Acta Psychologica 30 *Attention and Performance II* (*W. G. Koster, ed.*) 1969, 412–431

ON THE SPEED OF MENTAL PROCESSES

F. C. DONDERS [1]

While philosophy is occupied in the abstract with the contemplation of mental phenomena, physiology, having at its disposal the results of philosophy, has to investigate the relation between those phenomena and the action of the brain. In the domain of morphology that relation immediately leaps to the eye. Considering the known facts of comparative anatomy and anthropology, any doubt concerning the existence of such a relation is untenable. But physiology cannot be content with that general result. Along with disorders observed in the case of pathological changes, physiology tries to locate the various mental faculties as much as possible by experimentation, and especially to trace the nature of the action accompanying the mental phenomena. It therefore relates the study on chemical composition and the metabolism of its components with the investigation of the fine structure of the brain. It finds that with the loss of blood or suppressed action of the heart, consciousness is lost, it learns from this that the regular supply of blood is a necessary condition for mental processes, and concludes that metabolism is at the root of brain life. Further, it establishes that, as in all other organs, the blood undergoes a change as a consequence of the nourishment of the brain, and discovers in comparing the incoming and outflowing blood that oxygen has been consumed, that carbonic acid has been formed and that heat has been generated. It knows that the heat may have originated from other forms of energy, for instance from electromotive action that it may postulate in the brain, after proving

[1] This is a translation of Donders' article entitled 'Over de snelheid van psychische processen' which appeared in 'Onderzoekingen gedaan in het Physiologisch Laboratorium der Utrechtsche Hoogeschool, 1868—1869, Tweede reeks, II, 92—120'. An identical article appeared in: 'Nederlandsch Archief voor Genees- en Natuurkunde 1869, 4, 117—145'. The translation was made by W. G. Koster.

A translation into French entitled 'La vitesse des actes psychiques' appeared in 'Archives Néerlandaises, 1868, III, 269—317'.

A translation into German entitled 'Die Schnelligkeit psychischer Prozesse' appeared in 'Archiv für Anatomie und Physiologie und wissenschaftliche Medizin, 1868, 657—681'.

its existence in morphologically and chemically related nerves. It further aims at tracing, by means of continuing research, all phases of the chemical process in the living brain, and to follow closely the series of transformations, beginning with chemical energy and ending up with heat. And convinced that the phenomena can be reduced to laws only by measuring and evaluating, it will not rest before it has established, not only the nature of such conversions, but also their quantity and that of the converted substances, and has thus found the equivalent of the different forms of energy.

But will it ever be possible for the function of the mind to be included in the chain of transforming forces? As far as we can see now there is no prospect of this whatever. The essence of all forms of work and energy we know and measure, is motion or the condition of motion, and nobody can imagine how from motion, in whatever way combined, consciousness or any other sort of mental function my be born. In form and nature mental function has a character completely of its own, as we perceive it in the first place in ourselves. Nowhere does it show a transition or an affinity to other natural phenomena, and the law of conservation of energy, valid for all known natural forces and forming the guiding principle in every investigation, is absolutely powerless to bring the mental phenomena under its control. For, apart from their specific nature, which makes their creation from chemical bonds as inconceivable as their transformation into thermal movement or electrical movement, they can neither be measured nor evaluated, and we know no unit by which to express sensation, reason and will in figures.

The question physiology has to pose itself is simply this: What happens in the brain during the processes of sensation, reason and will? It is readily understood that this formulation does not and indeed must not, prejudge anything. But we must also admit that the complete answering of this question, i.e. a complete knowledge of the functioning of the brain, with which each mental process is connected, does not carry us a step further in the understanding of *the nature* of their relation. An explanation of mental phenomena, in the sense in which we consider phenomena explained, would be attainable only if they could be reduced to a universal law, such as the one on the conservation of energy, and, as we have seen, this possibility seems a priori ruled out.

But will all quantitative treatment of mental processes be out of the question then? By no means! An important factor seemed to be susceptible to measurement: I refer to the time required for simple mental processes. For answering the question whether we are entitled to apply the generally proved relation to special cases – in other words, whether we may assume that there is an absolute correspondence between diverse functions in the brain and the diversity in each particular sensation, each private mental picture, each expression of the will – it seems that the determination of that duration of time is not without importance. For a long time I intended to make attempts to measure it. In the session of the Koninklijke Akademie van Wetenschappen [2] of June 24, 1865, I gave an outline of the first results, obtained with the cooperation of Mr. De Jaager and of some other students at Utrecht University. These results were subsequently communicated in greater detail in a dissertation by Mr. De Jaager: 'Over den Physiologischen tijd der psychische processen'.[3] The idea behind these experiments – as is, for that matter, mentioned in the preface – originated from me, the methods used were suggested by me, and the experiments were carried out in the Physiological Laboratory and under my guidance. About the same time I gave an outline of the results obtained, with an indication of the methods, in a few popular lectures held at Utrecht and elsewhere. Finally, in a section-meeting of natural sciences and medicine of the 'Provinciaal Utrechtsch Genootschap' in 1866 I demonstrated and described two instruments used in my experiments, the noematachograph and the noematachometer.[4]

Meanwhile, as the research continued the results of the experimental results accumulated to such an extent that I lacked the time to handle them properly, and as the prospect of my being able to do so is still not favourable, I have decided to publish the main parts for the time being in a condensed form.[5] To simplify the survey several points concerning methods, special results, calculations, etc. will be added at the end of this report in separate notes.

