Experimental contribution to a theory of thinking / by Henry J. Watt.

EXPERIMENTAL CONTRIBUTION TO A THEORY OF THINKING.

By Henry J. Watt, M.A., Ph.D., *Carnegie Fellow.*[1]

For this thesis a long series of experiments was carried out. *Several hundred nouns* of common occurrence were printed in big type on cards and were shown to the observing subject one at a time by means of an automatic *card-changer* (Dr Ach's). A metal plate, which covered the card, sprang up, when a string was pulled, and by so doing closed an electric current, which flowed through a Hipp *chronoscope* and a *speaking tube* (Cattell's). The chronoscope therefore marked the time which passed from the appearance of the printed word until the first vibrations from the subject's voice broke the current in the speaking tube. This constituted the measure of the duration of the reaction and formed, with a full account of all the reproducible experiences of the observing subject, which were at once written down in full, and any other remarks he had to make, the experimental data of the thesis.

In contrast to previous experiments, on association definite *tasks* (*Aufgaben*) were given, which the subject had to accomplish in the reaction. These referred to what the printed word on the card signified, and were as follows: to classify it, to name an example of it, to name a whole to which it belonged, to name a part, to name another of the same class or another part of the same whole. Each subject performed

[1] This paper, which is to be regarded as an abstract of a thesis entitled, "Experimentelle Beiträge zu einer Theorie des Denkens" (Doctor Dissertation, Würzburg, 1904, *Archiv für die gesamte Psychologie*, vol. iv. Leipzig: Engelmann, 1904. Pp. 154), was accompanied by a letter from the author addressed to Professor M'Kendrick, of which the following is a paragraph:—

"I have made no attempt to sketch a physiological theory which would give a basis for the psychological factors I distinguish in my thesis. It is only just to those who know the possibilities of such physiological theories better than I do, to allow a clear account of psychological analysis to tempt *them* to any such undertaking. In several points, besides, as will be evident to you, my work goes rather to strengthen the hands of those who, for the present, want to work out their physiological material directly without any conclusions from psychological theory. The most we psychologists can hope meanwhile is, that some analysis of ours may suggest a new idea to some physiologist which he might try and investigate directly on physiological material. That would be something to be proud of! It will also be good if the impression gains ground that experimental psychology is an intelligible and exact science and not a mere play with dreams."

the experiments separately, and every care was taken, both in regard to technical details and to the way the experiments were carried out, that no disturbing factors should be present. The most of the work was done by four practised observers, and over three thousand experiments were made in all.

The following are *the results*. In almost every case the subject is able to accomplish his task correctly. His description of his experiences shows that there are in the main *three kinds of complexes of experiences*. Most frequently the subject follows one line right through the experiment, which then leads to the spoken word. In the other cases, he may seek a word which he does not find, and which he even afterwards cannot name, or he may have intended to say a certain word, but for some reason or other, wittingly or unwittingly, have said another. In general the first class, the *simple reproductions*, take place in a good deal less time than the other two classes, the *complex reproductions*, of which two the second named usually and naturally last longer.

Within each of these classes there are *three groups*. In the *first* of these the spoken word follows directly on the given optical stimulus, sometimes after a pause which can be described in no particularly definite way, sometimes with the assurance of the subject that between the stimulus and the reaction nothing whatever has been experienced. Such a reaction lasts in general a very short time, and in the second form a shorter time than any other kind of association reaction. In a *second* and very large class, a *visual representation* follows the stimulus. Directly after that, or after a short pause or a so-called search, comes the spoken word. These are a good deal longer than the first set, and sometimes longer, sometimes shorter, among themselves according to the detail and vividness of the representation and the frequency of occurrence of such reactions containing visual representations for the particular subject. *Last* of all come those reactions in which a *word-representation*, or some experience which could only be described in conceptual terms and not analytically according to its psychological content—call it a thought—appeared between the presentation of the word and the spoken reaction. These were often shorter than those containing visual representations and sometimes longer. It is not, however, contended in this classification that the reaction could take no other course. On the contrary, it is easy to see that we could have tone, smell, taste, touch and other such representations playing a part in the reaction, provided the conditions of experiment produced them. None of these were clearly present among these experiments.

