quality is the *sensum* with the implication of persistence as revealed through the tendency to go on into the same sort of conation. Similarly a percept composed of heterogeneous elements is one in which a *sensum* is qualified by other elements suggested in the ideal form.

Thus we have to recognise that a thing as perceived contains besides sensory elements other elements present to the mind only in ideal form. The act of perception is the conscious instrument by which these ideal aspects (in addition to, or in qualification of, the sensory ones) are revealed, and the ideal elements or tied ideas are only intelligible psychologically by reference to the qualifying or preparatory conations of the whole conative complex. But the ideal elements are themselves objective and non-mental. They exhibit their true relation to the sensory elements in the course of the perceptual process itself. For as the instinctive movement proceeds, the preparatory movement becomes effectual, and the previous effectual movement drops into the position of a qualifying movement, and there may be a new preparatory movement which qualifies the actual one. Correspondingly the object is revealed as sensory where before it was ideal, and ideal where before it was sensory. In the seen and touched orange the taste is suggested. In the tasted orange the seen roundness is suggested.

[1] Cp. A. Meinong, *Ueber die Erfahrungsgrundlagen unseres Wissens*, Berlin, 1906, 27.

8. C. *Reproductive conation or wish.* Instinct has introduced us to the existence of a qualifying or tied conative tendency. The next stage is the emergence of this tendency from the condition of being a mere qualification of another conation into independence as reproductive mental action. The object is correspondingly revealed as the free idea or image. Under this term are included not only objects of fancy, but those of memory in the wider sense of that term ('primary memory' of James), where an object which has once been perceived appears to the mind without being apprehended as belonging to the personal history of the apprehender or *remembered* in the strict sense. Whether the reproductive conation is merely a revival of a past conation or involves a further complexity, whether it occurs in the same anatomical places as the perceptual conation, or on some higher but connected level of the brain, is a question which fortunately it is not necessary to discuss here. The reproductive conation means anyhow the existence in the mind of a conation in the absence of the sensory object or rather in the absence of objects revealed as sensory. Its existence may be verified more easily in the more complicated case of a train of association or a play of fancy, just as the nature of perceptual process is more easily verified in a train of perceptions than in the more subtle case of a single perception. Perhaps it is made clearest by the instance of a free image suggested by a perception, as contrasted with the mere suggestion of a tied image—the familiar instance of a suit of armour leading on to the image of a tournament as contrasted with the perceptual experience in which the armour is said to look cold. In the second case if the hands go out to touch the metal the ideal cold becomes sensory. In the first case, the conation directed upon the tournament does not become sensory. It tends to do so, and may in persons of vivid temperament succeed. "All, idealists," said Mrs Browning of the French of her period, "too absolute and earnest, with them all, the idea of a knife cuts real flesh." Such impulsiveness, when it is theoretical, is hallucination. But while the free reproductive conation shares with the conation of the tied image its non-sensory character, it is unlike the latter (except when it becomes hallucination) in failing to become sensory, and the corresponding object is freed. What the occasions are which lead to the emergence of free images is by no means clear. But it is a commonplace to point out how large a share in this process is played by the frustration of what on a later level is called expectation. Free images are largely the offspring of disappointment. Imagine a person who put out his hand in order to feel the armour finding it not cold but warm, or finding that

it receded from his touch. Or the independence of the image may be achieved through the competition of divergent suggestions. Or the still commoner case may occur where a conation arises in the mind which is incompatible with, and is checked by, the sensory environment.

When such reproductive conation becomes sensory, the sensory movement invades the sensory organs, and, as indicated above, we have hallucination as in certain forms of dream[1].

It is more difficult to designate this form of conation on its practical side by a satisfactory name. It is more than instinct and less than desire. It might be called 'appetite' or 'appetition' because in hunger we are often aware of fleeting pictures of objects which would satisfy the hunger without our actually desiring them. But the name 'appetite' is more appropriate to instinctive impulses when discharged by internal excitement rather than by an external stimulus. The most suitable name is 'wish.' But this, too, is not free from objection; for, generally speaking, when we wish we not only have an imaged object, but we tend to expect it. It might be desirable to invent a technical term—perhaps 'appetition' would serve. But by whatever name the conation is best called, its existence and specific character are verifiable, and it is to be distinguished, though not sharply, from the next stage of conation which is 'desire' proper.

The main facts of association of ideas and free imagination (fancy) come under this head of reproduction or wishing. The law of association is expressed simply thus: that one part of a complex of conations tends to revive the remaining conations in a reproductive form. (*E.g.* when one of a group of friends is present we wish for the others[2].) Accordingly the law is less properly described in terms of the objects, for while it is true that objects which have been together are connected, and it is their connexion which makes us apprehend them together the first time, they are not revived as ideas because of their connexion, but the conation appropriate to the one induces the conation appropriate to the other, and that other is consequently revealed in idea[3].

[1] This is plainly compatible with the view of Prof. Freud that dreams are the fulfilment of a wish.

[2] In the famous meeting of Scotsmen where Charles Lamb expressed the wish that Burns were present, that was a fancy that passed before his mind, called up by association for a son of Burns was to be present. The objectors who said that that was impossible because Burns was dead mistook a fancy for an expectation, a wish for a desire. "An impracticable wish, it seems, was more than they could conceive."

[3] The objects themselves are not so much associated but (as Kant put it) associable or affined.

9. D. *Desire.* The next form of conation is familiar in the form of developed desire, which represents the struggle of a reproductive conation (wish) against a felt obstruction. Desire in general, as the word is commonly used, is directed upon a future object. But there are conations upon this level which are directed upon the past, to which the name is inappropriate. It is used here at the cost of apparent paradox, for want of any more comprehensive term which includes both varieties. The corresponding cognised objects are the images of expectation and memory. The word 'memory' is used in the strict sense. Both expectations and memories are more than mere images founded on previous experience. They are objects recognised as belonging to the future or the past; or, more strictly speaking, as belonging to the future or the past with which I personally am connected. Thus to take the case of a memory proper (or its process which is remembering), it is more than a recognised object, which is a percept. And it is, of course, less than something recognised as belonging to that past in which I have no share. I cannot remember the death of Caesar, though I can think of its date in time. Thus a remembered object (event) is remembered as mine. It is recognised as something which I experienced before. An expectation is a future object, recognised as belonging to me. Now the acts of expecting and remembering are the theoretical or speculative forms of the same conative activity which in its practical form is desire.

The main outlines of desire are fairly clear. An impulse is initiated whether by a present object, like food, or the idea of one. This impulse is baulked by some felt resistance, proceeding either from the outer world or from ourselves. Owing to this resistance, the object of desire assumes a free ideal form. It is the happening of some event A which now is not, like the fall of a ministry; or generally of some fact that A is B which it now is not. This object may be present in the form of imagery. But this is not necessary. I may simply desire that A be B, where B need not be imaged but may be only a predicate[1]. But in order not to complicate the discussion let us take the case where there is imagery. The object of desire assumes this form because the inhibited impulse lacks sensational intensity, and partly because it overflows into associated directions; and it does this the more, the more the hindrance

[1] This has been constantly made plain by Mr Bradley. When I apprehend that A is B the object is what Prof. Meinong calls an objective, and in the case of desire it is an assumption (*Annahme*). In remembering we do not always have a picture of the past, we may only remember that some event happened to us. Here we have the beginning of thought, and the boundaries of the corresponding mental processes of desire and will are not sharp. But it would introduce endless complexity into the text if I attempted to deal with all this, even if I were able to at present.

continues. Hence, in desiring, the more the enjoyment is delayed, the more fancy begins to weave about the object images of future fruition, and to clothe the desired object with properties calculated to inflame the impulse. But desiring is more than mere baulked impulse. This is only one element in the process. The other and distinctive element is the insurgence of the appetite against its hindrances. You can mark in desire the rising of the tide, as the appetite more and more invades the personality, appealing, as it does, not merely to the sensory side of the self, but to its ideal components as well. Desire then is the invasion of the whole self by the wish, which, as it invades, sets going more and more of the psychical processes; but at the same time, so long as it remains desire, does not succeed in getting possession of the self. All desire exhibits this 'tantalising' character. Correspondingly, the object of desire, in virtue of the enjoyed process which has been thus described, while still remaining ideal is contemplated in relation to the self or subject of experience so far as that self can be contemplated; and it is habitually so contemplated as the bodily part of the personality together with such other things as come to be inseparably connected therewith. This relation of the desired object to the self does not mean that the desired object is a *state* of myself, but only that it is directly or indirectly related to myself, and it thus acquires a warmth and intimacy, to use James's phrase, not possessed by an image of reproduction or fancy. The attention is still directed primarily on the event which the act of desire strains to realise, but in the course of the desiring process it is also distributed over the self, which the struggle in the mind tends to throw up into distinct consciousness not only in its enjoyed but in its contemplated form[1].

Now if you examine expectation, it is precisely these characters that you find. The object comes before us in idea, with the mark of the

[1] On the self as contemplated, see the paper on "Self as Subject and as Person" (*Proceedings of the Aristotelian Society*, 1910–11). The above may seem at first sight inconsistent with the statement in a previous page (§ 4, p. 247), that in desiring we do not attend to the movements or other organic reactions by which the object is realised, but it is not really so. We still attend to the object, but incidentally to the process we are made aware of its connexion with our own self. Every mental act means of course the presence, along with the object, of our own enjoyment, but it does not mean contemplation of the relation of the object to our self, unless that self is thrown up into contemplation as it is through the invasion of our subject or enjoyed self by the desired object. Observe also that while the contemplated self is in general our body and always centres in that, it may include things which are so organically connected with me, *e.g.* my child or even philosophy, as to form one group of external things centring about the body. Hence a desired object may have warmth even more from its relation to my child than to my body.

future on it, or of the not-yet (I shall endeavour presently to explain what this phrase 'the mark of the future,' used here for shortness, means). In so far as it is expected, we both prepare for its presence in actual perception and are stopped from so perceiving it. But it is expected and not merely imaged, because the conation invades and tends to overrun the whole self. In virtue of this invasion of the self, the expected future object is mine and belongs to my future. Thus, while expectation is relatively theoretical as compared with desire, it is often only incompletely theoretical. The interval between a cold expectation and a warm desire may be filled by expectations of varying degrees of warmth or by desires of varying degrees of coldness. Perhaps we speak more often of expectation when the impulse instead of urgent is calmer and more prospective, and instead of exciting a multitude of organic processes is more contemplative and allied to the purely theoretical. But, whether in pure or mixed form, its essential features are those of desire.

Remembering offers more difficulty. It will be convenient to use 'memory' for the object and 'remembering' for the process. Remembering may be described as an act of desire directed backwards towards the past. In certain phases of remembering, as in seeking to remember some forgotten incident of my life[1], the element of yearning or longing is obviously present. And if we ask why the desiring character of memory escapes recognition a reason may be assigned. Expectation is plainly in affinity with desire. But remembering is more theoretical than expecting. Expectation may be followed by present or sensory fruition, but the past cannot be restored, and accordingly that form of desire which is remembering is inhibited from practical issue and has lost its pungency. But its distinctive features are the same as those of practical desire and the more theoretical expectation. Only the memory has on it the mark of the no-longer or the past. Remembering shares with expectation and desire the invasion of our self by the process which corresponds to the past object. We are striving to bring this process along with its object into closer and warmer contact with our present self. Recognising the object as belonging to our past is this dragging of the object out of the past into myself which is present. And we may add, to revert to a principle enunciated earlier in this paper, it is not

[1] Even when I try to 'remember' something forgotten which is not something that happened to me, *e.g.* the first line of *Paradise Lost* or the date of the battle of Actium, I seem to have the implied consciousness that *I* knew it once, and so such examples of apparent mere ideas fall under this heading.

because the object is first before us as past or as a memory that we then in this way try to bring it from the past into fruition; but because the process which apprehends the past object (itself, of course, set going as a rule by some suggestion or other) invades the present self that the object appears to us a past object which belongs to us, that is as a memory.

The objection may at once be urged that many memories are unpleasant, and that, so far from desiring them, we seek to avoid them. This objection arises partly from the use of the awkward word desire. In truth a memory which we try to avoid presents a complicated situation. So far as we remember the object there is a theoretical conation (a desire) which seeks to bring back and hold the object. To this there is superadded a practical effort to get away from the unpleasant memory.

With practical desire is coordinated practical aversion, where the effort to avoid an object struggles against the fascination of the object which detains the attention. What is the theoretical form of aversion which is coordinated with remembering? It is the process of forgetting. We may verify this in the cases where we try to forget, just as we may most easily verify the nature of remembering where we consciously try to remember. It only sounds so paradoxical to equate forgetting and aversion, because in general forgetting takes place below the level of explicit consciousness. We do not in general try to forget as we try to remember. We are not aware of any struggle. This seems to suggest that the demarcation between desiring and wishing is less perfect than appears from the above account, and that it might have been preferable to take them as two stages in the same type of conation. But apart from this question of how far we can notice forgetting, we may observe that what forgetting means is not simply failure of objects to rise into consciousness, but rather their progressive fading from consciousness. Now in a large number, at any rate, of cases it has been made plain by Prof. Freud in his *Psychopathologie des Alltagslebens* that the forgotten object is disliked, and yet (fascinating us like the object of practical aversion) is perpetually trying to emerge into consciousness, and betraying its presence by the character of the mistakes of memory which we commit, or *mal à propos* words or acts. The forgotten object is not in such cases lost; though suppressed, it is not inactive.

There is one point in the above account of expectation and memory which requires elucidation. The object was described as coming before us with the mark of past or future upon it, and the phrase was used without further ado because the main problem was not to explain how an object comes to us as belonging to the past, but how it comes to be known as remembered, that is as belonging to me and my past. But just as we can say that an object external to us appears to us as an image and not a percept, because of the character of the conations which reveal it, or appears to us as green or red because of differences in the direction of the simple conation, so we may go on to ask what are the conative characters in virtue of which the pastness and futureness, which I suppose really to belong to the objects themselves, reveal themselves to us as such. The probable answer has been indicated in passing. We become aware of the pastness of an object in so far as the past object is one whose corresponding conation, itself enjoyed in time, tends to invade the person[1] without becoming perceptual, while the future object tends to invade the person so as to become perceptual. Here in the region of ideation is that enjoyed experience of order in time to which in the object corresponds contemplated order in time. Accordingly the above account which in respect of this matter might seem to beg the question at issue must be read with this qualification. The very act by which the object is remembered assures us of its more or less definite date in time. It may be added for completeness sake that to apprehend the date of an object in universal time, that is in time not apprehended as measured by the events of my particular experience, is to use not memory or desire alone, but thinking, and secures a further revelation of the object and, as compared with memory or expectation, implies a still greater advance in theoretical activity.

NOTE. In the above I have been dealing directly with remembering or expecting *an object.* It is clear enough that to remember a past event is also to remember my own mental state as it was in the past, the difference being one of interest. In general I am occupied with the object. But I may be interested in myself, and then the remembered conation itself stands out in prominence as contrasted with the object. But the process of remembering one's own mental state raises certain difficulties which I may treat shortly in a note. Remembering the object and remembering oneself are parallel and indeed numerically identical processes. But there are two differences arising from the fact that I contemplate the object but enjoy myself. First, the past object is presented to me in the only way in which it can, as an image or an ideal

[1] I am keeping to the case of 'remembering.' These expressions need slight modification for the simpler level of 'primary memory' or retention.

object, with the mark of the past. But now we have no image of our past mental state in the same form as we have an image of the past object. For we do not contemplate ourselves. We only have or enjoy the renewed mental process corresponding to the past object, though not renewed in the precise form in which it occurred, but in the form appropriate to the image of the past object. This is the meaning of having an image of ourselves, if that expression is employed. Second, it may happen that the same object happens to be present also in perception, as when I say to a man, you are the man I remember meeting yesterday. Here the percept is compared and identified with the separate image. But this need not happen. It is enough for remembering him that he should invade my present self, no matter with what objects I am presently concerned. But what need not happen as regards the object always happens as regards the self. I am perceptually enjoyed, and, though I need not be perceiving the old object, I at any rate am here. But, allowing for these superficial differences, the remembering of myself and of the object are the same. The mental process which I am said to remember comes before me with the mark of the past; it is on the backward edge of the 'broad' present of myself. But it is already enjoyed as a part of myself, being continuous with the remainder of myself. The distinct consciousness of its belonging to myself is the consciousness of that invasion which has been described. The remembering of my past state is the bringing a state of myself which is enjoyed with the mark of the past into greater dominance in my present as against the resistance of the processes now dominant in my present. As it becomes more and more vivid in my enjoyment, it still does not, except in case of hallucination, become present as an enjoyment concerned with a present object. If it did so it would be expectation.

The question might be asked: If the memory of myself is but a renewal of a past state invading my present self, am I aware of the renewal as being a renewal? The answer is no. To be aware of this renewal of an ancient state as a renewal would be more than remembering it or myself. It would be to have a theory about my present self, viz. that it is identical with an intellectual construction which I, or rather someone else, might contemplate and call my past self. But this, so far as it is a possible feat, is more than remembering myself. To be aware of the renewed mental state as a renewal would mean that I can compare it with an image of my past. But this image does not exist, in distinction from the renewal. Neither be it observed does it exist in remembering the object, so long as we confine ourselves to remembering. A remembered object is not the appearance of an image known to be an image of some past object, but only a past object known as past and known for mine. In the same way, to enjoy a renewed state and to drag it into intimacy with my present enjoyed self is to enjoy it as mine and past, and that means as belonging to me in the past.

Possibly there is still an obscurity as to the process of establishing this intimacy. Contrast the remembered mental state with the immediately preceding past event which I do not *remember* but still retain in my mind. That is immediately continuous, as I feel it fading away, with my present and forms indeed a part of it. But compare this with the memory of what I felt a week ago when I renewed acquaintance with a former friend. These mental states lie on the same side of me as the immediately preceding state, and not on the same side as the mental states just dawning in me on the side of the future. But to realise vividly that they are remembered past conditions is to recognise them as mine through this obstructed tendency of them to usurp my whole present. I hold them there instead of letting them slip back out of my mind as I do with the immediately preceding event which I am (we are supposing) not *remembering*.

If any difficulty is still felt, it must be, I think, because of the immense detail of our feelings which we may have in our minds when we recall a past state of ourselves. But so far as these are mental states the account given applies. So far as we are remembering our bodily condition, it must be carefully observed that this belongs to the objects we remember and enters as a constituent into their detail. There is no reason to doubt that the memory of our past states is enhanced if our interest can be spread also over their objects, which are indeed inseparable from them. But this raises no problem.

10. E. *Voluntary conation or thinking*. Volition may be described as the effective entry of a reproductive conation into the system of conations which constitute the self or subject of experience. The corresponding *cognitum*, *i.e.* the object present along with it, which assumes this form because of the character of the conation, is a judgment, or let us say in order to avoid confusion with the act of judging, a proposition, which is but the verbal expression of what is judged. The act of judging is the theoretical or speculative form of the act of willing. This is no mere statement of an analogy between willing and judging. Willing has been described as the self-realisation of an idea with which the self is identified. This is precisely what on any view of the logical import of propositions occurs in judging. The proposition, which to avoid

controversy is taken to be the common subject-predicate one, asserts that the more or less undefined subject is qualified by an ideal or universal predicate, which gives it definiteness, or we may say that the relatively abstract predicate is realised in the relatively concrete subject. What corresponds to the "identification of the realised idea with the self" is stated later. This is sufficient to justify a close analogy. But here the act of judgment is maintained to be literally an act of will. The proposition which is the *cognitum* of the judgment is the object willed.

This may briefly be further explained. According to the ordinary analysis of willing, the process is one in which an object first presented in idea[1] is converted into the same object presented in perception. Willing to lift the arm is the (enjoyed) conation by which the lifting of the arm, regarded as merely ideal, becomes the actual lifting of the arm. The realisation in the case of the practical or ordinary act of will is effected by the attachment of the willed object to the self of the willer. By this is meant that the willer's self becomes engaged in the work of bringing the willed object from ideal into actual existence. So far as he is able to produce the object willed, the conation of which it is the object, becomes involved in the complex practical machinery which constitutes his efficient self. The consciousness that this takes place is the so-called *fiat* of the will. Take as an example a case of willing where the object is something entirely external to me, *e.g.* that you should leave the room, or that half my fortune should descend to my son. Here I first entertain the willed object, your going out or your being out of the room, in idea, and then this idea is taken up into myself and becomes real so far as the realisation depends upon me. You may resist, but I push you out or order you out, and so long as the will is maintained, so long is the practical effort made to secure the real existence of your exit. In the case of the legacy I will the steps necessary to secure the future performance of the testament. I set in train the actual process by which half my fortune becomes transferred to my son.

It may be well to point out the contrast of this analysis to certain statements which are made about volition: (1) It is sometimes alleged that when I will I will a state of myself. In the cases mentioned above the object willed is something quite distinct from me, in one case

[1] Not necessarily in the form of imagery. It may be only the fact that the arm is lifted, presented not as a judgment but as an affirmation (without assertion), *i.e.* as an assumption.

something which will only exist when I have ceased to be. It is not the object willed which is necessarily a state of myself, but the act of willing itself which brings the object willed into intimate relation with myself, so that the object as a result of the willing becomes not a state of myself, but closely linked with me[1]. When I will to lift my arm or to go to London, I do, indeed, will a state of myself; and myself or my body is presented in idea at the initiation of the act of willing. But these, though perhaps the commoner, are after all only special cases. (2) It is sometimes said that, in willing, the object is a certain action or movement to be performed and that the attention is directed upon that. That may and does occur when we also will the means to the end, or when the means occupy our minds and we almost forget the end. But from the above account[2] it follows that the movements by which the will is effected externally are never the primary object, but are the expression, primarily, of the act of willing itself. Willing is attention to the end[3]. (3) Lastly, it may be observed that in the above another misapprehension has been guarded against. The object of will is not a percept or image, but is the qualification of something perceived or imaged. It is of the form A being B. Consequently, while it may take the form of an image of AB, the qualification need not be imaged and, as a matter of fact, is not always presented as a separate image. The object of will may be only *thought*.

The entry of the object of will into the conative system requires a few more words of elucidation. Any object whatever in so far as experienced is taken up into the total mass of conations which are the subject of experience. So, for instance, a mere reproductive conation with its corresponding image is more or less closely adherent to the subject. But it is relatively independent. In fact the lower forms of conation differ from will and also from desire by this relative want of systematic organic connexion with the whole. Willing is a process by which this independence is broken down and replaced by organic

[1] See the same point in connexion with desire, p. 257.

[2] See § 4, p. 247.

[3] This is James's statement. Cp. W. McDougall, *Social Psychology*, ch. IX. The real clue to understanding the nature of volition appears to be comparison with the process of trial and error in perception (cp. Baldwin, *Mental Development in the Child and the Race*, New York and London, 1895, ch. XIII.). In that process, the animal modifies his percept through uneasiness at failure to attain his end. In willing the modifications of the object perceived, what was called above the qualification of a percept, is thrown out into consciousness in ideal form. In the same way the animal remembers (I am using the word in the loose, general sense) the actions which were successful and repeats them. In willing man thinks of the means which have to be used to secure the modified situation.

connexion; and the linking up is enjoyed as such in the *fiat* or the feeling of consent. Secondly, while the new object is more intimately related to the conations which make up the self in the special sense of the personality, that is our habitual and highly organised interests, including our bodies, the mass of conations which constitute the subject includes not only these but all directions of activity of the subject. The contemplated object corresponding to all this mass of conations is the whole world as it occupies our minds, including ourselves as its centre. Hence in willing not only have we the consciousness that the act of willing is ours and consequently the event willed is related to us, but we have also the awareness that this event is a part of the world, has in this sense reality. The whole world, in the presence of which the subject exists, is enlarged by the introduction of some new object. Desire wants this sense of accomplished linkage of the new conation to the old. The linkage is still ideal. On the other hand the struggle which is characteristic of desire throws up into prominence the presence of the personality. And hence will most nearly approaches to desire, where the decision is clinched after conflict and suspense. Thirdly, the object willed is not the same in the preliminary stage, in which it is only entertained as it is in the actual *fiat* or decision, when it is realised. For, while there, as in desire, it is only entertained in connexion with the self, here that connexion is established. There is all the difference that there is between a percept and a mere image. The object revealed in the preparatory stages is revealed more fully in the actual volition itself, in the act of decision[1].

It is clear now what the object of volition is, which is thus entertained in the initial stage of willing and realised in the completed will. It is some matter of fact in the most general sense of that phrase, some connexion within the real world, which real world includes our self. This reality of the object is the cognitum which is the object of the *fiat* or *consent* of the will. In other words, it is in willing that objects (which on lower levels may indeed have been present with a 'coefficient of reality,' with a sensory 'tang') become known *as* real, where, once more, 'real' means non-mental reality and does not imply contrast to the illusory. Such a matter of fact is stated in a proposition. This proposition states the so-called end of the volition and states that end

[1] Thus I agree with Miss Wodehouse, *op. cit.* ch. XIII., that there is a difference of 'content' between the Assumption (*Annahme*) and the Judgment (*Urtheil*) and not a mere difference of mental attitude. But I should of course also assert that there is a difference of mental attitude, a difference in the apprehending process, as well.

as attained. Thus in our examples it is the proposition 'you are out of the room,' or 'half my fortune descends after my death to my son,' or 'my arm is lifted,' or 'I take a train to London.' The will *Delenda est Carthago* is directed upon the object *Carthago est deleta* or *deletur*. It is not the proposition 'you are about to go out of the room' (to confine ourselves to the one case), still less the proposition 'you *are* to go,' that is 'you are under constraint to go, out of the room.' The object in question is not necessarily conceived as future. It is the business of the act of will to secure its future existence. What is as a matter of fact future is thus made actual and present. This is the answer to an objection which might be taken, that if the object of the volition is a proposition, that proposition is not true. The answer is complex. In the first place, in judging 'you are out of the room,' I do not judge 'you are *already* out of the room' before I will it. Secondly, in so far as I will it, I do bring the fact which is expressed in the proposition into existence. Unless the action is deferred or obstacles occur, the object begins to be true in the act of willing it, that is it begins to be actual instead of being an idea. Lastly, while every judgment begins thus to be true in so far as it is willed, and willing ensures the reality of what it judges, we are not concerned to maintain that every judgment, because it is an act of speculative will, is true in the sense of being really true. We are only concerned with the fact that it is believed. The distinction of what is valid from what is objective has from the first been declared to be outside the scope of our inquiry.

In fact the logical proposition, unless it contains within its import a reference to time, does not itself refer to distinctions of time[1]. When I say 'this grass is green' I do not mean 'it is green at this moment, though it may not have been green at the last moment.' Propositions may of course refer to the future: 'it is going to rain'; or to the past: 'it has rained during the last hour.' But it is clear also that there may be a will directed to the future as such, as in the case of the legacy or any resolve—'half my fortune comes to my son, when I am dead.' I may also have a will directed upon the past, in the same sense as I may have desire for the past. But from the nature of the case, since the past is physically not to be restored, the will like the desire is in this case theoretical.

Thus in all practical volition the *cognitum* is a proposition, just as in all desiring the object is an idea of expectation or memory. In all

[1] This is not the same thing as saying that it is timeless or contains no reference to time.

theoretical judgment the act of judging is an act of theoretical willing. The *fiat* of the practical will is now the *fiat* of the speculative will, which is belief. The only difference between the two kinds of willing is that in the practical volition the conations issue in practical physical movements; in the acts of judgment they only lead to fresh mental actions or issue in speech. It must not, of course, be supposed that the act of will is in the same respect speculative and practical at once. When I will you to be out of the room, the proposition 'you are out of the room' is practically willed, not speculatively judged. After you have been put out I can make the judgment 'you are out of the room,' but it is now a theoretical will directed on the same object as before but with a different interest, and leading not to practical modification but to speculative advance.

The above account has been dealing with judgment as being the fundamental act of thought. A fuller treatment of the identity of willing and thinking would need to treat on the same lines the precise character of the other forms of thought:—conceiving, with its universals for objects, which are an element in judging; and inference, which is a judgment that includes the grounds of a proposition, where the grounds correspond to the means of a volition. These matters, and in particular the nature of conception, raise questions of difficulty. Other matters barely hinted at above are the nature of belief; and the important question of the relation of judgments proper to assumptions (*Annahmen*). But also there has been omitted the whole development of the object of the speculative will. What has been discussed above is singular propositions. There remains the psychology of judging universal propositions. Above all there remains the psychology of how science or true belief comes to be, if not created, yet extended and made possible by collective or social willing, which perhaps deserves to be made into a fresh level of mental development.

But the principal object of the paper has been not completeness, but to indicate how the series of conative acts are related to their non-mental *cognita*, and how they assume a speculative as well as a practical form. Tentative even as a sketch, it is still more so in some of its detail. But it is still more deficient in its failure to deal with the stages of feeling and in barely indicating the various stages of external movement.

THE MEASUREMENT OF MENTAL ABILITY OF 'BACKWARD' CHILDREN.

By A. R. ABELSON.

From the Psychological Laboratory of University College, London University[1].

I. Introduction.

1. *Problems to be solved.*
2. *Theoretical* v. *practical aims in research.*

II. Method of procedure.

1. *General character of the investigation.*
   The type of children chosen for the investigation; why they were chosen; schools from which they were taken; the conditions under which the research was carried out.
2. *Method of testing.*
   The tests that have been employed; tapping; crossing out rings; crossing out sets of dots; immediate memory for sentences; immediate memory for names of objects; immediate memory for numbers; immediate memory for commissions; auditory and tactual acuity; visual acuity; discrimination of length; discrimination of weight; interpretation of pictures; geometrical figures; general conduct of tests.
3. *Introspective examination during the tests.*
4. *Teachers' estimates.*
   Imputed 'practical intelligence'; 'scholastic ability.'
5. *Mathematical treatment of the results.*
   Correlational coefficients; reliability coefficients.

III. Results obtained.

1. *The degree of ability shown.*
   Some unexpectedly good performances; improvement with practice.

[1] I must take this opportunity of according my heartiest thanks to Prof. Spearman, under whose direction this research has been carried out; it is to his help that I owe in a great measure the results of this investigation.

2. *Reliability of coefficients obtained.*
   Present neglect of this point by observers; great difference in the ease or difficulty of obtaining reliability; table of reliabilities.
3. *Intercorrelation between the tests.*
   Tables of correlation between single tests; considerable correlation for the girls; smaller and more fluctuating one for the boys; remarkable tendency to equality of the intercorrelations; bearing on the theory of a 'common factor'; global correlations; nature of the 'general common factor'; quantitative estimate of 'general ability.'
4. *Correlations of tests with imputed 'practical intelligence'; discrepancy of opinion as to the value of teachers' estimate; table of correlations; improvement of correlations with practice; 'practical intelligence' as a criterion of 'general ability.'*
5. *Correlations with teachers' estimates of 'scholastic ability'; table showing these correlations; 'scholastic ability' by no means a criterion of 'general ability'; general impressions* v. *experimental tests.*
6. *Children's age.*
   The course of development in the case of 'backward' children; the significance of Binet's scale of tests.

IV. General conclusions.

Appendices I and II.

I. INTRODUCTION.

1. OF late years increasing attention has been given to the important study of mental deficiency and much work is being devoted to investigating this condition from various aspects. The value of this study is being more and more realised. There is a widespread and growing dissatisfaction with the current methods of diagnosing mental deficiency. Even the responsible authorities freely admit the present ignorance on most vital points. It is quite evident that many problems of fundamental importance remain to be solved.

One of the most difficult of these is to decide what is the real nature of mental deficiency. Is it a condition in which all mental functions are similarly affected, or is the defect only partial? And if the latter be the case, are the affected functions different in each individual?

Is corroboration given to the popular conception of a 'general ability,' or does the evidence indicate rather that every mental power is independent? Up to the present there has been disagreement on this point. Many of the earlier investigators assumed, although they had quite inadequate empirical evidence, the existence of a 'general ability' common to all mental processes. Others again have dogmatically assumed just the contrary and with no better evidence. More investigation will be needed before we can decide definitely on this question.

If corroboration of a 'general ability' is forthcoming, it will be necessary for us to determine what is the nature of this central function. Is it a factor always present in constant amount in any given test? Or does its extent depend not only on the test but also on the type of individual under investigation?

Furthermore, there is the great question of differences between the sexes. Does the intelligence of the boy obey the same laws as that of the girl and if not, how do they differ from one another?

Besides these theoretical questions, there are others of a more directly practical nature. Thus, it is important to determine what kind of examination is necessary for the recognition of mental defects in children. According to the prevailing practice, an examination by the medical officer, usually lasting under five minutes, is deemed sufficient for deciding whether a child is mentally defective or normal.

Out of this arise many questions of great importance. First, how far are the earlier investigators justified in taking success in the schoolwork as a measure of 'general ability'? In this connexion we shall have to consider how far the teacher's estimate of 'practical intelligence' is serviceable as a criterion of 'general ability.'

Next, as regards the significance of the data obtained, is the usual method of judging by general impressions a satisfactory one? If not, how can we best improve upon this method?

Moreover it is of essential importance that every performance should furnish information of a reliable character; otherwise it can only suggest misleading inferences. We shall have to decide what methods are most suitable for judging the reliability of the results.

The 'age scale of intelligence[1]' drawn up by Binet and Simon is of great theoretical interest as far as child development is concerned. There is little doubt that such a scale will be very useful for diagnosis in quite young children. But it is much more difficult to say how far

[1] *Année psychol.* 14e année, 1908, 58.

such a scale is of value for the more important class of children, those about whom the decision has to be made as to whether they are fit to return to the school for normal children after careful training in the 'special' school.

It will be interesting, in this connexion, also to obtain some evidence as to the course of development of these 'backward' children. Do they develop in the same way as normal children, and if not how do they differ from them?

2. Recently much has been done towards solving many of these problems. The mental tests drawn up by de Sanctis[1] and more especially those introduced by Binet and Simon[2] have opened up a vast field for research and their work is a distinct and important contribution to the study of mental deficiency.

In one important respect, however, the present investigation differs considerably from most of its predecessors. The above-mentioned writers have aimed at producing sets of tests of the greatest possible efficiency which might serve for practical purposes. They have succeeded in convincing even the more sceptical that such tests have already great practical value. But at the same time, these investigators have not gone deeply enough into the theories underlying the application of their tests. They do not know what these tests measure or signify. There is nothing to show whether their tests are better or worse than any others. They do not show how their tests can be improved, to what dangers they are liable, under what circumstances they are applicable, etc. The tests are isolated from the main body of scientific psychology. They neither derive much light from it, nor do they impart much to it. The present investigation, on the other hand, aims at supplying, as far as possible, this deficiency by elucidating the principles upon which such tests must be essentially based. This work must be interpreted accordingly. The tests I have employed are not meant to serve as models for blind imitation, but as demonstrations of the essential properties, good and evil, of such tests.

II. Method of Procedure.

1. *General character of the investigation.* The investigations described in this paper have been carried out on children only just

[1] De Sanctis, "Types et degrés d'insuffisance mentale," *Année psychol.* 1906, 70.

[2] *Année psychol., op. cit.*

below normality. These children form the well-known 'backward' type,—who, for one reason or another, are unable to cope with the work in the elementary school, but who, nevertheless, make considerable progress in the special school for 'mentally defectives.'

It was thought advantageous to carry out this investigation on 'backward' children rather than on those with marked mental defect, for several reasons. In the first place, it is easy to recognise feeble-mindedness in its marked forms, whereas children who are just on the border-line can only with great difficulty be classed as mentally defective or normal. In the second place, these merely 'backward' children are of all mentally defectives the most interesting, since they are the most amenable to treatment. Many of them may be suffering merely from arrested development of a temporary nature; the mental condition of the others is often, at any rate, amenable to improvement. Furthermore, the examination of these children is valuable from another point of view, for they represent the first downward step from normality and may therefore throw special light on the general course taken by this form of degeneracy.

The investigation was carried out in eight of the London County Council Schools for mentally defectives[1]. In all, 88 girls and 43 boys were examined. Only 10, 11, or 12 children were taken at each school, excepting where both boys and girls were examined. In this case, about 22 (11 boys and 11 girls) were chosen. This was as many as could be obtained sufficiently conforming to the type chosen for investigation. No child was included who had any form of marked physical defect, such as severe deafness, very defective eyesight, paralysis, etc. The children chosen were the least abnormal ones in the school. The labour of examining over a hundred children individually, each upon 18 (every test was attempted at least twice) and often on more occasions, was, of course, very considerable. Consequently the research took almost three years to complete.

All the tests were carried out under the best conditions possible. In each school, a special room was set aside for my use and nobody was allowed to enter or leave it during the performance of a test. One

[1] I must herewith acknowledge my thanks to the London County Council authorities for granting me facilities for carrying out this research in their schools. I must also say how greatly I appreciate the kindness shown by the headmistresses and teachers in these schools. They did all they could to help me in every way, often putting themselves to great inconvenience. They took a keen interest in the test and at times offered some valuable suggestions.

child at a time was taken and he was only given one test at any one sitting. In each case, I tried to make certain that the child was not suffering from any slight temporary derangement of health, such as headache or toothache. The first thing done was to make the child feel perfectly at ease. He was encouraged to do his best, and care was taken to give him, when he had finished, every assurance that he had done capitally. The children, in every case, entered into the spirit of the thing with eagerness; many of them looked upon the tests as a kind of game and did all in their power to outdo their school-fellows. Like most mentally defective children they were very sensitive to praise and they looked on my approbation as the highest reward. Only by such precautions as these can the children be got to do their very best at these tests.

2. *Method of testing.* In the course of this investigation I have attempted the measurement of as many abilities as could be conveniently arranged. At the start fourteen tests were tried and of these nine were ultimately selected as the most promising.

Tapping. The 'Tapping' test was introduced to measure simple motor action. A square with sides about 3 inches in length was marked out on a sheet of paper. The child was given a pointed instrument and was told to tap, as fast as possible during ten seconds, anywhere inside the square. The child was stimulated to tap as fast as possible. After the child had finished, the pricks in the paper made by the instrument were counted.

Three performances were allowed at each sitting. A first trial was allowed for preliminary practice, while the second and third attempts were entered in the records.

Crossing out rings. This was used for measuring quickness of sensori-motor coordination. It was one which had been previously employed by Prof. Spearman and Dr Hart. It consisted of an irregularly disposed line of small rings, as below:

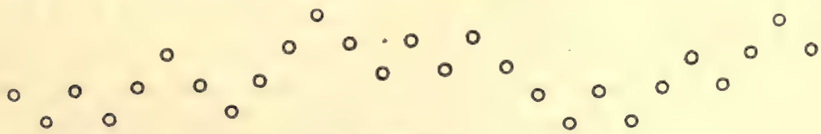


FIG. 1.

The child was given a pencil and told to cross the rings as quickly as possible. The time taken to cross one line of rings was recorded. Three attempts were allowed to each child.

The main difficulty of this test (and of many others) is that it involves two factors, speed and accuracy. It has been found best to keep the latter uniformly just at the breaking point. If the child is crossing with perfect accuracy, he is urged to go faster. If he frequently misses touching the ring, he is warned. The first attempt is devoted to attuning him in this way. The second and third furnished the record for the investigation.

Crossing out sets of dots. The next test was chosen for measuring one form of quickness of perception. The child was shown a paper on which were arranged five lines of dots in sets of three, four, and five, in any order (Fig. 2[1]). He was then asked to cross out all the 'fours' as quickly as possible, and in each instance the time taken to complete the five lines was recorded. Here again three attempts were allowed, and were utilised as in the former test.

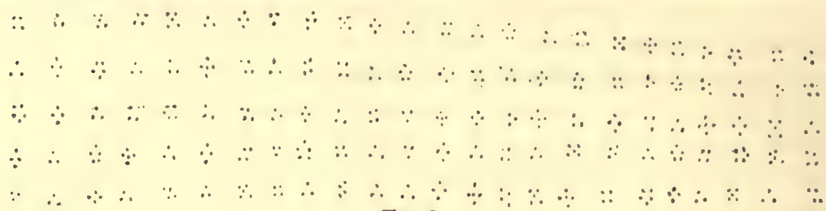


FIG. 2.

Memory for sentences. A series of sentences was drawn up and the child was asked to repeat each sentence after the experimenter. The first sentence was a very short one and each succeeding sentence was a little longer than the one before it. The following seven sentences form an example of this test.

1. The boy went to school.
2. All little children should love one another.
3. Jack showed me a watch which his uncle gave him last week.
4. In my room, there is a chair, a bed, a table and a cupboard.
5. Once upon a time there was a little girl called Mary who lived with her mother in the forest.
6. In a small town, far away, there once lived a kind lady who was very good to the poor people who lived near her.
7. The little boy came up to his mother and said: "I am very sorry that I did not tell the truth. I hope you will forgive me."

[1] The figure is a half-size reproduction of that actually used.

Memory for names. This also involved immediate memory. The experimenter first said the names of two objects and the child repeated them. Then three other names were given and reproduced. These served as a preliminary trial and were not reckoned in the actual record.

Then the names of four objects were given and the child had to repeat them at once; this was done four times, different objects being named in each case. Then five objects were named and the child had to repeat them to the best of his ability; this was done six times. Then six objects were named and repeated; this was done four times. The rate was about one word a second. The names of objects chosen were always monosyllabic, *e.g.* door, ink, tea, face, etc. In each case, the number of names remembered was recorded.

Memory for numbers. This test had to be discarded, as it presented certain difficulties, of which more will be said later. It was only attempted once, but its correlation with intelligence seemed, if anything, to be negative.

Memory for commissions. Below, I give an example of this test. The articles mentioned were arranged in random order upon the table. There were also a mantelshelf, a window-ledge, an armchair, some chairs and a water-tap in the room. Each set of commissions was given out twice, in case the child should have missed part of the task through disturbance of any kind. It should be noticed that here, as in the other tests, each task is a little more involved than the one preceding it.

1. Take up the matchbox and put it on the armchair.
2. Take the newspaper, put it on the cupboard and then put the india-rubber on the mantelshelf.
3. Give me the spoon, the brush, the ball and the watch.
4. Take up the matchbox, the book, the pen-knife and the scissors and put them on the window-ledge.
5. First tell me how many marbles there are inside the cup (one or two), then fill the glass with water, then put the ball on the cupboard and then give me the cup and saucer.
6. Put the saucer over the cup, then open the match-box and tell me how many matches there are inside it (one or two), then come and sit down again, then get up and open the book, then empty the glass of water and then give me the scissors.

7. Put the book, the saucer and the pen-knife upon the gas-stove, then put the brush next to the spoon, then go and open the door, then sit down on the armchair and then get up and put the matches which are on the table inside the matchbox.

Auditory acuity and tactual acuity. These had to be discarded after the first attempt, owing to the fact that they were too lengthy and difficult to arrange under ordinary conditions. For in the first place, the schools chosen were usually situated in districts where there was much noise from road traffic, and these disturbances made it impossible to proceed with the auditory acuity test. And in the second place, I found the responses to the applications of the aesthesiometer very unsatisfactory. The children seemed not to know when they felt one point or two but merely to give wild guesses.

Visual acuity. On the other hand, the visual acuity test gave every promise of being a very useful one, but owing to the great length of time necessary to carry it out, it had to be given up. A round black disc about three inches diameter was used for this test. On this black background a white circle was marked out $2\frac{1}{2}$ inches in diameter and about $\frac{1}{16}$ of an inch thick. There was a break in this circle about $\frac{1}{16}$ of an inch long. The disc was hung on a wall in a room or corridor about the child's eye level and could be rotated at will. The room was artificially lighted, all daylight being carefully excluded. Thus a uniform illumination was maintained throughout the whole experiment.

The child was now given a disc exactly similar to that described above. He was then placed a short distance from the one hanging on the wall and told to hold up his disc showing the break in the same position as it appeared to be on the one which was hanging. If the judgment was correct, the child was placed a little farther away (about 50 cm.), the disc was rotated so as to place the break in another position and he was again asked to hold up his disc showing where the break was. This was done until the child was placed at that distance when he just began to fail to see where the break was situated. The distance was now measured between the child and the disc. The child was then taken a long way out and by the same process was brought nearer and nearer until he just began to indicate correctly the position of the break in the white circle. The distance from the disc was then measured. Each of these processes was gone through five times with each child, and the average distance and deviation were calculated. This will be better understood if an example of such a test is herewith given.

Going away from disc	515 cm.	
Going towards disc		470 cm.
Going away from disc	490 cm.	
Going towards disc		540 cm.
Going away from disc	540 cm.	
Going towards disc		430 cm.
Going away from disc	535 cm.	
Going towards disc		400 cm.
Going away from disc	460 cm.	
Going towards disc		520 cm.

Average distance = 490 cm.

Deviations from the average 25, 20, 0, 50, 50, 60, 45, 90, 30, 30.

Average deviation = 40 cm.

The test was only attempted at two schools. Judging by the results obtained, the visual acuity as shown by the average distance gave little or no correlation with the imputed intelligence, whereas the average deviation gave a correlation with the intelligence, the more capable children showing a tendency to small average deviations. This result, I may add, fully corroborates the conclusions of van Biervliet[1].

Discrimination of length. This test gave very little trouble. A shortened method of right and wrong cases was employed. Two vertically upright lines were set up about an inch away from each other. One was a little longer than the other, and the child had to say which was the longer. The lines were arranged in the following manner[2], the figures expressing the length of the lines in centimetres.

TABLE I.

5, 7	6, 5·6	6, 6·3	6, 5·8	6, 6·1	6, 5·95
5, 6	6, 6·4	6·3, 6	6, 6·2	6·1, 6	6, 6·05
6, 5	5·6, 6	6, 5·7	5·8, 6	6, 5·9	5·95, 6
5·5, 5	6, 6·4	5·7, 6	6, 6·2	5·9, 6	6, 6·05
5·5, 6	6·4, 6	6·3, 6	6·2, 6	6·1, 6	6·05, 6
	6, 5·6	6, 6·3	6, 5·8	6, 6·1	6, 5·95
	5·6, 6	5·7, 6	5·8, 6	5·9, 6	5·95, 6
	6, 5·6	6, 5·7	6, 5·8	6, 5·9	6, 5·95
	6·4, 6	6, 6·3	6·2, 6	6, 6·1	6·05, 6
	5·6, 6	5·7, 6	5·8, 6	5·9, 6	5·95, 6

The whole set was gone through twice. Thus the subject had to make altogether 110 judgments. It will be noticed that the first pairs were very easy to discriminate, and that thereafter the difficulty gradually increased.

[1] "La mesure de l'intelligence," *Journ. de Psychol.* 1904, 1re Année, 225.

[2] The lines were carefully drawn under a magnifying glass. I have to thank Mr Boorman of the Shoreditch Technical Institute for arranging for the drawing of the lines for this test.

Discrimination of weight. This test was arranged after Binet's method. Six equal-sized cartridges of different weight were placed in random order before the child, and he was asked to arrange them in order of their weight. These defective children proved to have no idea of how to arrange the weights in order, and the result was hopeless confusion. The test was, therefore, abandoned.

Interpretation of pictures. Each child was shown eight pictures —one at a time—and was asked what they meant. It was found advisable to ask one or two questions so as to direct the child's attention to the more important details in the picture. But great care was always taken that these questions did not contain any form of suggestion and that the same questions were put to all the children. I had previously determined how the marking should be arranged, *e.g.* what kind of an interpretation was necessary for obtaining full marks, and how the marks were to be deducted in the case of faulty details in the child's interpretation of the picture. The child's recognition of the different objects in the picture was not taken into account as all the children seemed able to do this quite well. I took great care not to be biassed by any information the child gave which appeared to be the result of instruction. The criterion was not knowledge, but the ability to take in the meaning of the *whole* picture after careful inspection of it. For instance, on one of the pictures was a pet monkey. In order that those children who had seen a monkey should not be placed at a greater advantage than those who had not, I had to make sure beforehand whether they all had seen a monkey and if not, I showed them the picture of one. But when a child informed me that 'the monkey is frightened because the dog is coming up to him and barking at him,' and, moreover, gave intelligent reasons as to how he could tell the monkey was frightened, this correct interpretation showed that the child fully understood what was depicted on the picture.

The following were some of the pictures used for this test:

Reynolds' 'Portrait of Miss Bowles.' (Wallace collection.)

Mulready's 'Giving a bite.' (S. Kensington Museum.)

Yeames' 'And when did you last see your father?' (Walker Art Gallery, Liverpool.)

Faed's 'When the children are asleep.' (Walker Art Gallery, Liverpool.)

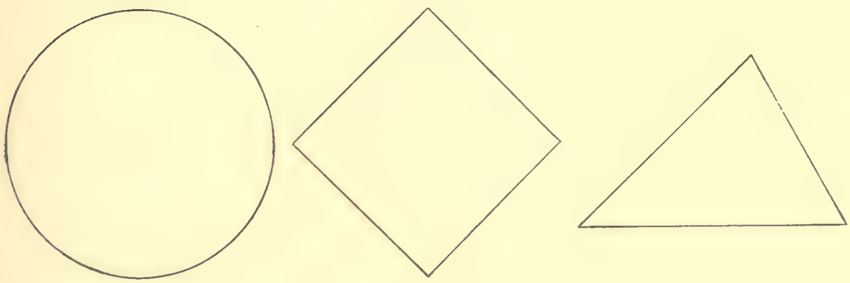
Mulready's 'Fair-time.' (Tate collection.)