---

[2] Royal Academy of Sciences. A translation of the official report is inserted in this volume p. 409—411 (translator's note).

[3] The correct title is: 'De physiologische tijd bij psychische processen'. This may be translated as: The physiological time of mental processes.

[4] See this volume p. 432—435 (translator's note).

[5] As far as we know a more detailed description has never been published (translator's note).

Barely 25 years ago, the time in which excited nerves carried their commands to the brain and the brain activated the muscles, was supposed to be 'infinitely short'. Johannes Müller, to whom the first place among the physiologists of his time is due, not only pronounced the conduction velocity in the nerves unknown, but he even went so far as to predict that the means of determining this velocity would no doubt always be denied us. And yet a short time later, in 1845, Du Bois-Reymond sketched in general terms a scheme for such a determination, and as early as 1850 Helmholtz carried out the measurements.

The method was simple. Helmholtz excited the motor nerves at two points, the one close to the nerve-muscle junction, the other at a greater distance. In both cases he determined the time that elapsed before the muscle contracted. The differences in times indicated the conduction time of the nerve between the two excited points, and from this the conduction velocity could be obtained. It appeared to be no greater than 100 feet per second. [6] This is a speed surpassed by birds in their flight, approached by race-horses, and attained by our hand when arm motions are quickest.

This result was obtained with frogs. The method could not be applied to warm blooded animals, to human beings in particular. Helmholtz adopted another course. He exited the skin at successive points, the one at a short distance from the brain, the other at a greater distance, and in both cases he responded to the stimulus received as rapidly as possible with a particular sign, such as some movement for instance of the hand. The time between the excitation and the response being thus known in both cases, the difference between these times was considered to be the conduction time for the difference in length of the sensory nerves: for this appeared to be the only difference in the two experiments. In this way the conduction velocity in humans was found to be 200 feet per second, which is double that of frog nerves.

It is readily seen that the course taken in the research was not irreproachable. In the first place it is difficult to make the stimulus act with the same force in different places on the skin, and with differences in force the physiological time appears to be not the same. But in addition, the method supposes that the duration of the process in the brain is quite independent of the place of excitation. Even a priori this is not probable. If one enters a room from two sides successively to do something there, it is unlikely that in both cases one will leave the

[6] About 30 metres per second (translator's note).

room through a third door within the same interval of time. And apparently the difference in delay is completely attributed to the conduction velocity. Thus it is not surprising that in repetitions of experiments, mainly keeping to the same method, very divergent results were obtained. We need not report them, however, because recently the conduction velocity of a motor-nerve has been measured successfully in man in as easy and conclusive a way as in the frog, and consequently with complete exclusion of the mental process of the brain. It is Helmholtz again who indicated the right course.[7] He excited the nerves of the muscles of the ball of the thumb in succession at the wrist and above the fold of the elbow, whilst elbow, forearm and hand were fixed immovably in a plasterjacket: in both cases the muscles referred to contracted and the moments of contraction could be recorded via a lever on the 'myographion'. The result obtained was very satisfactory. As a matter of fact, with a very small deviation a conduction velocity was found of 33 metres per second, which is only a little more than found in the case of frog nerves.

With this direct determination all the experiments, including those in which the brain influences the sensory nerves have now gone down into history, and what that means is common knowledge. Von Wittich[8] would indeed like to maintain the somewhat higher conduction velocity of sensory nerves found by him. But that is impossible: the similarity of sensory and motor nerves is in all respects too perfect to maintain the conduction velocity found by doubtful methods against the value established by reliable ones. To what degree a difference in duration of brain conduction may be deduced from a great number of experiments on the effect of the stimulated place such as I have conducted on various subjects, is a matter which I may further investigate in the future.

Thus the conduction velocity in the nerves is known and the prediction of Johannes Müller has been brilliantly belied. It is worth noting that the theory created the courage to venture upon the solution of the problems referred to as insoluble. In fact, the theoretical notion that propagation cannot be considered as a progressive force or movement, but rather as a chemical and associated electromotive process, renovating itself at each point, prompted the surmise that conduction

---

[7] Königliche Akademie der Wissenschaften zu Berlin. 29 April 1867.

