PITCH DISCRIMINATION IN THE DOG.

By G. V. ANREP.

(*Institute of Experimental Medicine, Petrograd.*)

WITHIN the last ten years considerable attention has been given to the investigation of the auditory capacity of dogs. The interest of the subject was not only aroused by a desire to prove conclusively that dogs are able to differentiate all the various properties of sound, but also in order to investigate the more intimate work of the brain by using the differentiative auditory capacities. The majority of treatises on the subject have been written by Russian authors under the guidance of Professor Pavlov. Selionyi(1) was the first to investigate the auditory capacities of the dog. He proved that dogs possessed the faculty of discriminating absolute pitch. His dogs were not only able to differentiate between quarter-tones, but could also distinguish combinations of sounds, such as chords, and recognise the most subtle distinctions in timbre. Repeated investigations on this subject, which had been originally attempted by Selionyi, led to the recognition of the marvellous perfection of the dog's auditory system. Makovsky(2) proved that they also discriminate the value of pauses between them. Boormakin(3) observed the striking fact that their auditory power is sufficiently developed to enable them to distinguish sounds of 80,000 and 100,000 vibrations per second, thus attaining a far higher limit than the human auditory system. Babkin(4) showed that they appreciate the value of the musical pauses occurring in four consecutive notes and can discriminate the order in which the notes followed each other. Tihomirov(5) gave instances which prove their wonderful capacity for differentiating between the force of sounds. Concerning pitch discrimination, Eliasson(6) obtained a discrimination of half-tones; Nicholaiev(7) between sounds of 30,000 and 27,750 vibrations, that is less than half a tone's difference; Nikiforovsky(8) got a differentiation between sounds of 461 and 435 vibrations per second. Finally Beliakov(9) obtained the highest point of pitch discrimination—that of $\frac{1}{4}$ and $\frac{1}{8}$ tone interval. His dogs were actually able to recognise the difference between

sounds of 800, 812 and 825 vibrations per second respectively. I have merely mentioned those authors who gave ground for asserting that the discrimination of sound is highly developed in dogs. I will not enumerate the various works which are inspired by this argument, and which concern the intimate activities of the brain.

The results of all the experiments carried out on the dog's auditory capacities show that they are quite capable of differentiating all the qualities of sounds: their pitch, timbre, force, source of origin and the order in which the sounds follow.

Of foreign authors there are the older works of Kalischer(10, 11) and Rothmann(12) who studied the same question, for the imperfect experiments of Goltz and Munk(13) can scarcely be taken into consideration. Whoever has studied the susceptibilities of animals to the effects of light, sound or other external influences, will realise how difficult it is to devise a really flawless method for the furtherance of this work. A closer knowledge of Kalischer's writings shows the imperfection of his methods. Although his results correspond with the data of the Russian authors, his method gives rise to too many fallacies to justify a blind faith in his conclusions. Rothmann's experiments were carried out by the same method as Kalischer's and the slight alterations in the method he made did not greatly improve it. Swift(14) was another follower of Kalischer who succeeded in demonstrating a rough differentiation between c' and e''.

In 1913 an article by H. M. Johnson(15) ("Audition and Habit Formation in the Dog") appeared in the *Behavior Monographs*. His results, however, radically denied one of the fundamental theories of our school. He states: "The experiments have failed in showing to what extent the dog is sensitive to difference of pitch. They have not established that he is sensitive to pitch difference at all." Professor Pavlov in consequence requested me to repeat the experiments on the dog's auditory system with due regard for the data presented by Johnson.

General observations on the methods of obtaining conditioned reflexes.

Modern English literature is but little acquainted with the views of the Russian school upon the reflex activity of the central nervous system. (See Bayliss' *General Physiology*.)

The science of reflex activity, that is to say of the reactions of the animal's organism to stimulations of various receptors, brought about through the central nervous system, is studied by the Russian school under two main headings.

Under the first heading we have all reflexes of a general nature, which form the inevitable characteristic of the whole class of animals under consideration; they are the immutable outward signs of the central nervous system, inherited and existent in each animal from birth to death.

The second group includes the reflexes peculiar to individual animals and acquired by them in the course of their existence. The organism at birth presents merely greater or lesser possibilities for the formation of reflexes of the second group and their formation depends only on the conditions under which the animal lives. The second division is therefore referred to as the group of "conditioned reflexes."

The method of conditioned reflexes is practically a method of experimental formation of new reflexes. In this method the central nervous system is subjected to conditions under which invariably new reflexes, *i.e.* new connecting links between any receptor organ and any effector organ, are formed. For the formation of a similar new connection any receptor organ can be utilised. Concerning the effector part of the reflex, there are three systems in the organism which all can be used—the so-called voluntary muscular system, the unstriated involuntary muscles and the glandular tissue. In conditioned reflexes we almost entirely use as effectors the salivary glands and for the following reasons: the salivary gland is a simple organ not a composite one, it is not like an extremity, which consists of several muscles; there are no tonic reflexes in the salivary gland which might interfere with our investigations of the newly formed reflexes; the salivary gland is far more independent of other parts of the body than the muscular system where the state of every muscle is influenced by all, or practically all, the remaining muscles of the body; the activity of the salivary gland can be graduated with far greater facility than that of the muscles.