Webster's 'Dame School.' (Tate collection.)

Steen's 'Santa Claus.' (Amsterdam.)

Millais' 'Boyhood of Raleigh.' (Tate collection.)
Meissonier's 'La Rixe.' (Private collection.)
Dauvant's 'Maîtrise d'enfants.' (Luxembourg.)
Gemmel Hutchison's 'The Vigil.' (Tate collection.)
Tassaert's 'Une famille malheureuse.' (Louvre.)
Collins' 'Sunday Morning.' (Tate collection.)
Netcher's 'Maternal Instruction.' (Wallace collection.)
Roos' 'Homelessons.'
Harcourt's 'Goodbye.' (Representing 3rd battalion Grenadier Guards leaving Waterloo Station, October 21st, 1899[1].)

Geometrical figures. The following figures were used for this test[2]:



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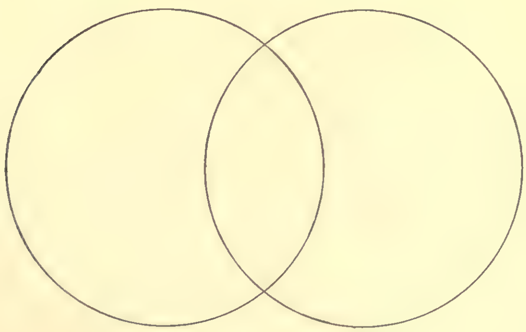
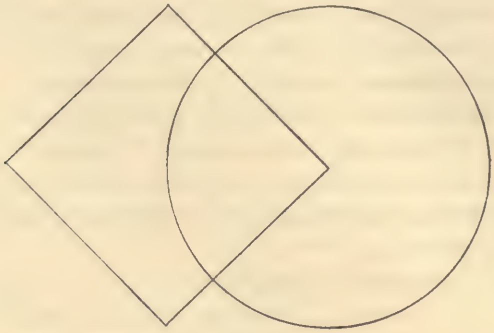


II

[1] Reproductions of most of these pictures on postcards can be obtained from the Muchmore Art Co., 93, Gt Russell Street, London, W.C.

[2] The circles were drawn in black, the squares in blue and the triangles in red.



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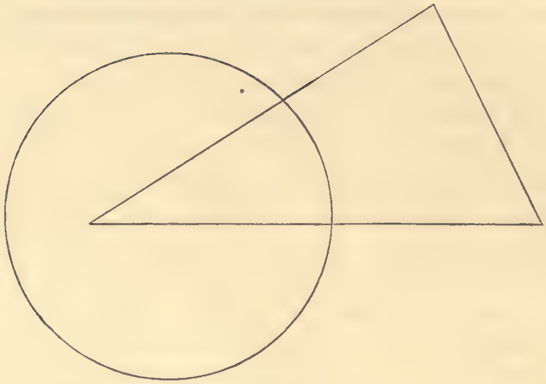

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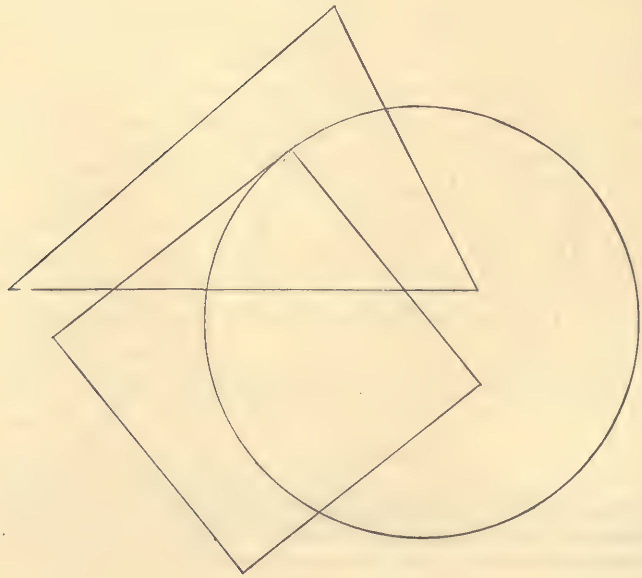

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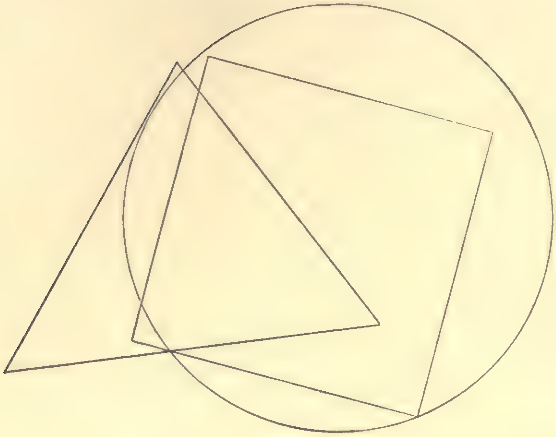

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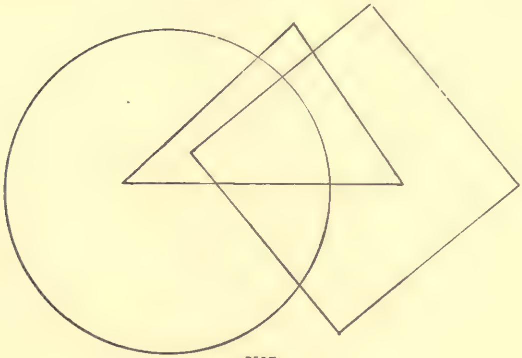

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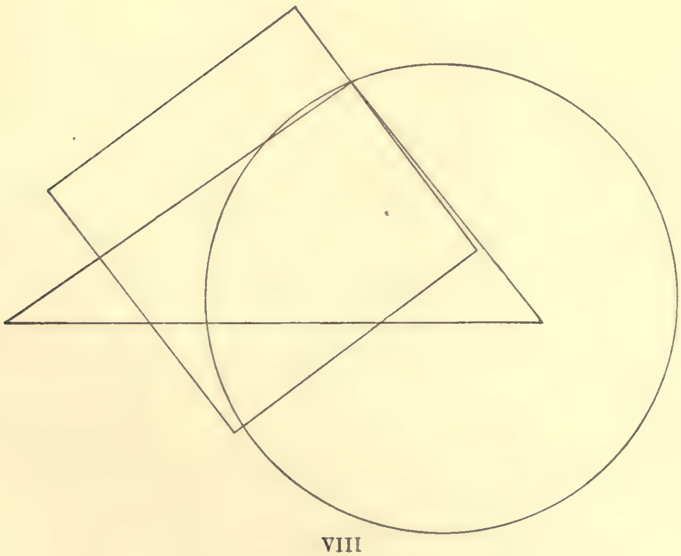

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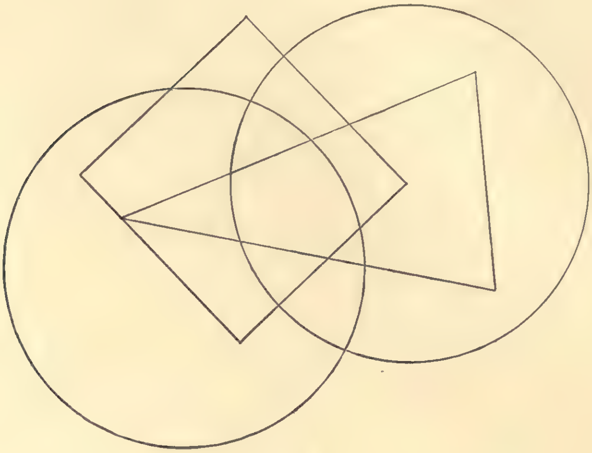

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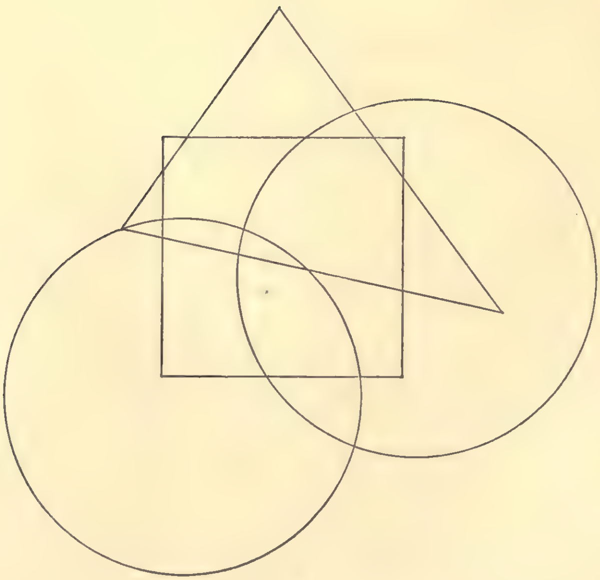

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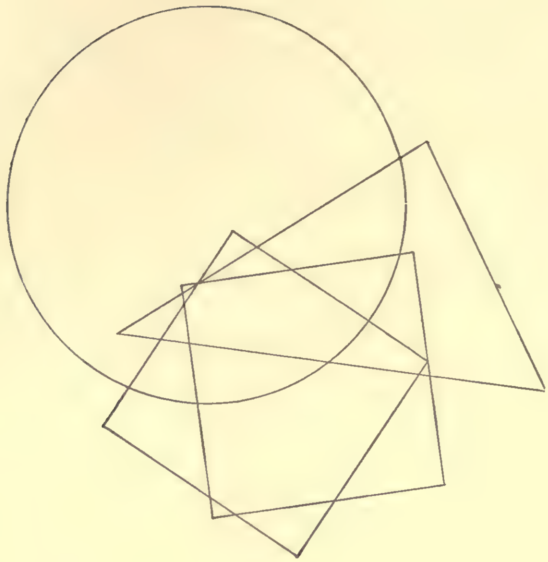

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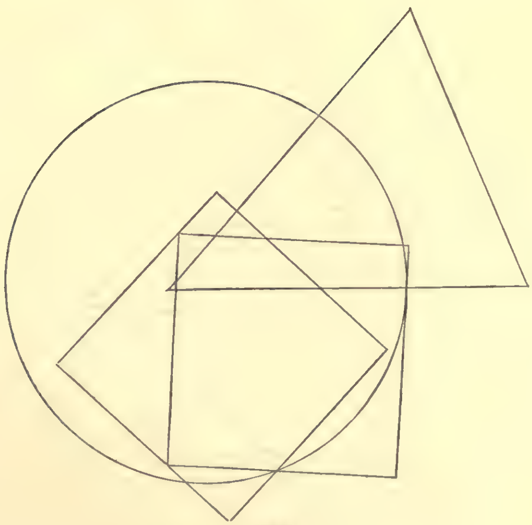

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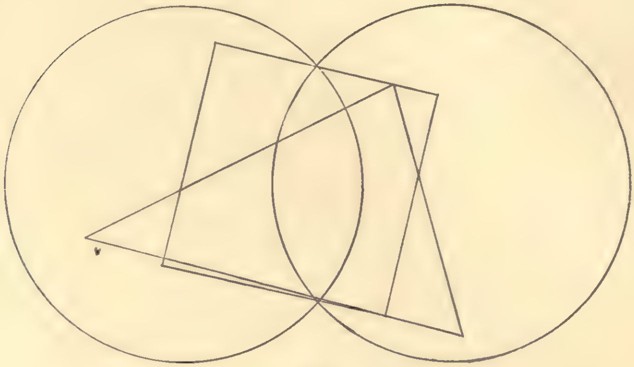

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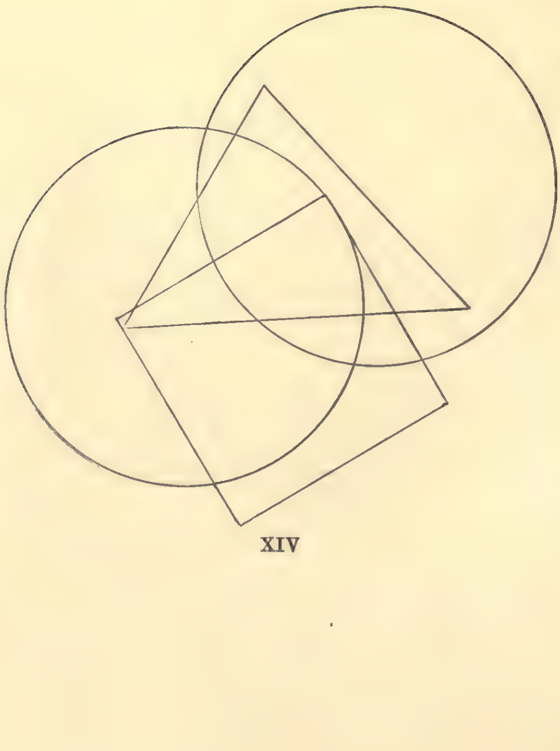

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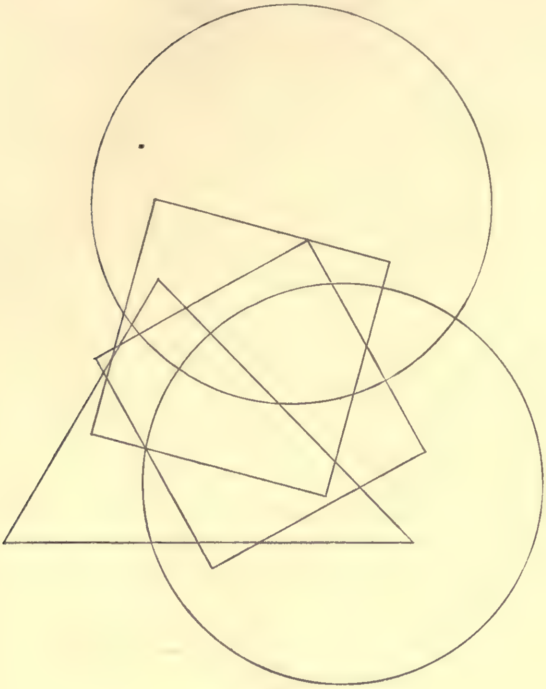

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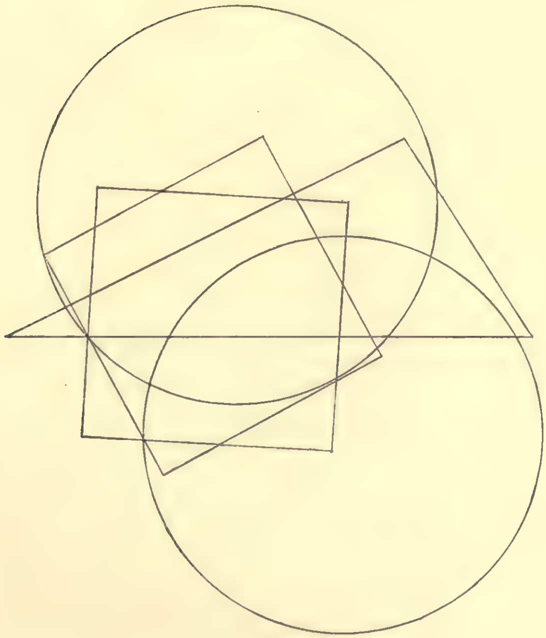

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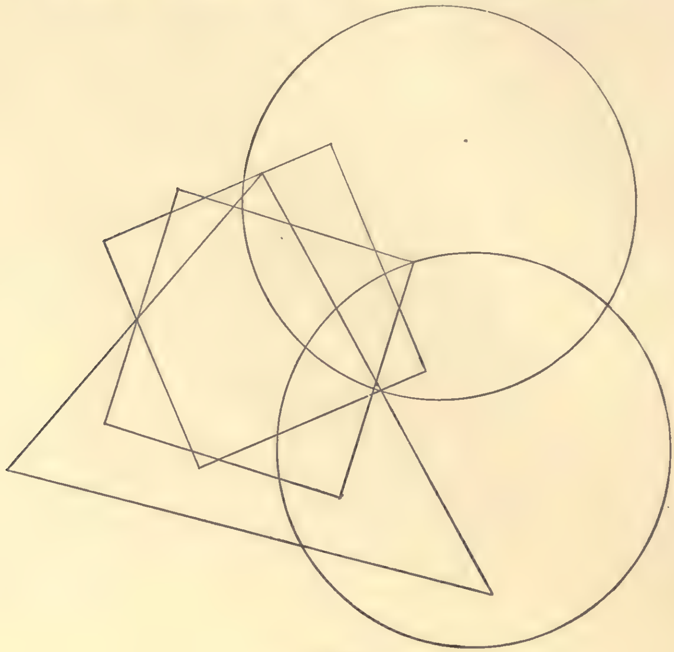

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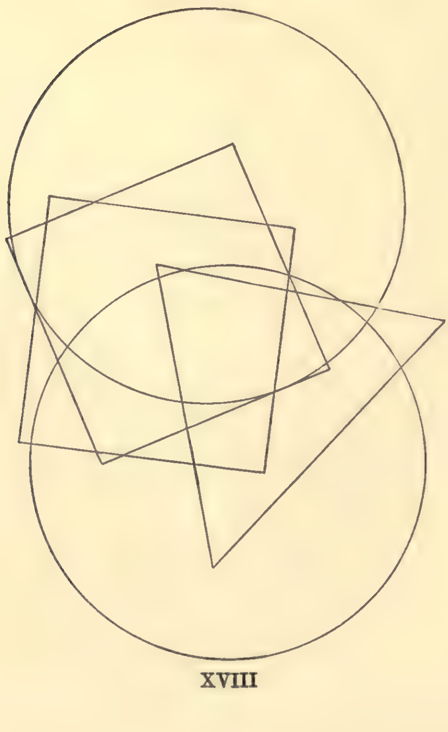

XVIII

In each case the child was given a soft clean brush, and asked to do the following:

1. Point in the square.
   „ „ circle.
   „ „ triangle.

(Repeat these out of order to see whether the child is quite at home with these figures[1].)

2. Point inside both circles.
3. Point in the square and the circle.
4. Point in the triangle but not in the circle.
5. Point in the square and triangle but not in the circle.
6. Point in the circle and triangle but not in the square.
7. Point in square and circle but not in the triangle.
8. Point in the triangle and square but not in the circle.
9. Point in the two circles but not in the triangle and square.
10. Point in the triangle and square but not in the two circles.
11. Point in the two squares but not in the circle and triangle.
12. Point in the circle and triangle but not in the two squares.
13. Point in the two circles and the square but not in the triangle.
14. Point in the two circles and the triangle but not in the square.
15. Point in the two circles and the two squares but not in the triangle.
16. Point in the two circles and the triangle but not in the two squares.
17. Point in the two squares and triangle but not in the two circles.
18. Point in the two circles and triangle but only in one square.

The figures were covered over while the order was being given. Each order was said twice, care being taken that the child fully grasped what he had to do. The figure was then uncovered and the stopwatch was immediately set going. As soon as the child made his judgment, the watch was stopped and the time recorded. If the judgment turned out to be a wrong one, the figure was covered and

[1] In addition, during the week preceding the giving of this test, the child was practised in recognising these three geometric figures so that by the time the test was tried the child could name them without the slightest hesitation.

the order was again given. The time taken in this case was added to the time taken in the first instance.

Nos. 1, 2, 3 and 4 were used for explanation and the times taken to execute these were not recorded.

I found afterwards that although most of the children were able to undertake No. 18, in many cases it caused a certain amount of confusion. So this was afterwards abandoned[1].

Of the 14 tests thus originally attempted, the following were eventually chosen as being most suitable for the conditions under which the work was done: 'Tapping,' 'Crossing out rings,' 'Crossing out sets of dots,' 'Memory for sentences,' 'Memory for names of objects,' 'Memory for commissions,' 'Discrimination of length,' 'Interpretation of pictures,' and 'Geometric figure test.'

General conduct of the tests. Throughout I took special care to see that every test was straightforward and that no confusing element entered into it. It is essential for the experimenter to make sure that the child is perfectly clear as to what is required of him. I made certain that no test was too easy or too difficult for these children. The best results were obtained when the tasks were such as the child could carry out with fair success. It was found advisable to begin the test with something very simple indeed, so as to give the child every confidence. The difficulty was then increased little by little, but it never reached a stage when the task was entirely beyond his capabilities.

3. *Introspective examination during the tests.* Some introspective work was attempted with a view to getting as much evidence as possible as to the nature of the processes involved in each of the tests. For this purpose, the tests were applied to some adults and normal children as well as to the children under investigation. Although the mental processes involved in any particular test may not be the same for all grades of intellect, any knowledge we can obtain from normal subjects may possibly throw some light on the mental activity of these defective children.

Unfortunately, I was unable to get any introspections worth recording from defective children and very little was obtained from the normal children that were examined. Nevertheless, by considering all

[1] This test was introduced by Dr Hart and Prof. Spearman, whom I thank for kindly lending it.

the evidence furnished by normal subjects old and young, some interesting details were furnished[1].

Further light was obtained by carefully observing and recording the external demeanour of the children, especially the defectives, during the conduct of the test.

4. *Teachers' estimates.* The head teacher at each school was asked to draw up a list of the children in order of their 'practical intelligence.' In each case, I carefully explained how the judgment was to be made: scholastic attainment and progress were for the moment to be entirely disregarded. The criterion was to be common sense for everyday worldly matters. The teacher was to ask herself which of these children she would soonest trust on an errand requiring the sharpest intellect and to take this into consideration when drawing up her list. For brevity this estimate will hereafter be referred to as imputed 'practical intelligence.'

At one school I was able to obtain two estimates of the same children, one from the teacher and one from the head-mistress. But the latter admitted that her estimates were largely based on the reports of the former, so that the two estimates could hardly be taken as independent. Accordingly the correlation between the two was as high as 0·96. In all the other schools I only obtained one estimate as the head-mistress was usually in charge of the class from which these children were drawn.

The teachers were also asked to draw up a list of the children under investigation in the order of their scholastic attainments. Reading and arithmetic were chosen for the purpose and two gradings were drawn up for each set of children; one in the order of their reading ability, the other in the order of their arithmetical ability. In all probability, if the children were given an examination in these subjects, the results would closely agree with those of the lists given by the teachers.

5. *Mathematical treatment of the results.* Having thus obtained records of the child's powers at a number of different performances, we must consider how to regard the results. The usual method is to pick for each performance a label out of a certain stock of conceptions used for that purpose, such as memory, observation, discrimination, motor coordination, etc. When, for instance, a person has successfully

[1] These and some further introspections I hope to discuss in a future paper. I must take this opportunity of thanking Dr Aveling, Mr Kaye and the others who kindly acted as subjects.

discriminated between the lengths of some pairs of vertical parallel straight lines, he is straightway declared to have good sensory discrimination. But this mode of expression tacitly introduces a very large assumption, for it implies the child to be good at all or most other kinds of sensory discrimination; and for this there is absolutely no evidence. On the other hand, if we abstain from making any such illicit deduction, it is hard to see how we are going to make any deduction at all. The fact of doing anything well has really demonstrated at most a power for that very particular performance only, and as in ordinary life no two performances resemble one another quite exactly, no isolated performance can give us precise information for subsequent use.

Evidently, then, the first step towards profitable knowledge must be to find out how events are interdependent so that the occurrence of one event enables us to form some conclusion as to the likelihood of another. And the only effective method at present known of measuring such interdependence appears to be that of correlational coefficients.

In the present work, therefore, I have made an extended use of these coefficients. There are several to choose from, with various special advantages and disadvantages. But, considering the many inevitable sources of inaccuracy attending such an investigation as the present one, there seems no appreciable advantage in choosing any coefficient involving long calculations. The present results are only able, and are only expected, to furnish rough approximations. I have, therefore, used Prof. Spearman's simple Footrule Method. Here the coefficient is represented by R and is obtained by using the formula

$$R = 1 - \frac{6D}{n^2 - 1}$$

where D stands for the sum of the numerical differences in station and n stands for the number of children.

Then R was changed, so as to give an approximate equivalent, to the Bravais-Pearson r, by using the formula[1]

$$r = \sin\left(\frac{\pi}{2} R\right).$$

A further explanation of the methods employed can be obtained by

[1] A modification of this formula and of the above formula for R has recently been suggested by Pearson, based upon assuming the Gaussian distribution. But the difference is too slight to affect any of the results in this paper, so that the simpler original formula has still been used. See Spearman, this *Journal*, 1910, III. 287.

referring to the works written on this subject[1]. It should be noted, however, that a correlation of 1·00 signifies complete correlation; a correlation of zero signifies that there is nothing in common between two series. A value above zero shows positive correlation, and one below it negative correlation.

As regards the probable error of this coefficient, the formula[2] suggested by Pearson has been used, *i.e.*

$$p.e. = \frac{\cdot 77\,(1 - r^2)}{\sqrt{n-1}}.$$

Also some evidence has been produced, that when correlations are calculated from such small groups as 10 or 11, both the coefficients and the probable error given by the formulae are slightly too small[3]. But the difference would scarcely be appreciable on the scale of accuracy of the present experiments.

Each correlation had first to be calculated separately for each school and then an average was taken for all the schools. This was done for each sex separately. Recent investigations go to show that such averages derived from a small number of cases (here 10 or 11) give results slightly smaller than the true one; but the difference is not great enough to disturb any of the present work appreciably[3].

Before proceeding to enumerate the results, it is necessary to note that each test was done twice. The result of each test was divided into two parts. The first half of the first performance was added to the last half of the second performance and the result thus obtained was correlated with that obtained by combining the second half of the first performance with the first half of the last performance[4]. Evidently this correlation gives a measure as to how far the results furnish a truly general measurement of each child's power. Hence this correlation has been aptly termed the 'reliability coefficient' of the test (Krueger and Spearman). It has been mathematically demonstrated that a low reliability coefficient greatly perverts correlations with other tests. Formulae may be used to compensate for this disturbance but it is far better to prevent its occurrence. Hence in the present investigation,

[1] For the study of these methods, reference must be made to Prof. Spearman's articles on the subject—"General intelligence objectively determined and measured," *Amer. Journ. of Psychol.*, 1904, xv. 201, and also "'Footrule' for measuring correlation," this *Journal*, 1906, II. 89. Other works will be referred to later on in this thesis.

[2] *Drapers' Company Research Memoirs*, IV. 19.

[3] See "Probable error of a 'correlation' coefficient," by 'Student,' *Biometrika*, 1909, VI. 302.

[4] For an explanation of this method see this *Journal*, 1910, III. 274–5.

when the reliability coefficient fell below ·70 or thereabouts, the test was repeated and the results of the first and third performances were thrown together and correlated with the results of the second. If, even when the test was attempted three times the reliability was still below about ·70, the test was again tried and the first and fourth results were thrown together and correlated with the result obtained by throwing together the second and third performances.

III. Results obtained[1].

1. *The degree of ability shown.* The ability shown by the children in carrying out these tests was unexpectedly high. Those teachers who were present during the performance of the tests were quite astonished at what the children were able to accomplish. For example, one boy and two girls did the whole of the 'commission' test (at the second attempt) without a mistake, and many others did it almost as well. Some of them executed the 'geometrical figure' tasks with remarkable rapidity,—one boy took 18 seconds the first time he attempted the test and at the second performance he did what was required immediately the orders were given and the figure uncovered. One girl took 36 seconds the first time and 18 seconds the second time. Quite a number of them did almost as well. Two or three children went through the 'discrimination of length' test without a mistake. The 'crossing out sets of dots' test was also done remarkably well. The pictures, difficult as they may appear to be, were interpreted with a success which was altogether unexpected. Thus the tests have thrown some interesting sidelights on the powers of these children. It is quite evident that we do not know what they are really capable of doing and that by means of these tests we have been tapping mental strata different from those which the school-work even touches upon.

As already indicated, on the test being done a second time, the children did even better than at the first performance. This improvement with practice was a marked feature in all the tests in the case of both boys and girls, except for the 'discrimination of length' test where the improvement did not always take place. It is probable that this was due to the fact that the children soon lost interest in so monotonous a test.

[1] As the introspective observations turned out to have small bearing on the main argument of the investigation, I have refrained from giving a systematic account of the introspective results, but I have quoted them occasionally in connexion with any special topics requiring them.

2. *Reliability coefficients.* Previous investigators have complacently regarded their results as always furnishing reliable general characteristics of the persons tested. But it is conceivable that a test may give quite different results every time it is tried, in which case it would be of very little use. In the present research, precise information on this vital point has been procured by means of the 'reliability coefficients' (page 291).

Viewed from this point it has actually been found that many tests, highly esteemed by their inventor, completely break down. For example, Binet, in his interpretation of pictures, states that three pictures are sufficient to measure the child's ability in this direction. But the present investigation shows that eight pictures had to be interpreted before the reliability could be considered satisfactorily; and as the test had to be attempted twice, as many as 16 pictures were used. Similarly the 'memory for numbers' test showed itself to be a very unreliable one, giving results which fluctuated excessively from time to time and affording no satisfactory indication of the capacity we wish to measure.

Extraordinary differences were found in the ease of obtaining reliability. In the case of many tests, reliable results could be procured in a few minutes; in others, they could scarcely be considered as satisfactory after half an hour. For instance, the 'tapping' test only lasted a minute or so, but it was one of the highest, if not the highest, in reliability. On the other hand, the 'interpretation of pictures' test took over a quarter of an hour and even then was not nearly as reliable.

The following values show the average reliability coefficients for the present tests, and appear quite satisfactory.

	Girls (88 examined)	Boys (43 examined)
Tapping	·92	·91
Crossing out rings	·90	·94
Crossing out sets of dots	·94	·97
Memory for sentences	·78	·79
Memory for names of objects	·74	·81
Memory for commissions	·73	·70
Geometrical figures	·80	·78
Comparison of lines	·84	·76
Interpretation of pictures	·81	·80
	Av. ·83	Av. ·83

In the following cases, owing to low reliability, the test had to be tried again :

Girls:—Memory for sentences, one extra performance at 2 schools
Geometrical figures, ,, ,, ,, 1 school
Discrimination of length, ,, ,, ,, 1 ,,
Commission, ,, ,, ,, 1 ,, and two extra in another.

Boys:—Commission, two extra performances at 1 school and one extra at another.
Geometrical figures, one extra performance at 1 school.
Interpretation of pictures, one extra performance at 1 school.

3. *Intercorrelations between the different tests.* Having devised a test at any rate able to inform us reliably about a child's power of doing that test itself, we want next to know how much it tells us about his other powers. On this point there seems formerly to have prevailed an almost incredible looseness of thinking. As has already been mentioned, investigators used naïvely to assume that just those things went together which had the same name (*e.g.* Memory).

TABLE II. *Girls.*

	Interpretation of pictures	Memory for sentences	Tapping	Memory for commissions	Geometrical figures	Crossing out rings	Discrimination of length	Crossing out dots	Memory for names of objects	Average Intercorrelation between tests	Average Probable Error
Interpretation of pictures		·06 **·49**	·05 **·61**	·07 **·30**	·06 **·42**	·07 **·26**	·07 **·26**	·07 **·25**	·07 **·33**	**·365**	·065
Memory for sentences......	·06 **·49**		·07 **·27**	·07 **·50**	·06 **·43**	·07 **·21**	·07 **·32**	·07 **·26**	·06 **·42**	**·362**	·066
Tapping........................	·05 **·61**	·07 **·27**		·07 **·14**	·07 **·33**	·06 **·42**	·07 **·37**	·07 **·21**	·07 **·30**	**·331**	·066
Memory for commissions	·07 **·30**	·07 **·50**	·07 **·14**		·07 **·31**	·07 **·30**	·07 **·43**	·07 **·31**	·07 **·34**	**·329**	·070
Geometrical figures.........	·06 **·42**	·06 **·43.**	·07 **·33**	·07 **·31**		·07 **·32**	·08 **·21**	·06 **·45**	·08 **·13**	**·325**	·069
Crossing out rings	·07 **·26**	·07 **·21**	·06 **·42**	·07 **·30**	·07 **·32**		·07 **·42**	·06 **·47**	·07 **·17**	**·321**	·067
Discrimination of length	·07 **·26**	·07 **·32**	·07 **·37**	·07 **·43**	·08 **·21**	·07 **·42**		·07 **·22**	·07 **·22**	**·306**	·071
Crossing out sets of dots...	·07 **·25**	·07 **·26**	·07 **·21**	·07 **·31**	·06 **·45**	·06 **·47**	·07 **·22**		·07 **·18**	**·291**	·067
Mem. for names of objects	·07 **·33**	·06 **·42**	·07 **·30**	·07 **·34**	·08 **·13**	·07 **·17**	·07 **·22**	·07 **·18**		**·261**	·070
Average										**·321**	·068

Intercorrelations shown in blacker figures.
Probable errors shown in ordinary figures.

TABLE III. *Boys.*

	Memory for sentences	Geometrical figures	Discrimination of length	Memory for commission	Interpretation of pictures	Crossing out sets of dots	Tapping	Memory for names of objects	Crossing out rings	Average Intercorrelation between the tests	Average Probable error
Memory for sentences		·09 **·39**	·10 **·17**	·10 **·38**	·10 **·36**	·10 **·21**	·10 **·20**	·07 **·66**	·10 **·02**	**·299**	·095
Geometrical figures	·09 **·39**		·10 **·21**	·08 **·56**	·10 **·36**	·10 **·17**	·10 **·07**	·09 **·52**	·10 **·07**	**·294**	·095
Discrimination of length	·10 **·17**	·10 **·21**		·10 **·34**	·10 **·21**	·09 **·46**	·09 **·38**	·10 **·18**	·10 **·31**	**·282**	·097
Memory for commissions	·10 **·38**	·08 **·56**	·10 **·34**		·10 **·22**	·10 **·04**	·10 **·38**	·09 **·38**	·10 **– ·15**	**·269**	·096
Interpretation of pictures	·10 **·36**	·10 **·36**	·10 **·21**	·10 **·22**		·10 **·15**	·10 **·25**	·10 **·30**	·10 **·27**	**·265**	·100
Crossing out sets of dots	·10 **·21**	·10 **·17**	·09 **·46**	·10 **·04**	·10 **·15**		·09 **·32**	·10 **·06**	·05 **·65**	**·257**	·091
Tapping	·10 **·20**	·10 **·07**	·09 **·38**	·10 **·38**	·10 **·25**	·09 **·32**		·10 **– ·08**	·08 **·46**	**·247**	·095
Memory for names of objects	·07 **·66**	·09 **·52**	·10 **·18**	·09 **·38**	·10 **·30**	·10 **·06**	·10 **– ·08**		·10 **– ·22**	**·225**	·094
Crossing out rings	·10 **·02**	·10 **·07**	·10 **·31**	·10 **– ·15**	·10 **·27**	·05 **·65**	·08 **·46**	·10 **– ·22**		**·176**	·091
Average										**·257**	·095

Intercorrelations shown in blacker figures.
Probable errors shown in ordinary figures.

The accompanying tables II and III show the actual correlation observed between each test and every other test. A glance at the tables will show that there exists a quite appreciable intercorrelation between the tests in the case of the girls; the values in the case of the boys show that the intercorrelations are somewhat smaller. It will further be noticed that the tests coming under the same designation, such as sensory, motor, memory, intellectual, etc., only correlate slightly better among themselves than with the tests coming under a different designation. Amongst the girls there appears to be no exception to the rule; the boys, however, show two exceptions. The one is the high correlation between memory for sentences and that for names of objects; these two tests were so extremely similar that the wonder is that the correlation was not higher still. The other exception is the high correlation between 'crossing out rings' and 'crossing out sets of dots.' This is not so easily accountable. True, the fact of both being 'crossing out' tests indicates a specific similarity between the two. But, on the other hand, the introspection of normal adults revealed remarkable differences, the former test appearing to depend essentially on speed of movement and the latter on speed of perception. Perhaps, however, this opposition is less marked in the case of defective children.

On the whole, we must say that the similarity between two performances has to be very marked for it to produce a specially high correlation between them. The degree of similarity indicated by the 'faculties' current in lay psychology, such as memory, judgment, discrimination, etc., is quite insufficient. It is obvious that the cause of the observed correlations must be looked for elsewhere.

A further examination of these correlations shows us that the most striking feature about them is their remarkably small difference of magnitude, especially in the case of the girls. This tendency to equality of the correlations had already begun to be apparent when the experiments had been done in four schools for boys and four for girls. To get more definite evidence on this point, it was decided to devote the remainder of the investigation to girls only, for it was noticed that the boys had such large 'probable errors,' that it was hopeless to attempt to make the research long enough to obtain very definite evidence from their results. The smaller 'probable error' of the girls, however, suggested that with these, only four more schools need be included so as to be able to get trustworthy evidence on this point. The remarkable tendency to equality must be expected to vanish if it were accidental, but should become even more accentuated if genuine[1].

On pooling the eight schools for the girls together, the differences between the intercorrelational values were found to have shrunk still further (as shown in Table II).

And even the differences that remain must clearly be attributed in part to the mere errors of sampling, whose general size is indicated by the 'probable errors.' It was, therefore, desirable to obtain quantitative evidence on this point. The necessary formula was kindly worked out by Prof. Spearman, showing how large apparent differences would be produced merely by the sampling errors in a set of correlations whose true values are quite equal to one another (see Appendix I).

[1] It should be noted that the much more extensive work with the girls than with the boys makes the results about the former much more trustworthy on all other questions in addition to the present one.

In the case of the girls the theoretical mean square deviation to be expected merely from the errors of sampling was found to be ·012. The actually observed deviation was just the same value. Of course, this exact coincidence must be ascribed to chance. But, at any rate, we arrive at the very novel and interesting result, that the observed deviations of value among the correlations are approximately the same as would be expected to arise merely from accidental errors; the true values of the correlations must hence be taken as very nearly equal to one another.

The same method can be applied to the boys, but, of course, with a good deal less expectation of accuracy. Here the theoretical mean square deviation comes to ·025. The actually observed one was ·034. Even this excess of the observed value over that to be expected by mere chance is very small. Moreover it is easily explained by the fact already mentioned elsewhere, that two instances (the two 'verbal memory' and the two 'crossing out' tests) were so similar as to cause high special correlations between them. If we omit these two correlations from the table the excess again practically disappears as in the case of the girls[1].

(We may compare this with the results obtained from normal children. The data most fairly comparable seem to be those of the larger group of children investigated by Burt[2]. There the theoretical mean square deviation is ·028. The actually observed mean square deviation, however, amounts to ·062.)

Further corroboration of our results was obtained by comparing one school with another. This was done in the following manner. A list of intercorrelations in order of magnitude was made for each school. If these differences of magnitude were really significant, the order should be similar in the different schools. To test this, the order for each school was correlated with that for each of the other schools. The average of the 28 correlation coefficients thus obtained was only ·08,—a result so near zero that the difference of size in the correlations would appear wholly due merely to sampling fluctuations.

Now comes the great question as to the bearing of this equality of correlations on one of the most fundamental and disputed problems of individual psychology. Does the equality confirm, refute, or is it

[1] The modifications proposed by Pearson for the formulae involved (see p. 290) would leave all the above values practically unaltered, merely changing the further decimal figures. If the estimation of the probable errors is too low (see p. 291), this can only make the ratio of the observed to the theoretical deviation still smaller.

[2] This *Journal*, 1910, III. 161.

indifferent to the theory advanced,—that in general the correlations between distinctly different performances are due to their depending more or less on one common factor—'general ability' as it may be called? The criterion of the common factor is the relation

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

where A, B, P and Q represent any of the performances; $r(A, P)$ is the correlation between A and P, and similarly for the others[1].

Obviously if all the correlations are equal, then

$$\frac{r(A, P)}{r(B, P)} = \frac{r(A, Q)}{r(B, Q)} = 1,$$

so that the theory of a common factor is corroborated as completely as possible.

Global correlations. Yet another corroboration can be obtained in the following manner. So far we have been considering the correlations between the results of the performances taken singly. But it is interesting to see the effect of pooling them together into what is called the 'amalgamated' or 'global' result. By this means the specific factors in each of the tests partly neutralise one another, so that any factor common to all the processes comes out with great prominence.

TABLE IV.

Name of child	The children's position in rank in the single tests									Total (= cols. 1—9 added together)	Global result (preceding column expressed in ranks)
	1	2	3	4	5	6	7	8	9		
	Tapping	Crossing out o's	Crossing out sets of dots	Memory for sentences	Memory for names	Memory for commissions	Geometrical figures	Lines	Pictures		
L. H.	3	7	6	5	5	8	3	8	1	46	4
L. E.	2	1	5	3	9	3	2	9	3	37	2
N. K.	4	4	3	7	4	4	5	3	4	38	3
E. C.	6½	3	4	10	7	9	4	7	5	55½	7
M. W.	1	10	10	1	6	1	1	1	2	33	1
K. S.	11	5½	1	5	10	5	11	4	8	60½	8
C. W.	8	9	11	2	1	6	7	2	6	52	5½
L. B.	9	5½	7	9	3	2	9	11	11	66½	9
M. M.	6½	8	9	11	11	7	10	6	10	78½	11
F. G.	10	11	8	5	8	10	6	10	7	75	10
E. W.	5	2	2	8	2	11	8	5	9	52	5½

[1] See Burt, this *Journal*, 1909, III. 159.

The following example will serve to show how the 'global result' was obtained. The eleven children are all girls, and the numbers show their order of merit in each of the tests.

(In order that the test which is being correlated should not bias the correlation, it was thought advisable to omit that test from the 'global result.' Thus 'tapping' was correlated with the amalgamation of all the tests except tapping; 'memory for sentences' was correlated with the amalgamation of all the tests except 'memory for sentences,' and so on.)

The following values show the correlations between each test and the 'global result.'

	Girls (88 examined)	Boys (43 examined)
Tapping	·52	·32
Crossing out o's	·47	·34
Crossing out sets of dots	·42	·40
Memory for sentences	·67	·46
" " names	·37	·40
" " commissions	·58	·40
Geometrical figure test	·49	·42
Comparison of lines	·39	·53
Interpretation of pictures	·57	·41
	Av. ·50	Av. ·41

It will be noticed that in the case of the girls the correlations are quite high. In the case of the boys, although they are not quite so high, yet they are very appreciable.

If it is true, as we have seen reason to believe, that the differences in the intercorrelations between the tests are merely chance ones, the effect of pooling should be predictable by calculation.

The equation[1] for this purpose is

$$r_{pq} = \sqrt{\frac{pr_1}{1+(p-1)\,r_1}}\sqrt{\frac{qr_1}{1+(q-1)\,r_1}}, \quad (a)$$

where $r_1 =$ the average correlation between the performances taken singly and $r_{pq} =$ the correlation of p pooled performances with q pooled performances.

In the present case $p = 1$ and $q = 8$, so that we get

$$r_{pq} = \sqrt{r_1}\sqrt{\frac{8r_1}{1+7r_1}}.$$

[1] This *Journal*, 1910, III. 275, Eq. (I) we make equation $x=y$ so that $r_{xy}=1$ and we get the above equation (a) immediately. As will be seen, the indices in Eq. (I) are abbreviated in (a).

In the case of the girls, as the mean intercorrelation of the tests is, as shown by Table II, ·321, we have

$$r_{pq} = \sqrt{\cdot 321}\sqrt{\frac{8 \times \cdot 321}{1 + 7 \times \cdot 321}} = \cdot 50,$$

which is precisely the value actually observed and given above.

In the case of the boys, we similarly get from Table II,

$$r_{pq} = \sqrt{\cdot 257}\sqrt{\frac{8 \times \cdot 257}{1 + 7 \times \cdot 257}} = \cdot 44,$$

a value which is nearly the same as that actually observed, viz. 41.

(This remarkable agreement also furnishes a very useful corroboration of the general applicability of the formula.)

The nature of the 'common factor.' The next question is whether there is any evidence as to the central factor. For the present the very neutral term 'general ability' seems at once justified by the actual observations, as it in fact merely summarises them. But we are not straightway justified in introducing the often used term: 'general intelligence.'

Some light on the essential nature of this 'general ability' seems to be afforded by the discrepancy of the present results from those given by adults; for in their case, the comparatively high correlation given by 'tapping,' even with the most intellectual performances, does not seem to occur[1]. This seems strongly to indicate that this 'general ability' is not a factor always present to a constant amount in any given test, but rather that this amount depends jointly on the test and on the type of the individual under investigation.

This would agree well enough with Burt's theory that the source of the 'general ability' is power of attention. Tapping may perhaps be a feat of attention for 'backward' children and yet be merely mechanical for normal people.

Further, the curious equality of all the correlations suggests as explanation the fact that all the tests were carefully selected and graded so as to call forth the full attention of these defective children. But plausible and important as this explanation may be for the theory of the nature of mental deficiency, it must still be accepted with great

[1] Prof. Spearman has found that 'tapping' (the results are not yet published) gives a low correlation with mental ability in the case of adults (normal and insane); correlations of intermediate magnitude were got on normal children by Burt (this *Journal*, 1909, III. 176); Bagley (*Amer. J. of Psychol.* XII. 193) obtains a low and even inverse correlation with tapping; and Binet's results (*Année psychol.* IV. and VI.) are very irregular.

caution, for it has been shown that attention—like all other expressions of lay psychology—really denotes a complex of many factors—intellectual, conative, sentient, etc. Even if these manifold factors really include among them the basis of 'general ability,' we have still to find out which of them actually constitute it.

The evidence available for this purpose consists chiefly in the introspection of the mental processes of normal subjects, and in reasonable deductions as to the probable mental processes of defective children, together with corroborative evidence derived from their outward demeanour. On the whole the evidence seems to indicate strongly that the common factor is not conative but intellectual. These defective children seem to make quite as much effort as normal people, but to experience greater difficulty. This is in conformance with Binet's observation, that such experimental tests almost completely eliminate individual differences of volition[1]. For the time being, the subject's will seemed to be replaced by that of the experimenter. Hence we must conclude that the defective performances are not so much the fault of the willing process itself, as of the ability to execute what is willed.

To go farther would be very speculative. But one may, perhaps, hazard the suggestion that the deficiency appears to consist generally in the lowering of the level of all performances needing *clear awareness.* To judge from recent physiological investigations, such a defect would imply a general impairment of some stratum, portion or function of the frontal, parietal or lower temporal areas.

But there appears no reason to suppose that this lowering of a person's general average level is incompatible with the fact that he is able to do a certain thing far better than others. Any special power which would have been very much above the normal average (had the individual been normal) may still be healthy even in the condition of general defectiveness; just as the subsidence of a continent may leave the mountain tops still projecting above the sea-level.

Quantitative estimate of 'general ability.' Whatever may be the essential nature of 'general ability,' the important practical question is, whether we are able as to our power accurately to estimate it. The answer is facilitated in the present case by the fact that the tests have been shown to be regardable, especially for the girls, as almost

[1] Binet et Simon, "Le développement de l'intelligence," *Année psychol.* 14e année, 1908, 77.

independent and about equally accurate measurements of 'general ability.' Hence perfectly true measurements of the latter would be given by an infinite number of such tests pooled together. But we can readily calculate the probable correlation of any one test with such an infinite number of further tests. We may use for this purpose the same mathematical formula which we have already found to be in extremely good consonance with our experimental results.

r_{pq} in the equation on p. 299 evidently gives us the required value at once if we put p equal to unity and q equal to infinity. It works out to be simply the root of the mean correlation between the tests. In the case of the girls, this is $\sqrt{\cdot 321} = \cdot 57$. In the case of the boys, this is $\sqrt{\cdot 257} = \cdot 51$. Such numbers convey to anyone familiar with correlational coefficients a perfectly clear idea as to the degree to which such tests would form a really reliable criterion in practice. It may be summed up by saying that the test would be reliable enough to be extremely useful as a guide in making a bet but hopelessly inadequate for a magisterial decision.

But we possess nine of these tests. What would be the diagnostic value of all nine pooled together? We can determine this just as before—this time putting p equal to 9, q being, as before, infinity. The correlation of the pool of the nine tests with the true values of the children's intelligence works out (in the same way as before) to be no less than ·90 in the case of the girls, and ·87 in the case of the boys.

These two correlations are of really high magnitude. The tests which taken singly showed themselves so inadequate, when pooled together constitute a means of diagnosis that can only be admired—especially when we recall the remarkable unreliability shown to characterise most usual forms of evidence or diagnosis.

A still further increase in the number of tests would make less further improvement than might at first be supposed. The same formula would easily show, for instance, that to raise the girls' correlations to ·95 would require as many as 19 tests.

4. *Correlations of the tests with imputed 'practical intelligence.'*

Common discrepancy of opinion. It will now be interesting to confront the tests with the teachers' estimates of 'practical intelligence.' As to the respective value of these, there still exists great discrepancy of opinion. The teachers' estimates have by some investigators been simply accepted as representing the exact truth, while others regard these estimates with unconcealed scepticism. The two opinions have only one thing in common: they lack precise scientific evidence.

Table of correlations. The following values show the correlations between the imputed 'practical intelligence' and the tests:

	Girls	Boys
Tapping	·42	·28
Crossing out rings	·43	·04
Crossing out sets of dots	·32	·28
Memory for sentences	·45	·18
Memory for names of objects	·18	·19
Memory for commissions	·53	·65
Geometrical figures	·42	·32
Discrimination of length	·35	·47
Interpretation of pictures...	·39	·52
	Av. ·39	Av. ·33
All the tests pooled together... ...	·60	·56

It is evident that between the estimates and the tests, a correlation exists, but only a moderate one, the average being ·39 for the girls and ·33 for the boys. But if we pool all the tests together, their correlation with the estimates rises to ·60 for the girls and ·56 for the boys. Thus at any rate the idea that the tests are merely laboratory artifacts, having no relation to ordinary life, falls to the ground.