[8] Zeitschrift f. ration. Medizin. 1868. XXXI, S.112 u.f.

in nerves could not be so extraordinarily rapid, and that the shortness of the nerves could not be an absolute obstacle to empirical determination.

I

Would thought also not have the infinite speed usually associated with it, and would it not be possible to determine the time required for shaping a concept or expressing one's will? For years this question has intrigued me.

We have described above the method used in the study of the propagation velocity in sensory nerves. In these experiments the time that elapses between the stimulus and the response includes a particular mental process. The same is true of those experiments in which the stimulus acted on one of the other sense-organs. On this subject the first comparative research was done by Hirsch, the well-known astronomer of Neufchâtel. The time elapsing between stimulus and response he called *physiological time*, and with unchanged response, for instance a movement of the hand, he found that this time was shortest after stimulating the skin (close to the brain of course), longer after stimulation of the ear, and still longer after stimulation of the eye. In general this result was confirmed by later experiments. From the combined experiments, including those of myself and my students, I calculated the physiological time for the three sense-organs mentioned, viz. touch, hearing and vision, as being approx. $^1/_7$, $^1/_6$ and $^1/_5$ of a second respectively.

But what proportion of this forms part of the mental process proper? On this point we are completely in the dark. In that short lapse of time much has to be done. When we follow the process from the moment of the stimulus up to that of the response, we can distinguish:

(1) the action on the sensory elements in the sense-organs;

(2) the communication with the peripheral ganglion cells and the increase required for a discharge ('Schwelle' of Fechner);

(3) the conduction in the sensory nerves up to the ganglion cells of the medulla;

(4) the increase in activity in these ganglion cells;

(5) the conduction to the nerve cells of the organ of conception;

(6) the increase in activity of these nerve cells;

(7) the increase in activity of the nerve cells of the organ of will;

(8) the conduction to thenervecells governing movement;

(9) the increase in activity in these cells;
(10) the conduction in the motor-nerves to the muscle;
(11) the latency in the action of the muscle;
(12) the increase in activity up to the moment of overcoming the resistance of the response.

The whole process can be completed in $^1/_7$ of a second; even $^1/_9$ has been found as a minimum. The times required for the separate stages of the process cannot be determined. Only the conduction velocity in the nerves can approximately be taken into account, and this then leads to the result that the mental process of conception and expression of the will lasts less than $^1/_{10}$ of a second, but it does not allow us to claim that it takes any time at all. The truth is that these experiments teach us only about the limits of the maximum, while as to the minimum they give no decisive answer at all.

The idea occurred to me to interpose into the process of the physiological time some new components of mental action. If I investigated how much this would lengthen the physiological time, this would, I judged, reveal the time required for the interposed term.

The first results of the measurements were mentioned in the above-cited communication to the Koninklijke Akademie van Wetenschappen (see note I).

In the first series of experiments identical electrodes were placed on both feet. The arrangement was such that by tilting a Pohl seesaw[9] an electric impulse could be delivered to either the right or the left foot. The experiments were now executed in two ways: (a) in the first it was known to which foot the impulse was to be offered and the response was to be made by the hand on the same side; (b) in the second it was not known to which foot the impulse was to be offered but the response had also to be made by the hand on the stimulated side. In the latter case more time was required than in the first, and the difference represented the time required for deciding which side had been stimulated and for establishing the action of the will on the right or left side. For the rest the whole process was identical in both cases. It appeared that the mental action interposed in this manner required $^1/_{15}$ of a second, as calculated from the average values. Previously it was found that response with the left hand lasted 0.009 second longer than with the right hand, which difference was taken into account.

[9] The Pohl seesaw is described by De Jaager (Inaug. Diss. De Jaager l.c. p. 16, 17 and plate I). It appears to be a sort of two-way switch (translator's note).

This was the first determination of the duration of a well-defined neural process. It concerned the decision in a choice and an action of the will in response to that decision.

The same experiment was made with stimuli acting on the sense-organ of vision. Here the physiological time was ascertained with a simple response to light and with a differential response to a red and to a white light.[10] In the latter experiments the response to red light had to be given with the right hand, to white light with the left. In this case the decision in the choice and the related response appeared to require more time than in the previous experiments: averaged over 5 subjects it was 0.154 sec; the lowest average was 0.122 in the case of Mr. Place; the highest, 0.184, fell to my debit, since my age was twice that of other observers. Later on we will return to the cause of the difference between stimulation of the skin and of the eye.

In these experiments the signal was given with one of the hands. Later I carried out another series of experiments in which the stimuli were special letter-symbols, either uncovered or suddenly illuminated by an induction spark, and the response consisted in pronouncing the sound. In this case the interposed mental process requires 0.166 ($^1/_6$ sec) as calculated from the averages and 0.124 ($^1/_8$ sec) as calculated from the minima. This method lent itself further to experiments in which one vowel-symbol had to be recognised not out of two but out of five and to be pronounced as a sound. In this way we made no fewer than 5 series of observations on different days and it appeared that, with the wider choice out of five, some more time is indeed required than with the choice out of two, viz. 0.170 as calculated from the averages and 0.163 as calculated from the minima (note II).