But what are these *conditions of experiment*? How does any one particular reaction come about and not another? The *first influence* at

work on the subject is the given *task*. This he hears spoken by the experimenter, and generally repeats to himself in words, *e.g.* "find a part!" "name an example!" or he may exemplify the experiment to himself, *e.g.* "animal—dog," and so on. The scanty description of the preparation for the experiment given in the subject's account of it does not help us to form a very clear idea of what the process itself is. It was found, however, as a series of detailed curves show, that of all the simple reproductions the percentage of occurrence of each of the three above-named classes changes regularly and similarly with each subject from one task to another. This leads to the assertion that the task has a regular influence on the *nature* of the experiences of each subject, which becomes particularly evident between the two larger groups of simple reproductions, those containing visual representations and those containing nothing at all. The change of task has a most decisive influence on the percentage of these classes, and a subject who has hardly a single visual representation when the task "classify" is given, may have them in 50 per cent. of the cases when the task "find a part" is given. Alongside this, a subject with 50 per cent. visual representations in the first case, may have 90–100 per cent. in the second. Moreover it is found that the *duration* of the reaction in each of these classes is also on the average dependent on the nature of the task. So too is the duration of the complex reproductions, but the percentage occurrence of these, out of all experiments made, is, curiously enough, quite *independent* of the nature of the task, as curves show. The attempt is made to explain this by a fairly probable consideration. The number of tendencies to reproduction which diverge from any one stimulus, must depend on the number of ideas with which the stimulus is associated. It is impossible to conceive how the task should change these, as an association must be presupposed before the task working with the stimulus could produce any reaction. The occurrence of a complex reproduction would depend then on the nature of the stimulus-word given and not on that of the task. The influence of the *task* has therefore to be carefully differentiated from that of the *stimulus*.

An analysis of the experiments worked with the fifth and sixth tasks shows that an experience which plays an *important* part in producing or leading to a reaction makes the reaction longer than when the experience only comes along with the stimulus or the reaction-word, that is, when it is only side-play, as it were.

States of consciousness *tend to persist* and to return more easily once they have been experienced. It is found that they come *more rapidly* after the first time. It is found, besides, that the task also tends to persist, for it also often comes to consciousness, in the form of a word-presentation

or the like, during the course of the experiment. In the great majority of cases this occurs only where some disturbing factor has been present, while the normal reproduction runs its course smoothly from beginning to end, as soon as the regular preparation for the experiment, *i.e.* the given task, has worked on the stimulus without any repetition during the experiment. The *repetition of the task* is therefore, we suppose, made necessary as soon as the task ceases to operate sufficiently well. This shows the exchange which goes on between representations and the task in operation. A suitable representation may introduce the task, which then, when it has ceased to operate effectually, may come to consciousness in similar representations. By means of such exchange it is possible to modify, strengthen, restrain, or check the task which is operating.

It has already been shown in experimental work on memory that the *rapidity of a reproduction* is dependent on the number of times the reproduction has occurred. In accordance with this it is found that the rapidity of such reproductions as those here described is dependent to a very large degree on the number of subjects who make any particular reproduction. The dependence is, of course, not supposed to be direct, but the co-ordination and the result presupposes that the number of subjects who make any given reproduction is a fair sign of the frequency of its repetition.

The result is very distinct and the exceptions can, as a rule, be explained by the record the subject gave of his experiences or by other experimental data. Further, if the average duration of each grade of frequency is co-ordinated with the change in the task for each subject, the *influence of the task* on the duration of the reaction *in each grade of frequency* is seen to be surprisingly similar to its influence in the previous cases. This means that the influence of the task is *independent* of the rapidity of the tendency to reproduction in itself, so that the influence of the stimulus-word is for the second time differentiated from that of the task. It is, then, probable that the rapidity of a tendency to reproduction from one point to another in the stream of succeeding ideas is something by itself, independent of the influence of the task operating at the moment. Whether the latter be to the increase of the former in every case remains to be settled.