Looking at these correlations in detail, the significant fact may be observed that for both girls and boys much the highest correlation is that with 'memory for commissions,' the coefficients being ·53 and ·65 respectively, and as we have not been able to correct for the teachers' errors of observation, the true values are, probably, a great deal higher still. It will be remembered that the estimates of 'practical intelligence' were based on 'which children could soonest be entrusted on an errand requiring the sharpest intellect' (p. 289). From the above specially high correlations, the sharpest intellect would appear very largely to consist of the children merely remembering to do what they were told [1].

Improvement of correlations with further practice. The results have shown that not only does the child improve with practice, but the correlations tend if anything to increase when the test is repeated.

The following values show the correlations between 'practical intelligence' and the different performances.

[1] See also Burt, *op. cit.* 144.

Girls.

	1st Perf.	2nd Perf.	3rd Perf.	4th Perf.
Tapping	·40	·37	·43	·53
Crossing out rings	·40	·34	·40	·43
" " sets of dots	·34	·31	·34	·45
Memory for sentences	·32	·54		
" " names of objects ...	·20	·28		
" " commissions	·28	·42		
Geometrical figures	·34	·42		
Discrimination of length	·27	·31		
Interpretation of pictures	·37	·42		
Av.	·32	·38	·39	·47

Boys.

	1st Perf.	2nd Perf.	3rd Perf.	4th Perf.
Tapping	·19	·17	·32	·32
Crossing out rings	·06	·06	·15	·07
" " sets of dots	·37	·27	·35	·31
Memory for sentences	·04	·31		
" " names of objects ...	·16	·15		
" " commissions	·06	·22		
Geometrical figures	·28	·26		
Discrimination of length	·27	·51		
Interpretation of pictures	·48	·35		
Av.	·21	·26	·27	·23

Girls.

	1st Perf.	2nd Perf.	3rd Perf.	4th Perf.
Tapping	·44	·40	·47	·59
Crossing out rings	·29	·35	·34	·39
" " sets of dots	·28	·43	·46	·50
Memory for sentences	·44	·56		
" " names of objects ...	·46	·43		
" " commissions	·39	·45		
Geometrical figures	·41	·50		
Discrimination of length	·46	·38		
Interpretation of pictures	·50	·53		
Av.	·41	·45	·42	·49

Boys.

	1st Perf.	2nd Perf.	3rd Perf.	4th Perf.
Tapping	·26	·17	·35	·41
Crossing out rings	·33	·30	·35	·35
" " sets of dots	·40	·40	·46	·46
Memory for sentences	·39	·38		
" " names of objects ...	·44	·28		
" " commissions	·19	·32		
Geometrical figures	·43	·33		
Discrimination of length	·35	·43		
Interpretation of pictures	·30	·41		
Av.	·34	·34	·39	·41

This improvement seems reasonable enough. When a child, especially a defective child, is given a task of some difficulty to perform —a task he has never before attempted—he is likely to be troubled by disturbing irregular factors (such as strangeness of procedure, difficulty of adaptation to new conditions and accidents of all kinds). When the task is done a second time, these disturbing elements, under favourable conditions, tend to disappear, and the child is able to do himself more justice. This increase in correlation with intelligence as a result of practice signifies that with practice the more intelligent children tend to excel in a higher degree than the duller; in other words, they improve more with practice. This well agrees with some results of Thorndike. Such tendency of the correlations to increase with practice is, however, very slight, especially in the case of the boys. The main point is that there is certainly no tendency for the correlations to diminish.

This result is not necessarily inconsistent with Burt's report, that in his case the correlations diminished on repeated trial. For the diminution was completely absent in the case of his more difficult tasks. His easier ones may well have been tests of intellectual power to normal children when first tried, but tending to become mechanical on repetition; in that case the more defective children would no longer be at such a disadvantage. Mentally deficient children, on the other hand, would not readily master a performance sufficiently to make it mechanical, but would have to continue to exert their full powers.

'Practical intelligence' considered as a criterion of 'general ability.' Next let us consider how far this 'practical intelligence' as estimated by teachers is serviceable as a criterion of 'general ability,' which, after all, is the main thing. It is possible, by means of the same formula as has been used above, to calculate the correlations which exist between this 'practical intelligence' and a *perfectly accurate* measurement of the children's ability, were such available; also between the tests and this perfectly accurate measurement. The calculations, which are very simple, will be found in Appendix II. The results are given in the following table.

Table showing calculated correlations which a perfectly accurate measurement of the children's 'general ability' should be expected to give with

	Teacher's estimate	A single test, on an average	Two tests pooled, on an average	All tests pooled
Girls......	·66	·57	·70	·90
Boys......	·65	·51	·64	·87

From this table we see that the 'practical intelligence' as displayed in these children's ordinary life has a distinctly better diagnostic value than a single test, but it is not superior to two tests pooled; and it is far inferior to nine tests pooled.

5. *Correlations with estimates of 'scholastic ability.'* The correlations with the teachers' estimates of 'reading ability' and 'arithmetical ability' can best be illustrated by being shown in a complete table of all the correlations of the tests with these estimates and with one another.

TABLE V. *Girls.*

	Imputed 'Prac. Int.'	Memory for commissions	Tapping	Crossing out rings	Memory for sentences	Interpretation of pictures	'Arithmetical ability'	Geometrical figures	Discrimination of length	Crossing out sets of dots	Memory for names of objects	'Reading ability'	Average Intercorrelation
Imputed 'Prac. Int.'		·52	·42	·43	·45	·39	·51	·43	·35	·33	·18	·43	·40
Mem. for commissions ...	·52		·14	·30	·50	·30	·41	·31	·43	·31	·34	·37	·36
Tapping	·42	·14		·42	·27	·61	·29	·33	·37	·21	·30	·26	·33
Crossing out rings.........	·43	·30	·42		·21	·26	·34	·32	·42	·47	·17	·27	·33
Mem. for sentences	·45	·50	·27	·21		·49	·16	·43	·32	·26	·42	·13	·33
Interp. of pictures.........	·39	·30	·61	·26	·49		·30	·42	·26	·25	·33	·00	·33
'Arithmetical ability' ...	·51	·41	·29	·34	·16	·30		·32	·26	·30	·30	·47	·33
Geometrical figures.........	·43	·31	·33	·32	·43	·42	·32		·21	·45	·13	·01	·31
Discrimination of length	·35	·43	·37	·42	·32	·26	·26	·21		·22	·22	·20	·30
Crossing out sets of dots...	·33	·31	·21	·47	·26	·25	·30	·45	·22		·18	·15	·28
Mem. for names of objects	·18	·34	·30	·17	·42	·33	·30	·13	·22	·18		·24	·26
'Reading ability'	·43	·37	·26	·27	·13	·00	·47	·01	·20	·15	·24		·23
Average.........													·32

It will at once be observed that neither 'reading ability' nor 'arithmetical ability' correlates very highly with the other performances. On the whole, they give results no better than the average single test. This is clearly against the assumption by some of the earlier investigators that scholastic attainment may be considered as the supreme criterion of 'general ability.' It also indicates that they were unwarranted in claiming that their tests are adequate for classifying the children for scholastic purposes.

In both girls and boys, the two kinds of scholastic intelligence appear to be very distinct—in fact, in the case of the girls, the two hardly go more with one another than with the tests or than the tests do with one another. In the case of the boys there does indeed appear much special correlation, the coefficient being ·74, but this is, perhaps,

TABLE VI. *Boys.*

	'Reading ability'	Imputed 'Prac. Int.'	Memory for sentences	Geometrical figures	Crossing out sets of dots	Discrimination of lines	Interpretation of pictures	'Arithmetical ability'	Memory for commissions	Tapping	Memory for names of objects	Crossing out rings	Average Intercorrelation
'Reading ability'............		·54	·20	·20	·40	·11	·19	·74	·41	·37	·20	·41	·34
Imputed 'Prac. Int.'	·54		·18	·32	·28	·47	·52	·46	·24	·28	·19	·04	·32
Memory for sentences......	·20	·18		·39	·21	·17	·36	·41	·38	·20	·66	·02	·29
Geometrical figures.........	·20	·32	·39		·17	·21	·36	·30	·56	·07	·52	·07	·29
Crossing out sets of dots	·40	·28	·21	·17		·46	·15	·39	·04	·32	·06	·65	·28
Discrimination of lines ...	·11	·47	·17	·21	·46		·21	·20	·34	·38	·18	·31	·28
Interpretation of pictures	·19	·52	·36	·36	·15	·21		·14	·22	·25	·30	·27	·27
'Arithmetical ability'......	·74	·46	·41	·30	·39	·20	·14		·10	– ·11	·32	– ·02	·27
Memory for commissions	·41	·24	·38	·56	·04	·34	·22	– ·10		·38	·38	– ·15	·24
Tapping	·37	·28	·20	·07	·32	·38	·25	– ·11	·38		– ·08	·46	·23
Mem. for names of objects	·20	·19	·66	·52	·06	·18	·30	·32	·38	– ·08		·22	·23
Crossing out rings	·41	·04	·02	·07	·65	·31	·27	– ·02	– ·15	·46	– ·22		·16
Average.........													·27

explicable by difference of training in previous schools. Those who had been taught longer or better than the others would tend to excel in both branches, and thus would arise a special correlation between them.

General impressions as compared with experimental tests. Let us now venture to compare the information given by the tests with that furnished by the teacher's entire experience of the children for many years. If this experience may be taken as wholly concerning the children's 'practical intelligence' on the one hand and their 'scholastic intelligence' on the other, it will appear to be almost exhaustively expressed in the three estimates furnished by them. In that case, as the practical experience is only worth about two tests and each of the scholastic estimates at most about one test each, the diagnostic value of all together might be equivalent to about four tests only. But, doubtless, something must be added to this in view of scholastic estimates being based on present proficiency. If the teachers had instead been asked to estimate what they believed to be the innate capacity of the children (as shown by their progress, etc.) for scholastic work, their estimates might have shown a higher value. How far, it is impossible to say. On the other hand, something must be deducted from the valuation at four tests, for, as we have already seen, the teachers' estimates do not appear to have been quite independent of one another. There is every reason to believe that the teachers, in spite of themselves, were biassed by the scholastic progress made by the children when drawing up their estimates for 'practical intelligence.' Moreover, the teachers' estimates have other disadvantages. They owned that unless they had known the child for a very long period indeed, they found it difficult to judge his 'practical intelligence.' They themselves stated that there were many disturbing considerations, such as age, precocity, and cunning, which made the judgment a difficult one. Moreover, even if the teacher could unfailingly rank his own pupils according to their abilities, we should still have no reliable means of comparing the pupils of one teacher with those of another. We should be as far as ever from obtaining a uniform standard whereby to select those children that need special treatment; it is thus impossible to standardise the teachers' estimates, whereas tests furnish such a standard without difficulty.

6. *Children's age.* Next as regards the children's age at the time of the test. The mean age, both for the boys and for the girls, was about eleven years. The mean deviation came to about one year. The extreme differences of age were considerable, the oldest children being about fifteen and the youngest about eight years old.

This wide range of age gives good scope for examining its correlations. We find that the girls' correlations of age with 'practical intelligence' is distinctly positive (+ ·48); this indicates that the older ones have been found by the teachers to be distinctly more intelligent on the whole. As there is a marked tendency to send back the children who do best in these special schools to the normal ones, the older children must represent a selection of the least favourable cases. As they nevertheless appear more intelligent than the younger ones, we must conclude that their practical intelligence continues to develop quite appreciably during the ages in question. This is corroborated by the correlation of age with the pooled tests which comes to the smaller but the still distinctly positive amount of + ·37. The correlation of age with 'reading ability,' however, is + ·24, and that with 'arithmetical ability' only + ·09. It is interesting to compare this with the recent work of Bonser, according to which the general ability of normal children ceases to increase markedly at a much earlier age[1]. The same conclusion had already been arrived at by Prof. Spearman[2].

In the case of the boys, the correlations of age with 'practical

[1] Bonser, *The reasoning ability of Children*, 1910.

[2] *Amer. Journ. of Psychol.* 1904, 285.

intelligence' and with the pooled tests are much less, being only $+\cdot 12$ and $-\cdot 09$ respectively. On the other hand, the correlation of age with scholastic ability is greater, being $+\cdot 35$ with 'reading ability' and $+\cdot 31$ with 'arithmetical ability.'

These sex differences appear rather too large to be wholly accidental, at any rate it is clear that as regards the boys also, the older ones do not do any worse in spite of being selected bad cases. These also then continue to develop later than ordinary children.

As both the teachers' estimates and the experimental tests show a correlation with age, this might conceivably be the real and only cause of their correlations with one another. But by means of Yule's partial coefficients, we can verify this supposition quantitatively. According to Yule's formula[1], if the influence of age is required to be eliminated, the correlation of 'practical intelligence' with the tests only changes in the case of the girls from ·60 to

$$\frac{\cdot 60 - \cdot 48 \times \cdot 37}{\sqrt{1 - \cdot 48^2}\,\sqrt{1 - \cdot 37^2}} = \cdot 54,$$

and in the case of the boys, it changes from ·56 to

$$\frac{\cdot 56 - \cdot 12 \times - \cdot 09}{\sqrt{1 - \cdot 12^2}\,\sqrt{1 - \cdot 09^2}} = \cdot 56,$$

i.e. it is as before.

The above throws some interesting light on the value of the work of Binet in drawing up a scale of tests elaborately graded to suit different ages. Such a scale is unquestionably of very great intrinsic interest. It is a most valuable document on child development. Also it is clearly indispensable for diagnosing mental deficiency in the case of very young children. But it seems much less needed for diagnosis in the practically important class of children chosen for the present investigation. It is very questionable whether all children of this class cannot be perfectly well suited by the same series of tests, differences of age being compensated for simply by allowing marks according to age.

Binet regarded the fact that his tests gave results closely conforming to the age of the children as showing that the tests were very reliable[2]. But really this conformity is not a good but a bad sign. It means that the tests are such as to depend less on the children's innate powers than on their stage of instruction. It is important to notice that

[1] See Yule's *Introduction to Statistics*, 1911, ch. XII.

[2] *Année psychol.* 14e année, 1908, 71–74.

the disturbance effected by age affects the diagnostic value of the experimental tests distinctly less than the personal estimates.

IV. General Conclusions.

1. There appears justification for the growing feeling that the prevalent methods of examining mental deficiency are inadequate. It is high time to abandon exclusive reliance on casual and intangible 'general impressions,' with all their vagueness, fallacies, tendency to mere dogmatic assertions and impossibility of standardisation. And when such general impressions have to be derived from a single brief interview, the present practice of making it a decisive criterion as to the child's future can only excite grave misgivings.

2. Methods of diagnosis must always have as ultimate criterion their degree of correspondence with later results otherwise obtained. The chief modern means of estimating correspondence in matters of such complexity as mental deficiency is that of correlational coefficients. In spite of many disadvantages, they at least afford explicit, quantitative, and readily verifiable evidence.

3. Our present results give no corroboration to the 'faculty' theory which apparently still lingers in lay psychology. Hence when those entrusted with the diagnosis of mental deficiency usually report that they have tested memory, observation, judgment, reasoning, muscular coordination, etc., this claim does not arouse confidence in their procedure.

4. The present investigation lends no support to other *a priori* estimates of the value of these tests, such as the frequent off-hand assertion that 'a child who can do such and such a test is clearly not defective.' Often an unquestionably defective child can perform a task almost incredibly well, while perfectly normal children may meet with surprising failure. Among other causes for this, success in a task depends very largely on the exact manner in which it is given, the degree of 'warming-up' allowed, etc. Moreover, the apparent intellectuality of a performance may be grossly misleading. For instance, the simple test of 'tapping' showed itself to have (for these defective children) just about as much diagnostic value as the far more plausible test of 'interpreting pictures.'

5. In fact the present work has shown that when the various tests have been properly constructed and suitably graded, any one of them is just about as effective for the present purpose as any other. The

difference in their diagnostic effectiveness proves to be no more than what may be expected to occur from mere accident.

6. All tests alike show themselves to be most untrustworthy when used alone. The cardinal principle in using them must be to pool several independent tests together. Means have been devised for estimating quantitatively the effect of this pooling, and it has been proved that even tests practically valueless in isolation become remarkably trustworthy in pools of from ten to twelve. Moreover, it has been shown that a surprisingly small difference in the nature of the tests is usually enough to make them sufficiently independent of one another.

7. Even more fundamental is the principle that the tests should be thorough enough to give results of some constancy. If a test is not even consistent with itself, it can only suggest misleading inferences about itself as well as about other things. To secure this reliability is not nearly so simple a matter as it might seem. Previous work on mental tests has almost all been vitiated by disregard of this point.

8. Far less important is the principle lately advocated with much emphasis of using a scale of tests graduated according to the different ages of the persons tested. Children on the border-line between normality and deficiency, who are at least eight or nine years old, are probably most satisfactorily examined by always using the same tests (not necessarily the tests used in the present investigation) and simply making allowance for age.

9. Considerable light appears to have been thrown by the present results upon the vital question as to the essential nature of intellectual deficiency. It seems to be a general lowering of that class of performances which is characterised by the need of clear consciousness. The reason for applying a large number of tests is not to gauge a number of different factors in ability but merely to obtain multiple evidence as to this one factor, the general level.

As the above conclusions from the present research indicate that mental tests may be of extreme value, it seems desirable not to leave unmentioned what I believe to be an important qualification concerning their usage. I should like to endorse strongly Dr Myers's recent emphatic condemnation of the use of them or of correlations in a merely mechanical manner[1]. Psychology has become a science requiring just

[1] "The Pitfalls of 'Mental Tests'," *Brit. Med. J.*, 1911, I. 195.

as systematic a training as any other. The excuse that the persons entrusted with the diagnosis of defective children have no leisure to learn it, is only valid for the present moment. It can no more continue indefinitely to be accepted than, say, the excuse that engineers are too busy to learn engineering.

Equally temporary is the alleged difficulty of finding sufficient time to carry out the tests. When we remember that a very considerable portion of our teachers' and children's lives are being spent in examinations to no useful purpose whatever, surely one half-hour can be found to examine a child properly in a matter vitally affecting his whole life.

APPENDIX I. (Kindly supplied by Prof. Spearman.)

Assuming that the true values of all the correlations between several series of values are equal to one another, it is required to find what apparent deviations between the correlations will be produced merely by the errors of sampling.

Let r_{xy} denote the true correlation between x and y, any two of the series.

Let a denote the mean value of all such correlations.

Let δ_r and δ_a denote the sampling errors of r_{xy} and a.

Let D denote the required standard deviation produced between the correlations by these sampling errors.

Then

$$\frac{n(n-1)}{2} D^2 = \sum_{xy} [(r_{xy} + \delta_r) - (a + \delta_a)]^2$$

$$= \sum_{xy} (\delta_r - \delta_a)^2, \text{ since the } r\text{'s are all equal by hypothesis,}$$

$$= \sum_{xy} \left[\delta_r - \frac{2\sum_{xy}(\delta_r)}{n(n-1)}\right]^2$$

$$= \sum_{xy} (\delta_r^2) - \frac{2}{n(n-1)} [\sum_{xy} (\delta_r)]^2 \quad \text{.........................} \quad (1)$$

$$= \Sigma (\delta^2) + \Sigma (\delta_p \delta_p) + \Sigma (\delta_p \delta_q)$$

where δ^2 denotes any of the squared errors contained in the sum on the right side of (1) on squaring out.

$\delta_p \delta_p$ denotes any of the products of the errors of two coefficients that have one index in common [*e.g.* r_{xp} and r_{yq}].

$\delta_p \delta_q$ denotes any of such products where neither index is common [*e.g.* r_{xp} and r_{yq}].

$$= \Sigma (\sigma^2) + \Sigma (r_{pp} \sigma_p \sigma_p) + \Sigma (r_{pq} \sigma_p \sigma_q)$$

on a mean of samples, where the σ's denote the standard sampling deviation of the coefficients involved.

r_{pp} denotes the correlation between the two δ_p's in the same bracket
r_{pq} " " " δ_p and δ_q " "

$$= A\overline{\sigma^2} + B\overline{r_{pp}\sigma_p\sigma_p} + C\overline{r_{pq}\sigma_p\sigma_q}$$

where the scoring indicates mean values, while A, B, and C are functions of n.

$$= \overline{\sigma}^2(A + B\overline{r}_{pp} + C\overline{r}_{pq}) + R,$$

where R is a value vanishing with the mean deviation of the σ's and usually small.

Then neglecting R and picking out the values of A, B and C from (1), we readily get, nearly,

$$D^2 = \overline{\sigma}^2\left(1 - \frac{2(n-2)}{n(n-1)}\right)\overline{r}_{pp} - \frac{n-2}{n}\overline{r}_{pq},$$

and taking rough approximations for $\overline{r}_{pp}$ and $\overline{r}_{pq}$ *

$$= \overline{\sigma}^2\left[1 - \frac{n-2}{n(n-1)}(2\overline{r} - \overline{r}^2) - \frac{2(n-2)}{n}\frac{r^2}{(1+\overline{r})^2}\right] \quad \text{......(2)},$$

where $\overline{r}$ denotes the mean value of the observed coefficients and $\overline{\sigma}^2 =$ their mean square 'probable errors' $\times$ 2·2.

In the case of the girls

$$\overline{r} = \cdot321,$$
$$\overline{r}^2 = \cdot103,$$
$$(1+\overline{r})^2 = 1\cdot719,$$
$$n = 9,$$

and mean square prob. error = ·0047, so that

$$D^2 = 2\cdot2 \times \cdot0047\left[1 - \frac{7}{9 \times 8}(\cdot641 - \cdot103) - \frac{2 \times 7}{9} \times \frac{103}{1\cdot719}\right] = \cdot0087.$$

In the case of the boys

$$\overline{r} = \cdot257,$$
$$\overline{r}^2 = \cdot064,$$
$$(1+\overline{r})^2 = 1\cdot560,$$
$$n = 9,$$

and mean square prob. error = ·0091, so that

$$D^2 = 2\cdot2 \times \cdot0091\left[1 - \cdot097(514 - \cdot064) - 1\cdot56 \times \frac{\cdot064}{1\cdot560}\right] = \cdot0178.$$

* These may easily be deduced from the values given for r_{pp} and r_{pq} by Pearson and Filon, *Phil. Trans.* A, CXCI. 262.

APPENDIX II.

It is required to find the correlation of variously sized pools of tests or estimates with the true values, namely those given by a pool of an infinite number of different tests or estimates. We will use the same formula as before, namely

$$r_{x_p y_q} = r_{xy} \sqrt{\frac{p r_{x_1 x_1}}{1+(p-1)\, r_{x_1 x_1}}} \sqrt{\frac{q r_{y_1 y_1}}{1+(q-1)\, r_{y_1 y_1}}} \text{ ......(1).}$$

Here let r_{xy} denote the correlation between two series x and y, x being the pool of an infinite number of independent estimates, and y being that of an infinite number of different tests. As x and y by hypothesis both give true values, $r_{xy} = 1$.

Let $r_{x_1 x_1}$ denote the average intercorrelation between single estimates for the girls; $r_{y_1 y_1}$ that between single tests for them, and therefore $= \cdot 321$; $r_{x_p y_q}$ that between the one estimate and the pool of nine tests, so that $p = 1$ and $q = 9$. $r_{x_p y_q}$ has been found $= \cdot 60$, so that

$$\cdot 60 = 1 \times \sqrt{\frac{1 \times r_{x_1 x_1}}{1+(1-1)\, r_{x_1 x_1}}} \sqrt{\frac{9 \times \cdot 321}{1 + 8 \times \cdot 321}} \text{ ....(2),}$$

from which $r_{x_1 x_1} = \cdot 44$.

Next let $q =$ infinity, everything else meaning as before. Then $r_{x_p y_q}$ denotes the correlation between the teachers' estimates and the true values. Utilising (1) and (2)

$$r_{x_p y_q} = 1 \times \sqrt{\cdot 44}\, \sqrt{1} = \cdot 66 \text{ ....(3).}$$

Let x now denote tests instead of estimates, while every other meaning is as in (3). Then $r_{x_p y_q}$ denotes the average correlation of single tests with true values,

and $= 1 \times \sqrt{32}\, \sqrt{1} = \cdot 57$ ....(4).

Similarly if $p = 2$, we get $r_{x_p y_q} = \cdot 70$ ....(5).

If $p = q$, $r_{x_p y_q}$ it becomes $\cdot 90$ ....(6).

In the case of the boys, the values corresponding to (2), (3), (4), (5) and (6) are ·42, ·65, ·51, ·64 and ·87.

MENTAL FATIGUE IN DAY SCHOOL CHILDREN, AS MEASURED BY ARITHMETICAL REASONING.

By W. H. WINCH.

I. The Problem stated.

This investigation is an attempt to give a direct answer to a question put to me by an experienced Head Master of an elementary school in London. "I do not suppose," he said, "with a school such as mine that any of my pupils are seriously mentally fatigued during any part of the school day: their health is not jeopardised by overstrain; that aspect of the case does not trouble me; what I want to know is where to put my lessons in my time-table. Shall I put problematic arithmetic, for example, at a time more favourable than the learning of poetry? Suppose I put my problematic arithmetic in the afternoon, will my boys really do any worse than if I keep it in the morning time-table? And my English composition, what is the best time of day for that? I know every subject can't have the best time of day every day, though, of my specialist advisers, each thinks that his own subject ought to.

(It was a Head Master who was speaking; I disclaim all responsibility.) I want to give the worst times to the things which will suffer least." I pointed out that there were books on mental fatigue which gave answers to questions of that kind. Whereupon he answered me by saying that he thought there had been too much measurement of fatigue of one function by the results of weakness in another. "I am unable," he said, "to accept so much 'transfer' without evidence, and I thought you were too."

I promised him to try to solve one of his problems, namely, the degree of superiority (if any) of early morning work over late afternoon work in problematic arithmetic.

II. First Experiment in a Boys' School.

i. *General Plan.*

The experiment was carried out with the whole of a Standard IV class, 60 in number, of an average age of 10 years 7 months at the commencement of the tests. The work was done in a hard-working and strongly disciplined boys' school situated in a rather poor neighbourhood in London. The teacher of the class, who administered the tests for me, was very steady, equable, patient and continuous in his teaching. His boys were not accustomed to bursts of great vigour, followed by quiet times. They were expected to work steadily and rapidly, though not hurriedly, throughout the whole day. They did not *appear* to have less mental energy at their disposal at one time of day than another; but, of course, if we could trust appearances, there would be no need of a science of experimental pedagogy. With such a class and such a teacher there seemed a likelihood of useful and unexaggerated results. Additional means were taken to secure reliability. A division of the class into two equal groups was made 'objectively' by a series of *tests of the same nature as those which were subsequently to be used as fatigue tests.* Test by test, the results of these preliminary tests were correlated, so that confidence might be felt in the results of the division when once it was effected.

These tests for the division of the class took place in the mornings just prior to the mid-day recess.

When the division had been effected, one of the groups—Group A—continued the tests in the mornings as before, but now about two hours earlier, and the other group—Group B—worked identical tests late in the afternoon.

A comparison between the morning and afternoon results may afford us a means of estimating the comparative freshness or fatigue for problematic arithmetic early in the morning and late in the afternoon.

ii. *Chronology of the Experiment.*

The experiment was commenced in February, 1911. A lesson with illustrative examples was given by the teacher in which he explained to the boys that they were to set down their *method of solution* of the problems given to them, but *not* to solve them numerically. Then, on February 15th, a practice exercise was given to the boys to accustom them to work the problems under test conditions. It was then thought that they were ready to start the preliminary tests, the results of which were to be used as a basis for dividing the class into two equal groups. The preliminary tests were given as follow:

Friday,	February 17th,	11.35 to 11.55.	Preliminary test		1.
Wednesday,	,, 22nd,	,, ,,	,,	,,	2.
Friday,	,, 24th,	,, ,,	,,	,,	3.
Wednesday,	March 1st,	,, ,,	,,	,,	4.
Friday,	,, 3rd,	,, ,,	,,	,,	5.

The Friday morning tests followed immediately after a lesson in Geography, given orally; and the Wednesday morning tests followed immediately after a Reading lesson.

On the results of these five preliminary tests the class was divided into two equal groups, that is to say, equal in their power to reason out the methods of solution for the arithmetical problems given.

Then about a week later followed the tests which, it was hoped, might indicate the boys' comparative freshness or fatigue for early morning and late afternoon work respectively. Group B worked in the afternoons the identical test worked by Group A in the morning. The tests were given as follow:—

Group A,	Wednesday,	March 8th,	9.40 to 10.0.	Final test	1.
,, B,	,,	,,	4.10 to 4.30.	,,	1.
,, A,	Friday,	,, 10th,	9.40 to 10.0.	,,	2.
,, B,	,,	,,	4.10 to 4.30.	,,	2.
,, A,	Wednesday,	,, 15th,	9.40 to 10.0.	,,	3.
,, B,	,,	,,	4.10 to 4.30.	,,	3.
,, A,	Friday,	,, 17th,	9.40 to 10.0.	,,	4.
,, B,	,,	,,	4.10 to 4.30.	,,	4.
,, A,	Wednesday,	,, 22nd,	9.40 to 10.0.	,,	5.
,, B,	,,	,,	4.10 to 4.30.	,,	5.

On the mornings of Wednesday and Friday the tests followed immediately after a Scripture lesson; on Wednesday afternoons they followed a lesson in Physical Drill, and on Friday afternoons they followed a lesson in Recitation—the repetition of poetry already learnt. No arithmetical work other than the tests was done on the same days as any of the Preliminary or the Final tests. But all other arithmetic lessons scheduled in the school time-table were given as usual.

Whilst Group A was working the Final tests, Group B was learning poetry in another room; and whilst Group B was working the Final tests, Group A learnt the poetry. Both Group A and Group B were tested on the results of their poetical work and, on the following day, the boys were told how many marks they had obtained both for their solutions of the arithmetical problems as well as for their memory work.

iii. *The Tests and the Method of Marking.*

Each 'test' contained five problems. The first problem was invariably a 'one-step' problem, the second a 'two-step' problem, the third a 'three-step' problem, the fourth a 'four-step' problem, and the fifth a 'five-step' problem. A specimen test follows:

1. A horse and cart cost £379. 10*s.* altogether. The horse cost £296. 15*s.* What was the value of the cart?
2. A man sold 10 gold watches for £33. 17*s.* 6*d.* each, and with the money he bought 59 chains. Find the value of each chain.
3. After buying 12 houses at £426 each, a man has enough money left to pay for 48 chairs at £1. 2*s.* 6*d.* each. How much had he at first?
4. *A* earns £5. 12*s.* 7½*d.* a month, *B* earns 10 shillings more than *A*, and *C* earns half as much as *A* and *B* earn together. How much will *A*, *B*, and *C* earn altogether in a month?
5. 37 gallons of wine, worth 17*s.* 9*d.* a gallon, are mixed with 93 gallons of wine at 14*s.* 11*d.* a gallon. How much must the mixed wine be sold at per gallon so as to make a profit of £5. 10*s.* altogether?

The solutions given by the boys were marked in accordance with the number of 'steps' which were solved correctly. To take one illustration; the fifth problem in the specimen test given above might be solved thus: 17*s.* 9*d.* multiplied by 37 gives the total cost of the more expensive wine; 14*s.* 11*d.* multiplied by 93 gives the total cost of the less expensive wine; the addition of the 37 and 93 gallons gives the total quantity of the mixed wine; the addition of £5. 10*s.* to the total cost of the mixed wine gives the selling price; and a division of this sum by the total number of gallons gives the selling price per gallon of the mixture.

For every rational step necessary to the solution, by any method, a boy received one mark.

It is not contended that all the 'rational steps' required in each problem are precisely equal in difficulty, nor is it contended that every one of the ten 'tests' or sets of problems are precisely of the same difficulty. As an indication as to how far we might reasonably expect 'steady' results to follow from the division of the class by means of such tests, the correlations between the results of each preliminary test with those of the 'tests' which preceded it and followed it were worked out.

iv. *Results.*

(*a*) The division into two equal groups.

Little regularity can be hoped for in the final results if the preliminary tests are of such a nature that the boys appear to change places much from exercise to exercise. If precisely the same test were given on more than one occasion there would still be some variations in the results. It is, indeed, an illuminating experience for a teacher to set the same test with the same boys two or three times under what may appear to him to be precisely the same conditions, and to correlate the results. The tests given in this experiment, though not identical, are regarded as approximately equal. The correlations were worked out from the actual marks obtained in each preliminary test by each individual of the sixty pupils who worked the preliminary tests. They were calculated on the Pearson formula, in which the coefficient of correlation or $r = \frac{\Sigma xy}{n\sigma_1\sigma_2}$. The results of preliminary test 1 correlated with those of preliminary test 2 to the extent of + ·780; those of test 2 with test 3 to the amount of + ·737; those of tests 3 and 4 correlated with a coefficient of + ·771; whilst the results of tests 4 and 5 correlated to the extent of + ·857. There were six boys who had been absent on more than one occasion during the preliminary tests; their names were removed and the remaining fifty-four were divided as shown in Table I.

It is obvious from the above table that we have obtained a fairly satisfactory division. As a further check the correlation of the A series with that of the B series was worked out for the total marks of the five tests. By means of rank formulae the correlation would of course be found to be perfect; and even estimated on the product-moment formula, which uses actual marks and not 'rankings,' the correlation between the two series amounts to + ·992. Group A scores an average mark of 40·4 for the five preliminary tests with a mean variation of

TABLE I, *showing the division of the class into two equal groups on the results of the Preliminary tests.*

Group A

Marks for 5 prel. tests	No. of boys	Test 1	Test 2	Test 3	Test 4	Test 5	Totals
60 and over	3	14·3	15·0	14·7	13·0	15·0	72·0
50 to 60	7	10·4	10·1	12·3	10·0	10·9	53·7
40 to 50	3	8·3	9·3	11·0	6·3	7·7	42·7
30 to 40	4	7·3	7·0	9·0	6·3	5·0	34·5
20 to 30	8	6·1	4·9	8·0	4·0	2·4	25·4
10 to 20	2	4·0	5·0	2·5	2·0	1·5	15·0

Group B

Marks for 5 prel. tests	No. of boys	Test 1	Test 2	Test 3	Test 4	Test 5	Totals
60 and over	4	13·5	14·0	14·5	14·5	14·8	71·3
50 to 60	5	11·6	12·4	10·0	8·4	11·0	53·4
40 to 50	4	7·3	7·8	10·5	7·5	10·8	43·8
30 to 40	4	7·8	6·0	9·8	5·0	5·5	34·0
20 to 30	9	4·9	4·9	6·9	4·7	3·6	24·9
10 to 20	1	1·0	2·0	3·0	2·0	2·0	10·0

TABLE II, *showing the comparative results of the early morning and late afternoon work of Groups A and B respectively—the Final tests.*

Group A (Morning)

Marks for 5 prel. tests	No. of boys	Test 1	Test 2	Test 3	Test 4	Test 5	Totals
60 and over	3	14·7	15·0	15·0	14·3	15·0	74·0
50 to 60	7	12·7	12·0	12·9	11·0	12·6	61·1
40 to 50	3	9·7	11·3	11·0	7·3	11·7	51·0
30 to 40	4	9·5	9·3	9·8	7·8	6·8	43·0
20 to 30	8	7·3	7·0	8·9	6·1	5·5	34·8
10 to 20	2	5·5	4·0	2·5	3·0	3·5	18·5

Group B (Afternoon)

Marks for 5 prel. tests	No. of boys	Test 1	Test 2	Test 3	Test 4	Test 5	Totals
60 and over	4	14·3	14·5	14·5	13·5	15·0	71·8
50 to 60	5	11·2	13·0	11·0	9·6	12·8	57·6
40 to 50	4	9·5	11·0	11·3	8·0	9·0	48·8
30 to 40	4	8·5	7·0	7·5	6·5	6·3	35·8
20 to 30	9	6·1	6·2	7·6	5·2	5·9	31·0
10 to 20	1	2·0	3·0	2·0	3·0	2·0	12·0

14·5, whilst Group B scores an average of 40·6 with a mean variation of 14·7.

(*b*) The difference between early morning work and late afternoon work.

It now remains to be shown what the results were when Group A worked tests early in the morning and Group B worked identical tests late in the afternoon.

An inspection of the table leaves little doubt as to the superiority of the work of the group working in the mornings. Group B has gone down as compared with Group A. But the series are still very highly correlated. Estimated by means of the rank formula $\rho = 1 - \frac{6\Sigma D^2}{n(n^2-1)}$ the coefficient of correlation is found to be + ·864; whilst calculated from the actual marks, not rankings, the correlation coefficient is + ·874.

It will probably be of advantage for the reader if I now show in one table the work of the two groups before they were divided and after they were divided.

TABLE III, *showing the marks of Groups A and B compared, section by section, (i) in the Preliminary tests, (ii) in the Final tests.*

	Group A			Group B		
Marks for 5 prelim. tests	No. of boys	Average per boy for 5 prelim. tests	Average per boy for 5 final tests	No. of boys	Average per boy for 5 prelim. tests	Average per boy for 5 final tests
60 and over	3	72·0	74·0	4	71·3	71·8
50 to 60	7	53·7	61·1	5	53·4	57·6
40 to 50	3	42·7	51·0	4	43·8	48·8
30 to 40	4	34·5	43·0	4	34·0	35·8
20 to 30	8	25·4	34·8	9	24·9	31·0
10 to 20	2	15·0	18·5	1	10·0	12·0

But I have not yet solved the problem proposed in this research in the form required by the Head Master to whose question I am trying to reply. He wishes to know what percentage of difference it would make to the work if he put his problematic arithmetic at the end of the afternoon time-table rather than early in his morning time-table. The following table may help toward such a solution.

The sections of Group A all show superiority over the corresponding sections of Group B, in some cases a very marked superiority. Working from the individual cases we find in Group A 2 pupils who improve

TABLE IV, *showing the percentage improvements compared of Group A and Group B from the Preliminary to the Final tests.*

	Group A (Morning)		Group B (Afternoon)	
Marks in 5 preliminary tests	No. of boys	Percentage improvement	No. of boys	Percentage improvement
60 and over	3	2·7	4	·7
50 to 60	7	13·7	5	7·8
40 to 50	3	19·4	4	11·4
30 to 40	4	24·6	4	5·3
20 to 30	8	37·0	9	24·5
10 to 20	2	23·3	1	20·0

50 °/ₒ and over as compared with 1 pupil in Group B; of those who improve 40°/ₒ to 50°/ₒ, 1 is in Group A and 1 is in Group B; of those who improve 30°/ₒ to 40°/ₒ, 5 are in Group A against 3 in Group B; of those who improve from 20°/ₒ to 30°/ₒ, 7 are in Group A against 2 in Group B; of those who improve from 10°/ₒ to 20°/ₒ, 4 are in Group A against 8 in Group B; of those who improve from 0°/ₒ to 10°/ₒ, 7 are in Group A against 6 in Group B; whilst of the pupils who show a loss from 0°/ₒ to 10°/ₒ, 1 is in Group A and 6 are in Group B. It is, of course, obvious that no great improvement can be expected from the pupils near the top in either group; the exercise would have had to be indefinitely extended upwards, as it were, to allow of that; and, as these tests were not used to involve speed as a factor, it was desirable not to set very long exercises.

Dealing with the groups as wholes we find the following comparative results.

TABLE V, *showing the comparative results of Groups A and B as wholes in the Preliminary and Final tests.*

		Preliminary tests					Final tests				
		1st test	2nd test	3rd test	4th test	5th test	1st test	2nd test	3rd test	4th test	5th test
Group A	Av. mark	8·4	8·2	9·9	7·0	6·9	10·0	9·8	10·5	8·4	9·1
	M.V.	2·8	2·8	2·7	3·1	4·0	2·9	3·0	2·9	2·8	4·2
Group B	Av. mark	8·0	8·1	9·4	7·2	7·9	9·0	9·4	9·6	7·8	8·9
	M.V.	3·3	3·5	2·3	2·8	4·0	2·3	3·3	2·7	2·6	3·7

Group A scores an average total mark of 40·4 (mean variation 14·5) in the Preliminary, and 47·8 (mean variation 14·5) in the Final tests; whilst Group B scores an average total mark of 40·6 (mean variation 14·7) in the Preliminary, and 44·6 (mean variation 14·6) in the Final tests.

Group A improves on its preliminary record to the extent of 18·3 °/ₒ whilst Group B improves on its preliminary record to the extent of 11·3 °/ₒ. This shows a difference of 7 °/ₒ in favour of the morning work.

v. *Summarised Conclusions.*

1. Under the conditions given in this experiment, with boys of this age and standard of mental proficiency, the difference of improvability in the rational solution of arithmetical problems for early morning and late afternoon work appears to be about 7 °/ₒ. One boy only in the morning group showed a loss on his preliminary record; he worked his final exercises with the following results, 6, 8, 11, 8, and then 2 for the 1st, 2nd, 3rd, 4th, and 5th test respectively. It seemed clear that some unusual factor was at work in bringing down the last mark to 2, but I am unable to suggest what it was.

2. No less than 6 boys in Group B show a loss on their preliminary record; so that we have, in the afternoon work in this subject, not only a general falling off as compared with morning work, but a fair proportion of cases in which the work appears futile.

III. EXPERIMENT IN AN INFANTS' SCHOOL.

i. *General Plan.*

A second experiment on the same general plan was carried out in an infants' school situated in a rather poor neighbourhood in London. The whole of a Standard I class near the commencement of the school year, consisting of 57 children—boys and girls—did the work. There were 4 children over seven, 1 under six, and all the rest between six and seven years of age. The average age at the end of May was six years and six months. As in the experiment previously recounted, the class was divided into two equal groups on the basis of several tests worked near the end of the morning session. Then one of the two equal groups worked similar exercises early in the morning session, whilst the other group worked the same exercises late in the afternoon session. A comparison, as before, between the work of the two groups in these final tests may indicate the degree of freshness or fatigue for work of this kind in the early morning and late afternoon for children of this age and mental proficiency.

ii. *Chronology of the Experiment.*

The four Preliminary tests were taken at 11.30 a.m. on Tuesday, May 23rd, on Thursday, May 25th, on Tuesday, May 30th, and on Thursday, June 1st, 1911. The time-table of work preceding the exercises was in all cases the same. The tests followed immediately after ten minutes' Physical Exercises which were preceded by a short Dictation lesson. No arithmetic other than that given in the tests was done on the days on which the tests were given.

The class was then divided into equal groups on the basis of the results of the given tests.

Two of the 57 children who began the experiment were absent on one or more of the days on which the Preliminary tests were given, and on this account were omitted from the equal groups. One other child, as we were left with an odd number, was also omitted, so that an equal number of children might be placed in each of the two groups.

A short Whitsuntide holiday intervened; then, after allowing a day or so for the children to become generally adapted to school work, the Final tests were given to Group A in the mornings, and to Group B in the afternoons.

The tests were in all cases administered by the Head Mistress, who had had considerable experience of experimental work.

Group A worked four tests at 9.40 a.m. on Thursday, June 8th, on Tuesday, June 13th, on Wednesday, June 14th (Thursday being a holiday), and on Tuesday, June 20th.

Group B worked the same tests on the same days as Group A, but at 3.40 p.m. instead of 9.40 a.m. Whilst Group A worked their tests in the morning, Group B had a reading lesson in another room, but with their own teacher; and whilst Group B worked their test in the afternoon, Group A took the same reading lesson in the same way as Group B had taken it in the morning. In every other respect the work of the two sections was the same during the whole period of the experiment.

The morning tests were preceded by a lesson in Scripture, whilst the afternoon tests were preceded by a lesson in Constructive Manual Work, which followed a 15 minutes' interval for Recreation.

iii. *The Tests and the Method of Marking.*

The tests set were arithmetical problems, which were called out twice clearly by the Head Mistress. It was of little use to write them down for children of this age and proficiency, since the written forms of many of the words would be unknown to them. The children were required to work the sums mentally and to write down the answers, and the answers only. The numbers involved were small since the test was intended to be one of arithmetical reasoning rather than one of numerical computation.

But it was quite impossible to ask these children to indicate how they worked the problems instead of writing down their numerical answers, as we did in the case of the Standard IV boys in the experiment previously described.

Children can give correct answers to problems with small numbers long before they can realise the processes by which they arrive at the results. To realise the process with small numbers is indeed an intervening stage on the way to working similar problems with large numbers which cannot be computed mentally. It was therefore necessary to mark their numerical results as right or wrong, and to accept these as indications of the rightness or wrongness of their methods.

It is quite possible that some children who added when they should have added, and multiplied when they should have multiplied, and so on, may yet have lost marks by incorrect computation; but the numbers involved in the problems were chosen so that almost the whole, though not quite the whole, difficulty should be of a problematic rather than of a numerical nature.

In each set of tests there were six problems—two involving addition, two involving subtraction, one involving multiplication, and one division. Each was a 'one-step' problem, and each correct solution carried one mark. It is not quite justifiable theoretically to give each problem the same mark, since they are not all of quite the same difficulty to children of this age; but it is a method of marking which is simple and practically serviceable.

The First Preliminary test follows:—

1. There were 18 trees in one field, and 4 trees in another field. How many trees were there altogether?

2. A man had 10 cows and sold 4 of them. How many had he then?

3. A man uses 6 nails to make one box. How many nails will he use to make 3 boxes?

4. There were 9 people inside an omnibus, and 5 people outside. How many people were there altogether?

5. A lady had 11 pennies in her purse, and spent 6 of them. How many pennies had she left?

6. A teacher had 12 farthings and gave them away to 3 boys, so that they each had the same number. How many did each boy get?

The Second Preliminary test contained 6 problems which were identical in principle with those of the First test, but the words and numbers were varied. Problem 1 in the first test became in the second test 'There were 12 books on one shelf, and 4 books on another shelf. How many books were there altogether?' Problem 2 in the first test became in the second test 'A girl has 9 sweets and eats 5 of them. How many sweets has she left?' and so on. Similar changes were made for each of the succeeding tests, both Preliminary and Final. They were all modelled on Test 1 so far as the 'principles' involved in them were concerned. But one further change was made from test to test. The problems identical in principle did not always occupy the same position in the tests. Problem 1 of the first test became Problem 5 of the second test, problem 2 became problem 3, problem 3 became problem 1. The *order* of the problems was varied from test to test.

iv. *Results.*

There were 27 children in each group and the maximum mark per test was 6.

I will give first the results of the groups as wholes. Group A obtained an average mark per child per test of 4·3 with a mean variation of 1·2 and a coefficient of variability (obtained by dividing the mean variation from the average by the average) of ·3 in the 4 Preliminary tests. Group B obtained an average mark per child per test of 4·3 with a mean variation of 1·2 and a coefficient of variability of ·3 in the 4 Preliminary tests.

In the 4 Final tests Group A scored an average mark per child per test of 4·8 with a mean variation of 1·3 and a coefficient of variability of ·3, whilst Group B scored 4·3 with a mean variation of 1·2 and a coefficient of variability of ·3.

I will now submit some more detailed comparisons.

TABLE I, *showing Groups A and B compared as wholes, test by test.*

		Preliminary tests				Final tests			
		1st test	2nd test	3rd test	4th test	1st test	2nd test	3rd test	4th test
Group A	Av. mark	4·5	4·0	4·4	4·4	4·4	4·9	4·9	4·9
	M.V.	1·2	1·4	1·3	1·3	1·5	1·3	1·3	1·1
Group B	Av. mark	4·5	4·1	4·4	4·3	4·0	4·4	4·4	4·5
	M.V.	1·4	1·5	1·4	1·3	1·5	1·3	1·3	1·3

The correspondence in the preliminary work, test by test, is remarkably close, Groups A and B are running, so to speak, 'neck and neck.' But in the work of the Final tests Group A—the early morning group —forges ahead and maintains about the same advantage, test by test, over the late afternoon group.

I will next submit a table which will show that this improvement of early morning over late afternoon work is common to the children of this age and class, to those well-endowed with the capacity required, as well as to those of average endowment.

TABLE II, *showing Groups A and B compared, section by section, in the work of the Preliminary and Final tests.*

	Group A			Group B		
Marks for 4 prelim. tests	No. of children	Av. mark per child per test. 11.30 a.m.	Av. mark per child per test. 9.40 a.m.	No. of children	Av. mark per child per test. 11.30 a.m.	Av. mark per child per test. 3.30 p.m.
24, 23	6	5·8	5·9	5	5·8	5·3
22, 21	5	5·3	5·7	6	5·4	5·0
20, 19	4	4·9	5·6	4	4·9	5·3
18, 17, 16	4	4·3	5·2	4	4·4	4·8
15 to 10	5	3·0	3·6	5	3·0	3·1
Below 10	3	1·3	1·1	3	1·3	1·6

The work of each corresponding section of the two groups is obviously remarkably close in the Preliminary tests, which were those worked at 11.30 a.m. And equally obvious is the difference of the work of corresponding sections when the times are changed and one group works at 9.40 a.m. and the other at 3.30 p.m. The advantage, in all cases except one, lies with the early morning workers. The lowest section of

Group B contained Edith W. whose work was exceedingly erratic, and varied very much from one test to another. In one of the Final tests she actually obtained 'full marks.'