Finally, the same experiments were carried out with stimulation of the ear. In this case the stimulus consisted in the sound of a vowel and the response was the repetition of the same vowel. Two subjects A and B are seated before the mouth of the phonautograph. Whilst the cylinder is turned, A utters a vowel and B has to repeat it as quickly as possible. The beginning of the vibration caused by the two sounds is to be seen on the line P (figure 1) at *a* and *b*, and the time lapse between the two can be deduced from the vibrations S of a tuning-fork recorded simultaneously. The experiments were carried out in two ways, (a) while the subject knew what vowel-sound was to be presented

[10] See De Jaager, l.c., p. 21—32: the method was not exact enough to trust the difference between the minima (compare note II).

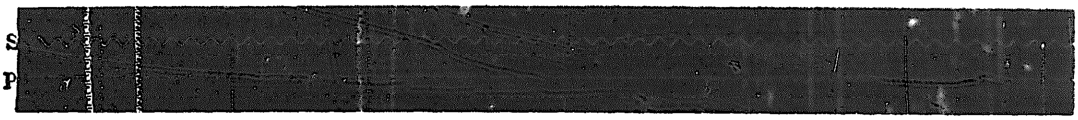

Fig. 1.

and had only to repeat the same sound as soon as possible, (b) while the subject did not know what vowel was to be presented, and thus had to form a clear idea of the vowel-sound in order to repeat it. In my first experiments with De Jaager I responded in 0.180 second on the average, in the case of a known sound *ki*,[11] and in 0.268 second in the case of an unknown sound, the result being a difference of 0.088 second; with longer times – especially at the beginning – De Jaager obtained an identical difference of 0.088 second. Later on, in seven series in which I myself had to respond to the sound, I found 0.201 second ($^1/_5$ second) on the average for a known sound and 0.284 second for an unknown one, hence a difference of 0.083 second (about $^1/_{12}$ second); as calculated from the minima this difference was reduced to 0.067 second (about $^1/_{15}$ second). In four other series, in which I had to repeat either the previously known stimulus or one out of *only two* unknown stimuli, the repetition of the known sound required a little less time (0.184 second) and the increment for the unknown sound was only $^1/_{18}$ (0.056 second) as calculated from the averages or $^1/_{16}$ (0.0615 second) as calculated from the minima.

In experiments with five vowel-sounds, carried out with three subjects of different, and generally young ages the increment found with an unknown sound was 0.088, 0.087 and 0.069 second respectively. A remarkable similarity, indeed!

Summing up the results obtained, it appears that the time required for a decision and the appropriate response is:

	second
(1) stimulation of the *skin;* choice, calculated from the averages	0.066
(2) stimulation of the *eye*;	
(a) two colours, choice, five subjects, calculated from the averages	0.184, 0.122, 0.159, 0.134, 0.172
(b) two vowel-symbols, choice, calculated from the averages	0.166
calculated from the minima	0.124
(c) five vowel-symbols, calculated from the averages	0.170
calculated from the minima	0.163

[11] Pronounced key (translator's note).

(3) stimulation of the *ear*;

(a) two vowel-sounds, calculated from the averages	0.056
calculated from the minima	0.0615
(b) five vowel-sounds, with myself, initially, calculated from the averages	0.088
later, calculated from the averages	0.083
calculated from the minima	0.067
The same with four other subjects	
calculated from the averages	0.088
calculated from the averages	0.069
calculated from the averages	0.087
calculated from the averages	0.088

Some of the differences immediately attract attention. First of all: why does the choice require less time in the case of differences in sound (0.056 second) than in that of differences in colour (0.122 second)? The answer is that the response given to the sound is the simple imitation which has become natural by training, more so than the conventional response with the right or the left hand in the case of differences in colour. Consequently, prolonged training will result in a higher speed of response in the case of colour experiments. With the imitation of vowel-sounds, on the other hand, the maximum speed appeared to have been effectively reached, and thus the values obtained here tell us immediately the minimum time required for decision in a simple choice, including the corresponding expression of the will, viz. $^{1}/_{18}$ second. However, with stimulation of the skin in which the response was likewise conventional (movement also of right or left hand) the same interposed process required only $^{1}/_{15}$ or 0.66 second,[12] thus little more than with the trained response to vowel-sounds. This result is not surprising either. We made the subjects respond with the right hand to the stimulus on the right side, and with the left hand to the stimulus on the left side. The tendency to respond in this way is already present as a consequence of habit or training: for, when movement of the right hand was required with stimulation of the left side or the other way round, then the time lapse was longer and errors common. I would make a final remark. The recognition of vowel-symbols and the response to them require about double the time needed for recognising vowel-sounds and making

12 Probably 0.066 second is meant (translator's note).

the appropriate response, and we have certainly as much training in seeing and pronouncing vowel-symbols as in hearing and repeating them. I was very struck by this result. The reason may lie in the different stages of the complex process. I believe, however, it must be looked for in the purely mental process. I calculated from joint observations with different subjects that the response to light usually requires somewhat more time than the response to sound. Combining the results, obtained with myself as a subject, of 8 series of experiments with responses to light and 12 series with responses to sound, I find that they are equal, viz. for the former 0.1953 second, for the latter 0.1952 second.