It has often been asserted that over and above more or less mechanical reproductions, which are often to be found in our mental experience, there is a large number of cases in which the decision is not uniformly and completely determined by regular laws, but in which a greater or less amount of scope is allowed for the usually indefinite activity called choice or selection by the attention and the like. But a thorough examination of the complex reproductions, in which no particular description was given of

the second tendency to reproduction, produces a large mass of evidence, partly from the record of the subject and partly from manifold combinations of the various experimental data, much too detailed to be described, in favour of the reproduction which actually took place. This shows that, if other conditions remain the same, it is the individual strength or *rapidity* of the tendency to reproduction *which determines the reproduction*, and not anything else. In other words, the influence of the task is the same for all the reproductions it makes possible. It is not meant, of course, that our everyday conception of choice has no meaning, but only that the influences which determine every event in our mental experience fall into two large groups, the operating task and the individual strength of the reproductions which come thereby in question. On the one hand, the task may find no reproductions, in which case no reaction can occur; and, on the other hand, the strength of the tendency to reproduction may be too great for the task to operate, in which case it forces its way out in spite of the task, or before any reproduction which the task favours has had time to become actual: in other words, a wrong reaction takes place. Otherwise, more or less suitable reactions occur. This is thought to be *valid for the whole of our mental experience*, because the very few cases which offered no explanation, contained no indication of any other determining factors, and are therefore to be placed alongside the others with the remark that in these cases the record of the subject or the experimental data were probably deficient, as can always occur in such experiments.

A detailed examination shows further that the general content, the vividness, and the frequency of our *visual representations* is dependent on the nature of the *task* in question. It is therefore probable that rather hasty generalisations have been made of the possible *types of mental imagery*. It could very well be, according to this result, that a subject who showed an entire absence of visual representation with the kind of task which has hitherto been given to determine the types of mental imagery, would with other tasks show quite a lively and detailed visual imagination. An example of almost such a case occurred among the subjects used for these experiments. It is probable, however, that one who has fewer and less vivid imagery than another with one task, will with another task again have less vivid and detailed imagery than the other.

The attempt to establish an *association by contrast* or by *similarity* is then discussed, on the basis of the experiments, and is rejected, because it is found to be impossible to show that similarity as such could determine an association. Apparent determinations of reproductions by similarity are found to dissolve into more detailed reproductions, which are themselves

determined by the factors already discovered. There is no reason to expect that the subject in his record should be able to give the reason for any reaction, or even always the previous mental experience by which the reproduction in question under the operation of the task was determined.

A detailed examination of the experiments with each task by themselves, leads to interesting results which tend to separate the task as a psychological factor still more from the tendency to reproduction in itself and from other factors. Interesting connections are shown between the logical relations contained in the tasks given and the psychological processes found in the experiments, in which the psychological simplicity and rapidity of happening are shown to be sometimes on the side of the logical simplicity and sometimes not.

In a lengthy *summary* the results are brought together under various points of view and several *theories* formulated.

After a short summary of *individual differences*, a *criticism of the distinction between motor and sensory reaction* is given. First of all, the facts are brought together to show that this distinction is a fairly good description of some differences between the subjects. The first basis of the distinction was the usual arithmetical mean, but of late it has been thought that the *curve of distribution* of the reaction-times gives a better foundation. This curve is formed by making a time equal to the probable error of all time-observations of the series the unit in the horizontal, and by setting the number of cases which occur at each such unit on the perpendicular. If the number of factors involved is small and limited, then this curve ought to rise to one or more symmetrical points. This is sometimes the case, especially in the motor reaction, according to the latest researches. It is evident, according to the last two of these, that the time of even the motor reaction can be shortened a good deal with practice, and the curves seem to show points at somewhat regular periods,—these periods being, however, liable to minimal displacements when the nature or quantity of the stimulus is changed. It is also indisputably true, that the *class* to which any experiment is to be reckoned, is not determined by the nature of the experiment after it has been made, but by the nature of the given *preparation*, the direction of the attention to sensory or motor elements. Here, then, we have again differences between what we call the task and the mere tendency to reproduction or any physiological basis for the latter. A *motor reaction* is, therefore, merely the quickest and most constant reaction possible, which constancy and rapidity are achieved by simple and constant conditions of experiment and of task especially. The long-practised so-called *natural reaction*, in which the task directs the attention specially neither to the stimulus nor to the movement which is