I propose to use one more method for comparing the work of the two groups, a method which could be used by teachers to give useful indications of a balance of advantage or disadvantage, though less satisfactory from some points of view than the analyses I have already given.

TABLE III, *showing the number of sums right, obtained by Group A and Group B in the Preliminary and Final tests respectively.*

In four Preliminary tests.

Group A					Group B				
30	children	worked	6	sums correctly	35	children	worked	6	sums correctly
36	,,	,,	5	,,	28	,,	,,	5	,,
16	,,	,,	4	,,	15	,,	,,	4	,,
6	,,	,,	3	,,	11	,,	,,	3	,,
9	,,	,,	2	,,	9	,,	,,	2	,,
8	,,	,,	1	,,	7	,,	,,	1	,,
3	,,	,,	0	,,	3	,,	,,	0	,,

In four Final tests.

Group A					Group B				
53	children	worked	6	sums correctly	35	children	worked	6	sums correctly
26	,,	,,	5	,,	27	,,	,,	5	,,
7	,,	,,	4	,,	17	,,	,,	4	,,
6	,,	,,	3	,,	10	,,	,,	3	,,
7	,,	,,	2	,,	11	,,	,,	2	,,
6	,,	,,	1	,,	4	,,	,,	1	,,
3	,,	,,	0	,,	4	,,	,,	0	,,

The children in Group A worked 468 sums correctly in the Preliminary tests and 514 sums correctly in the Final tests; whilst those of Group B worked 468 sums correctly in the Preliminary tests and 469 sums correctly in the Final tests.

The early morning work produced an improvement of 12 °/。 on the Preliminary tests whilst the late afternoon work produced no improvement in the group as a whole.

v. *Summarised Conclusions.*

1. If we measure the fatigue effect by the difference in the improvement made by the early morning and late afternoon groups we should be justified in estimating it at about 12 °/。 for work of this kind, with children of this age (from six to seven years) and standard of mental proficiency. But it is almost certainly higher than this, since a large proportion of the children, especially in the early morning work,

obtained full marks and could doubtless have done more correct work had it been given them to do.

2. No improvement appears to be shown in the work of the Preliminary tests themselves which were done at 11.30 a.m. The question may therefore be raised as to whether this problematic work might not advantageously be postponed till later in the school year.

3. The late afternoon exercises seem to have produced no general improvement on the Preliminary tests. Class work of this kind with these young and undeveloped children appears to be futile at this time of day, viz. 3.30 p.m. It is important to add the limitations as to age and mental proficiency, since I have clear indications that, with much older children, if such a difference exist between early morning and late afternoon work of this kind, it is certainly much smaller.

IV. Experiment in a Girls' School.

i. *General Plan.*

A third experiment on the same general plan was carried out in a municipal girls' school situated in a very poor neighbourhood in London. The whole of one class near the commencement of its educational year, consisting of 52 children, did the work, but some irregular children were necessarily excluded. The average age of the class was 11 yrs. 3 mths., and though the general standard of attainment was described as V, it would be fairer to regard it as IV. This will be obvious to experienced teachers when they read the problematic questions which were set.

The class was divided into two equal groups on the results of 5 sets of problems which were worked toward the end of the morning session. Then after some three weeks' interval, during which holidays for Coronation festivities took place and the continuity of the school work was much broken, the exercises were resumed, but one of the groups now worked early in the morning sessions, and the other worked the same tests late in the afternoon sessions. It was expected that both groups would improve somewhat, but unequally; the difference between the improvability (if any) in the early morning group and the late afternoon group being taken, as before, as a measure of fatigue for the function in question, viz. the rational solution of arithmetical problems. It may be well if I emphasize the points of difference between this school and the boys' school in which the experiment was made which I have described above.

Both the schools were in poor neighbourhoods, but the boys were very regular and very healthy, and problematical work in arithmetic had long been a good feature of the school work, whilst the girls were poorer, lived in a densely packed slum neighbourhood in the West end of London, and could not be described either as on the whole very regular or very healthy.

Moreover the problematic aspect of arithmetic had not received as much attention as the merely computational. The girls are considerably older than the boys though they are regarded as the same school grade or standard. If with wide variations in the conditions of the experiment we get similar results, the validity of our general conclusion will be strengthened.

ii. *Chronology of the Experiment.*

The five Preliminary tests on the results of which the class was divided into two equal groups were worked from 11 a.m. to 11.20 a.m. on Wednesday, June 14th, Thursday, June 15th, Friday, June 16th, Monday, June 19th, and Tuesday, June 20th. The tests immediately followed a 15 minutes' interval for Recreation, which was preceded by different lessons on different days, but in no case by any lesson in which arithmetic was involved, either directly or indirectly. Nor was any arithmetical work done in the afternoon of the days on which these tests were given.

After the Coronation holiday and a few days in school to allow for re-adaptation to school work, the Final tests were given. One of the two equal groups—Group A—worked the tests at 9.40 in the morning sessions, immediately after the usual morning lesson in Scripture, whilst the other group—Group B—worked the same test at 4.5 in the afternoon sessions. The afternoon tests followed different lessons on different days, but each was an oral lesson of some kind: no other arithmetic lessons or lessons involving arithmetic were taken on the days on which these tests were worked, either in the mornings or afternoons.

The tests were administered by the Head Mistress, who had had a good deal of experience in experimental work. Whilst the one group was working the tests, the other group read from their Reading books in another room under the care of the usual teacher of the class. The Final tests were done on Wednesday, July 5th, Thursday, July 6th, Friday, July 7th, Monday, July 10th, and Tuesday, July 11th.

iii. *The Tests and the Method of Marking.*

The tests were sets of arithmetical problems, which were written clearly on the blackboards and read out once by the teacher conducting the experiment. The first set follows:

1. A piece of material measures 708 inches and another piece measures 571 inches. How long would the two pieces be if joined at the ends? (One rational step.)
2. If I buy 4 pounds of butter at 1*s*. 4*d*. a pound, what change shall I receive out of a sovereign? (Two rational steps.)
3. James sold 25 oranges, and Tom sold 13 more than James did; how many did they sell between them? (Two rational steps.)
4. In each of two baskets there are 221 flowers. From one basket 100 are sold and from the other 56. How many were left in the two baskets? (Three rational steps.)
5. A master paid 10 shillings as wages to 5 boys. 2 boys had 1*s*. 9*d*. each, and 2 other boys had 2*s*. 7*d*. each. What wages had the fifth boy? (Four rational steps.)

I should have preferred to ask the children to write down how they would solve the problems rather than work them numerically, for I wished entirely to eliminate the computational aspect of the work. But there was not time enough to go through the necessary preliminary exercises in order that these girls should know what they had to do and settle down into their proper relative positions for a new kind of exercise. So I was compelled to ask the girls to solve the problems numerically.

The computational aspect was, however, eliminated practically, firstly, by the extremely simple nature of the numbers given, and secondly, by the method of marking. Every step in the working which was correct in method received a mark even though the result was wrong numerically.

The second set of problems was identical in principle with the first set, but words and numbers were altered, and the positions of the problems within the set were changed. Thus problem 1 of the First Set became in the Second Set 'A passage measures 864 inches, and another passage measures 503 inches. If the passages were joined at the ends how long would they be?' It was placed as number 5 in the Second Set, and not as number 1, as in the First Set.

As teachers would say, the *same problems* were given but with different verbal settings, different numbers and varying positions within the sets.

This was true not only for the Preliminary tests but throughout the whole experiment. All the sets of tests are supposed to be of the same difficulty in so far as the rational steps in method necessary for their solution is a factor in that solution.

iv. *Results.*

First let me give the results of the work of each group as a whole. The maximum mark for each child per test was 12, and there were 20 children in each group. Group A scored an average mark per child per test of 8·9 with a mean variation of 1·9 and a coefficient of variability (found by dividing the mean variation by the average) of ·21 in the Preliminary tests. The corresponding results for Group B were likewise an average of 8·9, a mean variation of 1·9 and a coefficient of variability of ·21.

In the Final tests the average per child per test for Group A—the group working early in the mornings—was 10·2 with a mean variation of 1·4 and a coefficient of variability of ·13. The corresponding figures for Group B—the group working late in the afternoons—were an average of 9·6, a mean variation of 1·9 and a coefficient of variability of ·20. Group A improved 14·6 °/ₒ from the Preliminary to the Final tests; Group B improved 7·9 °/ₒ from the Preliminary to the Final tests. The difference between these percentages, viz. 6·7 °/ₒ, is regarded as due to the fatigue effect of working late in the afternoon as compared with working early in the morning session.

But the above summary gives little indication of the degree of regularity or of the distribution of the results of the corresponding sections of Group A and Group B. In the following tables the work of the two groups is compared, section by section, in both the Preliminary and Final tests.

TABLE I, *showing the work of Group A and Group B compared, section by section, in the Preliminary tests.*

Group A

Marks for 5 prel. tests	No. of children	Test 1	Test 2	Test 3	Test 4	Test 5	Totals
55 and over	5	11·2	12·0	11·0	12·0	11·4	57·6
45 to 50	7	8·6	9·7	9·7	10·6	10·8	49·4
35 to 45	2	7·5	9·5	7·5	7·5	10·0	42·0
25 to 35	6	3·5	5·8	6·5	6·5	7·0	29·3

Group B

Marks for 5 prel. tests	No. of children	Test 1	Test 2	Test 3	Test 4	Test 5	Totals
55 and over	5	11·2	10·8	11·6	12·0	12·0	57·6
45 to 50	7	9·0	10·7	10·0	9·9	9·6	49·2
35 to 45	2	7·5	7·5	8·0	8·5	10·0	41·5
25 to 35	6	4·3	5·8	5·5	6·7	7·3	29·6

TABLE II, *showing the work of Group A and Group B compared, section by section, in the Final tests.*

Group A. Morning group.

Marks for 5 prel. tests	No. of children	Test 1	Test 2	Test 3	Test 4	Test 5	Totals
55 and over	5	11·6	12·0	12·0	12·0	12·0	59·6
45 to 55	7	10·1	9·9	10·8	10·6	11·3	52·7
35 to 45	2	9·5	10·5	9·0	9·0	11·0	49·0
25 to 35	6	7·8	8·5	8·7	8·8	8·8	42·6

Group B. Afternoon group.

Marks for 5 prel. tests	No. of children	Test 1	Test 2	Test 3	Test 4	Test 5	Totals
55 and over	5	11·6	11·8	12·0	12·0	12·0	59·4
45 to 55	7	10·4	10·0	10·6	10·3	10·7	52·0
35 to 45	2	9·5	7·5	9·0	9·0	10·0	45·0
25 to 35	6	5·2	7·0	7·7	7·7	7·2	34·8

It will probably be an advantage if I now present in one table the summarized results of the work of the sections of Group A and Group B in both the Preliminary and Final tests.

TABLE III, *showing the work of Group A and Group B compared, section by section, in both the Preliminary and Final tests.*

	Group A			Group B		
Marks in 5 preliminary tests	No. of children	Av. per child in 5 prelim. tests	Av. per child in 5 final tests	No. of children	Av. per child in 5 prelim. tests	Av. per child in 5 final tests
55 and over	5	57·6	59·6	5	57·6	59·4
45 to 55	7	49·4	52·7	7	49 2	52·0
35 to 45	2	42·0	49·0	2	41·5	45·0
25 to 35	6	29·3	42·6	6	29·6	34·8

An inspection of the above table shows a close equality between the corresponding sections of Group A and Group B in the Preliminary tests; it shows also that every section in both groups improved from the Preliminary to the Final tests, and it shows also that the improvement in the sections of Group A is greater in every case than the improvement in the corresponding sections of Group B. The percentage improvement for the sections of Group A is 3·5, 6·7, 16·7 and 45·7, as against 3·1, 5·9, 8·4 and 16·8 for the corresponding sections of Group B.

As the maximum mark for both the 5 Preliminary and the 5 Final tests is 60 it is obvious that the highest sections in both groups had little room for improvement, but this cannot be said of the other sections. The morning work seems especially favourable to the weaker and less competent pupils. But improvement is the rule rather than the exception, even with the afternoon workers. This, it will be remembered, was not the case with the afternoon workers in the infant school class previously dealt with. With these girls, however, the difference between early morning and late afternoon work is a difference in improvement, not a difference between improvement on the one hand and a standing still on the other. Of the group of early morning workers, 16 out of 20 improve on their preliminary record, 2 stand still, and 2 go down one mark. Of the late afternoon workers 16 out of 20 improve on their preliminary record, 1 stands still and 3 go down 1, 2 and 5 marks respectively. The relation between the improvements shown from test to test is, perhaps, more clearly shown in the following table.

TABLE IV, *showing the comparative results of Group A and Group B as wholes, test by test, in the Preliminary and Final work.*

		Preliminary tests					Final tests				
		1st test	2nd test	3rd test	4th test	5th test	1st test	2nd test	3rd test	4th test	5th test
Group A	Av. mark	7·6	9·0	8·8	9·4	9·8	9·7	10·0	10·3	10·2	10·7
	M.V.	2·7	2·4	1·8	2·2	1·6	2·5	2·2	2·2	1·9	1·6
Group B	Av. mark	8·0	8·9	8·8	9·3	9·5	9·0	9·3	9·9	9·8	9·9
	M.V.	2·5	2·2	2·2	1·9	1·6	2·6	2·2	1·9	1·8	1·8

v. *Summarised Conclusions.*

The difference between the improvability in work of this kind, with girls of this age and standard of mental proficiency, between early morning and late afternoon work appears to be about 7 °/₀. That it is really higher than this can scarcely be doubted, for the problems set were of such a nature that a considerable number of the girls obtained full marks towards the termination of the tests. The possible improvement, and consequently the difference in improvement, of the upper sections of Group A and Group B was therefore unduly limited.

A striking difference between the improvement of the lower sections of the class appears to result from the difference between early morning and late afternoon work.

V. SECOND EXPERIMENT IN A BOYS' SCHOOL.

i. *General Plan.*

A second experiment on the same lines was carried out in the boys' school previously referred to, but with a different class under a different teacher.

In this case the work was done with a Standard VII class (the highest class or standard of an elementary school) of an average age of 12 years 9 months at the end of the month in which the experiment took place. The boys were very regular and very healthy, and their teacher was a vigorous young man whose methods stimulated his boys to work with energy and enthusiasm.

The class was divided into two equal groups on the results of the tests in problematic arithmetic which were given toward the end of the morning sessions. Subsequently one of the two equal groups worked similar problems early in the morning sessions, whilst the other group worked identical problems late in the afternoon sessions of the same days. As in the experiments previously recounted, it is supposed that differences (if any) between the work of the early morning and late afternoon will be a measure of the boys' comparative freshness or fatigue for mental work of this kind at these times.

ii. *Chronology of the Experiment.*

Previous to the tests whose results were taken into consideration for the purpose of dividing the class, a lesson was given by the teacher in which the boys were instructed what to do. Then several sets of arithmetical problems were given under test conditions, so that the boys might settle down into their proper position relatively to each other, and that the division of the class might be made on results which were an expression of the boys' 'true form' for this kind of work.

Then on Wednesday, July 5th, Thursday, July 6th, and Friday, July 7th, all the boys in the class, forty-nine in number, at 11.30 a.m. on each day, worked three sets of problems, one set on Wednesday, one on Thursday and one on Friday. The tests followed different lessons on each day, but in no case did any preceding or following lesson on these days involve any arithmetical work.

After the class had been divided on the results of these Preliminary tests the Final tests were given on Wednesday, July 12th, Thursday, July 13th, and Friday, July 14th. One of the two groups—Group A—worked the tests at 9.40 a.m. to 10.5 a.m. following immediately after the usual morning lesson in Scripture. The other group—Group B—worked the same tests at 4 to 4.25 p.m. on the same days. The lessons preceding the tests for the afternoon workers were, on Wednesday, English Composition, on Thursday, Reading, and on Friday, English History. No arithmetical work other than the tests was done on any of these days by the boys of either group. Whilst the pupils of Group A were working the Final tests in their own class room, those of Group B were drawing objects in the school hall, and *vice versa*. The drawings and arithmetical tests were both carefully marked and the children were told their marks next day, but no errors were pointed out to them either in their arithmetical reasoning or drawing.

iii. *The Tests and the Method of Marking.*

Sets of arithmetical problems were written on the black-board and the boys were required to write down *how they would solve them*, but not actually to attempt their numerical solution.

The set given on July 5th, the first set whose results were used for the purpose of dividing the class into two equal groups, is appended.

1. Jones caught 70 fish, Brown caught 8 more than twice as many as Jones, and Smith caught 3 times as many as Jones. How many did they catch altogether? (Four rational steps.)

2. A ship and its cargo are worth £1600. The ship is worth £700 less than the cargo. How much is the cargo worth? (Three rational steps.)

3. Tom had a bicycle which was worth £5. He sold it for that sum and afterwards bought it back again for £3. He afterwards sold it for £6. 10*s*. What was his gain from first to last? (Three rational steps.)

4. If a ship can steam 16 miles an hour against a stream which runs at the rate of $2\frac{1}{2}$ miles an hour, how far could the ship steam in 4 hours with the stream? (Three rational steps.)

5. A man and a boy had a race of 4000 yards. The man ran at the rate of 330 yards a minute, and the boy at the rate of 220 yards a minute. The boy had a start of 1000 yards and won. How many minutes did he win by? (Four rational steps.)

6. *A*, *B* and *C* had 960 apples between them. *A* had 60 more than *B*, and *B* had 70 more than *C*. How many had *C*? (Four rational steps.)

7. How many whole planks 5 feet long can be cut from 18 planks, each 29 feet 6 inches long? (Two rational steps.)

The second set of problems was similar to the first, but the words and numbers were changed. Problem 1 of the second set corresponded to problem 1 of the first set. It ran 'Jack had 50 marks in an examination. Tom had 20 more than three times as many as Jack, and Harry had twice as many as Tom. How many had they between them'?

Problem 2 in the second set corresponded to problem 2 in the first set: problem 3 corresponded to problem 3, and so on.

All six sets were so constructed that they consisted of identical problems arranged in the same order; they were *rationally* identical, that is, for the wording of the problems and the numbers used were changed from test to test.

The marking of the problems depended upon the number of correct rational steps which the boys indicated in their solutions. Every correct step which was valid on any method of working received one mark.

iv. *Results.*

It will probably be sufficient in this case if I give in one table the marks obtained, test by test, and section by section, in both the Preliminary and Final tests for Group A and Group B.

TABLE I, *showing the work of Groups A and B compared, section by section, and test by test, in both the Preliminary and Final tests.*

Group A

Marks in 3 preliminary tests	No. of boys	Preliminary tests			Final tests		
		1st	2nd	3rd	1st	2nd	3rd
Over 65	2	23·0	22·5	23·0	23·0	23·0	23·0
60 to 65	4	19·8	20·5	21·5	21·8	21·8	21·8
55 to 60	5	17·6	19·0	20·4	20·4	18·5	20·0
50 to 55	7	16·6	18·0	18·6	17·6	18·7	18·6
45 to 50	2	13·5	17·0	18·0	17·0	17·5	19·5
Under 45	4	12·7	14·3	15·0	16·0	16·8	19·2

Group B

Marks in 3 preliminary tests	No. of boys	Preliminary tests			Final tests		
		1st	2nd	3rd	1st	2nd	3rd
Over 65	2	22·5	23·0	22·5	22·5	22·5	22·5
60 to 65	4	20·2	20·5	21·5	20·8	20·8	21·2
55 to 60	4	17·8	19·5	20·2	20·2	19·5	20·0
50 to 55	7	16·9	18·3	18·7	17·7	18·6	18·6
45 to 50	3	15·7	15·7	17·3	16·0	17·0	17·7
Under 45	4	11·2	14·8	16·0	14·5	16·8	19·0

An inspection of the table will show the high positive correlation between the results of the different tests; we are obviously dealing with a mental operation or sets of mental operations which are functioning very steadily. It will show further that both groups improve from test to test, though there is a slight set-back after the interval between the Preliminary and Final tests, very slight in the case of the early morning group, more serious in the case of the late afternoon group.

If the work of the corresponding sections be compared, test by test, in the Final tests, it will be found that the sections working early in the morning are absolutely superior to those working in the afternoon no less than 13 times, are equal to them 3 times, and are inferior on 2 occasions only. In the case of one section, for one test, there is undoubted superiority on the part of the afternoon workers. The marks for the third section (the boys obtaining from 55 to 60 marks in the Preliminary tests) of Group A run as follow: 17·6, 19·0, 20·4, and for the Final tests 20·4, 18·5, 20·0.

The corresponding marks for the corresponding section of Group B are, in the Preliminary tests 17·8, 19·5, 20·2, and in the Final tests 20·2, 19·5, 20·0. The afternoon workers show marked superiority in the second of the Final tests; they score 19·5 against 18·5 scored by the early morning workers, and there is no similar case throughout the whole table.

A reference to the results of the work of the individual pupils enables us to understand it. This section of Group A contains A. S. whose marks run as follow: Preliminary tests, 18, 18, 22; Final tests 22, 16, 22. On the afternoon of Monday, July 12th, the boy was not at school, but went out with his father for the rest of the day. On Tuesday morning he dropped from 22 to 16, and did not recover his old position until the next morning, Wednesday, July 14th.

But probably the comparison between the work of Group A and Group B will be more readily apparent from the following table in which the marks for the separate tests are not shown.

Obviously, the sections of Group A and those of Group B are nicely balanced in the Preliminary tests, and if the sectionizing itself gives any advantage, it lies with Group B rather than with Group A. But, if we look at the results of corresponding sections in the Final tests, we find a balance of advantage on the side of the sections of Group A. In every section but one (which contains A. S., whose case was described above) the sections working in the early morning are better absolutely,

TABLE II, *showing the work of Group A and Group B compared, section by section, in the 3 Preliminary and 3 Final tests.*

	Group A			Group B		
Marks in 3 preliminary tests	No. of boys	Av. mark of 3 prelim. tests	Av. mark of 3 final tests	No. of boys	Av. mark of 3 prelim. tests	Av. mark of 3 final tests
Over 65	2	22·8	23·0	2	22·7	22·5
60 to 65	4	20·6	21·8	4	20·7	20·9
55 to 60	5	19·0	19·6	4	19·2	19·9
50 to 55	7	17·7	18·3	7	17·9	18·3
45 to 50	2	16·2	18·0	3	16·2	16·9
Below 45	4	14·0	17·3	4	14·0	16·8

or show a greater advance upon their preliminary work, than those working late in the afternoon. The relative improvement of the corresponding sections of the two groups may be more clearly seen in the following table.

TABLE III, *showing the percentage improvements of Group A and Group B compared, section by section, from the Preliminary to the Final tests.*

	Group A		Group B	
Marks in 3 preliminary tests	No. of boys	Percentage improvement	No. of boys	Percentage improvement
Over 65	2	·9	2	·9 (loss)
60 to 65	4	5·8	4	1·0
55 to 60	5	*3·2	4	3·6
50 to 55	7	3·4	7	2·2
45 to 50	2	11·1	3	4·3
Below 45	4	23·6	4	20·0

* This section contains A. S.

But, so far, no table has been given showing the work of Group A and Group B, test by test, as wholes, nor has any direct indication been given of the relative variability of the work of the two groups. These comparisons are given in Table IV.

Summarizing the work of the two groups, it is found that Group A scores an average mark of 18·1 (mean variation of 1·9) in the Preliminary and 19·4 (mean variation 1·6) in the Final tests; whilst Group B scores an average mark of 18·1 (mean variation 2·0) in the Preliminary and 18·9 (mean variation 1·7) in the Final tests. Group A improves upon its preliminary record to the extent of 7·2 °/ₒ and Group B to the extent of 4·4 °/ₒ.

TABLE IV, *showing the work of Group A and Group B compared as wholes, exercise by exercise, in the Preliminary and Final tests.*

		Preliminary tests			Final tests		
		1st	2nd	3rd	1st	2nd	3rd
Group A	Av. mark ...	16·9	18·3	19·2	19·0	19·2	20·0
	M.V.	2·4	2·0	2·0	2·2	1·8	1·5
Group B	Av. mark ...	16·9	18·3	19·1	18·3	18·9	19·5
	M.V.	2·6	2·1	1·9	2·3	1·6	1·7

V. *Summarized Conclusions.*

1. For boys of this age and mental proficiency the difference between the improvability shown by early morning and late afternoon workers seems to be small, namely, about 3 °/ₒ.

2. Each group contains 24 boys, and of the early morning workers 21 improve on their preliminary record, 3 remain steady and no boy goes down; of the afternoon workers 16 show improvement, 4 remain steady and 4 go down. Even with boys of this age and capacity there are, it appears, a few cases in which the afternoon work appears unprofitable.

3. The difference between the groups is small, and the fatigue effect slight, even for work which is usually considered to demand such mental activity as that involved in the rational solution of untaught arithmetical problems.

VI. General Summary.

An attempt has been made, by the direct measurement of the functions concerned, to estimate the freshness or fatigue of day school children for arithmetical reasoning at different hours during the school day.

Experiments were carried out with four whole classes, differing in age and mental proficiency, in three different schools—infants', girls', and boys' schools—in different parts of London, south-east, south-west, and north-west, but all in neighbourhoods below the average in social class.

The method employed was that of equal groups. After a sufficient number of preparatory exercises had been given to produce steady results, a number of preliminary tests were set, on the results of which the classes were divided into equal groups. Then, in each class, one of the groups worked arithmetical problems early in the morning session and the other worked the same problems late in the afternoon session.

The difference in improvability shown by the early and late working groups is taken as a measure of fatigue for the function in question toward the end of the school day.

From an experiment in an infants' school, where the work was done with 57 Standard I children, boys and girls, between six and seven years of age, late afternoon work in problematic arithmetic appears to be useless. The group of children working at the end of the afternoon session made no improvement on their preliminary record, whilst the group working early in the morning effected an improvement which was decidedly over 12 per cent.

From two experiments, one with 60 boys and another with 52 girls, being Standard IV or Standard V children of an average age of about eleven years, the difference in the improvement shown by early morning and late afternoon work of this kind appeared to be about 7 per cent. But unlike the late afternoon workers in the infants' school, the late workers of the boys' and girls' classes showed, on the whole, a considerable improvement on their preliminary records amounting to about 8 per cent. in the case of the girls, and about 11 per cent. in the case of the boys. A few of the boys, and two or three of the girls, who worked late in the afternoon showed a loss on their preliminary records.

From an experiment conducted in a boys' school with 49 boys of about 13 years of age in a Standard VII class—the highest class of an elementary school—the difference in improvement shown by early morning and late afternoon workers appeared to be about 3 per cent. The fatigue effects seem to be very slight for boys of this age and mental proficiency; though even in this class there were three or four of the afternoon workers who showed a loss on their preliminary records.

The results generally are in accord with the opinions prevailing in the best current pedagogy. Mental work involving reasoning of this kind appears to be less and less affected by the fatigue engendered by the school day as the children rise in age and mental capacity. For the older and more proficient children the fatigue effects are very small indeed.

THE FUNCTION OF RELATIONS IN THOUGHT[1].

By CARVETH READ.

§ 1. Notes on the History of this Question.

As we ascribe 'genius' to poets and artists, to scientific discoverers and practical inventors, and moreover to military leaders and organisers of industry, it seems impossible to find any further general meaning for the term, in all its uses, than 'originality.' Effective originality of course, for the insane are often original, and the eccentric; but we do not ascribe genius to them on account of their bizarre inventions, though a few of them display something like it; and on the whole, in discriminating the true stock, we are obliged to trust to time, in whose course all sorts of failures disappear—amongst them, no doubt, some who deserved a better fate. And persistent originality,—for one swallow does not make a summer; and in adolescence many youths and virgins have hopes and give promises of originality that are not fulfilled. Abundance, indeed, is a usual mark of genius, unless it is hampered overpoweringly by social or domestic circumstances, or has its career cut short by disease or death. A certain stamp of individuality is also found upon the works of genius, but much more noticeable in some departments than in others,—in literature or art than in science or invention. We may, therefore, begin by inquiring into the psychological conditions of originality, and first of all in its intellectual aspect.

Not much originality will be found in the following essay: it starts at nearly every point from the observations and speculations of others. Ribot, in the first chapter of his *Essai sur l'Imagination Créatrice*, observes that original construction depends (1) on the breaking up

[1] Being an expansion of a paper read before the British Psychological Society on May 6th, 1911.

of habitual associative redintegrations, and (2) on the recombination of their elements through association by resemblance and the active assimilation of them. "The essential and fundamental condition of creative imagination on its intellectual side, is the power of *thinking by analogy*, that is to say by partial and often accidental resemblance. By analogy we understand an imperfect form of resemblance; the similar is a genus of which analogy is a species" (p. 22). But what species? According to the older and better meaning of the word, 'analogy' is a resemblance of relations. I was thus reminded of some notes made upon first reading Spencer on the associability and revivability of relations (*Principles of Psychology*, Pt II, cc. 6 and 8, and Pt VI, Special Analysis), in which he resolves all processes of thought and perception into a classification of relations: a position confirmed by his essay on *The Genesis of Science*. In these notes, which are amongst my oldest and most faded MSS., Spencer's doctrine is extended to processes of imagination and wit. Next, an essay by Professor Woodward, in the volume dedicated to W. James, three or four years ago, led me to experiment with my classes and other victims upon the function of relations in determining the course of ideas, and so to the results that are given in this essay.

Ribot's view of the importance of analogy (as he understands it) in occasioning originality of ideas is hardly any advance upon Bain's, who often dwelt upon the importance of the "fetch of similarity" in art and science. Bain's account of the matter is obscured by an unfortunate confusion of terms: he did not clearly distinguish, or did not sufficiently bear in mind, the distinctions between association and reproduction, and between reproduction and reinstatement. That he was aware of these distinctions is plain in *The Senses and the Intellect*, c. ii. §§ 2—3 in his account of the intellect: where he shows that "the power of similarity" is involved in the reinstatement of a past impression by a present one that is wholly or partially identical with it; that without such reinstatements the repetition of experiences could not strengthen the bond of contiguity; and that associations by contiguity are restored or recalled (if strong enough) by an initial reinstatement through similarity—as when I recognise a man in the street and then remember (or fail to remember) his name. But he also says that reinstatement is "a case of the operation of the associating principle of similarity, or of like recalling like" (§ 2): not that the recall is always noticeable; for where the identity is perfect "so quick and unfaltering is the process that we lose sight of it altogether; we are scarcely made aware of the existence of an associating link of similarity" (§ 3). Of course, as James Ward has shown, there is no such link. The laws of 'association' by contiguity and by similarity, as stated by Bain, are entirely different. The former says that mental states "occurring together or in close succession tend to grow together or cohere, in such a way that, when any one of them is afterwards presented to the mind, the others are apt to be brought up in idea." This is certainly a law of association. But the latter says that mental states "tend to revive their like among previous impressions or states." This is not a law of association at all, but entirely a law of reproduction. Only the influence of earlier psychologists could have kept up this confusion in Bain's mind. In applying the principle of similarity to explain the operations of our minds, he always treats it as the means of recall and the great source of original combinations. "The force of Contiguity strings together in the mind words that have been uttered together; the force of Similarity brings forward recollections (or passages) from different times and connexions; and makes a new train out of many old ones" (*The Senses and the Intellect,* 3rd ed., p. 469). But there may be something, he says, in our present mental state similar to something in our former experience without that circumstance having any influence upon the course of our thoughts. The power of perceiving, or of being affected by, similarities in our experience may be frustrated by the faintness of the similarity, or by diversity of the circumstances in which it occurs; and the overcoming of such obstacles depends on (1) acuteness of sense, (2) previous familiarity with the subject, (3) acquired delicacy or habits of attention (two different things), (4) low susceptibility, or comparative insensibility to the points of difference, and (5) natural superiority of endowment in this function of intellect (genius). The rest of the volume gives numerous examples of the dependence of original analyses and constructions in all departments of history, literature, art and science upon the power of reproduction through assimilation. I cannot find, however, that he ever mentions similarity of relation as a ground of reproduction, although the fact is involved in many of his examples.

Although it has seemed to me an act of justice to vindicate Bain's position as to the nature and functions of this "power of similarity," I must now add that it was anticipated by Thomas Brown in his *Lectures on the Philosophy of the Human Mind* (see especially Lectures 33 to 37). He divides our states of mind into (1) the external (sensations and perceptions), excited by external things; and (2) internal, excited by antecedent states, and divisible into intellectual states of mind and emotions. Intellectual states of mind are 'suggestions': and are either (*a*) 'simple suggestions,' merely images of the past, following one another without any feeling of relation necessarily involved in them; or (*b*) 'relative suggestions' or feelings of relation, which arise when two or more objects or ideas are considered together by us. Relative suggestion is nearly equivalent to comparison; but as this seems to imply voluntary activity, and relative suggestion may be involuntary, it is better expressed by "judgment, in its strict philosophical sense, as the mere perception of relation." Simple suggestion depends upon association by contiguity, resemblance (including analogy) and contrast: but the greater part of the associations that superficially seem due to resemblance and contrast may, by minute analysis, be reduced to contiguity—perhaps all (cf. James Mill). These are the primary conditions of simple suggestion; but in every particular case they are modified by secondary conditions, such as the liveliness of the original experience, frequency of recurrence, recency, absence of divergent associations, etc.; and amongst these he gives prominence to "original constitutional differences," which give rise to "great varieties of the general power of remembering" and lend a "greater proportional vigour to one set of tendencies of suggestion than to another," and hence to what "I conceive to constitute all that is inventive in genius,—invention consisting in the suggestions of analogy, as opposed to the suggestions of grosser contiguity." This he repeats again and again: contiguity is the principle of rote and habit; all originality depends upon analogy. But by 'analogy' he only means a shadowy or indirect resemblance, and it is of two chief kinds: (1) analogies of objects which agree in exciting the same emotions, to which we owe simile, metaphor and other devices of poetry; and (2) analogies of objects that may be means to the same end, whence arise science and practical invention. Although his copious illustrations frequently point to relation as the ground of analogy, he never notices it; and even in discussing relative suggestion as the condition of reasonings, he maintains that the conceptions or elements that constitute a train of reasoning arise involuntarily by simple suggestion, and that then comes the relative suggestion of their connection, that is to say, judgment (Lect. 49).

Recently, however, the effectiveness of relations in determining the course of ideas has been recognised by G. F. Stout in an article on "Apperception and the Movement of Attention" (*Mind*, Vol. XVI.: see especially pp. 48, 51). He shows that the difference between a mere train of ideas and a train of thought consists in this, that thought proceeds under the influence of apperceptive systems, proportional systems that exist in the mind and are "adapted to apperceive objects, in other respects most diverse from each other, merely because they agree in being capable of entering into certain relations." If there is in one's mind an associated complex *a b*, such that if *a* occurs it tends to revive *b*; then if, in a train of undirected ideas, a^n (partially resembling *a*) occurs, it will also tend to revive *a b*; but if in a train of thought a^n occurs, it will "call up, not *a* or *b*, but b^n which stands to it in a relation analogous to that of *b* to *a*"; because, in a proportional system, the relations of terms to one another are "more potent to excite interest and attract attention" than the character of the terms severally. "This modified working of the principle of association is not merely reproductive. I propose to call it *proportional or analogical production*" (p. 50). In his *Analytic Psychology* he calls it 'relative suggestion'; but if the phrase is taken from Brown, its meaning has been entirely altered. Stout gives the formula: "If the presented content *b* has formed part of a presented whole *b c*, then the presented content β, when it occurs, will tend to call up a $\beta\,\gamma$ formally corresponding to *b c*" (Vol. II. p. 52).

The function of relations in determining the course of ideas has been elucidated in another aspect by Lloyd Morgan in his *Comparative Psychology*, c. 4, where he shows that a relation may be effective independently of its terms; that if, for example (to take a formal case), there is a presentation $a:b$ it may revive any other experience that contains the same relation (say) $c:d$; although *a* and *b*, respectively, have no resemblance to *c* and *d*. Many of the ornaments of wit and poetry depend upon this principle. He quotes from Shelley's *Ode to the West Wind*:

> "Drive my dead thoughts over the universe
> Like withered leaves to quicken a new birth"—

and paraphrases it: "As withered leaves form a fertile soil for new plants, so will my dead thoughts form a fertile soil for new thoughts." Such ideas "may be expressed in the form of ratios"; and the law of them is that "similar ratios or transitions suggest one another" (p. 79).

This seems to me an important truth; but one or two expressions occur in Lloyd Morgan's exposition that are questionable. He calls these complex cases "associations by perceived similarity of relationship" (p. 77); but although (as I shall try to show) they imply associations, the associations implied are indirect and depend upon contiguity; the ideas are direct reproductions, as the law above stated indicates. And, again, they are not due to *perceived* similarity; the function of reproduction operates before there is anything to be perceived: and the perception of similarity can only, at earliest, accompany the rise of the simile into consciousness. Further we would qualify the remark that this law of the reproduction of ratios, "which is the basis of poetic imagery, is also the basis of scientific insight and inference" (p. 79). That ratios should suggest one another, without any resemblance of the terms, seems to be peculiar to some of the ornaments of wit and poetry. They are *instantiae ostensivae.* In scientific insight and inference (except where hypotheses may sometimes be accidentally suggested in a fanciful way) there is always an essential resemblance of the terms, as well as of the relations, in that which is material to the inference: thus in 2 : 3 :: 4 : 6, 2 and 4, 3 and 6 are homogeneous quantities. A curious result is that poetical or witty similes, depending entirely upon their relational elements, have the best claim to be called 'ratiocinations.' Yet the relations compared may have a much fainter resemblance to one another than those have that enter into reasonings, a shadowy or remote resemblance; and when the resemblance of relations in a simile (or in the metaphor into which it has been condensed) is too far fetched or supersubtle, the figure is called a conceit; in the effort to comprehend it its poetical quality is lost, and it becomes frigid.

In Herbert Spencer's *Principles of Psychology* (Part VI—Special Analysis) reasoning, quantitative and qualitative, perception and all modes and degrees of cognition are explained as intuitions of equality or inequality, likeness or unlikeness, between terms more or less connatural. The power of comparing the relations of highly abstract conceptions or intricately complex materials depends, like the power of perceiving space and bodies in space, upon the inheritance of forms of thought acquired in the evolution of our race, and educated by our own experience. In § 299, he explains analogy as such an intuition or classification of relations, differing from more precise forms "simply in the much smaller degree of likeness which the terms of the inferred relation bear to those of the known relations it is supposed to parallel": *e.g.* as the growth of an organism is to the subdivision of its functions, so is the growth of a society to the division of labour. The relations experienced in our past life determine in concrete matters the direction of our present thoughts. In deductive reasoning we begin not with an universal proposition but with a direct inference concerning that which engages us. "On the presentation of some object (a), there is suggested to the mind some unperceived attribute (b), as possessed by it. This act is simple and spontaneous; resulting not from a *remembrance* of the before-known like relations ($A : B$), but merely from the *influence* which, as past experiences, they exercise over the association of ideas." But if the validity of our inference seems doubtful, then the past experiences that determined it and that justify it are recalled, and constitute the major premise of an argument (§ 306). I am not aware that he anywhere notices the peculiar character of these rhetorical similes in which there is no resemblance of terms; but probably it is mentioned somewhere in his volumes.

To conclude these notes,—in his *Tatsachen und Probleme zu einer Psychologie der Denkvorgänge* (I—*Ueber Gedanken*)[1] Bühler discovers three chief thought-types—'rules,' 'relations,' 'intentions.' Intentions he describes as difficult to analyse, but as appearing to consist of remembered aggregate ideas. Rules, of course, are remembered relation-formulae. Hence, if the investigation is exhaustive, relations are left as the sole type of original thought.

§ 2. How Relations come to function in Reproduction.

To return to Ribot: he says that original construction presupposes dissociations and proceeds by analogy. If there are to be new imaginations, our associative redintegrations must be broken up, the habitual series formed by them must be dissolved. And he finds that the process of dissolution depends (a) upon selective interest, by which what is useless in a series drops out of it; and (b) upon dissociation by varying concomitants, as to which he refers to Sully and James. By these processes, chiefly, the elements of habitual series are sundered and made ready for recombination.

As for the necessity of such disintegrations, it might be urged that in a great part of normal human life, especially in youth, no such strict organisation has been reached as to require disintegration, nor yet in many of the higher animals; and that, even low in the scale of life, instincts may have more than one method of attaining satisfaction. Still it must be admitted that there is a general tendency to routine, that habits of thought as well as of action (including modes of expression) are easily formed; and that, although this disposition is in many

[1] *Arch. f. d. ges. Psychol.* 1907, IX. 334–350.

ways highly serviceable, yet, so far as it is realised, there is a hindrance to the free play of imagination. The conditions mentioned by Ribot as counteracting routine are certainly favourable to originality.

Selective interest, indeed, merely of itself, would produce no high degree of originality. If one has an experience *abcdefgh*, and *bdeg* disappear, because they are relatively uninteresting or positively disagreeable, the surviving *acfh* is in some sort original; and this process is, in fact, prominent in that sort of beggarly idealisation which consists in leaving out everything you dislike; and in the kindred realism, founded upon 'documents,' in which, for want of interest, the beautiful or noble things of life have never been entered.

Dissociation by varying concomitants is incomparably more important than this elementary sort of selective interest: it is a more or less complex case of divergent association. To divergent association may be traced the simplest sort of originality: if

$$\begin{array}{ccccc} A & & & & L \\ & B & & K & \\ & & C & & \\ & H & & D & \\ G & & & & E \end{array}$$

are two experiences having a common factor C, C is a point of divergence, and either $ABCKL$ or $GHCDE$ is an original combination. Under favourable conditions either of these may occur; but from such simple experiences alone originality is not to be expected; for what shall occur after C will generally be determined by the indirect influence of its antecedents. That is to say, under the influence of AB, C is more likely to be followed by D than by K; and under the influence of GH, C is more likely to be followed by K than by D. To obtain an original combination, therefore, there must be something else capable of overcoming the inertia of either series: if, for example, there is an end in view, X, and in another former experience X has been attained through KL, then, on arriving through AB at C, and having X in view, this convergent association upon K may suffice to make the association CKL effective, against the influence of AB in favour of CDE. One may often have gone down Portland Place, across Oxford Circus, to Piccadilly; and often from Marble Arch, by Oxford Circus, to the British Museum; and with some people there would then be a decided tendency, having started on either of these routes, to finish it. But if, on the way down Portland Place, it occurs to a man that he must verify a quotation at the British Museum, he can, on arriving at Oxford Circus, turn eastward; and this will be an original excursion.

Such simple divergence may explain the irrelevant originality of weak or insane minds, uncontrolled by any effective censorship, on the occasion of casual impulses.

Only carry further such a crossing of experiences as I have figured above, and you have James's diagram of varying concomitants

from which C "rolls out," an abstract idea in quasi-isolation, ready for recombination in all possible ways. Its isolation is caused by the mutual inhibition of its associative tendencies; this delays it, tends to raise its intensity, and to make it a possible object of separate attention. Whilst "in the case of perception, the efferent discharge that adjusts the sense-organ inhibits the tendency to cortical irradiation into associated systems[1]," resulting in dissociation, delay, and sensation-intensity; in abstraction, the mutual frustration of the divergent tendencies to cortical irradiation has parallel consequences.

Now relations, in the region of thought and imagination, are a sort of abstract ideas, products of the same process of dissociation as we have described above, and affording, in fact, in the case of likeness, coexistence, succession etc., the most extreme examples of it. They become, therefore, as much as other objects of thought (qualities, or 'terms'), possible elements in any work of reconstruction. Relations may themselves be terms of other relations. They are also as fit as any other elements of experience to operate as stimuli or starting points of reproduction. Hence several writers on association have, in fact, included relations amongst its conditions under other names, such as 'concepts' or 'qualities.' In Bain's statement of the Law of Similarity, the word "thoughts" might fairly be held to cover relations. Miss Calkins, in her monograph on *Association*[2], records a case in which someone, on reading a controversial paper on psychology, was reminded of St George and the dragon, and the authoress traces the connexion between the presentation and the reproduction to the "highly abstract

[1] This *Journal*, 1908, III. 329. [2] *Psychol. Rev.* (Monogr. Suppl.) 1896.

quality," hostility. But 'relation' is at least as good a name as 'quality' is, for the hostile attitude toward an opponent as the ground of connexion.

Perhaps, before entering further into the function of relations in the reproduction of ideas, I may be allowed a digression in order to consider another consequence of divergent associations, which is also important to original thought and imagination,—I mean condensation, which has been given so much prominence by Freud in his theories of dreams and hysteria. One result of divergent associations being competition amongst the ideas that are subexcited at the point of divergence, there is mutual inhibition. Mutual inhibition of ideas may (1) produce confusion of mind and helplessness, may bring an extempore speech to a premature close, may be a cause of stammering. (2) The effect may be, and normally is, so brief as to go unnoticed: under the convergence of other conditions some one of the reproductive tendencies is realised; and the course of construction goes on from that point. But (3) after the presentation at the point of divergence has led to one of its possible reproductions, that presentation may still persevere and give rise to another reproduction. If this happens, the second reproduction may, of course, be preferred for continuing the construction in hand. But what I wish to draw attention to is that it gives occasion to voluntary condensation, which is common in all departments of art and thought. Dryden, for example, purposing to portray an ideal hero, thought of Achilles, Rinaldo and Artaban, and condensed their qualities in his absurd Almanzor. So at least he tells us in the essay *On Heroic Plays* prefixed to the *Conquest of Granada*. You remember the story of Apelles. To take a greater instance, Milton's description of Hell is a voluntary condensation of Christian ideas with those of the sixth *Aeneid*. Literary illustrations of this principle are inexhaustible.

More interesting, however, and less obvious, is involuntary condensation. This process goes on in the subconscious life, and causes the reciprocal modification of memories that every psychologist has noticed; and its effect upon dreams, as explained by Freud, seems to me the best part of his theory of dreaming. But we are chiefly concerned with condensation as directly producing original combinations of ideas. It is possible for divergent tendencies of reproduction to be effective at the same time, so far as their elements are not incompatible. When this occurs we have direct involuntary condensation; and compatible elements combine in different forms according as they are (1) congruous, or (2) not congruous. Involuntary condensation, in normal life, is characteristic of some minds, especially under the stress of emotional excitement. Of the form of it in which the elements condensed are congruous the most remarkable result is the metaphor, which is a condensed simile. Instead of saying "as a smile upon the human face, so is sunshine on the landscape at morning"—the poet says "smiling morn." This is instinctive rhetoric: the relation of "smile to face" and "sunshine to morning landscape," being the same, is a point of divergent association for both members of the simile. In a quiet mind they may follow one another, and then the simile is fully expressed: in an active or heated mind they arise simultaneously, and are condensed into the metaphor. Hence we understand why the metaphor is an earlier figure than the simile: it is for the same reason that poetry is earlier than prose: the barbarous mind has not the patience or the discipline to deploy its ideas in the dull and orderly track of explicit eloquence. The simile seems to have been, at first, merely explanatory or expository, as in *Beowulf* (l. 1457) "A light stood from his eyes like flame"; and some rationalist critics have been so mad as to maintain that the whole use of the simile is explanatory. In time its ornamental value was perceived, and it was developed for its own sake, even with amplifications irrelevant to the comparison, as in Homer. I need hardly say that the *Iliad* is, in the history of culture, much later than *Beowulf*.

When the elements of an involuntary condensation are incongruous, we get illogical combinations, bulls and mixed metaphors. Mixed metaphor may be illustrated by the best known of Shakespeare's verses,

"Or to take arms against a sea of troubles."

In Hamlet's exasperated imagination his troubles excite at the same time the ideas of a host of enemies to be fought and of an opposing tide. In a calmer mood he might have taken his choice between

"Or to take arms against a host of troubles";

and

"Or with stout arms to stem this tide of troubles";

but to such doggerel he, in his distraction, preferred the great verse that has launched a thousand footnotes.

Another digression, and a very brief one: the mention I made of 'perseveration' reminds me of another possible source of original ideas —spontaneous ideas, which sometimes occur to us without traceable antecedents. If we admit the reality of the subconscious 'perseveration' of psychophysical disturbances—and this seems to have been proved beyond dispute—we must also admit that ideas may be aroused as the effects of remote stimuli, of whose operation we have no present consciousness; and, therefore, we need not regard 'spontaneous ideas' as exceptions to the principle of association.

To return to the function of relations in the reproduction of ideas: we have seen that the simplest form of divergent association may lead to an original combination. The more numerous the divergences at any point in a mental series, the greater the opportunities of original combination. Every abstract idea is necessarily a point of numerous divergent associations; for the varying concomitants amidst which it has arisen, though by mutual competition for the next place in consciousness they tend to frustrate one another, and so to isolate their common associate, yet remain associated, and are capable of reproduction, whenever the general state of mental tension gives one of them an advantage over the others. The more abstract any element of experience is, then, the more favourable it is to the new grouping, comparison and rearrangement of ideas; and, therefore, relations, being abstract ideas, many of them highly abstract, and some (such as likeness, difference, succession, coexistence etc.) the most abstract of all, are the most favourable to new combinations, and the most likely points of original departures, whether in analysis or construction.