Of course, such a complete resemblance is accidental, the more so as in some experiments single vowels were used, in others vowels with plosives, the corresponding physiological times differing a little (note III). Yet this shows that the physiological times for sound and light do not noticeably diverge in my case. I also believe I may assume that the distinguishing of two colours takes place as quickly as distinguishing two sounds, and that with enough training the time required to make a response to the distinction of the former sort might be reduced to that required by the latter. The reason for the difference is to be found, I suppose, in the form of the symbol, which is not so quickly distinguished by the mind as is the sound. To account for this difference we have to analyse somewhat more closely the impression of the sound and of the form of the symbol. On the retina that impression is very complex. A number of sensory elements, each transmitting to the brain the excitation it received together with its own local sign, are suddenly excited, and from this our conception of the form is constructed. The elements excited are quite different when the symbol is large from when it is small, and yet an *a* remains an *a*, an *i* an *i*. A small deviation of the axis of vision also makes the whole picture of the letter-symbol fall on different elements of the retina. Thus the process of conceiving of the form is necessarily very complex, and it is not surprising that it takes more time than that of an impression of light in general, or even a colour that acts on certain sensory elements or only represents a particular type of energy. For such a process, prolonged by the time required for differential expression of the will, 0.16 second is comparatively not much. And now, how is the concept of a sound formed? For many sounds the process may be as complex as for instantaneously illuminated small forms. After all, normal sounds are composed of a number of individual vibrations that also bring into action different nerve fibres and each pitch has its own system of nerve fibres that receives the impression: the only thing that makes the process appear more simple is the relation between the individual tones which, at each pitch, are again mainly what are called the harmonics. But, although the process is made up in that way for sound in general, this does not apply to vowels. Whatever the pitch of the voice with which it is produced, for each vowel the tuning of the oral cavity is absolute, as I demonstrated already ten years ago, and in this connection any vowel has its absolute, virtually invariable harmonics. Thus, with the sound of the same vowel at any pitch some of the same tones are produced, and each time some of the same nerve fibres are excited, which, when a vowel-sound may be expected, characterise it immediately. That is why the conception of vowel-timbre does not suggest such a complex process as is considered necessary for the conception of a vowel-symbol.

After measuring the combined time lapse in which the discrimination between two or more impressions as well as the corresponding expression of the will is possible, the question arose whether the times required for the two stages of this process could not be established separately.

It seemed to me one would approach this problem by making it a necessary condition that a response should be made only to one stimulus, ignoring all others.

Thus, vowel-sounds were established as stimuli without further indication, but only one, for instance *i*, had to be responded to with *i*, the others were not to be responded to at all. The subject strains his ears for the recognition of *i* and keeps the position of the parts of the mouth and the mechanism ready so that, upon recognising the *i*, he has only to breathe out to produce the corresponding sound, just as in the case where a subject knowing that an *i* was to be heard had to respond with *i*. In this sort of experiment no choice is now required for the response: only the distinction, the recognition of *i*, is interposed in the normal process. And, indeed, it was found that less time was required than for responding to each vowel sound with the identical sound. Of the many experiments carried out by myself in this way, I will only insert the results of three series, carried out on the same evening and in such a sequence that as far as fatigue played a role, it was distributed uniformly over the three experimental procedures:

(a) responding to a known sound;
(b) responding to an unknown sound;
(c) responding to one of the unknown sounds.

With each of these ways the average duration and the minimum were recorded:

thousandths of a second

for (a) the average duration 201, the minimum 170.5
(b) the average duration 284, the minimum 237.5
(c) the average duration 237, the minimum 212.6

the following values are now found:

	from the averages	from the minima	averaged
b-a =	83	67	75
c-a =	36	42	39

In these experiments, then, the time required for the conception of a certain sound (longer duration with method c than with method a) was only a little more than half of that required for that same conception combined with the corresponding expression of the will. The development of a conception takes 0.039 seconds for myself, i.e. nearly $^1/_{25}$ of a second, and a little less, just over $^1/_{28}$ of a second, is required for expression of the will. In the foregoing we have reported experiments with other persons who needed less time for the combined process. In these experiments, too, the time for both stages is probably to be divided into about equal parts. From the determination they made with the c-method this fact cannot, however, be deduced satisfactorily. It has appeared to me that to many people the c-method offers certain difficulties. They give the response, when they ought to have remained silent. And if this happens only once, the whole series must be rejected: for, how can we be certain that when they had to make the response and did make it, they had properly waited until they were sure to have discriminated? In addition, since the very vowel-sound to which a response has to be made is presented only now and then the method is invariably accompanied by the drawback that most revolutions of the cylinder remain unused. For that reason I attach much value to the results of the three series mentioned above and obtained with myself as a subject, utilising the three methods described for each series, in which the experiments turned out to be faultless (note IV).