to be carried out, also shows a regular curve of distribution. It is evident that in this natural reaction, too, the factors involved are constant and regular. The *sensory reaction*, however, is not nearly so liable to be regular, and it is supposed that this lies in the *greater complexity of factors*, because the curve of distribution contains not one, but several high points. This is made probable by its being shown that, in the curves of distribution of the experiments made, the average times of most of the big classes of experiments found and distinguished on the basis of the records, lie under the larger rises, and *vice versa*. It is then likely that, if the conditions could be kept as constant as they are in the shortest possible reactions, the curves of distribution would be quite as regular for any set of conditions whatever. Peculiarities in the form of the curve of distribution would then be *symptomatic* of peculiarities in the reactions or in the factors which bring about these, and thereby an aid to discovery. The distinction between sensory and motor reactions is, therefore, *not physiological but psychological* in the prime instance, and is not an exact distinction. It has to be split up into its elements, and when this is done nothing new is found.

This result leads to a more decisive way of looking at those reactions which, through frequent repetition, are held by many to become *unconscious* or *mechanical*. It is evident that, if reflexes be excluded from this class, a task is always necessarily presupposed for the accomplishment of such a reaction. The task may not have been given before each experiment, but it must at least have become operative. The stimulus is given and the reaction follows without any conscious links intervening whatsoever. There is no need to appeal to the unconscious even when everything else falls away except the essentials, task and stimulus.

The *method of subtraction* of different sets of reactions from one another, in order to find the duration of an act of recognition, of distinction, and of association, is subjected to a criticism. In order to find the duration of elementary acts, it is no guarantee to suppose that the contents of all experiments carried out with the same task are the same. First of all, those experiments which are really similarly composed, must be collected with the help of the experimental data and the records. An ideally complete reaction, made up of bits out of many different reactions, is of no use for this purpose. The scheme which has been the basis of this method of subtraction is, besides, very mechanical, much too mechanical for any one to suppose it to be based on data which are true, or likely to be found true in physiology. But even if the number and nature of the elements in an experiment were experimentally determined, it has to be remembered that it is not yet settled how exactly the task affects each element which goes

to make up the reaction. All this does not make the method impossible, but only for a long time purposeless.

If *association* be understood as the cause of the known fact and experience of reproduction, it may be *defined* as that by means of which it first becomes possible for one experience to be reproduced by another. Other definitions are found to rest on logical divisions, and to give no guarantee of unity in research. There can be only one kind of association, as far as we know, and on the basis of the previous results the later experience is never reproduced by the earlier by means of the *value* of the logical relations between them but only by the factors described above. The only conceivable condition for the origin of association is, that the two experiences shall have once been together or immediately successive in consciousness.

It is evident that, to form a *judgment*, the subject must have at the moment some experience, and, besides, some experience which consists of reproductions, because an absolutely new experience and nothing else could not be held to form a judgment by itself. An absolutely fixed and rigid system of reproductions, however, gives no judgments, but merely a succession of experiences under the one principle of association. Even the subjects themselves tend to decline the responsibility for judgments in which the reaction which constituted the judgment was determined by the overwhelming strength of a tendency to reproduction. The experimental conclusion drawn by Marbe is accepted, that if one confines oneself to the experiences between the stimulus and the reaction, there is no psychological criterion of the judgment. Outside of this limit, however, stands the *task* which, even if it is not identical in the sense of being always either visual representation or word-representation or the like, is yet *functionally identical*, and is the one factor which goes beyond the rigidity which the single tie of association would give. *The operation of a task makes the reaction* which is determined by or in spite of it, *a judgment* in reference to this task. This position must be met before the attempt can be made to set up hidden unconscious or rare experiences as the criterion of the judgment. It is also evident that the agreement of ideas with their objects, whether these be themselves ideas or not, can never be directly the aim in view. Such agreement, if it exists, can be only and merely the result of the operation of the factors enumerated, of which the one, the task, may of course include the conception agreement. For how would it be possible to proceed to obtain such agreement psychologically?