To illustrate this matter, let us suppose that I have reached the stage of intelligence at which relations have been relatively isolated as objects of thought, and that on different occasions I have compared A and B, C and D, E and F, and have observed that each pair was related by x, but that on each occasion I was mentally engaged in such a way as not to be reminded of the other cases; then, by 'contiguity,' x has been associated in my experience with the above three pairs of terms; and therefore if x is reinstated at any time, under conditions of mental disengagement, it tends to call up AB, CD, EF. Mutual frustration may prevent anyone of them from appearing; or they may appear one after the other—x persevering, whilst its terms vary: there is then an original series, whose members have never been brought together before. Rhetorical similes are extreme cases of this reproductive power of relations, but are the least important: the fabrication of the poet's fable (which is more important) depends throughout upon the representation of incidents and characters according to the analogy of experience, which is (for the most part unconsciously) the incentive to such reproduction, and also constitutes its *vraisemblance*; and according to the analogy of experience the hypotheses of science must also be conceived, or else they are not true; and the projects of invention, or else they are impracticable. In all these cases relations are the stimuli of reproduction; acceptance of ideas that arise in one's mind depends upon a perception of the analogy; and verification of one's ideas (where verification is required) consists in proving that the analogy—the likeness or equality of relations—holds good.

Relations are abstract ideas. But, it may be asked, are they not also facts of nature, are they not present in things perceived as well as in thought? Relations are found in nature, or empirical reality, when we are capable of recognising them there; and that is only after they have been formed in thought by abstraction. Having been formed, they assimilate the corresponding particulars (Hume's "impressions of relation"), just as the ideas of colour assimilate red, yellow, green and blue. Without ideas there is no analysis: the ideas of relation are the latest to be formed; and, until they are formed, in the growth of animal intelligence, comparison and inference can rise no higher than the stage of "practical judgment" described by L. T. Hobhouse (*Mind in Evolution*, c. 6).

Even in a civilised country, the more abstract relations are very feebly apprehended by most people; beyond a step or two they cannot follow an argument. Hence general reasoning produces no conviction, because it can take no hold of them; they suspect reasoning, and attribute their suspicion to shrewdness, experience, common sense, worldly wisdom—to anything but native incapacity. But of those who fully apprehend relations, the greater number still think and act for the most part in routine, admitting deviations only within narrow limits; similar relations, holding in separate groupings of their experience, do not readily excite one another's terms or coalesce, so as to bring together ideas that had never been brought together before. It happens to them sometimes, but not often, not profoundly: they are not sensitive to analogy, have few "fetches of similarity," such as occur to those to whom Brown, Bain and Ribot ascribe genius.

On what, then, depends this sensitiveness to analogy? It is useless to say that it is due to superior 'plasticity'; for that is just what it is, and what we are inquiring into. Is it that some men only learn abstract ideas and relations from language and social intercourse, and so never have full command of them, like the superior minds in which ideas, language and institutions have actually developed or have been renewed? But our question is—What is the difference between these two sorts of men, if there are such sorts, the learners and the makers?

It cannot have been due to greater experience, in the ordinary sense of the term, that Newton's mathematics were better than Hobbes's, or that Shakespeare's dramatic insight was better than John Fletcher's. Is it that one man compares his experiences more than another? Partly, no doubt; but this implies that his interest in the relations of things is greater: and why should that be? Or perhaps the vast mass of organic experience to which we do not attend, nevertheless registers itself in us, and contributes more effectively to the mental resources of one man than of another. That seems to me very likely; but what is the ground of this difference (of 'plasticity') in men? If greater experience, in the ordinary sense, were the cause of genius, age ought to be more original than youth: to those of us who are growing old, a cheerful doctrine, but contrary to experience: which shows that, in general, adolescence is the period of greatest originality, and that people are like pigs: the little veers show some intelligence, playfulness and amiability, but soon learn to confine themselves to practical affairs, pachydermatous in mind as well as body. All this points to something for Physiology to explain. Differences of function must be connected with differences of structure; but the psychical structure, as such, cannot be directly studied; it is always subconscious. There is, therefore, nothing for it but to appeal to the anatomy of the nervous system, presumably of the cortex. It may have been suggested that explicit relational thought depends upon some special layer of the cortex in the association areas; that where this layer is absent, relations, as such, cannot be apprehended; that as it develops and grows richer in neurones and their interlacings, plasticity or originality increases; but that as, in a system not very rich, some lines of connexion get canalised by frequent use, the rest are relatively closed; habits are formed, dulness supervenes, and the promising youth becomes a 'reliable' citizen. Does microscopic examination show any such difference of cortex between a chimpanzee and an Australian; between savages with languages of low relational structure and civilised men; between the brains of similar European children at 13 and at 17; between the brains of a 'reliable' dull man and of what is called a genius? At the age of 13—14 normal children become capable of working the highly relational theorems of Geometry: is there any development of the cortex at that age which may correspond with such an advance of mental faculty?

§ 3. Illustrations and Elucidations.

To avert misunderstandings, which on these subjects seem to be endless, let me say that the views suggested in this paper involve no doctrine of 'Association by Similarity.' That there is no such thing has been clear to nearly everybody in this country since the appearance of James Ward's articles on *Assimilation and Association*[1]. An interesting investigation by Foucault, entitled *L'Association de Resemblance*[2], gives experimental evidence that there is no association merely on the ground of resemblance. The author shows that resemblance of presentations has no effect upon association, unless it is noticed (comparison). When it is noticed, there is no longer mere association (and, if any, it is by contiguity), but a judgment; and it is not peculiar to a judgment of resemblance to strengthen associations: the same effect follows upon noticing contrast, incompatibility, cause and effect, means and end. The influence of relations upon the course of ideas, as I conceive it, is not due to association of relations by resemblance, but to the reproduction of other associations that have been formed by contiguity, with or without fully conscious comparison, through the incentive of some relation which is a common constituent of all those associations or judgments. Whether the comparison must always be fully conscious, or may be only a 'practical judgment,' I shall consider later. But that there is a strong connexion between the power of comparison and the function of relations in reproduction may be seen (as Miss Edgell observed to me) in such a problem as—to find two reds that differ in brightness as two given greys differ, or to find two tones that differ in loudness as two reds differ in brightness. The latter problem seems to be impracticable, and that is why I choose it; because it throws into prominence the distinctness of the relations involved as ideas. If we have such distinct ideas of relations, why should they not, as much as other ideas, affect the course of ideas; or, rather, why should they not have much more effect than any others, seeing that they are the most important element in every rational or imaginative process?

In quoting examples of these processes from the poets, one must feel compunction and ask forgiveness: it is as if one should catch a butterfly for exhibition and smudge its glorious wings in the handling of it. Let those who cannot endure such a spectacle turn away their faces. The following passage from *Tam O'Shanter* is a conveniently

[1] *Mind*, 1893–4, (N.S.) ii. 347, iii. 509. [2] *Arch. de Psychol.* Février, 1911.

condensed specimen of pure poetical analogy. Tam had been tasting the joy of life at his inn till it was time to go home:

> "Kings may be blest, but Tam was glorious,
> O'er all the ills of life victorious!
> But pleasures are like poppies spread,
> You seize the flower, its bloom is shed!
> Or like the snowfall in the river,
> A moment white, then gone for ever;
> Or like the borealis race,
> That flit ere you can point their place;
> Or like the rainbow's lovely form,
> Evanishing amid the storm."

Burns's thoughts are arrested by the transience of pleasure, how it quickly fades and leaves behind the dull and commonplace; and this relation of change, a partially conceived phase of causation, reminds him of all the other charming things that are shortlived, and quickly wither, or dissolve, or disappear. Except in this relation, pleasures, poppies, snowflakes and shafts of the aurora have no intellectual resemblance one to another. That they are all charming is, indeed, a condition of their being thought of by the poet, because else they would not harmonise with the mood under which the passage is written: to this point I shall return later. But their charm is not the incentive to their being thought of, or else many charming things might occur to him that were not transient. Transience, however, would sometimes be called a 'quality'; and this extension of the word 'quality' is, no doubt, one reason why the function of relations has so generally been overlooked. But how can it be the quality of a thing to cease to exist, and to give place to something else?

It is impossible to say what happened in the poet's mind when he thought of all these things; and we have, I believe, no record of how long it took him to write the passage. It seems to me probable that the mere conception of the analogies was facile and spontaneous. To compare great things with small, we all of us sometimes think of analogies, not only in composition but in conversation, when there is no time for intermediate processes; and, surely, we know that they spring up on the incentive of a relational datum spontaneously, so far as we can know anything of this sort without experiment.

The above analogies are very simple: much more complex ones might be found in the poets; but not to worry them needlessly, let us quote others without offence. Anthony Hope makes Lord Mapledurham say: "I sometimes think a lawyer's wig is like Samson's hair.

When he takes it off, he takes off all his wits with it." Here, it may be said, there is a resemblance between hair and a wig. True, but that makes no point; and a lawyer's wig *per se* never made anybody think of Samson's hair. It is the parallel between the cutting of the hair in the one case, and the taking off of the wig in the other, and their consequences each to each, that is the point of the witticism and the ground of the thinking. Does anyone suppose that this cost the distinguished novelist an effort or any mediate process of excogitation? Claparède says: "De même que l'œil enflammé sécrète une larme pour chasser la poussière qui l'irrite, de même le cerveau angoissé sécrète une métaphysique pour écarter le doute qui l'obsède." This is a double analogy: as dust is to the eye, so is doubt to the brain; and as a tear is secreted to wash away the dust, so a system of metaphysics is secreted to dissipate the doubt. Probably quite spontaneous: for one sees no mechanism, except the relational identities, by which the combination can have been brought about. A friend lately took a grease-spot out of my coat by means of a lighted match; but after the coat had been exposed to heavy rain, the spot again appeared: so I said, "You drove in the symptoms without curing the disease." This is also a double analogy condensed. Being then engaged with this essay, my attention was immediately attracted to the occurrence; and to the best of my judgment there was no mediate process. On seeing the spot again, the words 'driven in' were mutely in my mouth and throat; and then the sentence uttered itself, the idea being fully realised only with the act of speech.

From these cases it appears that a likeness of relations is enough to bring about the reproduction of terms that are similarly related: for no one will say that dust is like doubt, or the eye like the brain, or a tear like a system of metaphysics. On the other hand, none of these analogies can pretend to the character of reasoning. To reason we must have terms also that are equal or similar. As Spencer says, "In perfect quantitative reasoning, there is equality among the terms in Space, Time, Quality, and among their relations in kind and degree"; in perfect qualitative reasoning, there is "coexistence and connature among the terms along with connature and cointension among their relations"; in imperfect qualitative reasoning, the terms are never quite the same, and the relations, though connatural, are not cointense[1]. Thus in Darwin's and Wallace's extension of the Law of Population to all animals and to plants, the relations were seen to be the same;

[1] *Principles of Psychology*, § 307.

and the terms, though differing superficially, were seen to be the same in that which was material to the induction—indefinite power of multiplication. Consider the analogies by which W. K. Clifford, in his lecture on *The First and Last Catastrophe*, justified his belief that the physical world consists of molecules and ether. Light is a wave motion, because it exhibits a periodic variation in time and space like other waves; it is a motion of something that can do work, because radiant heat, having the same character as light, can expand bodies; it is not a motion of molecules, because its rate of translation is vastly greater than that of molecules; and because in passing through molecular substances (such as glass) it is retarded; and then the short waves are less retarded, as in the sea long waves travel faster than short waves; and the waves of light are absorbed by bodies (such as chlorine) whose molecules have the same vibration rate, as tones are absorbed by piano-strings having the same vibration rate. Here, again, the terms, light, heat, water, gas, sound, wire, are superficially different, but are the same in those mechanical properties that are material to the argument; and to have the same mechanical properties is the same thing as to have the same effects, or to stand in the same relations of causation. Both Darwin and Wallace, we have reason to believe, saw by a flash of insight (or "fetch of similarity") that plants and animals, multiplying indefinitely, come under Malthus's Law, and are therefore subject to the same "negative checks" as men are—war, pestilence and famine. Clifford, in his Lecture, only assembled analogies he had learnt; and each of the facts he cites had been proved by experiment independently; but the discoveries were not psychologically independent; the earlier discoveries of science determine men's minds to think of the later ones. How but by force of analogy?

Such rational analogies are not mental processes peculiar to trained and distinguished men of science. Explaining the excellence of his violin, Kubelik said: "Perhaps you may know that a steam engine can grow tired. Every man who shaves himself knows that the same thing can happen to a razor. How much easier it is for a violin! Now, this Strad of mine has never been tired. Joachim's was tired out by overplaying—by constant and wearisome vibration. But mine has been resting so long that it is better than on the day when it was made. Probably, too, it has suffered no shock or abrasion; for the wood of a violin is as sensitive as a man's brain, and can be injured as easily beyond repair." These are rational analogies, in each of which the "terms" are (*a*) molecular structures, (*b*) impaired quality of reaction;

and the relations are always the same causal process—modification of the molecular structure by friction. Probably they were intuitive analogies; but I believe in many cases such intuitions are led up to by a cue-word; which, in this instance, might be 'tired.' But how does the word do its work? Is it as a senseless vocable that happens to have been associated with 'engine,' 'razor,' 'violin,' 'brain,' and so brings them together? But (1) how came it so associated? What makes a man complain that his razor is 'tired'? Analogy. Or (2) suppose that somehow 'tired,' though a meaningless word, does bring together 'engine,' 'razor,' 'violin,' 'brain'; these things have no resemblance to one another, or connexion: the assembling of them does not constitute a thought. But suppose again, that the word 'tired' does its work in virtue of its meaning. What is its meaning? The relation of something whose activity is impaired to a former state of that something. This is the only tolerable view of how the word does the work of thought, namely, by the relational content of its meaning.

For further confirmation of our principles let us consider contrasts and antitheses. In some of the above examples (*e.g.* symptoms and disease) the terms are antithetic; and we shall find that many of the most elaborate antitheses (if not all) are similes; namely, similes of relations of contrast. The supposed law of association by Contrast has been generally rejected. That contrasted ideas re-excite one another cannot be disputed; but their association has to be explained on other grounds. Experience is a continual differentiation; and there often occur in it those arresting differences that we call contrasts or opposites; these arouse feeling, interest, attention, and so become parts of the whole moment of arrest; they are, therefore, associated, and afterwards readily reproduce one another. It would be absurd to discuss the way in which an adult mind uses contrast and antitheses without adverting to the effect of language. Whoever learns a civilised language becomes familiar with a vast irregular classification of all things and of their qualities and activities, that is to say, their relations to one another; and with various means of stating these relations. This is the background of every man's intellectual operations, the common source of platitude and paradox: for that repeats, and this denies (under special conditions) the conventional relations of words.

"Better to reign in Hell than serve in Heaven"

is a paradox to the accepted values of those regions: should an Abdiel reply—

"Better to serve in Heaven than reign in Hell," it would be a paradox on the accepted values of serving and reigning.

However original you may be when at work upon a problem, if I ask you to experiment in cold blood, and say—'Mention two opposites,' you will probably reply, 'Day and night,' 'Good and evil,' or 'Success and failure,' and so forth—all taken from the common stock. Under the influence of some recent experience, you may say 'Goodness and vice,' or 'Goodness and imposture,' but these, too, are from the common stock. For it is there implied and effectively known (though it may never have been expressed) that Goodness is opposite to Evil, and that vice is a kind of evil and imposture a kind of vice; and that whatever is opposite to a genus is opposite to all it subordinates; with other such relations and maxims concerning relations. Though disease and symptoms may not seem opposed, yet, if we may interpret popular thought by logical categories, disease comes under 'Substance' and symptoms under 'Accident,' so that they are opposed as sub-coördinates. Logically, then, we might speak of direct and indirect opposites; but whether they may not all be psychologically direct, I am not now prepared to say. In some minds perhaps, but not in others: or in some minds, in greater measure than in others, the common language, or the scheme of some special study, may be so lucidly present, as to admit of direct comparisons between all its parts—intuitive knowledge.

Simple opposites having been fixed by experience or learning, similes of opposition become possible—"As light is to darkness, so is good to evil"; and corresponding condensations, such as—"Children of light." "An English style, familiar but not coarse, and elegant but not ostentatious"—implies that coarseness is to familiarity as ostentation is to elegance; namely, the exaggeration of a certain quality of it.

No doubt, the first use of stating the opposite was to deny it in order to make the meaning clearer, as we see in primitive gesture-language. Then its power of raising the impression was felt, or instinctively used; then it was practised, and by degrees elaborate rhetorical antitheses appeared. John Lilly exploited this figure more ruthlessly than anyone else dared: "In the disposition of the mind, either virtue is overshadowed with some vice, or vice overcast with some virtue. Alexander valiant in war, but given to wine. Tully eloquent in his glosses, yet vainglorious. Solomon wise, yet too too wanton. David holy, but yet an homicide. None more witty than

Euphues, yet at the first none more wicked. The freshest colours soonest fade, the keenest razor soonest turneth his edge, the finest cloth is soonest eaten with moths, and the cambrick sooner stained than the coarse canvas." And so forth, without compunction, for hundreds of pages. It is obvious that the passage is a string of similes of contrast. The idea of a certain kind of contrast—vice overshadowing virtue—being dwelt upon, calls up the series of examples. The examples are not commonplaces directly borrowed from popular speech; from that he was protected in this and many other passages by the rule he sometimes made to himself that the opposed terms should alliterate; but in the first series of antitheses (so far as the opposed terms are really opposed) we find the indirect opposition mentioned above between a genus and the species of a co-ordinate genus: war (peace, indulgence) wine; wise (foolish) wanton; holy (impious) homicidal. As for Tully's eloquence and his vainglory, there is no opposition between them: neither in the sense of the words, nor in the experience of mankind.

One may observe, too, that the passage is a sort of reasoning, and that, accordingly, the terms of the relations, each to each, have a material resemblance as well as the relations; it is, in fact, a playing at reasoning; like so much literature, an imitation of reasoning.

The history of literature shows that the antithesis is a favourite figure with some authors and even with some ages. Next to Lilly, in our own literature, Pope, Johnson and Macaulay are its greatest masters, and even in their earliest writings it is fully at their service[1]. Does not this point to an original disposition of mind to this relational figure, congruous with the vivacious attitude in which they compose, and which is natural to them in that task, as tranquility and gravity are to others? According to Bacon, some men are naturally given to notice resemblances and others differences; and thence they are given to be interested in resemblances or differences, and become alert to perceive and to compare them. In creative minds their congenial experiences are, at need, freely recalled; and thus we get such extreme literary types as Shelley and Johnson. When a fashion has been set, the antithetic style can be in some measure cultivated, and may be generally practised; as we see it was in the 18th more than in the 17th or in the 19th century; for, on the whole, it is a prosaic and

[1] Lilly wrote *Euphues* in his 25th year; Pope the *Essay on Criticism* in his 21st; Johnson the preface to *Lobo's Voyage* in his 26th; Macaulay *On the Royal Society of Literature* in his 23rd.

expository figure, favourable to the satiric or didactic style, to ostentatious paradox, prudential qualification, and elaborate inanity. An author, alert to employ antitheses, finds them more and more readily; they become a habit, then a mannerism, and at last command the master they formerly served; so that Johnson can say that had Addison's "language been less idiomatic, it might have lost something of its genuine Anglicism," and Lilly can oppose vainglory to eloquence. This is form without matter.

As relations of causation and contrariety may be the incentives to reproduction of ideas, so may those of genus and species, size, distance, direction, etc.; but I will not discuss this matter at length, as it will appear plainly enough in the following account of some experiments.

§ 4. SOME EXPERIMENTAL EVIDENCE.

Literary and artistic construction, mechanical invention and speculative thought, all presuppose a purpose, end, or plan. It is in fulfilling such a purpose that reproduction by analogy becomes active; and the purpose controls the flow of ideas; inhibiting, for the most part, the useless, and, if they arise, determining their rejection. The author warms to his work, and ideas flow with greater abundance and felicity. When, therefore, we proceed to experiment upon the reproductive force of relations, having no purpose but to see what will happen, and being in cold blood, very original results are not to be expected, and in fact are rarely obtained. These experiments are no good test of ability: distinguished men and junior students and casuals give nearly the same sort of reactions, except so far as the better educated know more facts. But some of the experiments test knowledge; their results establish the painful paradox that if you do not know the facts you cannot think them, and that pure reason is pure incapacity.

After experimenting with my classes in 1910—11, I read part of this paper to the British Psychological Society, and induced the Society to repeat the experiments, in order that everyone might observe for himself what happened in his mind when working upon the incentive of a relational cue. It would occupy too much space to give the results of all these trials, and it would be rather monotonous: no doubt the answers of the Psychological Society will be the most interesting. Nineteen members (the majority of those present) handed in answers; but names were not asked for, and of course none will be given. Sheets of paper were passed around; the clues were read out slowly and distinctly; and ten seconds were allowed for reaction.

LIKENESS:

1. What is *like* frost in Spring?

People prematurely old	Heat in winter (untimely)
A child's death	Jelly on a warm plate (soon thaws)
Grief in youth	Dew on cold glass in a ballroom
Rain on a holiday (damaging)	Silvery bark of beeches just out
Disappointment (two answers)	Bloom on peach
Vice in a child	Moonlight on water.

(The last three or four are 'analogies' in Thomas Browne's sense of the term—picturesque associations.)

2. What is *like* an open umbrella?

A mushroom (ten answers)	A palm leaf. A shield
A sort of pine tree	A tent
Clouds held up	An insurance policy.
Half an orange-peel	

(Except perhaps the next to last, all these are entirely relational resemblances: that is, there is no likeness except in form or relation.)

CONTRAST:

3. What is *opposite to* a storm at sea?

Calm on land (eight answers)	Truce in war
Calm at sea (four answers)	A crystal watch-glass.
Calm in a mill-pond	

(The influence of the public stock of opposites is plainly shown.)

SUPERORDINATION:

4. As bird is to parrot, so is — ?

Animal to bird (two answers)	Vertebrate to cow
Mammal to cow (three answers)	Dog to terrier. 'Cat' to tiger
Mammal to horse—to dog—to tiger—to lion	Fish to sole. Flower to rose.

(In general the suggested 'Universe' vertebrate, limits the field of view.)

SUBORDINATION:

5. As solicitor is to lawyer, so is — ?

Apothecary to doctor	Fireman to civil servant
Teacher to professional man	Chauffeur to servant
Professor to teacher	Poodle to dog.
Reader to University Teacher	

SIZE:

6. As England is to Scotland, so is — ?

France to Portugal	Austria to Hungary
Spain to Portugal	Ireland to Wales
Canada to U.S.A. (two answers)	Wales to Yorkshire
France to Spain	Yorkshire to Norfolk
Prussia to Baden	India to Japan
Bavaria to Wurtemberg	China to India.

DISTANCE:

7. As Dover is from London, so is — ?

Dover from Brighton	Exeter from Bristol
Rugby from London	Birmingham from Windsor
Oxford from London	Brighton from Ware
Harwich from London	Liverpool from Manchester
Margate from London	Calais from Paris
Whitstable from Westminster	Calais from Rouen
Cardiff from Bristol	Amiens from Calais
Cardiff from Swansea	Berlin from Hamburg.

AS TO STYLE AND INTEREST:

8. Think of two musicians, of whom one is to the other as Milton is to Shelley.

Beethoven to Chopin (two answers)	Bach to Beethoven
Beethoven to Mendelssohn	Bach to Mendelssohn
Beethoven to Mozart	Bach to Debussy
Beethoven to Grieg	Bach to Chopin
Beethoven to Debussy	Handel to Mozart
Wagner to Mendelssohn	Handel to Bach
Wagner to Mozart	Beethoven to Wagner.

(Except perhaps the two last, these are all intelligible parallels.)

From Professor Woodward: As a church-organ is to a banjo, so is Hamlet to— ?

When Knights were Bold	Puck
Man and Superman	Dogberry
Sweet Lavender	Falstaff
Potted Plays	Micawber
Patience	Stocker
Tommy make Room for your Uncle	Bardolf
	Little Tich.

(Here some answers assume "Hamlet" to mean the play, others the prince. "Little Tich," however, is given under a confusion of ideas. He is not a character in fiction, but (so I am told) a popular artist at Music Halls.)

We next tried what ideas could be raised by mentioning relations without any terms. No answers were asked for, but two members jotted down what occurred to them.

(1) Think of two things *like* one another.

Ans. Egg to conic-section. Apple to orange.

(2) Think of two things *opposite* to one another.

Ans. Black to white. Sugar to quinine.

(3) Think of two things, one greater than the other.

Ans. Sovereign and sixpence. Moscow and St Petersburg.

In this experiment there were 19 subjects and 9 questions; the possible answers, therefore, were 171. The results showed:

Clear answers 120; no answers 35; unintelligible 4; confusions 10 (not counting "Little Tich"); inversions 2.

The inversions that sometimes occur are interesting: the commonest occur under Subordination and Superordination, next under Greater and Less. The causes of failing to answer, as I gather from notes occasionally inserted by subjects in their papers, are chiefly these: (1) Unpreparedness for the task. This explains the relatively small number of answers that may sometimes be given to the first question of a series. (2) Not hearing the question distinctly. (3) Not comprehending the question, either not at all, or imperfectly,—whence follow inversions and irrelevancies in the answers. (4) Slowness of reaction under limit of time. (5) Want of relevant knowledge. (6) Writing nothing, because the answer that occurs to one seems not good enough. (7) Writing nothing, because though ideas occur, words do not within the time assigned: this is mentioned several times.

Other experiments, in which no limit of time was imposed, showed, of course, very few failures to answer; but it cannot be said that the quality of the answers was any better than when only 10 seconds were allowed; although the time taken varied from 5 to 110 seconds, and averaged 32 seconds. In fact, the limit of time seems to be a stimulus calling forth greater concentration and, in some cases, an activity of thought more comparable to that of original composition.

Supplementing class-experiments by personal experiments, followed by introspective accounts[1] of what happened, we get, as usual in such work, the most curious and various pictures of the different subjects' mental processes. The results show that, in many cases, the relational

[1] My colleagues, Professor Spearman, Mr Flügel and Dr Wohlgemuth, have helped me in this and in other ways; and I must also thank Mr Clifford Sully, Mr T. Whittaker and several students and friends.

form of the question appears to introspection directly to determine the answer; but that this is by no means always true; for sometimes methodical and roundabout devices are adopted in order to arrive at a very simple issue; and although 'beating about the bush' takes a wider range when there is no limit of time, there may be time for a good deal of it in 10 seconds. Of course, when the task occupies more than a few seconds, the greater part of the mental processes involved in it cannot be recovered by introspection.

The relations that are most simply and directly reproductive of ideas similarly related, appear to be those of size. Here are some answers and introspections that illustrate the matter.

Q. I. (No limit of time.) *As an ox is to an elephant, —?*

Ans. 1. "So is fly to bluebottle. Expected three members of the proportion to be given, consequently did not respond at once. On falling to the task, process consisted of hearing and understanding the question; willing to answer it; then, without further conscious intermediation, dwelling on the relation of size. This was perhaps aided by images (1 to 2 p.c. of full percept[1]) of the ox and the elephant. (If I had not known which was which, their intrinsic character would not have been sufficient to identify the objects they stood for.) The idea of relative size became distincter; though the images, I think, disappeared altogether. The idea was not numerical, geometrical, verbal, or in any way represented by any conscious content. Then came the idea of a fly, with a 2 p.c. visual image of it. Then I willed to think of something in the same relation of size to it as an elephant to an ox. At once, with no further conscious intermediation, thought of a bluebottle." (Time 15 seconds.)

Ans. 2. "So is terrier to goat. Concentration on task for some time, till there were clear visual images of ox and elephant, together with conception of the proportion between them. Images gradually faded, leaving the conception. Searched for two animals with same relative sizes; trying first to get one animal, and then another larger or smaller by the right amount. Tried this plan several times without success. Finally terrier and goat suggested themselves together as a suitable pair; and after comparison with the other pair reaction followed." (Time 17 seconds.)

[1] That is, according to this subject's ingenious device, the intensity of the image is estimated to be about 1 to 2 p.c. of that of a percept.

Ans. 3 shows a curious substitution of degree for size. "So is a policeman to a magistrate. I was not quite prepared; but on realising that the question was completed, immediately the absence of great contrast struck me. I had a faint visual image of an ox, but nothing similar of an elephant; and the relative power of a policeman to that of a magistrate occurred to me immediately." (Time 10 seconds.)

Q. II. (Time allowed 10 seconds.) *As a marble is to a cannon-ball, — ?*

Ans. 1. "So is a child to a man. The roundness of the things suggested a baby's limbs, and so a child. The child and the marble both seemed capable of growth."

Ans. 2. "So is a mouse to an elephant. Saw a striped glass marble lying next a huge grey ball. It seemed humorous or freakish; and the next moment I saw in their places a mouse and an elephant."

Ans. 3. "So is a pea to a potatoe. The words 'so is a pea' occurred immediately, and were connected with (1) 'to a bean,' which was felt to be too small; then (2) 'to a melon,' which was felt to be too large; then (3) 'to a potatoe,' which was accepted. All the objects were imaged."

In all these cases, either the third and fourth terms of the proportion are thought of together automatically; or, if the third term is traceable to some association, the fourth term is selected from amongst all its possible associates by the relation. For, apart from the relation, there is no reason why 'fly' should suggest 'blue bottle'; 'child' 'man'; 'pea' 'bean'; 'melon' 'potatoe,' rather than anyone of a score of other things.

Problems of Distance are usually solved by visualising the map, but not always:

Q. I. As to Distance. *As Berlin is from Dresden, — ?*

Ans. 1. "So is London from Peterborough or Rugby. Visual image of map with Berlin and Dresden and country between them. Search for two towns similarly situated in relation to each other. Decided to start from London. Suddenly changed procedure to a consideration of the time of travelling from one town to another. Auditory-motor image—'Berlin, Dresden, 2½ hours.' Peterborough arose; then Rugby. Did not go into the reasons for this, but thought that I could if necessary. Reacted whilst still uncertain which town was the better." (Time 5 seconds.)

Ans. 2. "So is Paris from Berne. Heard and understood the task: willed to do it. First dwelt on the distance. 1 to 2 p.c. image of a map came to mind. Then incipient ideas of the time by rail. I intentionally inhibited these, and then got a quite imageless, yet fairly distinct, idea of the distance. Then willed to find a corresponding distance. Paris came up at once as a starting point; then S.E. as the direction. Then S.E. France came up, and then Switzerland and Berne. Accepted Berne without much confidence in the geographical accuracy." (Time 15 seconds.)

Ans. 3. "So is London from Birmingham. On hearing the task, knew at once I was ignorant of exact distance. Berlin, as capital of Germany, suggested London, capital of England; and then Birmingham occurred with '118 miles.' This was even accompanied by a faint visual image of an open A B C time table and a fainter auditory image of 118." (Time 5 seconds.)

[The A B C says "from Euston 113 miles": the distance from Berlin to Dresden Central is 112. Yet there was no conscious comparison. The subject may have once known the distance from Berlin to Dresden; and that knowledge, though it had become latent, was still capable of influencing his thoughts.]

Q. II. *As Moscow is to Rome, —?*

Ans. 1. "Saw a map of Europe. First located Moscow and Rome; then found London and looked around for some noteworthy place at the right distance. Failed to find one in the time given. Distance across the sea bothered me, as if it were not to be reckoned in the same way." (Time 10 seconds.)

Ans. 2. "So is Rome to Cork. Saw the places mentioned in question on a map, and said (1) 'Rome to Cadiz,' then saw that that was not far enough, and so (2) (as it were) groped my way to Cork." (Time 10 seconds.)

Relations of superordination and subordination give quick reactions, and usually commonplace results, as if the subject were working in a familiar routine. I give one or two examples of the commonplace, and others of the more original kinds.

Q. I. Superordination: *As Greek is to Athenian —?*

Ans. "So is animal to cat. Did not immediately fully realise the meaning of 'superordination,' when told that this would be the next task. Remembered all about the experiments on Superordination; knew I could recall the exact meaning, and even had a vague meaning present, but not clear enough. I therefore dwelt on the meaning. When the task was given, I had no further doubts about the meaning. 'Animal' came up at once, quickly followed by 'cat.'" (Time 7 seconds.)

Q. II. Subordination: *As king is to sovereign, — ?*

Ans. "So is picture to decoration. On hearing what was the kind of task tried to prevent myself from anticipating, and succeeded. On hearing and understanding the particular task, immediately had an impulse to repeat the former response [under superordination], merely inverting it. Inhibited this. Sight of picture on table suggested picture for one term. Picture + idea of 'more general' evoked 'ornament.' After some critical hesitation, I responded accordingly." (Time very short, but not exactly recorded.)

Q. III. Superordination: *As bird is to sparrow, — ?*

Ans. 1. "So is reptile to crocodile. Immediate reaction, words and thought coinciding, as soon as the task was heard and understood."

Ans. 2. "So is unity to complexity. Saw a diagram, and had a sense of many little points converging towards one point." (Time under 10 seconds.)

Ans. 3. "So is lion to feline genus. Heard the question reversed. Saw diagram illustrating relations of part to a whole. This became metamorphosed into picture of lion surrounded by a crowd of other specimen of his genus: the lion in bright light, the crowd in shadow." (Time under 10 seconds.)

To the stimulus of contrast or opposition, there is usually an immediate reaction; but if the problem contains complex data, the first reaction may not be logically satisfactory. Then if time is allowed, an analytic process sets in with a series of tentative solutions. The occurrence of these tentatives, and selection from amongst them, depends on the clearness with which opposition is conceived, and on the resources of the subject's knowledge, whether recoverable or latent.

Q. I. *What is opposite to ostentation of wealth ?*

Ans. 1. "Modesty in poverty. The question immediately called up the German verses:

> Dem kleinem Veilchen gleich, das in verborgenen blüht,
> Sei immer fromm und gut, auch wen Dich niemand sieht.

Ostentation has modesty as an opposite, and wealth poverty. But as the answer was being put down quickly, in order to have a short reaction time, it struck me that it was not correct, but that 'modesty in wealth' would have been better." (Time 10 seconds.)

Ans. 2. "Ostentation of poverty. Heard and understood the question. At once responded inwardly 'unostentation of poverty'—no conscious intermediation that I can remember. Then came the idea that two negatives cancelled one another; and then the answer 'unostentation of wealth'; but this seemed too poor. Then I reflected on the exact meaning of opposite, and wandered off into 'contraries' and 'contradictories.' Then came the thought of negativing the 'wealth,' which seemed to give a less trivial response." (Time 30 seconds.)

Ans. 3. "Concealment of poverty. Meaning of 'opposite' arose gradually, monopolised attention for some time, and soaked in slowly. Then turned attention to 'ostentation of wealth.' Opposite of 'ostentation' first thought of—concealment; then opposite of 'wealth'—poverty. Considerable difficulty in getting full meaning of 'ostentation of wealth' in focus at one time: therefore contented myself with the opposites of 'ostentation' and of 'wealth' taken separately." (Time 23 seconds.)

Ans. 4. "Concealment of wealth. Thought first of ostentation of poverty, but rejected it. Only vague notion present, no imagery, no conscious process. 'Concealment' only roughly expresses the meaning—deprecation of the idea that one is wealthy." (Time under 10 seconds.)

Ans. 5. "Secret humility. At first, I asked myself—'Opposite in one way, or in both at once?' Did not think in words. Is the opposite—'ostentatious asceticism,' or 'wealth without ostentation,' or 'wealth determinedly masked'?—this seemed to be before my mind, but not definitely. Then came the answer." (Time under 10 seconds.)

Ans. 6. "Ostentation of poverty; which includes a deep spiritual arrogance. Answer followed the question with no interval. On reflection, I am not sure whether one kind of ostentation can properly be opposed to another." (Time under 10 seconds.)

[Plainly the real opposition is of "deep spiritual arrogance" to shallow worldly arrogance. The thought was just.]

Q. II. *What is opposite to dignity of demeanour?*

Ans. 1. "Self-abasement. Sensations as of stooping down to pick up something out of darkness became translated into self-abasement." (Time under 10 seconds.)

Ans. 2. "Shamefacedness. This word occurred to me at once. I had a sense of its inadequacy, the need of adding some situation, which was represented by the word 'in,' but nothing further presented itself before the time was up." (Time under 10 seconds.)

This evidence seems to me to show plainly that the reactions are not simple associations, but are essentially determined by the idea of 'relation' involved in the task.

Reproduction by relations of likeness (simile in the ordinary sense) exhibits several degrees of complexity. The simplest case is the reproduction of similars by the presentation of a single object, such as may be illustrated by a case given above: 'What is like an open umbrella?' All the answers (except the last) mention some other single object; but this object (except 'tent') is not like an open umbrella in anything except form; that is to say, it is a complex of relations of distance and direction between various parts of the object, which on the whole is like the complex of such relations presented by an open umbrella. It involves the same mental process as the recognition of a photograph; which is not like the original in anything, except the distances and other relations between the features and lines of the face.

Next we have condensed analogies, such as—"What is like frost in Spring?" "Grief in youth." That is to say, as frost is to spring so is grief to youth: both seem untimely. For although I put this case second, it is not really simpler than such explicit analogies as "Two musicians who, for interest or value, are to one another as Milton to Shelley?" "Beethoven to Chopin." A further stage of complexity is reached in the example quoted from Claparède (p. 358).

Experiments with introspection show that, in the condition of 'cold blood' prescribed in a laboratory, the reaction is sometimes direct and sometimes roundabout. I give some examples.

Q. I. *What is like a yacht at sea?*

Ans. 1. "An aeroplane in the air. Confusion at first: repeated sentence several times before understanding it. Visual images, whose vividness seemed to inhibit thought. Answer came almost automatically from a state of considerable tension. I thought a yacht at sea is serving its purpose; so is an aeroplane in the air. (Had seen an aeroplane in the morning.)" (Time 7 seconds.)

Ans. 2. "Professor at a Board of Studies. Heard and understood the question. Had a 1 p.c. image, but a clear idea, of a boat tossing on stormy waves. It struck me that the task did not require the sea to be stormy; then the counter thought that there was nothing against it. Tried to react accordingly. Have no confident memory of the remaining conscious process; but think it merely consisted in attending to the boat on a stormy sea; and in the idea being followed by that of University teachers at a stormy meeting of a Board. Then came the volitional adoption of this idea, and the response." (Time 20 seconds.)

Ans. 3. "A soul enjoying life. I saw a small sailing boat on a calm sea, the sun shining. Immediately remembered a passage in Lytton's *Pilgrim on the Rhine*, where a boat vanishing behind a cape was likened to continued existence after death. With this I had a sense of the tranquillity and contentment of a life on such a vessel, free from care. A strenuous effort to find a concrete comparison failed. The gliding, the passing on of a life free from care returned to my mind, and prompted the above answer." (Time 60 seconds.)

Ans. 4. "The soul of man on the ocean of life. This commonplace simile instantly occurred to me; and so completely filled my mind as to exclude every other idea. I had too the impression that, though commonplace, it was so truthful that one might well entertain it. Answered accordingly." (Time under 10 seconds.)

Q. II. *Mention two dramatists who are to one another as Scott to Thackeray.*

Ans. 1. "No parallel. Heard and understood the question. First dwelt on my ideas of Scott and Thackeray. The former developed into simple, romantic, historical description; the latter into somewhat satirical actualism. Then I turned to the dramatists. Several came up, but I could find none in the least like either of the novelists. Process consisted essentially in trying to find dramatists *like* the novelists, not in directly using the relation between the novelists." (Time 10 seconds.)

Ans. 2 and 3. Pursued the same method and obtained no result. (Times 60 seconds and 10 seconds.)

Ans. 4. "Shakespeare and Ben Jonson. This occurred at the starting-point. The idea was (1) a contrast between largeness of reference—not merely the interactions of a few particular persons, but social and historical values—and, on the other hand, a world more limited to private interests (in the comedies), viewed by a satirist with a moral purpose; and (2) the thought that Thackeray, like Ben Jonson, is describable as a realist, whilst Scott, though to a less extent than Shakespeare, is an idealist." (Time under 10 seconds.)

Q. II. *Two painters as Mr Browning is to Mrs Browning.*

Ans. "Michael Angelo to Raphael. The relation presented itself to me as one of superior force with inferior grace; and then the painters immediately occurred to me—together with a sense of ludicrous disproportion. There were sketchy images that meant pictures, but none in particular." (Time, under 10 seconds.)

Q. III. *Mention two musicians (or two poets) who are to one another as blue to red.*

Ans. 1. "Brahms and Strauss. Dwelt on the ideas of blue and red. Idea of Strauss came up, and seemed distinctly red. Dwelt a long time on blue; at last Brahms came up. These were the only two that came up at all. There was in each case an intermediate idea between the colour and the musician, a very curious, vague, *Einfühlung*; it was objective, but the carrying object was quite indefinite." (Time 30 seconds.)

Ans. 2. "Debussy to Tchaikowsky. Began to analyse blue and red as to their feeling-tone. Blue was soft, reserved, cool and rather sad; red was brilliant, fiery and noisy. Thought these terms must be applicable to musicians. Had now a clear conception of the task, but found it very difficult. I seemed to hunt for a pair of musicians standing in the required relationship rather than for individuals. Debussy and Tchaikowsky arose. Debussy's reserve (seldom *forte* passages) harmonised well with blue; though I would rather call him 'green.' Of Tchaikowsky I seemed only to have a vague idea that he often wrote noisy and tumultuous music. Searched in vain for a better pair." (Time 85 seconds.)

Ans. 3. (Poets.) "Shelley and Swinburne. This was what first occurred to me." (Time under 10 seconds). The subject gives this curious explanation of the answer. "A long time since I heard some-one quoted as having said that the relation of Swinburne to Shelley suggested a metempsychosis. This was dismissed as fanciful; but in thinking it over, the idea occurred: if this was so, we might explain the difference between the two men by supposing that the soul had descended a stage by assuming a fiery instead of an ethereal vehicle."

Q. IV. *Two poets as grey is to green.*

Ans. "Wordsworth to Moore. The process consisted in finding first a grey poet. Cowper was thought of, then Wordsworth, and that seemed better. 'Green' suggested Moore explicitly, after two or three vague ideas, amongst which I believe Shelley was one, but not clear, Moore seemed exactly right (probably associated with the emerald isle)." (Time under 10 seconds.)

These answers I select from a greater number as typical. Some of them show clearly the direct power of a relation to raise a simile. Others exhibit indirect methods; and these are of two varieties: (*a*) Two things (a colour and a musician) may be thought of as similarly related to two other such things, because their absolute affective impressions are alike. From all one's experience of Strauss an absolute affective impression has been formed, and so of 'red'; and these impressions agree: and so with Brahms and 'blue.' Therefore, it is assumed, some same relation must hold between the musicians as between the colours: if A feels like C and B like D, then as A is to B, so is C to D. (*b*) The attempt is made to establish a parallel on a tacit *assumption* that, if two things (a novelist and a dramatist) can be described in the same terms (such as 'historical,' 'sentimental,' 'satirical'), they must stand in the same relation to two other things (novelist and dramatist) that can also be described in the same terms. If A can be described in the same terms as (or is qualitatively like) C, and B in the same terms as D, then as A is to B so is C to D. We may learn from this how profoundly relational systems determine our thoughts even without our being conscious of them. Here we see the influence of an assumption concerning proportion amongst qualitative terms, which has never become a recognised intuition and has never been formulated. But this need not surprise us; for in the same way it was assumed during many thousand years by everyone who used a standard measure—his fingers for number, or a cubit for length—that magnitudes equal to the same magnitude are equal; yet no one had thought it *in abstracto*, nor formulated it.

Still, it will be acknowledged by everyone that it is by no such mechanical procedure as we see revealed in some of these introspections (all by genial minds) that similes and analogies occur to us in the absorption of thought, or in the warmth of conversation. The reason is that when we are already interested in any subject, the mental conditions already exist, which these mechanical methods seek, however slowly and imperfectly, to establish: if we are discussing music or literature, ideas of the circumstances, qualities or aesthetic value of Brahms or Scott, are already present or sub-excited; and a state of tension exists, which is relieved from time to time by sudden combinations of ideas that have the same relations; and such experiences have always been described as 'flashes' of insight, or of wit, or of imagination—immediate and spontaneous.

Ideas that seem fanciful and trivial may be deeply bedded in analogies. One subject wrote: "As blue is to red, so is Shelley to Shakespeare," and added no introspective report. So the following dialogue took place—quite fluently—and was immediately written down:

Q. "What do you mean by saying Shelley is like 'blue'?

A. Like the sky: ethereal, subtle, refined, remote from common things.

Q. But these words have quite different meanings as applied to poetry and to the upper air.

A. Yes; Shelley's poetry is to common things as the sky is to the earth.

Q. But for my part, if I say 'Shelley is blue,' it seems to me to mean that the poet and the colour give me directly the same feelings. Though I do not see why; for 'blue' is cool and soothing; Shelley is not. If I really mean what you do, it must be subconsciously.

A. So it was with me, until you asked 'why.' Similarly I might say Shelley is greenish—meaning connected with the world of things, not merely with man: who is red, the colour of blood, stimulating, hot."

This subject's thoughts are deeply bedded in analogies: that is clear. But whose are not? The dullest man uses the common language, though not in an all-renewing and vitalising way; and it is well understood that language is made of metaphors. There is nothing new in this.

§ 5. Further Considerations.

Relations are abstract ideas, which, once established, assimilate the 'impressions' involved in the kind of experiences from which they have been abstracted; and so we find likeness, difference, change, direction, etc., in things perceived. Some particular impressions of relation may then be used as symbols of the abstract ideas; and they serve the purpose with more or less adequacy, according as they are directly involved in, or only indirectly connected with, those ideas. Lines to represent distance and direction are the most adequate, and combinations of them for size and shape. Since it takes time to traverse space, and since a body actuated by a force traverses space, lines may also be used to signify time and force; but when we come to the psychology of time and force, such symbols are extremely misleading; for they contain only one (and that the most objective) element of our immediate experiences of time and force. If they do not always mislead, it is when they are not suffered to substitute themselves for their meanings. Logical relations of genus and species are frequently represented by diagrams in which the genus is shown above its species. From of old, the transition from species to genus is the way up, and from genus to species the way down. It is a point of dignity. The power of Abstraction 'raises' man above other animals, and makes him akin to the gods who dwell 'above.' In fact the dignified hold themselves 'up'; they are often to be seen 'up' on a dais, or throne, or other eminence; and so the genus is 'up' and the species, and still more the vulgar particulars, are 'down.' The diagram is convenient for some purposes; but in both metaphysics and psychology nothing can be more grossly misleading. It represents the genus as existing apart from the species; and substitutes space-relations for relations of likeness and difference. With likeness and difference we come to ultimate experiences of cognition; which, for that reason, cannot be described or defined; and the thoughts of them, because the most abstract of all thoughts, are the least capable of adequate symbolisation. Besides every symbol is an object, but these relational activities are the sheer subjectivity of all cognition. Yet there is a strong impulse to symbolise them. One of the subjects who furnished me with an introspection of the thinking of opposites, reports that he 'sees' opposites as a line with a small circle at each end. Perhaps many people 'see' likeness as the mathematical symbol of equality, =. But this presents equal things to be compared; it has no resemblance to the cognition of equality. Spencer's symbolic device for representing reasoning, involves the same error $\left.\begin{matrix} a \\ : \\ b \end{matrix}\right\} = \left\{\begin{matrix} c \\ : \\ d \end{matrix}\right.$. This device presents to us things compared, not comparisons, nor inference. In inference the relation of a to b persists, whilst a and b lapse and c and d take their places.

To return to the conditions of original thought and imagination: we have seen that genius consists in (1) an unusual power of "thought by analogy"; understanding by analogy—scientific ratiocinations, inventive transfers of mechanical principles to new uses, the fable-making of poets and other artistic imitations, allegory, simile, antithesis, metaphor and other condensations. All think and imagine in this way, but some with far superior subtlety and reach. And all make mistakes and false analogies, supposing the relations of their materials to be the same when they are different. The causes of such a neglect of differences are: (i) absorption in the purpose to be served by the ratiocination or construction, which attitude favourably entertains whatever promises to help the fulfilment of our purpose, and inhibits the thought of whatever is discouraging; (ii) inability to perceive the differences that exist, through not having the concepts that are necessary to assimilate those differences.

(2) Another condition of genius is knowledge. It is true there may be immense stores of knowledge without genius; but no potential genius, no mere capacity of thinking by analogy will avail a man who does not know the relevant facts. The contemplation of light will never enable a man to think of it as a wave motion, by analogy with sound or with the waves of the sea, if he does not know that sound is propagated by atmospheric waves, or if he does not know what a wave is, and supposes that a wave of the sea is a lateral translation of the water; or, again, however much he may admire Goethe and his susceptibility to new ideas, that will never make him think of Memnon's pillar, if he never heard of Memnon's pillar.

(3) Genius, then, is not proportional to knowledge; but knowledge sets a limit to the activity of genius. What, however, is the nature of the knowledge that genius can draw upon; is it the same that serves other men, or can genius draw in greater measure upon resources from which the ordinary mind is cut off?

The sensitiveness to analogy that distinguishes genius must (it seems to me) be supported by extraordinary power of registering experiences (in general, or of some special kind), perhaps without consciously attending to them, or but slightly noticing them, in such a way that, although incapable of being normally reproduced, they influence by analogy future constructive or analytic processes. Such power of registration may be extraordinary in geniuses, but in some degree it is common to all men; and possibly all their experiences are registered in all men, and the difference of genius may lie chiefly in the availability of (normally) unreproducible experience for modifying future processes.