Meanwhile one could still doubt whether with the procedure used it is really the time required for a certain conceptual process that is being measured. Is it not rather the difference of time which we find between the determination of the nature of the vowel-sound and the simple hearing of it? Our answer to this is in the negative. Anyone who has carried out the experiments knows that where the object is only to give a response in general, the response is made to anything that happens. Although one is waiting under tension for a visual stimulus, one will also respond involuntarily to a sound, and vice versa; and likewise to a shock, an electrical impulse, in a word to any strong impression. One does not wait till one hears, but only till one perceives, and with the method used one thus finds the time that elapses between the first moment of perception and the complete conception of the nature of the sound, i.e. the time required for the development of a certain conception.

I carried out these same experiments, using the c-method, when the stimulus was *seeing* a vowel-symbol. The time required for recognition was then rather short, scarcely longer than with vowel-sounds. This results is certainly remarkable when one considers that the discrimination of vowel-symbols, as appeared to us in the experiments with the a- and b-methods, requires much more time than the discrimination of vowel-sounds. Yet I believe that it can be explained. In the experiments with the b-method one could not conceive beforehand what impression one was to receive: one even had to refrain from doing so in order to be able to respond with equal speed to any vowel-symbol that might be presented. A relatively long interval was now required for discrimination. On the other hand, in the experiments with the c-method of which we are speaking here, all vowel-symbols could, indeed, also be presented, but one had to respond to only one of them; with the others one had to keep silent, and thus one could and even had to have a conception of that one symbol in order to give immediately on establishing the equivalence of impression and conception, the response prepared in the mechanism. In experiments on stereoscopic vision published elsewhere [13] the great influence of a predetermined conception on the recognition of forms became very clear to me.

With other stimuli, too, such as electrical impulses on the skin, experiments may be made using the c-method, but here only in relation with a presented choice. Nor is one limited to the choice of a sound as a response. For on hearing all the sounds or only a predetermined vowel-sound one can make a movement with the hand, and the dif-

[13] Archief voor natuur- en geneeskunde. Dl. II, bl. 332 e.v.

ference again indicates the time required for a certain conception; but comparison with the differential response to each of the sounds is then excluded, and the experiments would not have served their purpose if I had not had the idea of recording the sound as a response.

All the above-mentioned results were obtained with an instrument described as a noematachograph.[14] The desire to analyse further the duration of mental processes led me to a method based on quite another principle than the one mentioned above, in which I used an instrument described as a noematachometer. I intend to report later on the results thereby obtained.

NOTES

Note I. In our determinations we used the graphical method. It is simple, safe, easy to employ and accurate enough for our purposes. If we were determining absolute, invariable values, we would have to aim at the greatest accuracy. Measuring the somewhat inconstant duration of mental processes, a determination in thousandths of seconds is sufficient, and simplicity and certainty must not be sacrificed to greater, but useless precision.

In our experiments three points are important:

(1) We need an accurately known chronoscopic unit. These are found in the recorded vibrations of a tuning-fork. The vibration speed of the tuning-forks used was determined directly by simultaneous recording of their vibrations and of the seconds of a clock. The slight influence of the temperature was found from the change in the number of interference waves with another tuning-fork held at a constant temperature.

(2) The moment of the stimulus must be recorded with precision under or on the tuning-fork line.

For various experiments this is done in different ways.

When an induction spark jumps from the writing metal spring of the tuning-fork through the paper on to the cylinder, then the moment is sharply marked on the chronoscopic line. An arrangement is easily made for the spark to be either only seen, or only heard, or (when leading a small part of the discharge through the body) only felt. In that way the physiological time may be determined *alternately* with response to each of the three senses. To obtain only one spark when breaking the constant current, which is effected during and by the turning of the cylinder, a spark-micrometer has to be inserted in the induction circuit, with poles which are nearly the maximum length of the spark apart. Such a discharge-spark may also serve to decide in the choice between colours (whether or not seen through coloured glass) and the discrimination between letter-symbols (illuminated by the spark). Whenever the discharge occurs between

[14] Archief voor natuur- en geneeskunde. Dl. III, bl. 105. (A translation of this article is inserted in this volume on p. 432—435 (translator's note).)

the poles of the micrometer, a mark is recorded on the chronoscopic line. In our previous experiments the method was not yet so accurate.