A *theory of thinking* has, then, to start from our experience as we know it. This presents to us no sharply defined states with beginning and end like printed letters, but only continued observation leads us to a more and

more detailed and exact description of our experiences. By means of experimental data we can work ourselves out beyond this position and formulate our factors more precisely. We decline to accept choice and apperception or contrast and similarity as exact or useful scientific conceptions any further. The tendency to reproduction which realises itself, *ceteris paribus*, is that one which, by reason of more frequent actualisation, possesses a greater speed of reproduction. The task, which is no doubt itself a wider and stronger tendency to reproduction, has been sketched in detail as an operative force, and its sphere of operation is doubtless much larger than we have been able to determine it to be. Over against any tendency to reproduction, the task can only overpower a limited amount of force, a circumstance which makes false reactions possible. Any theory of association which operates only with associations between two experiences immediately following one another, is thus seen to be insufficient, though this much must be presupposed in any theory. Physiology can, perhaps, not offer us more than this at present, but a more exact definition of psychological factors and their sphere of operation can only be welcome to physiology, while the prospect that physiology and psychology will one day be able to give an account of their material which they will find to be much more intelligible to one another than it is now, is by no means excluded. It seems probable at present that the *variable factor* is the strength or rapidity of reproduction and *not the task*, which is supposed to favour in equal strength all tendencies to reproduction which come under its influence. The operations of these two classes of factors on one another, which seems to be confined to a small area which contains at least our fully conscious experiences, is what we know as thinking.

It must not be supposed that the picture of his mental experience given in a subject's record is by any means complete. We see from these results that besides mere suppression of parts of a record, which is not presupposed, the subject may have forgotten something, or the tendencies to reproduction and the tasks which would have enabled him to give a full and accurate record may not have been present, or, for want of practice, very poorly developed. Even if forgetfulness is put aside, we have therefore no right to suppose that what is not in the record was not experienced. But granting this, what can we say about that part of experience which does not come fully to consciousness in reproductions and judgments? A mere mechanical succession of events in consciousness seems to us obviously intelligible, as soon as it happens in fact. What we do not understand is the *meaning* contained in the reference of one experience to another, whether it reproduce or be reproduced by this other. The reaction refers to the stimulus, and, under the influence of the task, brings to fuller consciousness

something which was latent in it, although, as we have seen, no other fully conscious elements need be found either in the record or by experimental investigation. There are, besides, several elementary experiences which cannot be further analysed into psychological components, but can only be rendered by one or many reproductions. Such experiences are the more indefinite conceptual states of consciousness, what is often called feeling (other than pleasure and pain). Such experiences may besides be introduced by representations, for example, word-representations, and they are then to be exemplified by conceptions and tasks. All this points to an *insufficiency of consciousness* to give a full knowledge of our subjective experience. The only means we possess for supplementing this deficiency, is to contrive that every part of our consciousness shall be operated on by tasks capable of bringing as much as possible to full consciousness in reactions or judgments. At the same time, this conception of the insufficiency of consciousness starts out from conscious experience and does not necessarily imply notions like the *unconscious*, which lie further afield and are as yet more or less indefinite and unsettled. The *great advantage of the experimental method* is, that it enables us, by grouping of data and by a more exact knowledge of the elementary factors of experience, to overcome the insufficiency of our direct introspection.

The thesis closes with a critical discussion of general representations and conceptions.