The evidence for this position is as follows: (*a*) We all know what it is to become aware that something has happened which, at the time, we were not fully aware of: the favourite example is the striking of the clock. If this be put down to perseveration, what is the durability of the impression left by it? May it not have the same quality as the traces of what has been learnt by rote, rapidly declining in vivacity and completeness, but for long periods, perhaps for life, leaving effects, which, in the case of matter learnt by rote, can be shown by greater ease in relearning? E. W. Scripture's experiments[1] go to prove the influence of unnoticed circumstances upon the reproduction of ideas.

(*b*) We may see the effects of unrecoverable and (as far as memory goes) not specially noticed experiences in the formation of abstract ideas; which do not depend upon our remembering the 'varying concomitants' by which they were gradually isolated. Total, aggregate, cumulative ideas show the same process: they bear the traces of many experiences that cannot be recovered, and as to many of which it does not seem probable that much attention was formerly given to them. After having been in a room, I may retain for a time a picture of its size, shape, and arrangement of furniture, in which I certainly felt no interest. When this picture has faded, will not some effect of it remain?

Consider, too, the nature of an 'absolute impression,' which may be all that remains of vast contexts of experience: *e.g.* the impression that remains of the style of some author, such as Carlyle, of whom one may not be able to quote a single sentence. This impression is made up of multitudes of experiences of all degrees of interest; some that were intensely dwelt upon, but many more that were rapidly hurried through and taken as a matter of course. Such is the *Platonisches Gefühl* or *Shakespeareanisches Gefühl* that enables the happy owner to recognise or reject a dialogue or drama by intuition, and saves the trouble of collecting evidence. These impressions, by the way, are formed by the compounding of relations—such as orders of construction, grammatical preferences, cadences,—when no terms (single words) need be remembered.

The force of burlesque depends entirely upon 'absolute impressions': as, in *Rejected Addresses, the Baby's Début,*—mimicking the feebler sort of Wordsworth; or *Cui Bono,*—aping the 'Byronic' Byron, without in either case borrowing one phrase from the originals.

Most imitations and most of the plagiarisms of literature are due to these absolute impressions, and to the resurgence of turns and cadences, sometimes words and phrases, that were not recognised as to their source, or else they would have been rejected. Milton's plagiarisms from *The Faithful Shepherdess* in *Comus* and from *Christ's Victory*

[1] *The New Psychology*, London, 1897, ch. 13.

in *Paradise Regained*, are doubtless of this kind. In reading Charles Lamb one is often reminded of Sir Thomas Browne, though unable to recall a single illustrative passage; and probably Lamb knew that he was imitating Browne, but not any particular passage.

So with painters, musicians, actors and all sorts of artists.

(*c*) There is, moreover, abundant evidence in favour of my suggestion to be drawn from various forms of hypnotic and quasi-abnormal phenomena. It is admitted that under hypnotic and hypnoidal conditions and in states of 'abstraction,' many experiences can be recalled which the ordinary waking mind cannot recall; and Morton Prince has shown in cases of dissociated personality, that many circumstances in a given situation, that escape the notice of the subject in possession at the time, may be fully observed and remembered by the other co-conscious subject: they are therefore organically registered. He reports[1] the introspective analysis of *B*, a secondary personality split off from *C*, to the effect that nearly everything that happens is perceived by some part of the mind. If *C*, being at a certain time dominant, is attentive to something, reading a book or what-not, all other things are shut out from her; but *B* perceives them. *C*, again, does not remember her dreams; but *B* knows them to be very vivid, and also knows what happened in the house at the same time. Is it then unreasonable to suppose that normal subjects are affected by many experiences that they do not notice, and others that they very faintly notice, as well as by those they attend to; that such experiences are, in some measure, registered, so as to influence future reactions of the organism; and that in some men these experiences are, in an extraordinary degree, influential or even capable of recall? Each man, however, having his own intellectual character, is influenced chiefly by experiences that nourish that character—in whose intense activities they are re-awakened.

The foregoing discussions in §§ 2, 3, 4, assumed nothing of this, but rather that if (say) *AB*, *CD*, *EF* were all capable of revival through the common relation *x*, it was because they had all been observed in that relation and compared. That is a needless limitation. A relation is potentially present, though not yet discriminated, in a practical judgment; in which *AB* or *CD* is perceived, or thought of, as a connected whole, though the manner of connection remains obscure. Such a judgment may be subsequently raised to the status of a relational judgment in the very process of thinking or imagining; for example,

[1] *Journ. of Abn. Psychol.* (Dec. 1908—Jan. 1909).

the practical judgment *AB* (formerly established), having the same potential form as the relational judgment *CD*, may on the occurrence of *CD* be assimilated by it, and thereby the latent relation x, which it contains, may for the first time be perceived. The development of relations by abstraction implies this, for they arise from contents in which they do not (as such) exist; and it is only as they are abstracted that it becomes possible to analyse the contributory experiences. My hypothesis is that the shaping of present consciousness by the long accumulating influence of forgotten experiences, and the revivifying of old, faint, dormant records by present analogies, are far more active in some men than in others.

How little knowledge, in the ordinary ways of learning, went to the poetry of Keats, or to the earlier essays of Spencer, or to those works of Bacon that were written by Shakespeare! Yet how much effective knowledge! Whence came it? From extraordinary sensitiveness to everything that subserved their constructive instincts, and extraordinary faculty of interpreting hints by analogy. It is not necessary to learn everything in order to know it: you can often see that it must be so, because the analogies of the case require it: you often find that you know what you never attended to, such as the tones of a man's voice. Take the hackneyed case of Shakespeare and Ben Jonson. Ben Jonson had learned incomparably the more; Shakespeare's effective knowledge was incomparably the greater. But no one can suppose that Shakespeare drew his characters from deliberate observation, and provided them with dialogue from the resources of his memory. He had observed men and women, but was not always 'on the watch'; he heard them converse, but kept no records; he witnessed the frank display of passion and caprice in a great age, but Lear or Cordelia, Rosalind or Perdita he never saw. There must have been data even for these, but not particular assignable data: they were never to be found either as a whole or in parts. It was not by analytic attention, but by marginal absorption, that the elements of their being were assimilated; not deliberately, but by nature and grace. Ben Jonson, on the other hand, was a great observer, and sedulously incorporated his observations with his work. The plainest proof of this is to be found in his Roman Tragedies, the dialogue of which is full of translations. He was proud of it, and published *Catiline* with footnotes: demonstrating the propriety of his language by quoting the sources. The result is that these plays, in some ways noble and admirable, are found by most people to be unreadable. Particular fact is not the stuff of imagination, and matches badly when patched into its fabric. Compared with the epic founded on myth, every historical poem is a failure; historical plays and novels are (with few exceptions) inferior to genuine fictions; so are documentary realists to creative masters: because imagination is not made of particular fact, but of infinite analogies of things, and of things that were never observed or thought of until analogy called them to life.

It does not follow from this that observation has no place in imaginative work. Nothing else, for example, can give local colour, if that is desirable. Hence Scott before writing *Rokeby* studied the local flora; and explained that imagination, left to itself, becomes repetitive and monotonous. But how little of the great art of the world depends upon local colour! The important effect of observation upon literature is due to the growth of absolute impressions. When literature or art becomes traditionary, so that the impressions of each generation are derived almost entirely from former works, there is a cessation of development; and the only remedy for this is such a change that a rising generation may derive a stronger absolute impression from man and nature, or from some foreign culture, than from the traditionary models. It is not necessary that actual experiences should be taken into literature. The *Decameron* consists for the most part of old stories retold with a new impression of life; it contains also a few anecdotes of fact—all wretched.

(*d*) Some originality accrues to the man who sees what others cannot see; because, like a traveller in a strange country, or a man with a telescope, the facts are present to him and not to others. To a good psychologist the facts of experience lie open, as they do not to others; and we must include amongst psychologists many a poet and novelist who is neither indebted to text-books nor recognised by them.

A systematic thinker may discover things because his system needs them. Sometimes, no doubt, he invents them; but waive that. Is it not true that some men's minds are made according to Nature; so that even their preconceptions are apt to be true, and therefore the facts which their hypotheses demand are apt to be discoverable?

Most men cannot see things, because they are blinded by misleading preconceptions and false analogies. We are all of us in the habit of passing over innumerable things which we do not understand, and the understanding of which seems not practically to concern us; but the more original of us, in whom curiosity is strongest, are from time to time arrested by the commonplace and investigate it. Even those who notice what others pass by, and are disturbed in their customary acceptance of nonsense, have not always the courage of their judgments. They are afraid of dull people, the guardians of conventional beliefs, critics and great authorities; and so their ideas are smothered in the birth. Fear is as fatal in art as in war.

But that which is, above all, favourable to genius, in the more exclusive use of the term, is the inward condition, attitude, mood or rapture in which it works. Ribot[1] has discussed this condition: inspiration, he says, has been compared to hypermnesia, intoxication, and somnambulism. It cannot be explained by hypermnesia, because the creative activity is the very opposite of the routine of memory. Intoxication is factitious, and its activities are without the direction and control that give unity to works of genius. Certain somnambulists have a nearer resemblance to the state of inspiration: inspiration is an eruption of the subconscious mind, which does the work, and dictates the results to the normal subject; it is an exclusive state in which the number and intensity of imaginations monopolise consciousness and shut out the influence of outward things, except so far as they can be made to subserve the work; the work retains the impersonal character of the subconscious life. As to the nature of subconsciousness, the sagacious author will not commit himself, but leans (I believe) to a physiological interpretation of it.

This account of inspiration, or rapture, seems to be true, so far as it goes. Still, in confining hypermnesia to extraordinary manifestations of rote memory, it is needlessly strict. Surely we might use the term for an exaltation of the power of recalling past experiences, though the order of recall should not be by rote, and, therefore, should not be opposed to creative imagination. Hypermnesia implies a release of the power of recall from something that normally restricts it; and release is characteristic of genius. Intoxication (of the right kind), besides the superficial but unusual fluency it allows, is also a release from the fear of censorship, and so far excuses the dictum "that wit and humour consist in saying what others dare not." The same thing is true of somnambulism, and explains, not only why the subject of hypnosis will do absurd things, but why he acts better than in the waking state: he is no longer liable to stage-fright: his thoughts concentrated upon the rôle, everything he has ever known about it recurs to him, and there is nothing to repress whatever histrionic gifts he

[1] *Op. cit.* Pt I, ch. 2.

may have. As I have elsewhere[1] observed, there are ages in which every sort of censorship, conventional, traditionary, authoritative, is relaxed, so that every man breathes more freely, is more himself, and genius is relatively abundant.

In somnambulism, again, the normal dominant ideas of the subject are, in large measure, displaced by the idea of the operator, or by whatever idea he suggests: the subject becomes relatively impersonal. The normal subject is an adaptation to the normal life; and his dominant ideas of self, father of family, churchman, farmer, etc., control his actions, his plans, his memory. The tendency is for him to remember what subserves his purposes and to forget the rest. But much that seems to have been forgotten can be recalled under hypnosis; because, the dominant ideas having been subverted, the lines of association, through which most of his memories had been organised under those ideas, are broken; and therefore the inhibition which they exercised over all the other dormant traces of experience is relaxed; there is a release of the dormant or 'subconscious' mind, and the possibility of its contents falling into new combinations, if excited by new dominant ideas through analogy; so that, if the subject be told he is an actor, though he have never conceived himself in that way before, he immediately recalls all that he has ever seen or heard of acting, and behaves accordingly, oblivious of his farm and unsuspicious of playing the fool. Such then is the condition or attitude of inspiration or rapture into which the genius sometimes falls, and which constitutes his genius; for in ordinary life he may or may not be noticeable. In this state he is under a new dominant idea, his task —a mathematical problem, or a tragedy; he thinks of everything that serves that purpose, and may not be much disturbed by anything else: he is not afraid. The work is impersonal, because the ordinary dominant ideas of self and station are displaced; and we may even (with Schopenhauer) call his activity generic, seeing that what differentiates the individual is not so much his total experience, as the station and circumstances that narrow his life and restrict his thoughts and sympathies.

Probably the peculiar nature of this state of mind has been concealed by the facile description of it as 'attention.' It is attention; but so is every sort of dissociation, except the insane kinds that depend on the dissolution of attention. Attention is always a dissociation, since concentration implies inhibition; and (and being known

[1] *Natural and Social Morals*, ch. x. § 6.

to everybody) it seems to me the natural starting-point for the investigation of all other kinds of dissociation; but it is merely the starting-point. Generally, rapture is spontaneous attention, and its processes, at their best, are automatic: Carpenter's *Mental Physiology* is still worth reading upon this point. As it is spontaneous and apart from the ordinary activities of life, it is allied to play; but the mode of this alliance needs reconsideration. The spontaneity of genius implies some instinctive proclivity which is gratified by the work undertaken; and curiosity and constructiveness seem to be the original impulses which, modified in various ways, give rise to science and art. But if great works are undertaken, spontaneity will not suffice for their execution; the task must be returned to day by day; inertia must be overcome, weariness dismissed, neglect and disparagement forgotten. Homer needed a stonger will than Achilles. So if the will is weakened (like Coleridge's by opium) nothing great can any longer be achieved. But when the man has successfully invoked the Muse and fallen again to his labour, he enjoys that activity which Schopenhauer described as a first approach to Nirvana, having shut out all the care of the world; and offers (if I may say so) another analogy to intoxication; like Tam O'Shanter,

"Glorious,
O'er all the ills of life victorious."

MEMORY AND FORMAL TRAINING.

By W. G. SLEIGHT.

(From the Psychological Laboratory of University College, London University.)

V. Deductions.

1. *As to general improvement.*
2. *As to the existence of a general memory function.*
3. *As to the development of general psychical factors.*
4. *Alternative view.—Previous explanation by common factors.*
5. *Elements most effective in 'transfer.'*
6. *Reciprocal interference through differences amidst similarities.*
7. *Reciprocal interference through contrast of feeling-tone.*
8. *As to whether the effects of 'transfer' are enduring.*
9. *Effects of 'direct' and 'indirect' training compared.*

VI. Summary.

VII. Appendix.

I. INTRODUCTION.

THE psychological problem of 'transference' and 'spread of training,' connected as it is with the pedagogical theory of 'formal discipline,' has been for many years a fruitful source of experimentation and scientific discussion. Whether mental abilities, developed by special training in one kind of data, are transferable to any or all other kinds of data is a question which still divides psychologists into two schools. One asserts that special training does, under certain conditions, ensure general training; another, that it neither does nor can. Between these two extreme views, both of which are based upon experimental research, an almost unlimited number of degrees of compromise is logically possible. Volkmann[1] has shewn the possibility of transference of touch sensitiveness from the left to the right hand; Coover and Angell[2], from the sorting of cards to typewriter reactions, and from the discrimination of sound to that of brightness; Thorndike[3], from many complex functions to many others. In Scripture's[4] Laboratory, surprising degrees of transference of ability from one hand to the other were recorded; and Judd[5], by an interesting class experiment of

[1] Volkmann, "Ueber den Einfluss der Uebung auf das Erkennen räumlicher Distanzen," *Ber. der Kgl.-sächs. Ges. d. Wiss.* (Math. Phys. Col.), 1858, x. 38.

[2] J. E. Coover and F. Angell, "General Practice Effect of Special Exercise," *Amer. J. of Psychol.*, 1907, XVIII. 328—340.

[3] Thorndike and Woodworth, "The Influence of Improvement in one Mental Function upon the Efficiency of Other Functions," *Psychol. Rev.*, 1901, III. 247—61; 384—395; 553—564.

[4] Scripture, Smith and Brown, "On the Education of Muscular Control and Power," *Studies from the Yale Psychol. Lab.*, 1894, II. 114—124.

[5] Judd, "Special Training and General Intelligence," *Educ. Rev.*, June, 1908, 36—7.

aiming darts at a target under water, was able to demonstrate a considerable spread of training. Neatness, taught as an ideal, was found by Ruediger[1] to be transferable to some other subjects; and Squire[2] has demonstrated that neatness, cultivated in one subject without reference to ideals, has remained merely a specific habit.

Approaching more closely the subject of the present paper, we find Meumann[3] declaring for an absolutely general transference of memorising power, and Winch[4] for specific transference of very considerable proportions. G. O. Fracker[5], too, obtained the result that with some observers even greater memory improvement was found in certain matter other than in that practised. Thorndike and others, however, find no evidence whatever of any general effects of practice but, on the contrary, obtained repeated proofs that the area influenced by special memory training is exceedingly limited.

It is therefore clear that the investigations carried out have not up to the present led to any agreement upon fundamentals.

The present enquiry is limited to this problem of transference so far as it touches memory. Experiments are described which once more attempt to discover whether practice in one kind of memory work increases the power of memorising in general; and further, if the transference is not general, to what degree, in what directions, and under what conditions it occurs.

In view of the very conflicting nature of the results hitherto obtained, the discussion of the problem in its present form must largely consist in an evaluation of experimental procedure. It seems inadvisable merely to add another set of experiments to those already made, without first ascertaining by careful analysis wherein the weaknesses of the latter consist. By so doing, the ground will be made ready for other experimental conclusions which cannot be invalidated by the mere assertion of the reliability of other results.

[1] Ruediger, "The Indirect Improvement of Mental Functions through Ideals," *Educ. Rev.*, Nov. 1908.

[2] Squire, quoted by Bagley in his *Educative Process*, 208.

[3] Ebert and Meumann, "Ueber einige Grundfragen der Psychologie der Uebungsphänomene im Berichte des Gedächtnisses," *Arch. f. ges. Psychol.*, 1904, IV. 1—232.

[4] Winch, "The Transfer of Improvement in Memory in School-children," this *Journal*, 1908, II. 284; 1910, III. 386.

[5] Fracker, "On the transference of Training in Memory," *Psychol. Rev.*, *Monogr. Suppl.*, 1908, IX. 56—102.

II. PREVIOUS INVESTIGATIONS.

One of the most widely known researches of this kind, which has exercised very great influence in both America and England, is that carried out by Herren Meumann and Ebert at the Zürich Psychological Laboratory. Professor Meumann obtained results of so pronounced and definite a nature, and upon the truth of which so much of the practice of teaching and of education generally depends, that it becomes necessary to investigate with the greatest care the experiments upon which he bases his inferences. The problem which he set out to solve he states thus: "Is there a general memory function which can be perfected upon any material involving the use of memory, or, on the other hand, must we posit related or unrelated special memories[1]?" To answer these questions he arranged a series of experiments which lasted from November, 1902, till August, 1903. A group of six adults submitted themselves to a number of memory tests covering a large field of material. The object of the first series was to determine the existing condition of the memory ability in these several directions. On the conclusion of this 'Initial Condition Test,' all the observers were subjected to a lengthy one-sided mechanical practice, which took the form of memorising four rows of twelve nonsense syllables each daily for eight days—altogether, therefore, thirty-two rows. Nonsense syllables were selected as being the most mechanical material possible, and as being at the same time little related in form to the bulk of the subject matter which formed the material of the tests. A second series of similar tests was then applied to ascertain what was the effect, if any, of the practice. A further period of practice of the same kind and amount was given, and immediately afterwards a third series of tests.

The tests given were of the following nature:

A. Those which ascertained the powers of 'immediate learning'; that is to say—where the material could be grasped in one act of attention, and therefore was presented only once. These consisted of tests in (I) meaningless material, comprising (*a*) letters, (*b*) numbers, and (*c*) nonsense syllables; (II) material with meaning—(*a*) words (concrete and abstract nouns of one syllable); (*b*) German words with Italian equivalents, in couples, the couples being without logical connexion; (*c*) verse; (*d*) prose extracts.

[1] *Op. cit.* 5, 209.

B. Those which ascertained the powers of 'prolonged learning'; that is to say—where a greater amount of material was used, and more than one repetition was allowed. These included again tests in (I) meaningless material, namely, (*a*) nonsense syllables, and (*b*) visual signs; (II) material with meaning—(*a*) German and Italian words as before, (*b*) verse, and (*c*) prose extracts.

At the conclusion of the experiment, a considerable and general improvement appeared in the results of the second series, and a still more considerable advance in those of the third. The important question propounded at the outset he therefore answers thus: "There are no doubt related memory functions which can be perfected upon any material involving the use of memory, the development taking place proportionately to the degree of relationship between the practice and the test material[1]."

To Professor Meumann belongs the inception of the idea of this form of experiment; he was the first to attempt with any completeness an answer to this problem. If any subsequent attempts to elucidate the same vexed question meet with greater success, it will only be because it has been possible to build upon his work. But results so definite, and inferences from them of so far reaching importance as those of Professor Meumann, demand a careful examination of the conduct of the entire experiment.

In the first place, there are several serious and fundamental objections to his general mode of procedure.

1. The number of observers employed in the experiments was very limited, being only six. This number was further reduced to two in the case of three out of the seven tests in Immediate Learning. In one or two cases an observer who found himself in poor disposition for the test was withdrawn; in another, an observer was omitted in the second, and re-admitted in the final test. In itself the use of only a small number of observers may be perfectly legitimate, and even scientifically preferable. There are cases, however, like the present one, in which the experimenter seeks by using a number of observers, and averaging their results, to eliminate possible individual variations and to discover general mental tendencies; but to attempt this by calculating an average percentage of improvement based on the results of tests with only two observers, and in the absence of absolutely necessary precautions, appears to be a very precarious method of reaching general inferences.

[1] *Ibid.* 200.

2. Professor Meumann attributes the entire cause of the improvement in the tests to the practice in nonsense syllables, thus making no allowance for the 'direct' training to be derived from the tests themselves. The effects of this error are probably aggravated by the fact that many of the tests were, to those taking part, of an unfamiliar nature, in content, in method of presentation, or in both; progressive familiarity with their nature is sufficient in itself to bring about a great improvement in the later tests. Some means should have been devised by which it would have been possible to estimate the amount of improvement due to 'direct' training and that due to 'indirect[1].' The failure to do this has rendered the experiment, so far at least as its chief aim is concerned, of little value.

3. We have no guarantee that in these experiments the second and third series of tests were of the same difficulty as the first. An uncertainty of this kind clearly renders the experimenter's percentages of improvement of questionable validity, and indeed leaves still open the main problem as to whether there was improvement at all. When an experimenter makes use of simple sense material, such as colour and sound, which were employed by Netschajeff[2]; or even of material such as Meumann in part used, that is, numbers, letters, and nonsense syllables, where different arrangements of the parts make practically no difference to the difficulty of memorising; in these cases, it may be legitimate to do as was done here. But one of the innovations which made this experiment interesting and highly suggestive, consisted in using test material of an educational nature, an attempt which, while exposing in a very clear manner the ultimate pedagogical value of this kind of work, opens the door to a number of difficulties, one of which is that of securing equivalence in the three series. It appears impossible, for example, in the case of logical associations such as form the bases of prose and poetry, to make three tests exactly equivalent in difficulty. Some procedure should have been devised for coping with this defect.

4. The question as to whether the improvement made can best be represented by percentages is discussed more fully in another part of this paper; it is perhaps sufficient to observe here that it hardly seems a fair procedure to compare an improvement of 20 °/ₒ with another of 10 °/ₒ, when the former represents an advance from, say, 50 to 60

[1] By 'direct' training is meant practice upon the actual test material; by 'indirect' training is meant practice upon material other than that used in the tests.

[2] *Ztsch. f. Psychol. u. Physiol. d. Sinnesorg.*, 1900, xxiv. 321.

correct items, and the latter from 90 to 99. In fact the more nearly we approach our limit, the more difficult improvement becomes, and this is not brought out by the use of percentages.

In addition to the general objections just urged, there seem grounds for calling in question the validity of almost every test used in these experiments. We shall attempt to expose only the most prominent deficiencies.

(*a*) *The Number Test.* It would have been better here to avoid such numbers as 10, 20, 30, etc., which were introduced "to vary the row[1]"; or if inserted, care should have been taken, seeing that such numbers make a line easier to memorise, that they were equally distributed among the rows. The number of members and also of units among the members in a row should have been constant, the units being easier to memorise than the tens.

(*b*) *The Letter Test.* Vowels being used as well as consonants, there was a constant and varying degree of danger that the observer would be able to construct word associations. This danger could have been reduced to a minimum by omitting vowels altogether.

(*c*) *Nonsense Syllables.* This test followed immediately after the two preceding,—a sequence very likely to produce fatigue, with consequent irregularity in the results. As Müller[2] points out, too, the syllables used do not conform to the principles upon which Ebbinghaus and he constructed them, and which Meumann himself imagines and says he is following.

(*d*) *One-Syllable Nouns.* As there is probably some difference in difficulty in memorising abstract and concrete names, some precautions should have been taken that throughout all three tests there was the same distribution of the two classes of words.

The test was taken "as soon as possible[3]" after the three just mentioned, when the observer must have been considerably fatigued.

In the first series of tests only two persons took part; in the second and third, six persons, including the former two. If we base our calculations upon all six persons, this test shews the greatest improvement of any; if upon the two persons, it ranks as fourth—a difference tending to make one sceptical as to the significance of either.

(*e*) *German-Italian words.* Two persons only took part, and each

[1] Meumann, *op. cit.* 11.

[2] *Ztsch. f. Psychol.*, 1905, xxxix. 117.

[3] Meumann, *op. cit.* 32.

had a different task set. M. did not know Italian; W. knew it a little from his travels. We cannot therefore feel certain in the case of W. that he did not meet more familiar words in one test than in the others. Again, M. had to reproduce both German and Italian words, W. only Italian. For example, M. reproduced six words without error (that is, three couples); W., four Italian words (the German being given). It is evident that this is not one test, but two, which are essentially distinct. Nevertheless, Meumann works out the average for the two observers as five, a calculation which gives information of no scientific value.

(*f*) *Poetry.* In this test the method of estimating appears arbitrary, other equally reasonable methods leading to widely different results. The experimental procedure consisted in reading once a line or more of verse, containing a gradually increasing number of words; noting the number of words where the first error was made (Error Threshold), and also the point where the number of errors reached one-third of the total number of items in the extract. W.'s Error Threshold in the first test series is put down as twelve words; no notice being taken of the fact that he made only two errors when the lines contained twenty-two words. Similarly P.'s Error Threshold is given as eighteen in Test 2; but in twenty-six words, he only missed one. If we examine the state of things when, for example, twenty-two words were reached, we get the following results:

P. (in Test	I)	made	2	errors;	W.—	2
„ („	II)	„	4	„ ;	„ —	6
„ („	III)	„	2	„ ;	„ —	10

This indicates in both cases a marked deterioration instead of an improvement. And although by calculating the number of errors made when 20, 24, 26, and 28 words were reached, and striking an average, we obtain a certain percentage of improvement; this percentage is small compared with that given by Meumann on his basis of calculation. It is, therefore, impossible to place any reliance upon the percentage figures which are used to represent the various degrees of relationship between practice and test material.

Moreover, while with some tests it was difficult, in this test and in that of *Prose* which immediately followed, it was impossible to compare results, since the material of the different tests must have varied very much in difficulty.

An examination of the tests used in determining the effects of practice upon prolonged retention shews similar errors.

(*a*) The testing of nonsense syllables seems to have been conducted with great laxity; for we find that the rule that a row was to be considered as learned when it could be reproduced without error in the order given, was by no means strictly obeyed. The ground for this criticism is found in such a statement as "the reproduction was indeed without error, but somewhat hesitating and in part in the wrong order[1]," and in the reference to a reproduction being in "striking reverse order[2]." Such laxity could not fail to produce disturbing irregularities, and it is therefore not surprising that the percentages of extra repetitions needed after an interval of twenty-four hours, was so small when compared with those of other investigators.

Further, the valueless nature of comparisons based upon percentages is here brought out very strongly. In this test, where the same material was relearned twenty-four hours later in order to ascertain the saving effected, Meumann has failed to notice that these percentages are not independent of the length of the series; it is therefore impossible to judge the improvement entirely according to percentages. They would be different according as we took a longer or shorter series.

(*b*) From an examination of the entire material used in the first series of optical figures, and of the examples he gives of that used in the second and third (where such signs as z, τ, π, suggest names at once), it is quite evident that the first was the most difficult and that the large percentages of improvement (41 °/ₒ and 76 °/ₒ) which the results shew, indicate very little more than this decrease of difficulty—a fact which invalidates Meumann's percentages completely. The same neglect to frame tests of equal difficulty, with the consequent invalidation of the percentages of improvement, occurs in the three final tests.

(*c*) In the test using as material German and Italian words, we are told that 20 °/ₒ of Italian words were selected so that their meaning should be recalled by means of Latin or French equivalents; but the difficulty of framing such a test would be enormous. Moreover no account is given of the means taken to meet the difficulty.

(*d*) In the poetry tests two verses of Schiller's *Zerstörung von Troja* were learned until they could be repeated without error. Here again no one would allege that Schiller's long poem contains verses of exactly the same degree of difficulty or ease of apprehension.

[1] *Op. cit.* 88. [2] *Ibid.* 90.

There is therefore no basis for comparing the results of the three tests.

In addition, there appears to have been much irregularity in the conduct of this test. We are told that B.'s results were omitted because he learned four verses; in Test 2 he was readmitted. Another observer, M., dropped out midway; he was not in the mood.

In face of methods so open to serious criticism, we cannot agree with a recent writer who asserts that the experiments just examined "leave but slight room for doubt that the main outlines of the investigation will stand the test of time[1]."

With the intention of attempting a solution of the same problem, G. O. Fracker arranged an ingenious and skilfully framed series of tests again with intervening practices. The relation between the test and training series, he says, was "planned in order that the elements concerned in transference might be determined by analysis of the final results[2]." To effect this object, he arranged definitely 'gradated' degrees of resemblance between the test and the practice, some of the tests coinciding with the practice in 'form' and others in 'content.' This was worked out in detail in his four principal tests as follows:

1. Four shades of grey were successively shewn by means of the psychergograph[3]. After an interval of four seconds they were presented in a different order. During the interval which immediately followed this, the observer had to give the order of presentation of the first four shades. The test was carried through on similar lines to the end, duration of presentation and interval time being kept regular throughout. The shades of grey were graduated (1, 2, 3, 4) so that the observer had only to give the order by means of four numbers.

2. A series of nine tones, consisting of four different tones of varying intensity, was sounded by means of a tuning fork and a telephone, and during an interval of nine seconds, the observer reproduced the order in numbers as described in the previous experiment.

[1] W. B. Pillsbury, *Educat. Rev.*, June, 1908, 19.

[2] *Op. cit.* 57.

[3] This instrument consists of two parts, the stimulator and the recorder. The stimulator is an apparatus which gives the operator the means of exposing the test material for a given length of time and at given intervals without allowing the observer to know at any time what particular signal shall appear. The recorder was not used in this experiment. For a full description see Seashore's *University of Iowa Studies in Psychology*, III. 5.

3. A series of nine greys, consisting of different arrangements of the same four different shades as in Test 2, was presented, and during the nine seconds' interval between this series and the next, the observer gave the order in which they had appeared. Here again the responses were made by means of numbers.

4. Four tones, composing the major pianoforte chord, were sounded, the order, duration, rate and response intervals being the same as in Experiment 2, and the observer was asked to reproduce the order by giving the names—doh, me, sol, doh.

The remaining four tests, which, so far as one can judge, did not resemble the practice, consisted in:

5. Learning by heart a number of stanzas until they could be repeated without error.

6. Reproducing nine geometrical figures exposed for ten seconds upon a card, drawing them in relative positions and proportions.

7. Writing within a given time all that could be remembered of nine numbers of two figures each, read out at the rate of one per one-and-a-half seconds.

8. Recording from memory the distances (15, 20, or 25 cm.) traversed by the arm in moving the finger along a glass rod.

The training consisted in practising the memory for the order of four tones of the same pitch but of different intensities. Except for the fact that the content consisted of sound, the practice resembled the test upon the four shades of grey. The responses were here also given by numbers. To bring out clearly this very interesting arrangement, we may summarise the relation between the first four tests and the training series thus:

Test 1	resembled the	practice in	'form' but not 'content.'
Test 2	„	„	'content' but not 'form.'
Test 3	„	„	neither 'content' nor 'form.'
Test 4	„	„	partly in 'form' and partly in 'content.'

It is worth noticing that as regards the resemblance between the practice and Test 4 ('four tones') the latter differed from the training series in (*a*) dealing with differences of pitch instead of intensity, (*b*) the nature of the response (namely—by names instead of numbers), and (*c*) the method of production, the sound being produced by a fork with the aid of a telephone instead of by a piano.

In all there were twelve observers, eight of whom took both test and training; four, the tests only. The numerical results of the experiment may be set out briefly as follows:

Average Percentage of Improvement.

	Trained	Untrained
1. Four greys	36	4
2. Nine tones	22	11
3. Nine greys	19	10
4. Four tones	10	−2
5. Poetry	6·1	1·7
6. Geometrical figures	13	8
7. Nine numbers................	4	0
8. Arm movements............	0	−1

On the basis of the correctness of these values, Fracker comes to the conclusion that a general development of some psychical factor has taken place.

A critical examination of the experiment reveals one very important weakness. The very inadequate number of observers gave rise to a series of irregularities in the results:

(*a*) For example; if in the test of the nine greys we omit the remarkable performance of one of the trained observers (a leap from 39 °/₀ to 95 °/₀ of correct replies), there is but little difference between the results of the 'trained' and the 'untrained,' a difference namely of 13 and 10 instead of 19 and 10, as given above.

(*b*) The same fact emerges when we look into the calculations of the test in geometrical figures. Omitting again observer 5, whose jump this time is from 45 °/₀ to 80 °/₀, we arrive at respective improvements of 9 °/₀ and 8 °/₀ instead of 13 °/₀ and 8 °/₀.

(*c*) We see the same kind of irregularity in the results of the 'untrained,' due to the same general cause. In the number test, for example, one observer's results shew a fall from 39 °/₀ of correct replies in the first, to 31 °/₀ in the second test. This may indicate almost anything. Accompanied with its probable error, it would probably appear as a mere chance variation. Assuming it to be such, we must conclude from its size that the number of observers was insufficient. In such calculations it is quite evident that the number used should be such that the averages remain practically unaffected by the withdrawal of one of the individual results. This is not the case here; for if the work of the observer who shewed such a large retrogression is withdrawn, the result is materially changed. We may say that the average drawn from the four cases is scientifically as valueless as that drawn from three. Indeed, by using the results of only three observers, we find that only two tests (the 'four greys' and the 'four tones') shew a decided superiority of trained over untrained, a result to be expected from the nature of the tests and the kind of imagery employed, and closely in agreement with the introspections of the observers themselves.

Such irregularities as those just described are inevitable when results are based upon so small a number of observers. The remedy is to be found either in planning a much longer investigation of each individual—and such increase in length would involve many new difficulties—or in making use of a much larger number of observers, so that mere chance irregularities would, on averaging, cancel one another; and with either of these methods it would be further necessary to calculate the probable error in order to demonstrate how far the adequacy of scale had been achieved.

In view of this weakness, and the consequent irregularities, Fracker's results are as good as could be expected. They interestingly suggest—though they do not amount to a proof—that the coincidence of such definite 'form' as appears in the 'four greys' and the 'four tones,' *is* effective in transfer. The coincidence of 'content' in the 'nine greys' appears less effective, especially if that modification of the figures is adopted which was suggested above, reducing the respective improvements to 13 and 10 %.

The results further suggest, what Fracker failed to note, that the transference to tests coinciding with the practice neither in 'form' nor in 'content' does not occur to any significant extent.

On the whole, therefore, we find in this experimenter's investigation no evidence for the view that there occurred a *general* development of some psychical factor; on the contrary, it furnishes us with some corroboration of the opposed view, that the training has been of a specific kind, most effective where the test matter resembled it most closely, and ineffective where this resemblance ceased.

We shall conclude this consideration of recent investigations with a critical examination of some important work carried out in England. Mr W. H. Winch has for some years been conducting experiments[1] somewhat similar to those carried out by the writer, and shortly to be described. As the results and inferences are very different from, and indeed opposed to, those of this paper, an examination of his methods

[1] This *Journal, op. cit.*

and deductions may contribute to a solution of the main problem. For this purpose, only his most recent work will be investigated. Omitting details, his arrangements were as follows:

Three independent experiments were carried out in three different schools. In one school a series of three preliminary tests of prose-substance memory was given on one day in each of three successive weeks. Side by side with these were given three tests in rote memory for meaningless things (letters), for the purpose of finding the correlation between tests and practice. On the results of the three prose-substance tests, the class was divided into two groups of equal ability so far as those tests were concerned, one of which (Group A) was practised for three days, one day in each successive week, for twenty minutes, in rote memory work of a similar kind to that from which the correlation was obtained. The other group (Group B) spent the same period in drawing difficult geometrical designs. After the practice the groups were again united and then worked one final test in prose-substance, similar in kind to the preliminary tests. Summarising, we obtain the following results:

Group A shew in the final test an improvement of 21 %.
Group B „ „ „ „ 10 %.

That is, the practised group exhibits a superiority of 11 % over the unpractised.

Turning now from the tests to the practice medium, Mr Winch finds that here Group A has improved by 13 %. But this 13 % contains, he says, besides the improvement due to practice, that due to growth also. He deducts, therefore, 2 %,—a figure which, for some unexplained reason, he assigns as the amount due to growth,—and infers that the practice which produced an improvement of 11 % (13 minus 2) in the practice medium itself, has brought about a transference of improvement of 11 % in the tests. He thus arrives at, as he calls it, the striking result "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[1]."

It is perhaps unnecessary to discuss the question of 'growth,' as it seems by no means clear that in the short period during which the tests were carried on, there can be any appreciable 'growth.' This is probably nothing more than the effect of the last test exercise, which

[1] *Ibid.*, 1910, III. 394.

naturally itself constitutes practice. We will rather turn to more fundamental matters.

Stated briefly, the following objections may be made:—

1. The results are extraordinary when we consider that they are the product of only three practices of 20 minutes each, and they seem to require an unusual amount of evidential support. For it must not be forgotten that this training is nothing more than three practices extra to the many which the ordinary school work furnishes, so that the difference between 'unpractised' and 'practised' is not the difference between zero and three, but between say one hundred and one hundred and three.

2. Before this investigation, some of the children must certainly have done much more rote memory work than others, and therefore, according to these results, they ought to have excelled their companions both in rote and in substance training. This should have produced a considerable correlation between the two; whereas nothing of the sort occurred, the correlation between the two being only ·262.

3. Mr Winch's method of calculation tends to obscure the facts. For the purpose of comparing the results before and after practice, he takes the average of all the three test results before practice and compares that average with the one result after practice. It is clear that the average results of the three tests would not be nearly so high as the one result obtained from the last of these three. The comparison of chief interest to the reader is that between the last of the three tests before practice and the one after practice. The failure to give this very much obscures the values of the percentages he gives. It is quite possible that the percentage improvement of the 'practised' over the 'unpractised' would be so small that the probable error would shew it to be nothing more than a chance variation.

4. It is not at all evident that the tests are tests of the so-called substance memory. Less than six lines of very simple matter was read *three* times to the class. Some children would, with three presentations, learn this extract by heart, others would be able to learn a great deal of it by heart. That is to say, the tests are largely rote memory exercises.

5. Further, the difficulty of marking such exercises is enormous. Although Mr Winch has used a system of which the unit is very carefully chosen, the shortness of the extracts and the inevitable variation of the unit must produce unconvincing results.

6. In a somewhat similar experiment performed in another school, the children had, instead of one practice in each of three weeks, one

practice in each of thirteen weeks. In the latter case the percentage improvement of 'practised' over 'unpractised' is much lower, namely 7 per cent. instead of 11 per cent. This is rather surprising; but if we make the further deduction which Mr Winch urges, namely one due to 'growth,' it becomes still smaller. Moreover, as in the first school 2% was deducted for a three weeks' growth, how much ought we to assume to cover a period of thirteen weeks? Such a calculation might sweep away the entire improvement.

There seem therefore reasonable grounds for resisting the large claims made in different parts of the text and even the very modest conclusion of the author "that improvement through practice in rote memory for things with and without meaning, is followed by improvement in substance memory for stories[1]."

The experiments just described seem therefore to be still far from decisive. They were selected for examination as being the best known and most important attempts to reach a solution of this problem; nevertheless they have not succeeded, so far as can be seen, in avoiding errors of experimental procedure or inference, errors which appear to invalidate the results entirely. The present writer's investigations, now to be described, represent a renewed attempt to solve this question.

Before entering upon the description of the experimental part of this work, the writer wishes to express his indebtedness and thanks to all those who have enabled him to carry through a somewhat arduous piece of work;—to Professor Spearman, Director of the Psychological Laboratory at University College, London University, who especially assisted in the inception and development of the general plan, and in the statistical evaluation of the results; to the Education Committee of the London County Council, who granted the use of the schools for this purpose; and to the teachers who not only forgave the necessary interruptions to the school work, but lent their valuable help to the conduct of the investigation. Lastly, it is a pleasure to testify to the unfailing cheerfulness and assiduity of both school children and students, who bore long periods of practice and testing with undiminished zest.

[1] *Op. cit.* 405.

III. Experimental Series A (with school children).

The general arrangement of the experiment was as follows:

A series of ten different kinds of tests was first given in order to ascertain, within these limits, the memorising power of each child at the outset. This is here called the 'first cross-section.' Then followed a practice period of three weeks, during which, for thirty minutes daily, on each of four days per week, various kinds of memory training were carried on. The second test series, consisting of exercises similar to the first, was then taken for the purpose of discovering whether any improvement had occurred. It is here referred to as the 'second cross-section.' This was succeeded by a further period of practice equal in amount and distribution to the first, and making use of the same kind of material and procedure. Lastly, a third series of tests was given in order to ascertain whether there was any corroboration of the previous results. The results of this last series are here named the 'third cross-section.'

Three schools, dealt with in succession, were used in these tests—schools referred to in this paper as X, Y, and Z. In each school a sixth standard was made use of, which consisted of 21, 28, and 35 girls respectively. The average age was 12 years 8 months.

A careful, but naturally only a partially successful, attempt was made to compose three series of tests of equal difficulty. In several tests this may be said to have been practically secured, as for example, in those upon 'points,' 'nonsense syllables,' and 'letters'; but it was out of the question when dealing with material such as prose, poetry, and prose substance. In order to eliminate this difficulty, a point to which no experimenter has hitherto paid attention, and one which is vital in any attempt to solve the present problem on these lines, three test series were made (as in the case of Meumann), but each of the three schools took them in a different order, thus:

School X took (I) test series A, (II) B, and (III) C.
" Y " (I) " " B, (II) C, " (III) A.
" Z " (I) " " C, (II) A, " (III) B.

By taking the average of the first cross-sections of all three schools, then of the second cross-sections of all three, and finally of the third, it was possible to assume that any improvement or deterioration observable in the results of the second and the third cross-sections was due to some other cause than that of a decrease or increase in the intrinsic difficulty of the tests.

The average of (I) XA, YB, and ZC, formed the first general cross-section; of (II) XB, YC, and ZA, the second general cross-section; and similarly of (III) XC, YA, and ZB, the third general cross-section. The answer to the problem which this experiment seeks to solve is therefore to be found in a comparison of the results of (I), (II), and (III).

Six months after the completion of the experiment in school Y, and in the case of school Z, one month after the conclusion of its tests, a fourth cross-section was taken. This was done in order to ascertain in how far the improvement discovered after the third cross-section was an enduring one. The results of this series, however, do not stand upon the same footing as the others, as it did not enter into the cyclic arrangement explained above. Although the attempt was again made to obtain similar tests, it could of course succeed only approximately. Some scientific reliability can, however, be placed even on this fourth test series, owing to a precaution now to be described, which was taken throughout the entire investigation.

Thorndike first suggested that groups of children might be formed whose average ability in certain directions was equal, and Winch has developed and incorporated the idea in most of his recent work. The present experiment exemplifies the same procedure on a somewhat larger scale. In each school after the first cross-section the children were arranged for each of the test series subjects in an order of merit corresponding to the relative number of correct items. Each child's numbers representing the different positions for each subject were then added in order to calculate an order of merit for the whole series. With this final ranking as the basis, the whole class was divided into four equal sections, sections that is to say, in which the average memory power, as exhibited in the first cross-section results, was the same, or at least the same within a few decimal places. They were of course unequal so far as any individual subject was concerned, though even there the difference was not great. Three of these groups were practised during a period of three weeks in a particular kind of memory work. One group was made to learn by heart 'poetry,' another 'tables[1],' and the other to reproduce the substance of prose selections. One group had no practice whatever in memorising, occupying the time with arithmetical problems or with some other task involving little or no memorising.

Hereafter the section which underwent no training is referred to as

[1] For explanation of this practice see p. 405.

Group 1; that which practised the memorising of 'poetry' as Group 2, of 'tables' as Group 3, and of the substance of prose selections as Group 4. Since the average ability of each of these four groups was approximately equal, so far as the memory tests of series 1 were concerned, it was assumed that a second series of exactly similar tests would, unless some other factor or factors intervened, produce approximately identical results. Now the only factor introduced in the case of three of the four groups was that of practice in the subjects mentioned above. In the case of Group 1, this factor was not allowed to enter; any general difference which appeared in the results of test series 2 between the three groups and the first must have owed its origin to the presence or absence of this factor, viz. practice. Before it could be assumed, as Meumann assumed, that any improvement observable in the second cross-section was due to practice, it had to be shewn that the improvement made by the three practised groups was superior to that made by the unpractised group. It is not to mere improvement that we must look for any solution of the problem, but to superiority of improvement of the individual three groups over the one. By such a method alone could it be hoped to ascertain clearly and with certainty what the effect of practice was, and even in the fourth test series to place any scientific reliance upon the answer to the problem as to whether the effects of practice were enduring.

No attempt was made to estimate numerically the 'direct' effect of the practice, that is, in the practice material itself. The conditions under which the experiment was carried out did not admit of this. The fact that actual improvement did occur as a result of the six weeks' practice is, it is true, in part an assumption, but a very warrantable assumption. We can find no authentic cases where under normal conditions anything but improvement has taken place during such practice. Moreover, since in each succeeding test series, as we shall see later in more detail, even the unpractised children shewed improvement, due to the small amount of practice yielded by the tests themselves, we may reasonably assume that the much longer practice had a similar but stronger effect.

The three kinds of practice were conducted orally. The 'poetry' group repeated line by line after the experimenter until the average child could repeat the whole without help. To maintain interest, individuals were occasionally called upon to recite alone, but nine-tenths of the work was done collectively. From 20 to 30 lines were memorised each day. The matter of the poems was generally fairly within the comprehension of the children, although some were distinctly too difficult, as, for instance, Shelley's *Skylark* and *The Cloud.* All the selections learned, except two, had the characteristic of rhyme; the two exceptions were taken from *Hiawatha.* Lines of all lengths and rhymes of all kinds and arrangements were employed.

The same procedure was followed in the 'tables' practice; as material were used multiplication, pence, and metric tables of all kinds, squares, vulgar fractions with their decimal equivalents, distances from London to the chief towns of England, etc.

In 'prose-substance' practice the selections were scientific, geographical and historical, or took the form of simple narrative. The piece was read twice to the children, who then wrote out the gist as well as they could remember it. Sometimes one long extract, and sometimes two shorter ones were given, in order to maintain the attention and interest.

Care was taken that each group should work under similar conditions. For example, the group which underwent no memory training was never allowed to have the impression that it was in any way handicapped or under conditions not similar to those of the others. This precaution, which, so far as it is possible to judge from the accounts of other experiments, has hitherto received no attention, was very necessary to obviate the possibility of any laxity or lack of interest on their part. A further precaution, and again one it is believed not hitherto taken, was made use of. In order that each practice group should receive an equal amount of training, the three supervisors, of whom the experimenter was one, took each group in turn for one week, thus:—

	Group 2 (Poetry)	Group 3 (Tables)	Group 4 (Prose Subs.)
1st week......	W. S.	M. S.	A. J.
2nd week......	A. J.	W. S.	M. S.
3rd week......	M. S.	A. J.	W. S.

and the order was reversed in the last three weeks of training.

The ten tests comprising a cross-section represent a fairly wide field of memory work. They were selected out of twenty-two which were used by the writer in a preliminary experiment with an entirely different set of school children in order to determine the relative value of such tests for the purpose in view. The final selection of the ten tests was made after a careful study of the whole twenty-two and their results. Many improvements naturally suggested themselves,—improvements in the way of making them reliable; of hitting upon the best and most convenient length of test; of choosing out of the many ways of presenting the material that best adapted to the age of the children and to ease of evaluation; and indeed of avoiding difficulties of all kinds. By these preliminary tests, too, the whole procedure was standardised, so that every test of the whole experiment could be carried on with perfect regularity and sameness. In fact, they formed for the experimenter a training ground, where the whole machinery for the following experimental series was perfected, so that in the actual investigation it worked with ease and precision. It was very necessary that the tests should be short and easy to manipulate with classes of school children; that, so far as possible, they should appear to develop out of the ordinary school work; and lastly, that they should be representative of the many different mental processes involved in memory work. Hence they include verbal logical associations with one presentation, and with several; with and without rhythm; demanding exact and also substance reproduction; arbitrary verbal associations, in couplets and continuous; of letters, syllables, and names; and lastly, spatial associations with one presentation and with several.