A vowel-sound that strikes the ear may also bring into vibration the spring of the Scott-König phonautograph or of a more simple piece of apparatus constructed for me by König. If the distances are equal, this will happen at the same moment the ear drum is struck. Under the chronoscopic curve, the spring records a straight line until it is set into vibration by the vowel-sound. In such a way the moment of excitation is precisely recorded.

(3)The signal of the response must also be recorded with precision on or below the chronoscopic line. The usual electromagnets, whose delay varies with current intensity, were abandoned in our later experiments. The movement constituting the response is recorded without the intervention of any outside mechanism. A vertical light wooden rod, revolving without friction on its longitudinal axis, has on its top a horizontal spring, that records on the cylinder. Near to its bottom end it has a horizontal bar that, held between two fingers, may be knocked away, and which causes the recording spring to deviate at the same moment. With a view to a choice to be made the arrangement may be such that the piece is knocked away either to the left or to the right side.

The response to vowel-sounds is the repetition of the vowel-sound which is recorded by the phonautograph on the same line as that of the stimulus (compare fig. 1). For vowel-symbols the response is given also as a vowel-sound. When any stimulus is reponded to alternately with a movement of the hand and with a vowel-sound, the difference in time resulting from the difference in response may be found (compare note III).

The great advantage of the use of tuning-fork vibrations as chronoscopic units is that the cylinder may be rotated freely by hand. The duration of the process is found by counting the number of vibrations recorded between response and stimulus, irrespective of their length, and a constant speed of rotation is not highly important. The rotation occurs in about one second and with each rotation one trial is made, after which the handle again rests in the place from which one started. The cylinder I used had a diameter of 19 cm and a length of 25 cm. It could be used, at choice, with a spiral or a circular movement. On one roll 20 or more trials could be recorded one after the other. The trial was always made near the highest speed of rotation. and then, with 261 vibrations per second, fifths and even tenths of a vibration could be read quite well. At the end of the experiment the paper was cut along a line approximately corresponding to the start and the end of all rotations, so that on the oblong sheet each continuous tuning-fork curve corresponds to one trial. The trials are then numbered, and to each trial are provided any comments that are considered necessary before the lamp black is fixed with alcohol containing varnish.

At first we mostly used a tuning-fork producing 261 vibrations per second. Solidly fixed, it was brought into vibration some seconds before each trial by pulling out and upwards a wedge gently clamped between the arms of the tuning-fork. This action at once provided a warning signal that the stimulus would soon follow. Later on we had at our disposal the tuning-forks made by König that are kept in vibration by electromagnetism after the principle used by Helmholtz for the synthesis of vowel-sounds.

Note II. For the duration of the mental processes I attached particular value to the minima found.

The differences we find certainly depend for the greater part on real differences in the duration of mental processes. The moment of the stimulus is precisely recorded, as well as that of the response. The probable error in the different non-mental terms cannot be measured, but it certainly will not be significant. We simply have to accept the values as they are found and be content with the knowledge of maxima, minima and averages. We do not wish to study here any further the basis of the differences. Only this may be noted, that the degree of tension and of dismissal of all other thoughts has a considerable influence. Distraction during the appearance of the stimulus is always punished with prolongation of the process. In this connection, however, it is clear that the minima found are the purest values. they represent the smoothest and most undisturbed course of the process. By subtracting the minimum values from all the times found, one obtains from the differences a clear insight into the deviations from ideal regularity, and from this something may no doubt be deduced about the cause of the deviations. The significance we attached to the minima compelled us to determine the difference between the minima – in addition to the difference between the averages – of two series of experiments differing by the interposition of a mental stage. It would certainly be dangerous to rely only on the minima. There is a possibility that if the subjects cannot control his tension well, he responds before the stimulus has affected him. Then too small a minimum is found; with inefficient subjects the response sometimes came even before the stimulus. If this difficulty is met by repeating the experiments with relatively long intervals, for instance of one minute, without a warning that the stimulus is forthcoming, the stimulus often finds us less alert and the ultimate minimum limit is reached with difficulty. Even with a warning shortly before each experiment, so that the tension need not last long, a great number of experiments is required to approach the attainable minimum. For that reason the two series whose minima are to supply us with the difference we are looking for, have to be large or at least equal. We have never neglected to determine also the difference in the averages. They protect us from the profound error to which a thoughtless use of the minima could lead. And apparently their difference in two series represents also the duration of the mental process interposed in one of the series. It was, however, to be foreseen that for the same term the differences between the averages would be somewhat larger than those of the minima, for of course the disturbance that causes the longer duration of the process is reflected the more in the mental stages the more complex the latter are, and as a consequence is most significant in that series in which a new mental stage is interposed. So for that term we have to expect a minimum only in the minima of the whole process. In general, the result came up to our expectations.