In detail the tests were as follows:—

I. *Prolonged Learning.*

1. *Points in Circles.* This was an adaptation of a test used by Macdougall and Burt[1]. Circles of 18″ in diameter were drawn upon white cardboard, one upon each sheet. Within the circles were heavy black spots of $\frac{3}{4}$″ in diameter, and varying in number from three to six. The children were provided with paper of foolscap size, ruled into squares of $\frac{1}{3}$″, upon which circles of four squares' radius had been traced. One of the large cards was hung in front of the class, the circle upon it being covered. An exposure of one second was then given, immediately after which the children attempted to reproduce upon their own paper the positions of the spots. It was previously explained that the position of every spot upon the plain large circle corresponded with a junction of lines upon the foolscap. Each card was exposed six times, the children thus making six attempts to remember the exact positions relative to the circle and one another. No one was allowed, however, to alter or fill in any circles left totally or partially blank, as her estimate of the positions became more correct with repeated views. The marking consisted in estimating the number of correct positions.

[1] Burt, this *Journal*, 1909, II. 150, 151.

2. *Dates.* Two series, each consisting of six dates and their corresponding events, were repeated by the children after the experimenter a given number of times, the number of representations being announced beforehand. The event was then read out and the children wrote the date. The order of testing was always different from that of the repetitions, although the same order was maintained in every cross-section. In marking, each figure, correct and in its proper position, counted as one, except in the case of the first two numbers, which together counted as one; a number correct but out of position received no mark, but where two correct numbers occupied each other's place, one mark was given.

3. *Nonsense Syllables.* These syllables, constructed according to the rules of G. E. Müller[1], were printed in letters 3″ long and 1½″ wide in white chalk upon a blackboard disc which was made to revolve behind a screen, and were exposed to the class by means of a rectangular aperture in the screen. The rate of revolution was kept constant. The eight syllables of each series were exposed in succession five times, the children repeating them aloud as they appeared. They were previously told to emphasize the second member of each couplet. Immediately afterwards the experimenter repeated the first word of each pair, and the class attempted to write the associated syllable, that is, the next following.

4. *Poetry.* A stanza of from eight to twelve lines was first read through to the class; the whole class then repeated each line after the experimenter, always reciting the stanza from beginning to end. After a given number of repetitions (the particular number was announced at the beginning of the test) the children wrote all that they could remember. When finished, their writing was covered, not to be seen or touched again. The piece was then given a few more repetitions and a second attempt made to reproduce it; the correct items in each attempt were arranged, and constituted the test result. One such selection by Blake was as follows:—

> "The sun descending in the West,
> The Evening Star does shine;
> The birds are silent in their nest,
> And I must seek for mine.
> The moon, like a flower,
> In Heaven's high bower,
> With silent delight
> Sits and smiles on the night."

[1] *Ztsch. f. Psychol.*, 1894, VI. 99 ff.

5. *Prose.* The material for this test consisted in a short literary extract, such as the following:—

"At the usual evening hour / the chapel bell began to toll / and Thomas Newcome's hands outside the bed / feebly beat time. / And just as the last bell struck, / a peculiar sweet smile shone over his face, / and he lifted up his head a little / and quickly said 'Present,' / and fell back. / It was the word we used at school / when names were called, / and lo! he / whose heart was as that of a little child, / had answered to his name / and stood in the presence of the Master." /

The general procedure adopted here was similar to that of the previous test, the repetitions in this case being for the first attempt six, and for the second three. Logical portions were first marked out as indicated above, and adhered to throughout.

6. *Prose Substance.* A piece of prose, well within the comprehension of the children, was read twice to them, and they were asked to write the substance of it. This method was adopted in preference to the questionnaire, because although not so easy to mark and assess numerically, it involved a smaller expenditure of time; when treated in this way it became a more usual form of test to the children, and it also avoided the difficulty of suggestion. In practice it was quite easy to assess. Every correct fact was given one point; for example, such a phrase as "fierce little scorpions," or "the warm sunny South," received three marks, one for each so-called fact, viz. 'fierce,' 'little' and 'scorpions,' whether the exact word was given or not. The whole of the exercises were marked by the experimenter, a rigid uniformity being in this way maintained. The nature of the test is best understood by an example:—

"Plenty of these fierce little scorpions, which hide under stones by day and come out by night, may be found in the warm sunny South, and though they look so like crabs, they are real land animals. They have no means of spinning, and have a poison dart in the tail, yet they belong to the spider family, as may be seen by their four pairs of legs, their sharp pincers which take the place of the feelers of insects, their claws, which are part of their mouth-pieces and are fixed to the jaws, and narrow slits under the stomach through which they take in air to breathe."

II. *Immediate Learning.*

7. *Map Test.* A large map of the world, on Mercator's projection, was hung upon the wall before the class. Each child had a corresponding outline map upon the desk in front of her. A long pointer was used by the experimenter to shew a certain position on the wall map; at the moment of pointing, the name of the place was called out, such

distinctive names as cape, bay, river, etc., being omitted. The wall map was then immediately covered, and the names of the places again called out, upon which the children placed a cross on their own map, indicating as exactly as they could the precise positions. The first sixteen places were given out two at a time, the remaining twenty-four three at a time. Names were always read out in the same order as first given, so that it was almost purely a test of space relations. The method of marking was as follows: the extent of the spatial error was measured in twelfths of an inch; all errors of more than 2″ and all omissions, counting as errors of 2″.

8. *Dictation.* This consisted of a piece of continuous prose divided into intelligible and grammatically complete portions, beginning with eight and increasing in length gradually to nineteen words. Each portion was dictated once, the children immediately writing what they remembered. The result was given in the number of correct words.

9. *Letters.* Consonants only were used in this test in order that there should be very little tendency on the part of the children to seek secondary associations. The test was composed of 16 series of letters; numbers 1 and 2 consisted of four letters each; nos. 3 and 4 of five; 5 to 8 of six; 9 to 12 of seven; and 13 to 16 of eight letters each. Each series was dictated by the experimenter once, the children immediately attempting to reproduce. In the marking, an omission or addition was reckoned as one error; if the letter was one place out of position, as half an error; if more than one place out, three-quarters of an error. The total number of errors was deducted from the entire number of letters to find the total of correct items.

10. *Names.* Forty-four common Christian names and surnames were used in this test, dictated first in two's, of which there were two, then in three's, of which there were eight, and lastly in four's, of which there were four series. Thus, after the experimenter had read two, three or four double names, he repeated a surname and the children were asked to write down the Christian name which belonged to it. The names were not given in the order in which they were first read, so that it was necessary to try to associate each surname with its particular Christian name. The number of correct names gave the measure of the individual's ability.

The following general precautions were taken:—

(*a*) Each child was provided with a large sheet of blotting-paper, which she moved slowly down over her paper as she wrote her answers, the chances of copying being thus reduced to a minimum.

TABLE I.

Group 1 (no Practice)					Group 2 (Poetry Practice)				
	Section					Section			
Sch X.	I	II	III	IV	Sch. X.	I	II	III	IV
N.	79	110	95		W.......	47	31	51	
C.	51	98	62		C.	44	63	41	
F.	90	58	87		R.	80	56	45	
H.	80	79	62		S.	56	51	88	
R.	80	85	102		D.	48	62	64	
C.	27	43	37						
	407	473	445			275	263	289	
Average	67·8	78·8	63·6		Average	55	52·6	57·8	
Sch. Y.					Sch. Y.				
H.	94	74	113	67	C.	58	37	52	48
B.	129	91	134	78	W.......	56	117	125	122
K.	62	64	92	65	L.	54	74	89	83
R.	57	95	84	102	C.	86	120	129	—
C.	98	113	105	90	V.	72	144	132	127
W.......	56	69	82	62	C.	52	110	80	—
F.	32	57	100	93	M.......	92	51	97	65
	528	563	710	557		470	653	704	445
Average	75·4	80·4	101·4	79·6	Average	67·1	93·3	100·6	89
Sch. Z.					Sch. Z.				
M.	47	98	92	—	W.	33	31	41	49
J.	77	104	98	145	Z.	57	126	86	—
M.	116	121	137	125	L.	123	105	100	113
D.	43	53	71	48	C.	80	127	119	127
C.	132	155	150	—	H.	48	53	53	79
D.	54	74	54	57	K.	125	116	176	100
P.	93	116	95	94	P.	118	154	142	122
C.	65	74	60	78	W.......	43	46	45	—
	627	795	757	547		627	758	762	590
Average	78·4	99·4	94·6	91·2	Average	78·4	94·7	95·2	98·3
Averages	67·8	78·8	63·6	—		55	52·6	57·8	—
of the	75·4	80·4	101·4	79·6		67·1	93·3	100·6	89
3 schools	78·4	99·4	94·6	91·2		78·4	94·7	95·2	98·3
	221·6	258·6	259·6	170·8		200·5	240·6	253·6	187·3
Average	73·9	86·2	86·5	85·4	Average	66·8	80·2	84·5	93·6

Points in Circles.

Group 3 (Table Practice)					Group 4 (Prose Subs.)				
	Section					Section			
Sch. X.	I	II	III	IV	Sch. X.	I	II	III	IV
A.	89	58	100		F.	50	69	58	
P.	37	56	95		M.	93	55	96	
Z.	46	61	38		S.	45	56	50	
G.	74	90	45		I.	39	39	51	
T.	58	49	100		T.	28	86	73	
	304	314	378			255	305	328	
Average	60·8	62·8	75·6		Average	51	61	65·6	
Sch. Y.					Sch. Y.				
M.	76	95	133	137	H.	38	56	33	38
D.	76	60	61	91	M.	89	84	95	100
P.	76	71	120	142	D.	54	68	98	—
S.	101	95	121	—	Z.	80	92	136	—
F.	62	90	126	122	T.	76	53	89	79
H.	68	58	105	96	L.	81	81	74	84
N.	58	59	57	—	M.	55	103	120	118
	517	528	723	588		473	537	645	419
Average	73·9	75·4	103·3	117·6	Average	67·6	76·7	92·1	83·8
Sch. Z.					Sch. Z.				
M.	103	152	138	155	S.	51	91	65	72
J.	84	116	94	102	C.	65	57	61	81
M.	62	55	55	—	K.	87	82	100	63
D.	60	121	137	140	A.	80	111	97	132
C.	77	119	120	99	R.	43	64	63	73
D.	75	97	126	92	E.	90	75	65	83
P.	45	61	53	—	J.	37	65	94	—
C.	31	55	56	52	H.	23	48	51	80
D.	70	92	67	99	W.	35	51	51	58
W.	40	66	73	59					
	647	934	919	798		511	644	647	642
Average	64·7	93·4	91·9	99·7	Average	56·8	71·6	71·9	80·2
	60·8	62·8	75·6	--		51	61	65·6	—
	73·9	75·4	103·3	117·6		67·6	76·7	92·1	83·8
	64·7	93·4	91·9	99·7		56·8	71·6	71·9	80·2
	199·4	231·6	270·8	217·3		175·4	209·3	229·6	164·0
Average	66·5	77·2	90·3	108·6	Average	58·5	69·8	76·5	82·0

(*b*) All answers were written with a lead pencil, in order to obviate technical writing difficulties.

(*c*) Full time was allowed for every answer, so that every child did all that she was able.

(*d*) So far as the experimenter could effect it, no test was begun or carried on unless every child appeared to be giving her attention.

(*e*) Careful explanation as to what the child was expected to do was made, and every unfamiliar test was preceded by a short practice.

The scheme of tabulation was necessarily somewhat complex, owing to the large amount of statistical data furnished, and the numerous groupings. It was worked out in detail as follows:—

1. The results of each of the four cross-sections for each school were dealt with at first separately. They were then rearranged according to the practice groups which were formed upon the basis of the figures of the first cross-section, as already described. For illustrative purposes the arrangement according to schools and groups for one test is given in Table I.

The above table shews, therefore, for the one test (*a*) the actual number of correct items obtained by each child in all three schools; (*b*) the kind of training each child received; (*c*) the average number of marks obtained by each group, first in each school, and then in the three schools combined. Similar lists were made for each of the ten tests. The average numbers of correct items for all the test subjects, corresponding with the final averages of Table I, are set out in full in Table II.

2. From the last chart are obtained the improvement or retrogression made by each group in the second, third and fourth cross-sections. They are indicated in Table III as plus or minus quantities.

As the method employed here for calculating improvement is a departure from the usual percentage procedure adopted by Meumann, Winch and most of the experimenters in this branch of research, some explanation may be useful and perhaps necessary.

The usual expression of variation as a percentage of the initial value is suitable when the inequalities between the initial values of different individuals are due to their derivation from tests of different lengths. For, say that one person does twice as long a test as another, he thereby—other things being equal—has the chance of getting twice as many marks. If now on a subsequent occasion he again does twice as long a test, he obviously has the chance of improving by twice as large an amount. To make things equal we clearly require not the absolute amount of change, but the ratio of this amount to the initial amount, and such a ratio is furnished by the percentage.

TABLE II.

		Section I	Section II	Section III	Section IV
Points	Group 1	73·9	86·2	86·5	85·4
	,, 2	66·8	80·2	84·5	93·6
	,, 3	66·5	77·2	90·3	108·6
	,, 4	58·5	69·8	76·5	82·0
Dates	Group 1	14·4	15·3	15·1	18·1
	,, 2	14·7	16·8	20·4	15·0
	,, 3	18·9	21·9	21·3	19·8
	,, 4	17·7	17·1	20·1	14·4
Nons. Sylls.	Group 1	20·7	20·7	22·8	26·8
	,, 2	19·8	24·9	27·3	29·1
	,, 3	19·2	24·9	28·2	29·1
	,, 4	21·9	21·0	24·6	26·1
Poetry	Group 1	58·5	62·4	63·8	62·9
	,, 2	56·5	59·4	57·9	60·7
	,, 3	60·3	60·9	64·4	66·5
	,, 4	59·4	63·4	64·7	63·7
Prose (literal)	Group 1	109·8	117·4	118·6	141·0
	,, 2	101·9	107·3	107·5	145·5
	,, 3	108·1	113·0	115·6	148·7
	,, 4	104·6	113·7	118·3	144·7
Prose Subs.	Group 1	27·5	28·8	30·5	41·0
	,, 2	23·5	24·8	24·7	25·9
	,, 3	23·5	27·1	27·1	36·4
	,, 4	22·8	28·8	28·3	36·6
Map Test	Group 1	63·9	65·9	72·4	72·5
	,, 2	65·9	65·1	81·9	77·1
	,, 3	65·9	64·0	74·5	74·6
	,, 4	68·3	66·8	78·7	66·8
Dictation	Group 1	134·1	135·9	139·0	141·3
	,, 2	129·6	130·9	130·0	142·0
	,, 3	129·3	130·3	132·8	140·5
	,, 4	129·8	133·6	134·7	141·6
Letters	Group 1	76·1	78·9	80·2	80·0
	,, 2	79·2	81·7	82·6	85·8
	,, 3	76·5	78·4	80·8	81·1
	,, 4	78·7	81·1	82·4	84·0
Names	Group 1	32·7	41·5	41·4	43·5
	,, 2	34·7	39·9	42·7	47·5
	,, 3	35·3	39·7	42·1	46·1
	,, 4	35·5	41·5	45·9	42·7

On the other hand this percentage method is useless and even misleading when the inequalities are due to different grades of practice, or to different kinds of material dealt with; for it assumes that when one person has been so much more practised than another as to get twice as many marks in the same number of tests, it therefore becomes twice as easy for him to make a further improvement of a given number of marks. Whereas really the contrary is the case; the more skill he has already developed in the test, the harder it is for him to make further improvement.

A similar consideration clearly applies to different kinds of material.

To make such heterogeneous amounts at all comparable, the usual statistical device is to take as units the mean variabilities of the classes of performance compared. This seems reasonable; for if one class furnishes on the whole more variable measurements than another, it is almost equivalent to saying that variation in this class is correspondingly easier than in the other. By making the variability our unit of measurement, we eliminate this inequality in, it appears, the fairest possible manner. This has the further advantage that the tests marked by the number of corrects are made comparable with those marked by the number of errors; and it is scarcely possible to do this with the method of percentages.

The method of computation was therefore as follows:—

(*a*) The differences between the average correct number of items in Sections I and II, I and III, I and IV, were divided by the standard deviation[1], thus:—

$$\frac{\text{Av. II} - \text{Av. I}}{\sigma}, \quad \frac{\text{Av. III} - \text{Av. I}}{\sigma} \text{ etc.,}$$

where σ is the average of the three standard deviations obtained from the three schools.

(*b*) The resulting values were multiplied by 100 to remove decimals.

Thus, illustrating from the first two results upon Table II:—

$$\frac{86{\cdot}2 - 73{\cdot}9}{\frac{1}{3}[\sigma(\text{Sch. X}) + \sigma(\text{Sch. Y}) + \sigma(\text{Sch. Z})]}$$
$$= \frac{12{\cdot}3}{\frac{1}{3}(20{\cdot}5 + 19{\cdot}9 + 29{\cdot}1)} = \frac{12{\cdot}3}{23{\cdot}2} = {\cdot}53.$$

Omitting Section IV, the full results arrived at in this way are as follows:—

[1] By this is meant the root of the mean square deviation from the mean. For further explanation, see Yule, *An Introduction to the Theory of Statistics*, ch. VIII.

TABLE III.

Shewing Improvement and Retrogression of

	Section II compared with Section I	Probable error	Section III compared with Section II	Probable error	Section III compared with Section I	Probable error
Points ...... Group 1	53	12	1	13	54	13
,, 2	57	12	18	13	76	13
,, 3	46	12	56	13	102	13
,, 4	48	12	29	13	77	13
Dates......... Group 1	8	8	27	8	36	8
,, 2	20	8	35	8	55	8
,, 3	29	7	−6	8	23	8
,, 4	−6	8	30	8	23	8
Nons. Sylls. Group 1	0	9	26	8	25	8
,, 2	63	9	30	8	92	8
,, 3	70	9	41	8	111	8
,, 4	−11	9	44	8	33	8
Poetry ...... Group 1	31	13	11	13	42	13
,, 2	24	14	−13	14	11	14
,, 3	4	14	28	14	32	14
,, 4	32	14	10	14	42	14
Prose......... Group 1	32	7	5	7	37	7
(literal) ,, 2	23	8	1	8	24	8
,, 3	21	8	11	8	32	8
,, 4	39	8	20	8	59	8
Prose Subs. Group 1	16	8	21	8	37	8
,, 2	16	8	−1	8	14	8
,, 3	44	8	0	8	44	8
,, 4	74	8	−6	8	67	8
Map Test ... Group 1	13	11	43	10	57	10
,, 2	−5	11	112	10	107	10
,, 3	−12	11	70	10	57	10
,, 4	−10	11	80	10	69	10
Dictation ... Group 1	13	8	22	8	35	8
,, 2	9	8	−6	9	2	9
,, 3	7	8	18	9	25	9
,, 4	27	8	8	9	35	9
Letters ...... Group 1	20	5	9	4	29	4
,, 2	17	5	6	4	24	4
,, 3	13	5	17	4	30	4
,, 4	17	5	9	4	26	4
Names ...... Group 1	77	10	−3	10	74	10
,, 2	46	10	25	11	70	11
,, 3	38	10	21	10	60	10
,, 4	53	10	40	10	92	10

Group 1 refers to the 'Unpractised,' Group 2 to the 'Poetry practised,' Group 3 to the 'Tables practised,' Group 4 to the 'Prose Substance practised.'

3. It is evident that we now have the means of comparing the improvements, due to different kinds of practice, with one another and with those which took place in the case of the 'unpractised.' The differences between the 'trained' and the 'untrained' are given in the next table, which is directly derived from the preceding one. Thus in Table III, in the first test subject (Section III compared with Section I), the improvements were respectively:—

Group 1 (untrained)	54,	
,, 2 (trained with Poetry)	76,	representing a superiority of 21,
,, 3 (,, ,, Tables)	102,	,, ,, ,, 48,
,, 4 (,, ,, Prose Subs.)	77,	,, ,, ,, 23.

The corresponding results (again omitting Section IV which will be dealt with later) are shewn in full in Table IV.

Before making any use of, or inferences from, this table, it is very necessary to realise the meaning of the statistics there given.

It has often been observed that when small differences occur between such statistical measures as averages and percentages, it cannot at once be assumed that those differences are the effect of definite and assignable causes. They may easily result from very indefinite and complex causes, such as give rise to fluctuations in the proportions of heads and tails in tossing a coin. In 100 throws, we might obtain 52 heads and 48 tails, but the small difference would not lead us to conclude that there was any bias in the coin. In the next series of 100 throws, the figures might be reversed or different. Or similarly, if on sampling a community by taking any 1000 men and 1000 women, we find that there are 100 red-haired men and 110 red-haired women, we should not conclude that that represented the actual proportion in the entire community. Other samples would furnish other figures. Luckily, there is a means of estimating how far these sampling fluctuations may be expected to range. Through the 'probable error' we are enabled to judge whether an observed result should reasonably be regarded as significant, or whether it might quite well be mere chance. And the probable errors have a further *rôle.* Many a time when an author complacently rejoices over the striking uniformity and consistency of his results, the calculation of the probable error would reveal this consistency to be quite incompatible with the nature of the case. The processes of selecting, elaborating and presenting the experimental data offer—in spite of the most honest intentions—a fatal scope for self-suggestive bias. And nowhere is this bias more insidious than when it consists, not in warping the data, but in unduly smoothing

TABLE IV. *Showing Differences between Trained and Untrained.* Comparison of Sections.

		Section II compared with Section I	Probable error	Section III compared with Section II	Probable error	Section III compared with Section I	Probable error
Points	Group 2	4	16	17	18	21	18
,,	3	−6	16	[55]	18	(48)	18
,,	4	−4	16	28	18	23	18
Dates	Group 2	11	10	8	11	19	11
,,	3	(20)	10	[−33]	11	−12	11
,,	4	−14	10	3	11	−12	11
Nons. Sylls.	Group 2	63	13	4	11	66	11
,,	3	70	13	15	11	85	11
,,	4	−11	13	18	11	8	11
Poetry	Group 2	−7	19	−24	19	−31	19
,,	3	−26	19	17	19	−9	19
,,	4	1	19	−1	19	0	19
Prose	Group 2	−9	11	−4	11	−14	11
(literal) ,,	3	−11	11	6	11	−5	11
,,	4	6	11	15	11	(21)	11
Prose Subs.	Group 2	0	11	(−22)	11	(−22)	11
,,	3	(28)	11	(−21)	11	7	11
,,	4	58	11	(−27)	11	(31)	11
Map Test ...	Group 2	−18	16	[69]	14	[50]	14
,,	3	−26	15	(27)	14	1	14
,,	4	−23	15	(37)	14	13	14
Dictation ...	Group 2	−3	11	(−28)	12	(−32)	12
,,	3	−5	11	−4	12	−10	12
,,	4	14	11	−14	12	0	12
Letters	Group 2	−2	7	−3	6	−5	6
,,	3	−6	6	8	6	1	6
,,	4	−2	6	0	6	−2	6
Names	Group 2	(−31)	14	28	15	−3	15
,,	3	(−38)	14	24	14	−14	14
,,	4	−24	14	[43]	14	17	14

Group 2 refers to the 'Poetry practised,' Group 3 to the 'Tables practised,' Group 4 to the 'Prose Substance practised.'

Note 1. The meaning of the numbers in italics, within square brackets, and parentheses, is explained on p. 419.

Note 2. If any difficulty is found in appreciating the practical value of the above numbers, they can perhaps be rendered more readily intelligible by the following considerations:—Our unit, the 'standard deviation,' may be taken as being approximately equal to the middle deviation X ·6745, this middle deviation being that which as many persons exceed as fall short of. Where, therefore, the above table shows an improvement of, say, 67, it means that the 50th person out of one hundred has improved so much that he is now as good as formerly the 25th person was. Again, where the table shows an improvement of 33, the 50th person out of one hundred has attained a performance about halfway between those of the 50th and the 25th persons formerly.

them out, so that a merely accidental result comes eventually to look as if based on the most solid evidence.

Before leaving this part of the work, it is necessary to give a short explanation of the results of the fourth cross-section. That a fourth series of tests was given in the case of two schools has already been noticed. As more than a year had elapsed since the experiment was concluded in school X, and as very many of the children had either left or been removed to other classes, it was found impossible to institute another set of tests. In the other two schools, however, although a certain number of children had since left, it was found possible to still maintain groups of equal average ability by omitting one or two additional group members. It will be remembered also that the cyclic arrangement could not touch this fourth section, and it is particularly this last fact that renders it impossible to compare the absolute improvements of this section, as shewn in Tables II and III, with those of the three first sections. Later in this paper, a comparison, so far as it may be legitimate, will be made between the results of this section and those of section III, in order to estimate if possible the duration of the effects of practice. For the present, the section will be omitted.

Analysis of Results.

1. In studying Table III and Table IV, it is necessary to bear in mind that many of the values may be insignificant, that is, may be signs in reality of neither improvement nor retrogression, but merely chance variations due to sampling. It is therefore useless to deal with them as they stand, without considering also the size of the accompanying probable error. Where, for example, in the first figure of Table IV we obtain a positive superiority of 4, accompanied by a probable error of 16, we are unable to assume that the 4 represents anything beyond a chance variation. The probability that a figure is significant becomes of course stronger as it gets larger in proportion to its probable error. When it is only twice the size of its probable error, we cannot but be still very uncertain as to its real significance. In the following analysis of the results it has been assumed that for any number, taken by itself, to possess significance, it should be at least three times the size of the probable error; and to present really strong evidence, it should be five times the size. By this assumption, it is by no means implied that smaller numbers are not actually in

accordance with the truth. Only, without further evidence, this truth is hidden from us.

2. An examination of Table III shews us that there has been a nearly general improvement in the work of both the trained and the untrained. Only in eleven cases out of the total of one hundred and twenty does there seem to be any retrogression, and five of these appear in the results of the second cross-section, when practice and tests might generally be supposed not to have exercised their full influence. After six weeks of training, the results (given in Column 6 of Table III) shew improvement in every case.

If we consider the whole of the figures in company with their probable errors, a difference is observable. From about thirty to thirty-seven of these values are probably insignificant, and may simply express a normal variation in the performance of the tests. Nevertheless, it is clear that in three-fourths of the results there has been improvement. This is due, at any rate in the main, to the fact that each test served as practice for the corresponding following test and contributed to bring about the improvement. We shall regard this improvement, therefore, in the case of Group 1, the untrained, as the 'direct' effect of practice involved in working the tests, and any improvement made by the other groups over and beyond this, as due to the special training exercises. It might seem hardly necessary to reassert, were it not that at least one experimenter has failed to note the point, that the particular improvement indicated in Table III cannot be regarded as entirely the effect of the special training, since Group 1, which exhibits improvement, underwent no training whatever.

3. Let us next see whether the special practice has had any effect of an 'indirect' nature, that is, has improved performances other than those practised; and let us first consider whether there has been any such effect of a general character. For this purpose we must study the figures of Table IV.

Three scales of certainty of significance have been indicated in Table IV. The numbers in thick type without brackets represent those values which are about five times the size of their probable error, and which it may be assumed possess a real significance; the numbers in square brackets represent values of between three and five times the size of their probable error; those within parentheses, values which are between two and three times, and therefore of doubtful significance; the remaining numbers, values which we must assume to be possessed of no significance whatever.

Four out of the ten test subjects shew no superiority of 'practised' over 'unpractised' in any practice group in any of the four cross-sections; and in the remaining six, there is no *general* effect of practice visible. Only about ten items out of a total of ninety give any reliable indication that the practice has exercised some influence. It is clear, also, that there has been no *general influence* of training, either of a favourable or unfavourable kind, as would prove to be the case, were the positive and negative significant values between them to exhaust the table. There are some real significant improvements and some real significant retrogressions; but there are also many cases where neither improvement nor retrogression appears. Two explanations of this might be given. In the first place, it might well be that the individual positive results exactly counterbalance the negative, and we should then have to infer that the practice has influenced the test work for good in some, for ill in other cases. It is impossible, however, to accept this explanation; it is in the highest degree improbable that these negative and positive values should so exactly cancel one another in such a large number of cases—76 out of 90 instances. We are therefore compelled to accept the alternative that in certain subjects the practice has in no way touched or influenced the test work. The 'centres' employed appear to have functioned in complete isolation from those used for the several kinds of practice. This is a point to which hitherto insufficient attention has been paid. Fracker, for instance, seems inclined to overlook instances of this kind, and to consider merely those where improvement has occurred. Judd, on the other hand, has performed a considerable service by drawing attention to the negative values. Where retrogression has taken place, he rightly finds the cause in the practice, and at once takes it for granted that connexion between practice and test is established. This must be admitted, and the admission is far reaching. We must, I think, further insist that where there has been neither improvement nor retrogression there can have been no influence, and that where there has been no influence the mental processes have probably been independent. There is ample evidence in Table IV that the influence of practice has been confined to particular tests and groups; indeed, the most striking feature of this table is the number of neutral items, that is, items which indicate that the practice has left a particular test quite uninfluenced. There is therefore nothing to warrant the assumption of a general memory development.

As the results in Table IV are thus incompatible with any single general tendency, there is no justification for taking averages. Such averages would have no scientific meaning, but merely show whether the positive or negative values happened to preponderate. To obtain a useful meaning, each case must be studied on its own individual merits.

In the first place, let us examine the effect of practice upon what appear to be very kindred performances.

1. The training exercises of the group practising reproduction of the substance of prose extracts were exactly similar to the corresponding tests. The training was therefore 'direct' and produced, as we should expect, considerable improvement over and beyond that obtained by the other groups. This superiority was, however, somewhat reduced in the third section—a curious fact which can only receive explanation if we take into account the practice, large in amount, which the whole class had been undergoing for years in this subject. For it must be remembered that this exercise resembled fairly closely one of the most common performances in daily life and school routine. The incident of listening to a narrative, or account, or explanation, in such a way as to retain the gist, is repeated a hundred times a day. The so-called 'school composition' possesses many of the same features, with the difference that in the reproduction, attention has to be given also to the form—sentence construction, choice of words, etc. In the practice and tests of this kind the children clearly understood that no attention need be paid to the form; only the substance was wanted. In so far, therefore, as this omission of form was concerned, the exercise was for them new. The element of form, however, was one which school children very easily dispense with; indeed it has been found exceedingly difficult to make them, after years of practice, pay any attention to it. It is easy to understand, therefore, that they took early advantage of the permission to omit it, and set themselves at once to the memorising part of the exercise with concentrated effort.

From the fact just observed, that the exercise is of such common occurrence in ordinary life, it might be reasonably inferred that 'saturation point' was not far distant when the practice was started, and that no very great improvement was possible. Facts confirm the inference. We find that after three weeks of training, their maximum was reached, in which they shewed a marked superiority to every other group. How is it that in the remaining tests they not only failed to keep up this great superiority, but were overhauled by the children of other groups, and even by those who received no practice? Table III proves that they did not lose ground, but maintained their position and even continued to improve. It was the other groups which, after the second test, made such progress as to bring them level. The question seems, therefore, to find its only true answer in the fact that the tests themselves formed for the other groups the training necessary to bring them to their maximum power; that 'saturation' point was sufficiently near for them to need but very little exercise to reach it.

2. The training exercises in 'poetry' and 'tables' appear to resemble very closely the tests in 'poetry' and 'dates' respectively—at any rate, in the material employed. We might, therefore, on *a priori* grounds, expect to see a considerable degree of 'transfer' of memorising power. This is not the case, however. So far as regards the results of the training in 'verse' (see Table IV), we have no reason to assume that there was any transference at all. The children who practised learning poetry shew no improvement in the 'poetry' test relatively to the other children who did not practise poetry, and to those who were not trained at all. Similarly, with those practised in 'tables'—a material which was selected as practice medium on account of being apparently closely allied to the test subject of 'dates.' That there are important points of similarity in the material of 'tables' and 'dates' is clear, and also certain similarities in the methods of testing and training, both being learned auditorily, the children having to depend in both cases entirely upon sound. Whether they transformed these sounds into visual images is a question which does not affect the problem. The results tend to shew, however, that the common elements have been effective only in a negative direction; for in the third section of 'dates,' this group exhibits actual retrogression. It is moreover quite evident that there are important dissimilarities in the two materials. In the method of presentation, too, there were substantial differences. For example, during the training, the children repeated after the experimenter until the given number of items was memorised; in the tests they were always informed how many repetitions were to be allowed. A different distribution of attention was probably only one effect of this difference of procedure. It would therefore appear that these striking dissimilarities, occurring in the midst of important similarities, have resulted in mental confusion, in an inability to make use of the common elements.

With reference to the 'poetry' practice and tests, much the same considerations must be urged. Stanzas memorised as tests were chosen with an eye to their simplicity; they were all well within the comprehension of the children, and none but a mentally deficient pupil could fail to understand them. The 'practice' stanzas were not necessarily chosen on account of their simplicity; some were indeed unnecessarily difficult. In addition to this, which of itself might have sufficiently accounted for the facts, the children were always told in the tests how many repetitions would be given; in the practice, never. It may have been, therefore, as before, that the different distribution of attention was more than enough to counterbalance the other common elements.

Further, the practice work in this subject resembled very closely the ordinary school work in such a subject, except that it consisted purely of memorising, without exposition. Such school exercises are often carried on after the first few readings of a poem in a mechanical fashion, and with a very widely distributed act of attention. In the tests, however, the children gave all the attention of which they were capable.

It is evident that the old *a priori* methods are useless in the treatment of such problems; and further, that it is equally impossible to frame any such general law as that of the 'common element.' Thorndike's treatment of the matter appears too simple. Provided two subjects contain common elements, then training in the one, he says, will be transferred to the other. The results before us prove that the problem is so complicated that each case needs special investigation.

We will now examine the effects of practice upon what appear to be less kindred performances.

1. The test subject which exhibits the greatest effect of practice is 'nonsense syllables,' and it is here that the training in 'poetry' and 'tables' seems to have exercised a very powerful influence. The facts requiring analysis are as follows: The groups practising 'poetry' and 'tables' shew in the second series of the 'nonsense syllables' test a considerable advance beyond the standard reached by the unpractised group; in the third test series they remain at the same high level.

Before attempting to explain these facts, it is useful to place in strong contrast with them the effect of the training in 'prose substance' upon the same test, namely, 'nonsense syllables.' Strangely enough the practice in this case seems to have left 'nonsense syllables' entirely uninfluenced.

We have to consider, therefore, on the one hand, the striking improvement in 'nonsense syllables' effected by practice in memorising 'poetry' and 'tables,' and on the other hand, the no less striking absence of improvement in the same test shewn by those who practised the reproduction of the substance of prose extracts. We have to ascertain whether there is some special cause of these apparently contradictory figures; whether there exists amidst so much diversity of data some common element or elements responsible for these results.

So far as the school children are concerned, we have no introspections to direct the search for these common elements. An attempt was made to obtain from the children some analysis of the processes involved, attempts which met with little success. The high degree of suggestibility and the almost entire absence of introspective tendencies made the effort useless. In the experiments with students, to be hereafter described, the case was different, and an account of their introspective work will be given later.

Rhythm as a common element appears to be the predominant feature in the two cases of improvement. This must have become very rapidly an aid in memorising the nonsense syllables, since the greatest progress was made quite early in the experiment. The fall in the last results is not due to a loss in the effects, since absolute improvement continued. It is due to the great advance made by the 'untrained,' probably through a now complete familiarity with the test, that is, through 'direct' training.

It is worthy of notice that the improvement made by those practised in 'tables' is in all three columns greater than that made by the group using the other medium (poetry),—a superiority represented by the ratios 70 to 63, 85 to 66, and 15 to 4. This result points to the existence of other important common elements in the one case; for example arbitrary associations,—on the one hand between word and word, and on the other between word and number. The associations in the poetry are of course different.

Turning to the group trained in 'prose substance' we find strong confirmation of the 'rhythm' explanation. In this practice rhythm is entirely absent, and indeed it is difficult to discover any common element at all. One was auditorily presented, the other visually; one consisted of connected logical speech, the other of disconnected meaningless words; one was arhythmic, the other rhythmic. The two exercises seem to touch each other nowhere; and this is mirrored in all the results, which shew that the practice leaves the test work entirely uninfluenced. This particular result, which is based upon a test resembling very closely one of Meumann's and one used later by Winch, deserves special attention.

First of all, it cannot be contended that the practice was insufficient to produce any effect. Not only was this practice long and thorough, but its results were very pronounced. From Table IV it will be seen that this group, and this group only, shews great improvement in the *test* subject of 'prose substance.' The practice was therefore efficient. The table shews us, however, that this efficiency was confined to very narrow limits, that it was narrowly specific in its influence. We must conclude that no relation exists between these two particular kinds of memory work, between, on the one hand, the memorising of nonsense syllables,—a purely mechanical memorising exercise, involving meaningless material and arbitrary associations,—and on the other hand, the memorising of the gist of a prose passage,—work involving the formation of intelligent connexions and logical organisation. The two seem to have functioned as quite independent, quite disparate mental processes, change in either not affecting the other.

Thus from a survey of the facts disclosed in the 'nonsense syllables' results, we arrive at the conclusion that the presence of certain common elements has produced a transfer of power, and that in this particular case the absence of such common elements has left the function uninfluenced.

2. Practice in 'verse' seems to have had a deleterious effect upon the exercise of 'dictation,' although the figures (minus 28 and minus 32) are not sufficiently large in relation to their probable error to be taken as certain evidence of this negative effect. It may well be that the two processes have no relationship and function independently of each other. The two exercises appear, however, to have elements in common both in 'content' and 'form.' The material of both consisted of logical sentence constructions, and was read aloud to the subjects. The attention had also to be of a very concentrated kind in both tests. But evidently the points of dissimilarity are highly important. The mental attitudes involved in 'immediate' and in 'prolonged' learning work must be very different. At any rate the element of rhythm which we have already seen exercising considerable influence in other learning work was absent in the one case. In the absence of introspection and of certainty that the recorded figures represent a significant retrogression, it is useless to seek further reasons for this result.

3. Let us now examine the effect of the training in 'prose substance' reproduction upon the other subjects of the tests. All but the prose test seem, according to Table IV, to stand completely outside its influence. So far as material is concerned, all except poetry, prose and dictation, appear to have little in common with it. Of these three,

'dictation' was a test in 'immediate learning,' in which only one presentation was given, reproduction following immediately. The mental processes involved in this kind of learning work would be very different from those involved in memorising the substance of prose extracts; and it is probably due to that fact that this test escaped the influence of the practice. In the case of 'poetry,' the predominating factors, such as rhythm and word-for-word memorising with a number of repetitions, are perhaps sufficient to account for the entire absence of influence by the practice in prose substance, which naturally gave no opportunity for the use of rhythm and but little for any verbal memorising. The test in learning 'prose' literally may, according to the figures of Table IV, Section III, have benefited from the training in 'prose substance' reproduction, probably on account of the common features of material and in part of method of presentation.

4. We have now to note an improvement of a very unexpected nature, due, we must believe, to the effects of practice, that namely recorded by the group practising 'tables' in the test named 'points in circles.' At first sight there seems to be no particular reason for this apparently arbitrary result, and what follows is only put forward as a hypothesis which fits the facts as they stand. The explanation arises from, and is borne out by, the later introspections of 'experimental series B'; but it has been thought better to complete here the review of the results of series A, leaving to the introspections of the older observers of series B the task of confirmation.

In the first place we have seen rhythm occupying a predominant place in this form of practice. Visual imagery was also used, but would play a far less significant part; indeed, where rhythm is the predominant factor, it would be difficult for visual imagery to take an important place, since the attention of the observer would be occupied chiefly with the auditory and motor imagery involved in rhythm. It is quite possible, however, that in practising 'tables,' visual imagery would tend to become more and more explicit. The necessity for this kind of conscious organisation would naturally make itself felt more and more. The children would gradually come to feel the inadequacy of the merely mechanical repetition, and would, in the course of the practice, add to their first method that of visual imagery, the imagery of words and figures. In fact, the hypothesis here stated is nothing more than the assertion that of the three factors involved, namely, auditory, motor and visual imagery, the two first, being already highly developed and embodied in the use of rhythm, have little capacity for a further extensive development; while the last, namely, visual imagery, in that it has received but little training, has plenty of opportunity for development. This expansion of visual imagery would tend to make itself felt in certain kinds of subject-matter more than in others. For example, such imagery might easily receive a further impetus in test subjects such as 'points,' 'dates' or 'nonsense syllables.' Not of course in 'prose substance,' where only two repetitions were given, and where the visual imagery naturally used consisted in images of the scenes, a piece of mental construction on the part of the child, and altogether different from the actual visual images of the material presented. Nor probably in 'poetry' or 'prose,' literally memorised, for the latter of the two reasons just mentioned. Provided, then, no other factor hinders, we should expect an extension in the use of visualisation in the three subjects mentioned. In the test upon 'points,' we have a confirmation of this theory; the gradual augmentation, by the practice of 'tables,' of the factor of visualisation, finds its greatest possibilities in this particular test. If the figures for this test mean anything at all, they indicate the gradual employment of some such factor, which by hypothesis is that of visual imagery. Why then do we not see the same result in the case of the tests in 'dates' and 'nonsense syllables'? Simply because a hindering factor exists, namely, 'rhythm.' Visual imagery in these tests is still too fused with, and subordinated to, the auditory and motor imagery constituting rhythm; where the element of rhythm is strong, visual imagery cannot play a free *rôle*.

5. With regard to the apparently significant figure 43 in Column 3 of Table IV, where, in the test upon 'names,' the group practising 'prose substance' reproduction appears to shew an improvement beyond that made by the untrained, it is necessary to observe (1) that section II shews no significant value which might support the supposition that 'transfer' has taken place; and (2) that the large size of the figure 43 is due to the great but still insignificant negative value of the corresponding figure – 24 of Column 1.

Both reasons apply in part to the values recorded in the 'map test.'

IV. EXPERIMENTAL SERIES B (WITH STUDENTS).

A short time after the completion of the somewhat lengthy experiments just described, a further and quite independent series was carried out (October to December 1910), with two classes of Training College

Women Students of the average age of 18—19. Variations in ages were very small, as both classes were in the first year of training as teachers.

The observers were, relatively to the various theories as to the general or specific nature of 'training,' exactly in the same position as the children. They knew nothing about the matter, and were therefore insured, so far as their introspection was concerned, against the extremely strong influence of a teacher's opinions, of suggestion, of prejudice, and of preconceived notions.

Owing to obvious limitations with regard to time and to control of studies, it was impossible to extend the experiment to the same dimensions as the previous one. It was, in fact, undertaken as one of the ordinary demonstrations of scientific procedure in experimental psychology in a Training College course of pedagogy, and had the double object of seeking confirmation or contradiction of the earlier series of experiments, and of introducing the students to the process and methods of introspection.

Instead of four cross-sections, only two were made. The first, or 'control experiment,' consisted of six tests, and on the results of these the class was divided into four groups of average equal ability, as before. One group was then practised in the memorising of poetry, another in tables, another in the reproduction of the substance of prose selections, and the other received no practice at all. This training was carried on for twelve consecutive days (omitting the Sunday), for half-an-hour each day, at the end of which time the second series of tests was given to ascertain the effects of the training. To secure equivalence in the two test series a cyclic arrangement was followed similar to that already described. Class I of the students began the experiment with test series A, and concluded it with test series B; Class II, *vice versa*. Each class undertook to preserve faithfully complete silence upon the tests set them, so that both classes should remain in ignorance of the nature of the following series of tests. If any doubt should exist as to whether these women students were able to keep the secret and preserve the scientific nature of the experiment, the results offer sufficient evidence.

For the practice in 'poetry,' cyclostyled selections of verse were handed to the members of the group, and were memorised by whatever means were found individually suitable. From 30 to 60 lines, varying in number according to their length, were learned daily. If a student found she could not complete the work in half-an-hour, it was understood that she should not continue beyond that time. It is evident that the training here described differed considerably from that which the school children of the corresponding group underwent. The students had the material presented to them in written form, and chose their own procedure. The children received theirs in auditory form, and were therefore less free in their choice of method.

A similar plan was followed with the training in the memorising of 'tables.' Here again, however, there was a difference. Since the material practised included population, import and export tables, foreign and English coinage systems and other data of a somewhat irregular form, it gave far less opportunity for the employment of rhythm.

Between the training of students and the school children in the reproduction of prose selections, there was little difference except in length and difficulty.

The two cross-sections consisted each of six tests of the following kind:

1. *Dates.* A series of ten was dictated, event and associated date being repeated after the experimenter. The list was repeated six times, after which the students reproduced the date appropriate to the event read out. The order of the test was different from that of the repetitions, but was the same in both cross-sections.

2. *Nonsense Syllables.* Here again, the procedure resembled that of the preceding experiment, except that there were twelve instead of eight syllables in each of the three series of the test; and for these, five repetitions were given. The marking was carried out as before.

3. *Poetry.* This consisted in repeating after the experimenter line by line a stanza consisting of about eighty words. The repetitions were preceded by one complete reading of the poem by the experimenter. The number of words correctly written out constituted the measure of the individual's ability.

4. *Prose.* A prose extract was memorised in exactly similar fashion. One of these extracts was as follows:

"I have thoroughly tried school-keeping, he writes, / and found that my expenses were in proportion, / or rather out of proportion, to my income; / for I was obliged to dress and train, / not to say think and believe accordingly, / and I lost my time into the bargain. / As I did not teach for the benefit of my fellow-men, this was a failure. / I have tried trade, / but I found that it would take ten years / to get under way in that, / and that then I should probably be on my way to the devil." /

For this piece, one complete reading on the part of the experimenter and four repetitions were given, after which the students wrote what they remembered, and the number of correct words was taken to represent the student's memory within the limits of this test.

5. *Prose Substance.* This consisted as before in reading twice a short prose extract, after which the substance was reproduced in writing. In order to make definite marking possible, pieces were selected which contained fact without reflexions, and a mark was given for every fact in the way employed in the previous experiment.

6. *Letters.* This was the only test in immediate learning, and differed from those of Experimental Series A in being extended to nine letters at one dictation.

TABLE V.

Shewing Improvement made in Test Series II.

			Improvement	Probable error
Dates	Group 1	Unpractised	3	17
	,, 2	Poetry practised ...	35	19
	,, 3	Tables ,, ...	63	17
	,, 4	Prose Subs. ,, ...	−1	17
Nons. Sylls....	Group 1	Unpractised	66	9
	,, 2	Poetry practised ...	100	10
	,, 3	Tables ,, ...	75	9
	,, 4	Prose Subs. ,, ...	4	10
Poetry	Group 1	Unpractised	14	11
	,, 2	Poetry practised ...	47	12
	,, 3	Tables ,, ...	−12	11
	,, 4	Prose Subs. ,, ...	7	11
Prose (literal)	Group 1	Unpractised	35	14
	,, 2	Poetry practised ...	43	15
	,, 3	Tables ,, ...	−1	14
	,, 4	Prose Subs. ,, ...	18	14
Prose Subs....	Group 1	Unpractised	16	22
	,, 2	Poetry practised ...	8	24
	,, 3	Tables ,, ...	65	24
	,, 4	Prose Subs. ,, ...	68	22
Letters.........	Group 1	Unpractised	34	9
	,, 2	Poetry practised ...	9	10
	,, 3	Tables ,, ...	30	9
	,, 4	Prose Subs. ,, ...	7	9

It is to be regretted that the test in memorising spatial relations, namely, that upon 'points,' had to be omitted. It might have been very useful in elucidating the unexpected result obtained in the schools.

Its introduction would, however, have necessitated extending the tests into the next term with the Christmas holidays intervening and disturbing the practice. The other three omissions, namely, the tests in 'names,' in 'dictation' and in 'geographical places,' were possibly less important. All three were tests of 'immediate memory,' and were probably sufficiently represented by that of 'letters.'

TABLE VI.

Practised Groups compared with Unpractised.

			Section II compared with Section I	Probable error
Dates	Group 2	Poetry practised ...	32	25
	,, 3	Tables ,, ...	(59)	24
	,, 4	Prose Subs. ,, ...	−6	24
Nons. Sylls....	Group 2	Poetry ,, ...	(33)	13
	,, 3	Tables ,, ...	9	13
	,, 4	Prose Subs. ,, ...	−62	13
Poetry	Group 2	Poetry ,, ...	(33)	16
	,, 3	Tables ,, ...	−27	16
	,, 4	Prose Subs. ,, ...	−7	16
Prose (literal)	Group 2	Poetry ,, ...	9	21
	,, 3	Tables ,, ...	−36	20
	,, 4	Prose Subs. ,, ...	−17	19
Prose Subs....	Group 2	Poetry ,, ...	−7	33
	,, 3	Tables ,, ...	49	32
	,, 4	Prose Subs. ,, ...	(52)	31
Letters.........	Group 2	Poetry ,, ...	−24	13
	,, 3	Tables ,, ...	−3	13
	,, 4	Prose Subs. ,, ...	(−27)	13

Note on probable errors of Table VI. The probable errors accompanying the results in the three test subjects of 'poetry,' 'prose' and 'prose substance' were obtained in the way previously described, but were based this time on considerably fewer data. There is reason to believe that they should be somewhat smaller; in this case the values of the corresponding numerically stated improvements would be slightly enhanced.

The other test conditions and arrangements were exactly similar to those of the experiment with school children.

The figures which correspond to Tables I and II are omitted, and only those similar to Tables III and IV given. Thus, Table V represents for each group the improvement or retrogression made in test series II;

and Table VI, the superiority or inferiority of each practised group to the unpractised.

ANALYSIS OF RESULTS.

1. From Table V we may gather that on the whole the second series of tests was better done than the first. This is quite in accordance with the previous experiment (see Table III), and with general psychological experience. In detail, it may be noted that only three results are minus quantities, and these are too small to be considered significant; so that not one result can be positively affirmed to be retrograde. About eleven of the twenty-four results are almost certainly significant, and these are all plus quantities.

2. Table VI, however, demands a closer analysis. We obtain there the comparison which chiefly interests us—that namely between the progress of the 'practised' and the 'unpractised.' A glance is sufficient to shew that there is no *general* superiority of the former over the latter. Of the significant values, four are plus, but two are minus. Considering each value individually, we may note that they are of three kinds, viz. (1) those which, taken in conjunction with the size of the probable error, give fairly certain proof of improvement of 'trained' beyond the 'untrained,' (2) those showing retrogression of the former as compared with the latter, and (3) those which tend to prove that the performances of the two were equal. It is especially in this last type that we find proof of the specific character of the training. For even if the training did not produce general improvement, it might conceivably have had a general influence for good or evil; but we see that at least ten groups have probably been entirely unaffected by the practice. Unless we arbitrarily assume that here the good and evil influences just balanced one another, the theory of specific training receives from these results confirmation only less strong than that obtained from those of Table IV.

3. We have already noted that only one form of practice exactly resembled one of the tests, that, namely, in 'prose substance' reproduction. We will examine the effects of this practice.

(*a*) In the first place, the test in this subject was, as we should expect, and as was also the case with the children, better done by those practised in this kind of work than by any other group. We may infer therefore that the training had an immediate and 'direct' effect.

(*b*) On the other hand, we see that the same training had a disastrous effect upon the memorising of 'nonsense syllables,' a very decided retrogression occurring here. The results indicate, therefore, that, in the case of observers of this age, the effect of this kind of practice upon the memorising of certain mechanical matter is actually injurious.

The introspective accounts, as usual, present occasional large individual differences. On the whole, however, they are in as good agreement with the objective results as could be expected. One student finds the second test in 'nonsense syllables' "slightly easier," although she cannot say that the practice produced this; another that "it is much easier," although she also refuses to see the cause of this greater ease in the practice exercises. On the other hand, three students assert that these exercises actually hindered certain mechanical subjects, and mention 'dates,' 'letters,' and 'nonsense syllables,' as examples. One says that she "got to like the practice exercise, and was interested in the extracts; other work became on the other hand less interesting." Another that "it seemed easier to remember scenes that could be pictured and less easy to remember unconnected matter; it seemed so uninteresting after the other." In this simple language and superficial introspection, it may be that the central relationship has been seized. There is not, however, a consensus of opinion, most of the students perceiving no connexion, although they describe the practice as very interesting, and the nonsense syllable test in particular as "very difficult."

The untrained, on the other hand, express no particular lack of interest, three asserting that they "enjoy" the exercise. The balance of opinion seems therefore to be on the side of those who assert that the prose substance practice had a deleterious effect upon subjects involving mechanical memorisation. Moreover, the accounts add a very reasonable explanation of this injurious effect. Thus most of the trained observers express a strong feeling of repugnance for mechanical memorising; the 'untrained' experience no such feeling. It is here then that we must locate the connecting and disturbing factor, namely, in the strongly contrasted feeling-tone accompanying the exercise.

In Experimental Series A we have noted that there was no transfer either of a positive or a negative kind. The two powers appeared to function quite independently. In Series B, on the other hand, we have just seen that the practice did have some influence, and the introspections give a very intelligible explanation. The difference between the results recorded by children and those of older observers should not occasion any surprise. The records of the two Series, with their explanations, are quite independent of one another; and the fact that mechanical memorising work is more repugnant to students than to school children is sufficient to account for the difference.

(*c*) With regard to other subjects, the group practising 'prose substance' reproduction remains in the same position it occupied in the first cross-section. The functions employed in the training exercises appear to have been quite independent of those engaged in the test subjects. Results, introspections, and agreement with previous figures all tend to demonstrate and emphasise the narrowness of the area influenced by the training.

4. We will next consider the effects of the practice in memorising 'tables.'

(*a*) In 'nonsense syllables,' the results again shew some dissimilarity from those of the Experimental Series A. Previously, there was a great improvement; in the present case, none. It will be remembered that the exercises in 'tables' performed by the students were amenable only in a very slight degree to the employment of rhythm. We should therefore expect to see this appear in the results, and it actually does; for the members of this group remain practically in the same position as the unpractised group. The training seems to have had no influence whatever upon the test subject. Although the two exercises contained important common data, for example, arbitrary meaningless associations, these seem to have served in no way to connect the two complex functions.

In the introspective work as to the effect or non-effect of the practice, one observer states that in the second test nonsense syllables "seemed harder"; two others assert that they "derived no help from the practice"; another, that she was "not helped; nonsense syllables always seemed easy." The introspections upon the method of memorising the nonsense syllables are unanimous in according to rhythm the predominant position in the process. Moreover, the students, possessed of keener powers of discrimination than the children, perceived from the very first the necessity of employing rhythm in the nonsense syllable test. It appears, therefore, that the absense of rhythm in the practice exercises, and its predominating presence in the nonsense syllable test, rendered the other common elements inoperative.

(*b*) There is some evidence (see Table VI) that practice in 'tables' led to transference of ability in an exercise somewhat similar, namely, the learning of 'dates.' The improvement its members recorded in this test is greater than that made by any other group, and much greater than that made by the unpractised. The school children, it will be remembered, gave no such evidence. How can we then account for the fact that the students did, apparently, find common and usable processes in the two functions? That they did so, is borne out by the introspections, such as "'dates' were helped by the practice, as I was able to connect figures and words better"; "I used the same method in both"; "I was very much helped in 'dates' by the practice, because I practised seeing the figures in imagination"; "'dates' seemed easier after the practice; I was better able to concentrate my attention upon this kind of work"; "the learning of 'dates' was much helped because of the practice in visualising figures"; "'dates' were helped by the rhythm I used in the tables."

Summarising the introspections, we find that the large majority of the members of this group depended upon visualisation, asserting (1) its development through practice in tables, and (2) conscious employment of it in the memorising of dates. Only one observer used rhythm. The other statements do not necessarily contradict those of the majority. It might be that the student who "was able to connect figures and words better" meant what others more clearly said; or that the better concentration of the "attention upon this kind of work," spoken of by another, referred to this improvement in visualisation. On the whole it would not be unfair to infer that the development of a special kind of visualising power through practice in tables was made use of in the test in 'dates.' Words and figures formed the material of this special kind of visualisation. The nonsense syllables remained apparently outside the range of this development. This would seem to be due partly to the difference of material, but probably more to the presence of rhythm, which would act on nonsense syllables, as we have before noticed, with sufficient power to render the employment of any other factor impossible.

5. (*a*) The group which practised verse we should expect to do well in the test in 'poetry.' In the former experiments, however, reasons were adduced why those expectations met with disappointment. Here the result is different, but perhaps not very much so. The figures may represent an improvement, but at all events not a great one. For the smallness of the superiority, as before for its absence, we must find grounds partly in the great differences in the methods of presentation in practice and test, and partly in the fact that the students were, in this subject, not very far from 'saturation

point.' On the other hand, the practice exercises and the tests were of fairly equal difficulty, and in the former the observers chose their own method of learning. In some cases, at least, the method was modelled upon that used in the test. For example, one observer allowed herself a certain number of repetitions and made great efforts to memorise the piece within the given number; one other enlisted the services of a friend to read line by line, she herself repeating after her. Probably also the maturer powers of perception of similarity, and hence of conscious adaptation, possessed by the students made the possibility of transfer greater than in the case of the school children.

At the end of the practice exercises five students asserted that they "noticed no improvement in the power of learning poetry." This unawareness of progress may perhaps be attributed to the force of suggestion. The five students expected a very noticeable improvement, and being disappointed in this, overlooked the actual smaller improvement which did occur. Expecting one hundred, they failed to appreciate ten—a very common psychological phenomenon. All five affirm, however, that they were able to concentrate attention more easily on this task. The other observers agree in asserting an improvement in their power of memorising poetry. The majority of the latter kept the logical sequence at first predominant, fixing their attention on the meaning and seeking connexion between facts; then relaxing this method of idea-organisation, they gradually trusted more and more to mechanical means of impression, such as that of maintaining a very distinct rhythmic action, beating time with finger or foot. This, they say, was true of the individual practices, and became more marked as the practices proceeded.

In the introspections made after the final test, the disagreement just noted almost disappeared. Only two of the five observers still failed to notice any improvement, and one of these "did not think the rhythm helped." She relied upon a clear comprehension of the meaning, and never allowed herself to repeat mechanically. The other three apparently found it easier to make a comparison between the two tests than between the practices, and merely assert that they found the last test much easier. One member of the group ascribed the improvement to a greater power of visualising the scene depicted in the poem, and she was thus able to pass more quickly to a purely mechanical repetition; another gave the same reason, adding that the influence of rhythm was also great.

We thus have some reason to think that the figure 33 in

Table VI represents a significant value; that the practice actually did influence the test work, owing to the presence of various common elements.

(*b*) The training in memorising verse seems, as in Experimental Series A, to have exercised some influence upon the learning of nonsense syllables. A transfer of practice effects has taken place, giving rise to a fairly considerable degree of improvement in the unpractised material. That the superiority of this group over all the others is not as large as that shewn by the corresponding group of school children is probably due to the fact that most of the older observers, as we have previously inferred, very quickly grasped the idea of the necessity of rhythm in memorising nonsense syllables; in other words, the effect of one test exercise in this subject was very much greater than was the case with the children. Hence we should hardly expect them to exhibit such great differences after practice.

Of the students trained in memorising poetry, six assert that the rhythm, of which they made constant use, helped them greatly in the nonsense syllable test; only one observer finds the cause of her improvement in a "development of the power of visualisation."

It is fairly evident, therefore, that the common feature in the two processes was rhythm; and it seems to have played an important part notwithstanding other disparate elements. It is probable that the very disparity of the other elements rendered the influence of rhythm more powerful because more easily separable.

In no other test does the influence of this practice appear; its effects have been quite local, quite specific.

V. DEDUCTIONS.

1. The main problem which was attacked in the experiments described appears to have received a very definite answer. If the results of the test are to be trusted, we may regard the solution as given in the simple formula, "Specific memory training is specific in its effects." Or, keeping in mind the prevailing ideas upon the subject, we may state it negatively: "Special memory training does not effect a general development in the power to memorise."

2. May we now, from the absence of general improvement, at once infer that there is no general memory function? Meumann, even though he believed in a general improvement through practice, refused

to see the cause in the development of a general memory function, in the sharpening of a 'memory faculty.' Among other reasons why he rejected such an explanation was the fact that the improvements were greater or smaller according to the degree of similarity between practice and test. But it has been shewn above, as we believe, that his evidence and, consequently, his inferences are untrustworthy. The further possibility, however, has been urged, that a faculty of memory might exist which could be trained by a one-sided practice, but which could not exhibit its development clearly because the very result of its activity, a habit, a strong psycho-physical disposition, fettered it. In an interesting article upon "The Relation of Special Training to General Intelligence" in the *Educational Review* of June, 1908, Judd seems to give expression to this view. He says "The amount of practice given in No. 2 was much greater in quantity, and more radical in type than with No. 5, but the reactor remained relatively unaffected. This means of course that when the reactor first came to the experiment, he was open to all kinds of suggestions. He was in the habit-forming attitude; he easily took on the effects of practice. But after the training which he received with line No. 5, he was less capable of acquiring new adjustments. He was no longer in the habit-forming attitude." As a page or two later Judd speaks of the 'faculty of observation,' and of an inherited 'general function,' it is not unfair to assume that he would also speak of the 'faculty of memory'; he presents us, therefore, with the hypothesis of a general memory function fettered through practice by the formation of specific habits. It is clear that we must agree with Judd as to his facts and their importance. We have seen that in some cases practice precludes new adjustments. If we were to maintain as well his rather fanciful theory of a simultaneously developed and fettered general memory function or faculty, we should be forced to the inference that the more unlike the material and procedure of practice and tests, the greater would be the improvement. This is contradicted by the results of the present experiments. In a certain sense, therefore, we may consider it proved that there is no general memory function which can be sharpened upon any material; that, in the well-known connotation of that term, there is no such thing as the 'faculty of memory.'

3. There still remains the further question as to whether there is a general training of some psychical factor such as attention or imagery. Meumann found in the general development of attention one cause of his remarkable improvements. James R. Angell, in an

enlightening article upon "The Doctrine of Formal Discipline[1]," confesses that, "so far as those several forms of attention have divergent elements in them, and certainly there are many such divergencies, both of sensory content and of motor attitude, we shall hardly be entitled to look for beneficial effects in the use of one form of attention as a result of discipline in another form of it." But nevertheless he believes "that the most important elements in the whole situation are capable of statement in terms of attention"; and he finds in voluntary attention the universal factor which, employed upon any topic, in any direction, undergoes a discipline, "no small part of which is to be found in the habituation which is afforded in neglecting or otherwise suppressing unpleasant or distracting sensations." Much of this is pure assumption, especially as to whether the power to suppress the unpleasant sensations of one kind of activity is transferred to those of another kind. At any rate, the results of the present experiments shew no such general development.

4. If all these hypotheses of a general function are incompatible with the present experimental results, no less so is the opposite extreme view, namely, that every kind of memorising is entirely independent of every other kind. As usual, a more accurate knowledge of the actual facts proves all these early *a priori* conjectures to have been far too simple. On the whole, the influence of one kind upon another appears to be much less effective and general than has commonly been supposed; often it appears wholly absent. But in other cases, again, it is distinct and important. In place, therefore, of the old, easy, dogmatic assertions or denials of this influence, modern psychology and pedagogy have to face the exceedingly difficult problem of tracing out the precise conditions upon which this transference of ability from the practice to some other performance really depends.

The chief explanation hitherto suggested is that of 'common elements.' Suppose that the two performances in question are represented by the symbols a, b, c, d, e, and e, f, g, h, j, respectively; that is, they have a common element in 'e,' which may represent certain common elements of data, method of presentation, method of memorising, or other conditions. It has been maintained that practice in one of these performances included practice in 'e,' and that to this extent, the other performance must benefit also. But in our previous analyses, we have seen that different individuals are affected by the presence of the common element in different ways.

[1] *Educat. Rev.*, June, 1908, 9.

A. The individual mind was sometimes able to make use of the functional relation expressed in '*e*.' In this case we found joint improvement. From the introspections it is clear that

(*a*) some achieved the improvement with the knowledge of the exact point of similarity between the two processes, consciously applying this knowledge;

(*b*) others improved, conscious that a similarity existed, but without knowledge of the exact point of similarity;

(*c*) others improved, without any consciousness of the fact of similarity.

B. The individual mind was, in other cases, unable to make any use of the element of similarity. This condition was accompanied by one of the following characteristics:

(*a*) the observer perceived no similarity; or

(*b*) aware of some similarity, he was still unable to make any use of it owing to other conditions.

C. The mind was hindered by the presence of some common element. Here we found actual retrogression, and here, too, the individual was either

(*a*) unaware of any similarity; or

(*b*) aware of it, was incapable of modifying the process upon which improvement depended. The very consciousness of the common element, in the midst of other foreign elements, caused mental confusion.

Thus in the 'theory of common elements' we have not yet reached the whole truth. The mere fact that two memorising exercises possess in common a certain kind of material, or method of presentation, is not sufficient to produce transference. We require, over and above this, that the common element should be *usable* by the individual. And for this purpose even awareness of the common element is not essential. Unconsciousness of it does not always eliminate its influence. Nor does consciousness of it invariably make it effective; although it is doubtless true that awareness of the usable common factor or factors may give rise to earlier and greater transference.

The analysis must go deeper. It may be that some factors are so intimately fused with other elements that the whole complex cannot be disintegrated, just as certain features of a habit form so integral a part of it that they invariably set that whole movement off, and only with the greatest difficulty any other. Thus if *a, b, c, d, e, f, g, h, j*, represent the elements of one complex function, and *k, c, d, p, l, m, s*, represent the elements of another, we cannot infer that there will be what has been called 'transfer.' It may be that '*c*' and '*d*' are so intimately connected with the other elements that no transfer is possible. Mere identity, therefore, in a portion of the elements, is not sufficient. Improvement in any single mental function need not improve the ability in functions partially identical. It is evident that in such cases as the last, there are many possible modifications. The common elements may never be separated; or perhaps the second function may be repeated so frequently that associative links between *c*, *d*, and *p*, *l*, *m*, *s*, may become stronger than with *e*, *f*, *g*, *h*, *j*; in neither case will there be any 'transfer' of improvement; rather, indeed, an increased difficulty in forming and strengthening a new combination.

Certain elements common to two functions stand, however, in a different position; they are more easily separable from their associative connexions. They may owe this position to the fact that they form a prominent constituent in many such functions, and are being brought continually into exercise. They become dissociated by means of varying concomitants. There are, for example, rhythm; visual, auditory and kinaesthetic imagery; concentrated attention to material of a certain kind. The frequency of occurrence of such prominent process elements, where other conditions are not too strongly opposed, may enable the observer to disintegrate the complex function and to make use of the practice element in another connexion. We see therefore that the amount of improvement in practice capable of producing a given amount of improvement in any other material must vary according to the number and nature of the common elements, and the individual mind.

It seems clear, moreover, that even where identical elements occur, and are usable by the individual, the 'transfer' is generally certain to meet with some opposition, an opposition taking the form of the necessary effort of disintegration and separation from the first associates. So that Thorndike's statements seem to require some modification when he says that "the change in the second function is, *in amount*, that due to the change in the elements common to it and the first"; and that "the change is simply the necessary result upon the second function of the alteration of those of its factors which were elements of the first function, and so were altered by its training[1]"; and the example he uses to enforce his view, namely multiplication and addition, where

[1] Thorndike, *Principles of Teaching*, ch. xv. 248.

the addition practice is an easily separable and indeed quite distinct part of the process of multiplication, is a peculiarly exceptional one. We must, on the contrary, infer that all 'transfer' is to some extent a wasteful process, in that, beyond the mere improvement of certain elements through 'direct' training, there is the additional necessary expenditure of mental effort in the disintegration of the common element or elements from the old, and integration with the new associates.

5. Let us now consider in detail which of the common elements have actually proved themselves most effective in the transfer of improvement. The data seem divisible into four chief classes. We have in the case of the practice in verse: (*A*) *data of material*, consisting of (1) logical verbal sequences, (2) poetical and therefore unusual inversions, (3) comprehension (often difficult), (4) rhythm, (5) rhyme, and (6) given length (from thirty to forty lines); (*B*) *data of method of presentation*, (1) purely auditory, (2) the general idea given by one initial complete reading, (3) repetition line by line after the experimenter, and (4) repetition until generally known; (*C*) *data of method of learning* (varying according to the individual), (1) prominently auditory or visual or motor imagery, (2) imagery of scene or action described in the poem—total or partial, (3) diffused attention, (4) the first few repetitions of a logical kind, that is, with attention to meaning, (5) nine-tenths of the repetitions primarily mechanical—chiefly rhythmic, and (6) feeling tone (agreeable); (*D*) *data of other conditions*, (1) small group of observers in separate classrooms, (2) the work far less rigid than in the test, and (3) the testing oral, partial and occasional.

Such an analysis cannot of course pretend to any degree of thoroughness, although it reveals several features which have hitherto escaped notice, but which are nevertheless essential parts of the data, having considerable weight when the question of transference arises.

(*a*) Of all these factors, a very considerable *rôle* must, as we have seen, be assigned to attention—not to attention in general but to its specific character. It would seem that practice in a particular medium cultivates a particular kind of attention, which can only function under similar conditions, such as similarity of material, method of presentment or of other data. It is true that all the observers declare that their power of attention has *generally* improved; but this is completely controverted by the results. The supposition that the development is general probably arises from the fact that in all the subjects which were memorised—both test and practice—a degree of development took place in each specific form of attention connected with the separate tasks. It is easy to see how these several specific forms of attention came to be regarded as one general power applicable in equal degree to any and every situation. We must therefore regard the psychical factor of attention, as constituting, in one or other of its innumerable specific forms, part of the data, and doubtless an important one, in any complex memorising function.

(*b*) Let us next enquire whether the power of visualisation has been affected, and if so, in what way. The introspections of the students practising 'verse' give no indication, except in one case, that during the twelve days of training any development of word imagery took place. The results of both experimental series are in agreement with these introspections.

Examining the introspections and work of the students practising 'tables,' we find that (1) eight of the twelve observers already perceive at the end of this short practice a development of visual imagery of a special kind, and (2) an improvement in the test upon 'dates' occurs. We have already noticed that the 'tables' practice of Series B gave less opportunity for the use of rhythm and more for visualisation than that of Series A. The element of rhythm having been almost eliminated, the observers were able to find in visualisation the explanation for their success in dealing with 'dates.' In no other test besides 'dates' is this 'transfer' to be seen—neither in 'nonsense syllables' nor 'poetry,' where rhythm is predominant; neither in 'letters,' where we might expect it, except for the difference in method of presentation; nor in 'prose' literally memorised, where the visual imagery was of a very different kind. We must come therefore to the conclusion that the training in 'tables' resulted, for both students and children, in the development of a very special kind of visual imagery, which, under certain conditions, could be employed in the memorising of related subjects, such as 'dates' and 'points.' It is upon the introspections and inferences here discussed that the hypothesis explained on page 426 depends.

Turning to the last group, namely, that which practised 'prose substance' reproduction, we find all the observers asserting the development of a power of visual imagery of that special kind necessary in constructing the scene described in the extracts read to them. Nevertheless, with the possible exception of the 'prose' literally memorised in Experimental Series A, no other test subject has felt the effects of this development. In 'dictation' and 'poetry,' where it would appear as if the same kind of imagery might have been effectively used, the presence of other conditions has hindered 'transfer.' The whole subject of visualisation affords further proof of the very narrow limits within which the spread of training takes place.

(*c*) In calling an element 'common,' authors of course only mean that it is of a similar kind; and the question arises as to how perfect this similarity needs to be for purposes of transfer. Here we are in close agreement with Thorndike, when he states that the transfer may be affected by "a very slight amount of variation in the nature of the data." To find confirmation of this, it is only necessary to remember how the small differences between the practice of 'poetry' and 'tables,' and the tests in what were considered very similar exercises, namely, 'poetry,' and 'dates,' respectively, affected the influence of the practice, and lowered it nearly to vanishing point. Moreover, neither poetry nor prose substance appeared to have any influence on the memorising by heart of 'prose'—a fact sufficiently astonishing when we note the presence of so many identical elements.

It is worth while noticing, however, that Thorndike's dictum just quoted refers almost exclusively to variation in material. His tests were usually so arranged that the elements of material in the two exercises were the only ones which differed to any appreciable degree; so that the word 'data' must for him represent, at least most prominently, the idea of 'material.' It is doubtless true, that in Thorndike's experiments, however slightly the material was modified, a loss in the power to memorise the new material was observed. But upon the basis of the present experiments, we must limit the scope of this principle. It would appear that an external similarity, such as that of material, is not always sufficient to produce transfer, and may indeed, without other factors, be of but little importance. Other similarities, such as method of procedure, imagery, special form of attention, logical organisation of matter, have proved considerably more important. It is in these directions that we must generally look for usable common factors. On such a theory it is easy to understand why a great difference in the material as, for example, that between 'nonsense syllables' and 'poetry' should in no way hinder transference. Such results may have the effect of disturbing many of the presuppositions of an unscientific pedagogy.

The explanation just offered confirms the conclusion to which other experimenters have come as to the great difference in the mental processes involved, on the one hand in prolonged, and on the other, in immediate learning. If we omit the two very doubtful cases of Table IV, we have no result in all four immediate learning tests (consisting altogether of forty-eight items in the table), which shews with a fair degree of probability any influence of the training. Such results can be brought under the principle which appears more and more prominently as we analyse the figures of the whole experiment, the principle, namely, that improvement only takes place where the two exercises have *usable* common elements. We cannot say that immediate learning and prolonged learning are two entirely disparate functions; there are common elements, but usually these are more than counterbalanced by important dissimilarities. For example, the material of the two exercises may be very similar; but the differences in the methods of presentation, involving such differences in the process of memorising, as distribution of attention, etc., may more than counterbalance the similarity of material so that improvement in the practice may fail to bring about any improvement in the allied subject.

(*d*) With the modification in the meaning of the word 'data' which we have indicated,—a meaning which must now include not only 'content' but 'form,'—we can again agree with Thorndike when he says that "the loss in the efficiency of a function trained with certain data, as we pass to data more and more unlike the first, makes it fair to infer that there is always a point where the loss is complete, a point beyond which the influence of the training has not extended[1]." And further, "the rapidity of this loss, that is, its amount in the case of data on which the function was trained, makes it fair to infer that this point is nearer than has been supposed[2]."

6. Great similarity may be counterbalanced, or more than counterbalanced, by important differences. In other words, a difference in the midst of many similarities may be sufficient to prevent transfer, or may even cause retrogression, owing to the resulting confusion and hesitation in the method. For example, the procedure in the practice of verse was to allow as many repetitions as were found necessary for complete memorising. This method encouraged a mechanical procedure. In the corresponding test it was announced that only a given number of repetitions would be allowed. Each observer had thus to change his method of learning. Each found that it was now necessary to give

[1] Thorndike and Woodworth, "The Influence of Improvement in one Mental Function upon the Efficiency of Other Functions," *Psychol. Rev.*, 1901, VIII. 250.

[2] *Ibid.*

attention to the scene described and the meaning; and that the most successful result could no longer be obtained by the mechanical method used in the practice. Thus we had, instead of the mechanically, the logically formed association, with a consequent loss of transference. This may appear, in its extreme form, as reciprocal interference, represented, not certainly, but with some degree of probability, by some of the results of Table IV.

7. A very unexpected element may enter into such experiments as the present ones. The practice may tend to become very distasteful or very pleasant, with possibilities of all the intervening degrees of these qualities. In the first case, attention would very necessarily be of an effortful kind, and a recent writer (Angell) has laid very great stress on the fact. "From this point of view," he says, "it may well be that such studies as the 'Classics' and certain forms of Mathematics have a peculiar value in affording the maximum of unpleasantness diluted with a minimum of native interest, so that a student who learns to tolerate prolonged attention to their intricacies, may find almost any other undertaking, by contrast, easy and grateful[1]." And again, "It is held by certain psychologists that although each form of sensory and ideational attention involves a special and peculiar motor attitude not found in any other form in which attention may be exercised, it is nevertheless true that there is a general attitude on which each of these special forms is grafted, which remains as a constant background for them all. Of course, if this contention be true, and I am disposed both on theoretical and experimental grounds to think that it is, there would be some matrix common to all acts of attention, and any development whatever would affect this central core in some degree[2]." The writer confesses that he is here on distinctly speculative ground. Our own experiments were not framed in such a way as to throw much light upon the hypothesis that unpleasant training makes other performances easy by contrast. For all the practices, though long and entailing considerable effort, failed to produce any distaste; but on the contrary, so far as the children's demeanour and the students' introspections correctly indicate their mental attitude, proved distinctly interesting. These experiments do, however, throw some light upon the reverse phenomenon, in which exceptionally pleasant training makes other performances hard by 'contrast.' For where the training is of a very pleasant kind (exemplified in the 'prose substance' practice), we find a

[1] Angell, *Educat. Rev.*, June, 1908, 9—10.

[2] *Ibid.* 10.

considerable decrease in the power of memorising other and very dissimilar material. According to the introspections, pleasant feelings, aroused by intelligent organising operations, seem to have given rise to feelings of repugnance to work of a mechanical nature. The unpleasantness of the mechanical work, strengthened by contrast, has hindered the performance of the task.

The 'laws of contrast' are known, and include many important limitations, such as propinquity, resemblance in all non-contrasted elements, etc. The results with which we are here dealing, and for which we have attempted to account by the alternation of emotional processes, harmonise quite well with these laws, but not at all with the formation of stable habits, as contemplated by Angell.

8. We will next consider the question of the duration of the effects of practice. For this purpose it will be necessary to study the figures of section IV which records the results of the series of tests given after the completion of the cyclic series at the end of a further interval of time, during which no group received any training. In doing this, we must not forget that although some of the figures in this section, as shewn in Table II, represent much higher values than those of the other sections, it would not be fair to regard them all as having any real bearing upon the present question, for the following reasons:

(*a*) The fourth cross-section stands outside the cycle of equivalent test series; from which it follows that the tests may have been more or less difficult. From the size of the figures we might infer that they were much easier.

(*b*) The groups in this section, although still equal in average memory ability according to the calculation based on the first cross-section, were not identical with those of the other sections.

(*c*) School X is not represented at all.

If the fourth section, like the first three, had been within the cycle of tests as described, the question of permanence of improvement could have been directly solved by calculating the difference between sections III and IV. Under the present circumstances, this would be an obviously unfair procedure since, unless equivalence existed, we should be uncertain whether a large difference was due to permanent practice effects, or merely to the greater or slighter difficulty of the test material.

On the other hand, material that is made use of in some instances, such as 'points,' 'nonsense syllables' and 'letters,' cannot be said to increase or decrease in difficulty to an appreciable extent. Two other tests, namely, 'dates' and 'geographical positions' may approximate to this condition. We will therefore study the problem with reference only to these five tests.

TABLE VII.

		Section IV compared with Section III	Probable error
Points	Group 1	−33	15
	,, 2	−34	16
	,, 3	+14	16
	,, 4	+26	15
Nonsense Syllables	Group 1	+49	11
	,, 2	+60	11
	,, 3	+15	11
	,, 4	+30	11
Letters	Group 1	+ 7	9
	,, 2	−17	9
	,, 3	−26	9
	,, 4	+16	9
Dates	Group 1	−19	12
	,, 2	−71	13
	,, 3	−45	12
	,, 4	−32	12
Map Test	Group 1	−12	9
	,, 2	+ 3	10
	,, 3	+24	9
	,, 4	−31	9

Group 1 refers to the 'Unpractised,' Group 2 to the 'Poetry practised,' Group 3 to the 'Tables practised,' and Group 4 to the 'Prose Substance practised.'

In Table VII a comparison is made between the results of sections III and IV, shewing, by the same method used in Table III, the improvement or retrogression made in section IV by each group; Table VIII indicates how the figures of each trained group compare with those of the untrained.

In order to make the comparison between section III and section IV a fair one, School X and the children who had to be omitted (as described) from section IV, were also eliminated in making the calculations of the two last Tables, so that the figures of both sections are based upon exactly the same numbers and upon the same children. It is clear that by treating section III in this manner it has been necessary to set aside the cyclic arrangement by means of which equivalency in the tests was secured. It depends, therefore, entirely upon the correctness of the assumption we have made, namely, that the five tests here dealt with are of equal difficulty in sections III and IV, as to whether the two sets of data are comparable.

Dealing first with the question of the permanence of the effects of 'direct' practice, that is, the practice derived from the tests themselves, we see from Group 1 of Table VII that the test on 'points' shews a possibly significant retrogression. When we further take into account that this result is obtained *after* the additional 'direct' training involved in doing test series 3, it seems not unfair to assume that the loss in the effects of 'direct' practice is in reality greater than this figure indicates. In order to be quite clear upon this point it is perhaps worth while to restate the fact that the results recorded in section III are based upon the practice contained in working the *two* previous tests, so that in the same way the results of section IV record not only the effects of the interval without practice, but also the effects of the 'direct' practice involved in working section III. It is therefore impossible, at any rate here, separately to measure the effect of the loss of practice, and we must be content to infer that if there has been a loss it has occurred in spite of the 'direct' practice obtained in doing section III, and it can only owe its origin to the long cessation of practice. Applying this argument to the figures of Group 1 throughout Table VII, it would seem reasonable to conclude with tolerable certainty that in no case have the effects of direct practice augmented; and with less certainty that in four of the five cases there has been a loss.

Let us now turn to the more complicated figures of the 'indirect' practice, that is, practice upon material which is not similar to that of the tests. In Table VIII the effects of the 'direct' practice have been eliminated, and in that Table are shewn for the same five subjects (1) a comparison of sections I and III, indicating the improvement made by the trained over and above that made by the untrained, and (2) a similar comparison between sections III and IV.

(*a*) In Group 3 of the tests upon 'points,' section III shews a tolerably significant improvement (+ 69), and section IV indicates that some further improvement (+ 47) may have been made even after the long interval without practice. This advance cannot be attributed to the 'direct' practice involved in working section III of this test, since it was not general to all the groups which underwent training; it can only be put down to the effects of the 'indirect' practice. This is the only case where the effects of 'indirect' practice give any indication of having persisted during the interval; but even here, the size of the probable error makes such an inference very uncertain. There is no reason to see in the figures for Group 4 in the same test anything more than a chance variation. Similarly, and for the same reason, in the map test it would not be fair to attach much importance to the figures (+36) of Group 3, seeing that that Group shews throughout the corresponding tests no other effects at all of 'indirect' practice.

TABLE VIII.

		Section III compared with Section I	Probable error	Section IV compared with Section III	Probable error
Points	Group 2	26	22	−2	22
	,, 3	69	22	+47	22
	,, 4	−26	21	+59	21
Nons. Syllables...	Group 2	23	15	+11	15
	,, 3	57	15	−34	15
	,, 4	0	15	−19	15
Letters	Group 2	10	13	−24	13
	,, 3	6	13	−33	13
	,, 4	0	13	+9	13
Dates	Group 2	64	17	−52	17
	,, 3	16	17	−26	17
	,, 4	−20	17	−13	17
Map Test	Group 2	7	13	+15	13
	,, 3	−14	13	+36	13
	,, 4	−15	13	−19	13

Group 2 refers to the 'Poetry practised,' Group 3 to the 'Tables practised,' Group 4 to the 'Prose Substance practised.'

(*b*) In 'nonsense syllables,' Group 3 shews that in section III 'transfer' took place. This we also noted in Tables III and IV, attributing it to the influence of rhythm. Table VIII would tend to indicate that some of this influence has been lost during the interval of repose. In the case of 'letters,' however, the figures do not warrant any assumption of this kind, since we find that the influence of 'indirect' practice upon the memorising of this material in all the sections was *nil*.

(*c*) In the test upon 'dates' Group 2 records in section III what would appear to be a significant figure (+64); also in section IV but this time in a negative direction. This indicates apparently a considerable loss in the 'indirect' practice effects. No great reliability can, however, be attached to such an inference, for in this test in particular we are far from certain that the tests were of equal difficulty. It is probable that this defect, namely, lack of equivalency, which may indeed have existed in some degree in all the exercises whose results are shewn in Tables VII and VIII, was present to a very considerable extent in this particular test.

Our investigation of this question has thus led us to the conclusion that the effects of both 'direct' and 'indirect' practice are usually not permanent. In the one exception to this generalisation, namely, Group 3 of the test in 'points,' in which the 'indirect' practice effects do appear to be permanent, or at least to continue for some time beyond the moment when practice ceases, the common and usable element effecting 'transfer' was a specific kind of visualisation.

For confirmation of these inferences we must await the results of other experiments framed more directly with the view of solving this question.

A brief account of one feature brought out by the present experiments, a feature representing the nearest approach to the practical and pedagogical application of the results, may perhaps fittingly conclude this paper. I refer to the comparative influences of 'direct' and 'indirect' training. Having before us the results of a group which had no 'indirect' training (that is, the unpractised group), it is possible to institute a statistical comparison of the two kinds of training. We may do this by comparing the improvement effected by this group with the improvement made over and above this amount by the other groups. The former improvement is the effect of 'direct' training only, while the latter is the result of the 'indirect' training. Thus, in Table IX, the first figure indicates that '53' represents the advance made in the second series of tests by the 'unpractised' group, and may therefore be considered as the 'direct' effect for all groups. The '4' indicates that the group practised in 'poetry' improved in the second series of tests by 57 ($= 53 + 4$), as may be seen from Table III; the '4' therefore represents, if significant, the 'indirect' effects of practice.

TABLE IX.

Direct and Indirect Training compared. School Children.

		Section II compared with Section I		Section III compared with Section II		Section III compared with Section I	
		Direct	Indirect	Direct	Indirect	Direct	Indirect
Points	Group 1	53	—	1	—	54	—
	,, 2	53	4	1	17	54	21
	,, 3	53	– 6	1	55	54	48
	,, 4	53	– 4	1	28	54	23
Dates	Group 1	8	—	27	—	36	—
	,, 2	8	11	27	8	36	19
	,, 3	8	20	27	– 33	36	– 12
	,, 4	8	– 14	27	3	36	– 12
Nonsense Sylls.	Group 1	0	—	26	—	25	—
	,, 2	0	63	26	4	25	66
	,, 3	0	70	26	15	25	85
	,, 4	0	– 11	26	18	25	8
Poetry	Group 1	31	—	11	—	42	—
	,, 2	31	– 7	11	– 24	42	– 31
	,, 3	31	– 26	11	17	42	– 9
	,, 4	31	1	11	– 1	42	0
Prose (literal)...	Group 1	32	—	5	—	37	—
	,, 2	32	– 9	5	– 4	37	– 14
	,, 3	32	– 11	5	6	37	– 5
	,, 4	32	6	5	15	37	21
Prose Subs.......	Group 1	16	—	21	—	37	—
	,, 2	16	0	21	– 22	37	– 22
	,, 3	16	28	21	– 21	37	7
	,, 4	16	58	21	– 27	37	31
Map Test	Group 1	13	—	43	—	57	—
	,, 2	13	– 18	43	69	57	50
	,, 3	13	– 26	43	27	57	1
	,, 4	13	– 23	43	37	57	13
Dictation.........	Group 1	13	—	22	—	35	—
	,, 2	13	– 3	22	– 28	35	– 32
	,, 3	13	– 5	22	– 4	35	– 10
	,, 4	13	14	22	– 14	35	0
Letters............	Group 1	20	—	9	—	29	—
	,, 2	20	– 2	9	– 3	29	– 5
	,, 3	20	– 6	9	8	29	1
	,, 4	20	– 2	9	0	29	– 2
Names............	Group 1	77	—	– 3	—	74	—
	,, 2	77	– 31	– 3	28	74	– 3
	,, 3	77	– 38	– 3	24	74	– 14
	,, 4	77	– 24	– 3	43	74	17

Group 1 refers to the 'Unpractised,' Group 2 to the 'Poetry practised,' Group 3 to the 'Tables practised,' Group 4 to the 'Prose Substance practised.'

A similar summary is given for Experimental Series B in Table X:—

TABLE X.

Direct and Indirect Training compared. Students.

			Section II compared with Section I	
			Direct	Indirect
Dates	Group 1	'Unpractised'	3	—
	,, 2	'Poetry practised'	3	32
	,, 3	'Tables practised'	3	60
	,, 4	'Prose Substance practised'	3	– 4
Nons. Sylls....	Group 1	'Unpractised'	66	—
	,, 2	'Poetry practised'	66	34
	,, 3	'Tables practised'	66	9
	,, 4	'Prose Substance practised'	66	– 62
Poetry	Group 1	'Unpractised'	14	—
	,, 2	'Poetry practised'	14	33
	,, 3	'Tables practised'	14	– 26
	,, 4	'Prose Substance practised'	14	– 7
Prose (literal)	Group 1	'Unpractised'	33	—
	,, 2	'Poetry practised'	33	8
	,, 3	'Tables practised'	33	– 36
	,, 4	'Prose Substance practised'	33	– 17
Prose Subs....	Group 1	'Unpractised'	16	—
	,, 2	'Poetry practised'	16	– 8
	,, 3	'Tables practised'	16	49
	,, 4	'Prose Substance practised'	16	52
Letters.........	Group 1	'Unpractised'	34	—
	,, 2	'Poetry practised'	34	– 25
	,, 3	'Tables practised'	34	– 4
	,, 4	'Prose Substance practised'	34	– 27

Now the 'indirect' training was very much longer than the 'direct,' extending over a period of three weeks, and consisting of twelve practices. Each practice lasted from twenty-five to thirty minutes; the test piece, on the other hand, lasted on an average twenty minutes, including in this the time necessary for writing. If we consider that the 'indirect' was at least twelve times as great as the 'direct' practice, we shall probably be taking far too low a figure; for, although there were twelve times as many practices as any one test, each practice was very much longer, so that we should not be far wrong in assuming that there was twenty times as much 'indirect' as 'direct' training.

There were other available methods of finding such a number. For example, in the case of some tests it would have been more exact to compare the number of repetitions allowed in the test with the number given during the whole period of practice. Thus in the test with 'dates,' eight repetitions were given; during the practice in 'tables,' an average of twelve repetitions daily, that is altogether one hundred and forty-four. By this method of calculation the amount of indirect practice was 18 $\left(=\frac{144}{8}\right)$ times as great as that contained in the test. This method, however, was unsuitable in the case of some tests, and was therefore not adopted.

Thus to interpret the two last tables fairly, it would be necessary to divide every 'indirect' value by twelve or an even much larger number. It is evident, then, that the general value of 'indirect' is very far below that of 'direct' training. By averaging the ratios of the three columns of Table IX, we obtain the approximation that one 'direct' practice is worth about 400 'indirect'; and by using the whole of the results of section IV, we obtain a ratio of one hundred and forty-four to one. By a similar method with Table X, we find that the 'direct' practice is worth one hundred and thirty-four times the 'indirect.' It is of course impossible to deal with the individual items of the tables in the manner described; partly on account of the magnitude of the probable error and partly because the actual fluctuations are considerable.

Without, however, attaching undue importance to the actual figures of the ratios, there is ample evidence in the calculations already made, that the effects of 'direct' outweigh immeasurably those of 'indirect' practice.

Provided then that the results are reliable, there seems to be very considerable evidence for discrediting the theory known as 'formal training.' We are led by the results of the present investigation to the general pedagogic conclusion that specific memory development is most easily and efficiently brought about by specific training of the particular capacity. He would be open to the accusation of foolishness who, with a view to perfecting himself in one direction, should practise in another, however closely related. It is not accidental, but according to the rigid laws governing all mental progress, that the British sailor, who receives no formal training, is known as the 'handy man.' It is unwise to dogmatize; general principles are not often so clearly defined and closed to modifications as this one seemed at first sight to be; and indeed, in view of such an opposing theory as we have seen developed by Meumann, Winch and others, it is necessary to lay special stress upon the simple principle without modifications.

VI. SUMMARY.

1. There appears to be no general memory improvement as the result of practice, nor any evidence for the hypothesis of a general memory function.

2. There would seem instead to be a very large number of related and unrelated memory functions, of a more or less complex kind.

3. The relation which produced transference is not necessarily (*a*) an external relation perceivable by an external observer, nor (*b*) a relation perceivable by the learner; but (*c*) a common factor, of which the individual mind makes use, consciously or unconsciously. The individual's awareness of the usable common element may produce an earlier and greater effect.

4. The existence of common elements in two memorising processes, though necessary, is not sufficient. To involve 'transfer,' the common elements must be separable from the complexes in which they occur. This process of disintegration usually renders the improvement brought about in the related subject smaller than that reached in the practised subject.

5. The factors which chiefly make for the transfer of memorising power are similarities of a fundamental nature, such as specific forms of attention, imagery, rhythm; in short, similarities of procedure. These will, within limits, vary for the individual mind.

6. A small change in one of these may be very effective in hindering joint improvement; but a change in material may produce little or no difference. This confirms a distinction often drawn between 'immediate' and 'prolonged' learning. It has also important pedagogic consequences.

7. Slight differences in the procedure may bring about a loss of 'transfer,' and the point where this loss is complete is much nearer than has generally been supposed.

8. Differences in the midst of great similarity in the mental processes involved may lead to loss of 'transfer,' or even to reciprocal interference.

9. Reciprocal interference may also be produced when the mind has to pass from an exercise involving a strong pleasurable feeling to one accompanied by a highly contrasted feeling-tone. This is illustrated

in the opposition between the operations involved in logical and mechanical memorising.

10. The effects of 'indirect' practice do not in general appear to last long beyond the period when practice ceases.

11. The effects of 'direct' practice are in general incomparably greater than those of 'indirect' practice.

VII. Appendix.

Method of finding the Probable Error.

Let any variable, x, be measured twice, resulting in x_a and $x_b = x + d_1$ and $x + d_2$ respectively, where d_1 and d_2 are the errors of measurement. It is assumed that d_1 and d_2 are uncorrelated with one another. Then the probable error of the average sum of the two measurements will, making use of known probability formulae, be given by

$$\text{p.e.} = \cdot 6745 \times \sqrt{\frac{\Sigma\,(d_1+d_2)^2}{n\,(n-1)}}\,.$$

But
$$\Sigma\,(d_1+d_2)^2 = \Sigma\,(d_1-d_2)^2$$
$$= \Sigma\,(x_1 - x_2)^2$$
$$= n\,(\sigma_{x_1}{}^2 + \sigma_{x_2}{}^2 - 2r_{x_1 x_2}\sigma_{x_1}\sigma_{x_2}),$$

where 'σ' and 'r' have their usual signification.

And σ_{x_1} in most cases nearly equals σ_{x_2} or if not, can easily be made to do so, so that

$$2\sigma_{x_1}\sigma_{x_2} = \sigma_{x_1}{}^2 + \sigma_{x_2}{}^2;$$

hence
$$\text{p.e.} = \cdot 6745\sqrt{\frac{(\sigma_{x_1}{}^2 + \sigma_{x_2}{}^2)\,(1-r)}{n-1}} \quad \text{.........................(1).}$$

In seven out of the ten subjects used as memory tests, we have the two measurements required, in the shape of the total 'odd' and total 'even' results. Thus when 40 items are memorised and an attempt made to reproduce them, the total of correct odd items (Nos. 1, 3, 5, 7, etc.), and the total of correct even items (Nos. 2, 4, 6, etc), are treated as two measurements of the memory in this particular test.

For the seven tests mentioned, the method was then as follows :—

(1) The probable error was first obtained for each group exemplified in Table I.

(2) For Table II the probable error of the pooled three groups from the three schools respectively was taken as approximately

$$= \frac{\text{p.e.}_{\cdot 1} + \text{p.e.}_{\cdot 2} + \text{p.e.}_{\cdot 3}}{3\sqrt{3}},$$ since p.e.$_{\cdot 1}$, p.e.$_{\cdot 2}$ and p.e.$_{\cdot 3}$ were always very similar amounts ....(2).

(3) For Table III the probable error of the improvement of Second over First Section, etc. $\left(\text{e.g. } \frac{\text{Av.}_{\cdot \text{II}} - \text{Av.}_{\cdot \text{I}}}{\sigma_{\text{I}}}\right)$ was taken from

$$(\text{p.e.})_{np} = \frac{\sqrt{\text{p.e.}_{\cdot \text{I}}{}^2{}_{np} + \text{p.e.}_{\cdot \text{II}}{}^2{}_{np}}}{\sigma_{\text{I}}}, \text{ or approximately } \frac{\text{p.e.}_{\cdot \text{I}\,np} + \text{p.e.}_{\cdot \text{II}\,np}}{\sqrt{2}\,.\,\sigma_{\text{I}}} \text{......(3),}$$

where $\text{p.e.}_{\text{I}np}$ = p.e. given by equation (2) for a group in section I,

$\text{p.e.}_{\text{II}np}$ = p.e. for the corresponding group in section II,

σ = standard deviation,

and $(\text{p.e.})_{np}$ is the required probable error of the improvement.

(4) The probable error of the difference of improvement between practised and unpractised (Table IV) was taken from

$$P = \sqrt{(\text{p.e.})_p^2 + (\text{p.e.})_{np}^2}, \text{ or approximately } \frac{(\text{p.e.})_p + (\text{p.e.})_{np}}{\sqrt{2}} \quad \ldots\ldots(4),$$

where $(\text{p.e.})_p$ = the p.e. of the practised found by (3),

$(\text{p.e.})_{np}$ = " " non-practised " "

and P is the required probable error of the difference of improvement.

From three other test subjects, namely, 'poetry,' 'prose' and 'prose substance,' it was not possible to obtain the two series of measurements necessary to obtain the probable error. The method of obtaining the p.e. in their case was as follows:—

(1) In the three cross-sections of the other seven test subjects, the results for section I and section III were pooled together, then the pool was regarded as one, and section II as another set of measurements of the same real values. The errors of these measurements were found in the same way as in (1). They represented, not as previously, the error due to momentary variations, but that due to variations occurring over a much longer period, the period, namely, between the cross-sections. The pooling was justified by finding that the improvement of II over I was, except for accidental deviations, just equal to that of III over II. Hence, pooling I with III produced a result that might be expected to differ from II by chance variations only.

(2) These were compared with the p.e. already obtained; thus, for example, in the case of the test in 'nonsense syllables,' the p.e. based on the pooling amounted to ·38; that based upon the odds and evens of each separate test was, averaging for the three sections, ·46; the ratio between the two being ·75. For all seven subjects of all three schools, the probable errors obtained in the two ways described were compared, giving 21 ($=3 \times 7$) ratios, which represented an average ratio of ·75, with a mean deviation of ·05.

(3) The probable errors obtained from the poolings in the case of the three exceptional test subjects were next multiplied by this ratio, the resulting figures being then considered as giving at least some rough approximation to the probable error found by the regular method already described.

Note. The smaller probable error derived from the pools is, of course, due to the fact that they involve more data. The question of the relative sizes of these probable errors will be discussed in detail in a subsequent paper.

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