Note III. It may be not unimportant to investigate how much more time one response takes than another with the same stimulus. For different sounds and for movement of the hand I can supply a contribution based on 91 precise observations, all made on myself on various days. The response was made partly to light and partly to sound impressions. The sounds were vowels, either preceded by the plosives *p*, *t* or *k* or not. Those compared were, for instance, *pi*, *ti*, *ki* and *i*-or, in fact, not *i*, but *i* preceded by the *hamza* of the Arabs, which is started with closed glottis: the hamza (almost a gentle cough) is in fact a plosive, but in most languages it is neglected. It is not neglected by good singing-masters who often have to fight against the impact of the hamza at the loud beginning of vowels. Without a sufficient degree of absence of the *hamza*, the sound is not generated suddenly enough to record the start precisely. The movement of the hand consisted in pulling out by its handle a wedge gently clamped between the arms of the tuning-fork. Its vibrations as well as those of the vowel-sound were immediately recorded by the phonautograph. We restrict ourselves to communicating the final results in vibrations of 261 cycles per second:

	Response							
Stimulus	Vowel		Vowel with *p*		Vowel with *t*		Vowel with *k*	
	average	min.	average	min.	average	min.	average	min.
Light	43.3	41.5	45.1	40.5	53	48	49.3	46.5
Ditto	50.8	48	52	52	58.7	56	50.8	47.5
Sound	50	43.6	58,3	53	53.2	48	61.3	60.7
Light	56.8	53.2	56.5	54.5	59.3	53.7	61.2	58.9
Average of all series	50.22	46.57	52.97	50	56.05	51.42	55.65	53.27

It appears that without exception the vowel preceded by the plosive takes more time than the simple vowel with hamza and that *p* gives less delay than *t* and *k*, as the mechanism would already suggest. The delay as calculated from the averages and from the minima, in brackets, is:

for *p*	for *t*	for *k*
2.75 – (3.43)	5.83 – (4.85)	5.43 – (6.7)

In three series of observations we were able to compare the response with sounds and those with the hand movement mentioned above, and in each case we found that more time was required for the hand movement: in fact, in the first series of the table the average was 52.7 and the minimum 51, that is 9.4 and (9.5) vibrations more than for the vowel; in two other series in which the vowel was not known beforehand the values were 3.95 and (6.63) and 4.85 and (6.93) more than for pi.

Note IV. From the 51 series of experiments performed, counted and calculated, one will be reported here in full. Of two other series the results will be given. In these series the same roll contains alternate experiments made with the a-, b- and c-methods, i.e. with response to known, to unknown and to one out of a set of unknown sounds. In method c always some of the sounds are not responded to: on roll XVI B, of which we communicate here the results in full, there are only 15 determinations on 22 curves, because with the a-method the response once failed to be made owing to distraction, and with the c-method, six times the response was not made, as it should have beeen.

August 21, 7 P.M.; Messrs Hamer and Donders in front of the phonautograph. H. calls; D. responds. Tuning-fork = 261 vibrations.

Method a. *Ki* to be responded to with *ki*.

Serial number	Stimulus	Response	Number of vibrations	
1	*ki*	*ki*	45	average = 51.5
2	*ki*	not forthcoming		minimum = 45.
3	*ki*	*ki*	54	
20	*ki*	*ki*	53	
21	*ki*	*ki*	60	
22	*ki*	*ki*	45.5	

Method b. Unknown sound, to be responded to with the same sound.

Serial number	Stimulus	Response	Number of vibrations	
4	*ko*	*ko*	77.5	average = 74.33
5	*ke*	*ke*	72	minimum = 72
6	*ki*	*ki*	72	
17	*ki*	*ki*	76	
18	*ku*	*ku*	74.5	
19	*ke*	*ke*	74	

Method c. Of the sounds only *ki* has to be responded to.

Serial number	Stimulus	Response	Number of variations	
7	*ku*	—		average = 63.37
8	*ki*	*ki*	71.5	minimum = 59
9	*ki*	*ki*	61	
10	*ka*	—		
11	*ku*	—		
12	*ki*	*ki*	62	
13	*ke*	—		
14	*ki*	*ki*	59	

In the next table the results of the separate trials on this roll are combined with those on two similar rolls, 38 determinations in all being obtained on the same evening.

	Number of vibrations				
Methods	XV	XVI A.	XVI B.	average of the observations	minimum of 38 determinations
a.	56.66	49.66	51.5	52.41	44.5
b.	74.83	73.08	74.33	74.08	62
c.	60.83	60.5	63.37	61.89	55.5

Now one finds:

	from the average			from the averages of all observations	from the minima
	XV	XVI A.	XVI B.		
b-a	18.17	23.42	22.83	21.67	17.5
c-a	4.17	10.84	11.87	9.48	11

And for the three rolls in combination:

	from the averages		from the minima		averaged	
	vibrations	thousandths of a second	vibrations	thousandths of a second	vibrations	thousandths of a second
b-a	21.67	83	17.5	67.05	19.585	75.03
c-a	9.48	36.32	11	42.15	10.24	39.24