The principal bases, the unalterable law for the formation of temporary or conditioned reflexes demands that the action of the effector organ should coincide with a stimulation or with the traces left by a stimulation of any receptor organ. It is quite easy to awake the action of the salivary glands. Any inborn "unconditioned" reflex, producing a flow of saliva, can serve as the basis for the formation of a conditioned reflex. The introduction of food or of an emetic stimulus will throw into action our effector organ, viz. the salivary gland. If this action is provoked several times during a stimulation of any receptor organ, a link will be formed between the stimulated receptor and the action of this salivary gland and the mere stimulation of the given receptor organ, even unaccom-

panied by an unconditioned stimulus will irrevocably throw the effector into action—thus a conditioned reflex is formed. Such a conditioned reflex, as I have stated, can be formed by any stimulus, visual, olfactory, auditory or cutaneous.

The experiments on pitch discrimination were carried out by the Russian school with salivary conditioned reflexes—those of the other authors, including Johnson, with motor reflexes.

As an argument against the salivary conditioned reflexes Yerkes(16) presents a whole series of objections, corroborated by Johnson: the latter writes: "Yerkes has remarked that there are several obvious disadvantages to this method: conditioned reflexes die out with repeated stimulation; the quality of food given conditions a remarkable variation in the flow, and the rate of secretion is also conditioned by the time interval between stimuli. To this I would add the suggestion that there may be also a tendency to rhythmical change in the rate and quantity of secretion. Moreover, Selionyi tells us that the animal was affected by the 'kind of movements' made by the experimenter who had to be careful neither to move too quickly, nor to hold himself too rigidly quiet."

None of these objections are based on serious grounds. Conditioned salivary reflexes never die out, if sustained by unconditioned reflexes. Only if the conditioned stimulus is applied for a long time (several days) without the unconditioned one, does the conditioned reflex gradually die out. It would be quite easy to observe any irregularities in the degree of secretion, seeing that salivary reflexes are very easy to measure. The force of the reflex is immutable and, where identical conditions are maintained, it offers practically no variations at all. Quality of food, as stated by Johnson, can undoubtedly exercise a certain influence, seeing that the conditioned reflex is practically a diminutive copy of the unconditioned one, on which it is based. A change in the unconditioned reflex does effect a similar change in the conditioned one. But an unvarying, unconditioned reflex will produce a constant conditioned reflex. It is, of course, unthinkable, that a certain conditioned reflex should be sustained by alternate unconditioned stimuli—meat, biscuits, acids or others, as each of these agents will create its own particular conditioned reflex. According to this, conditioned reflexes should be made only by the same unconditioned stimuli throughout, as a change in the last would give absolutely different conditions and form different and new conditioned reflexes. By maintaining these conditions the same throughout, the objections of Johnson are confuted. It should

be remembered that the force of the unconditioned stimulus affects the strength of the conditioned reflex and that in consequence, an increase or decrease in the quantity of food stimuli would produce a similar change in the strength of the conditioned reflex saliva flow. Therefore one must use not only the same food as unconditioned stimulus, but also the same quantity of it in order not to alter the stimulus in strength. The time intervals between stimuli, as Johnson remarks, really do influence the strength of the reflex to a certain extent. But, first of all this influence is so small that it can not alter the results and secondly, this influence exerts quite definite and well-known alterations in the excitability of the central nervous system. Each stimulation is followed by a short increase and then a short decrease of the excitability, after which the reflex soon comes to its normal strength. These alterations are very small, do not usually exceed 3–6 p.c. of the reflex value and cannot interfere with the results of pitch discrimination experiments. In regard to Johnson's statement that there may be also a tendency to rhythmical change in the rate and quantity of secretion, I may say that in the course of more than 20 years' studies on conditioned reflexes, no one in Pavlov's laboratory has ever met with it. The behaviour of the experimenter himself does strongly affect the conditioned reflexes, as every change in it brings new conditions into play and interferes with the previous complex of conditions under which the given conditioned reflex was obtained. At the present time the influence of the experimenter as a source of interfering stimuli is excluded by absolute separation of the experimenter from the animal.

It would be ideal if the animal could be subjected exclusively to the stimulus under investigation, but this ideal is impossible of achievement, as the animal itself is a constant living source of innumerable stimuli. These stimuli often affect the muscular system very powerfully, although the salivary glands are not, strictly speaking, so dependent on them. It may be impossible to attain the ideal, but we can at least approach it. In order to do so the animal should be protected from all extraneous stimuli and this has been successfully accomplished in the new laboratory of the Institute. The thick external walls with double windows hardly admit a sound or a vibration from without into the experimenting room. The room itself is divided into an inner and outer part. The investigator works in the outer part of the room where a whole battery of rubber balls connects him through numerous mercurial electrical contacts with the inner part of the room. Compression of one or another of the balls causes a stimulus in the inner part of the room where the animal is

placed. The inner part of the room is separated from the outer one by a ceiling and by walls over two feet thick. The walls and the ceiling are made of compressed turf and a double massive door closes on pressure. Repeated experiments proved that the very loudest conversation alone penetrated from the outer to the inner room and then only as a subdued murmur. Street noises and vibrations were absolutely inaudible, likewise all noises associated with the work in the outer part of the experimenting room. The inner part, intended for the animal, is windowless and illuminated by electric light. A periscope is inlaid in the wall in order to see the dog and the inner part of the room during experiments. In the middle of the inner room is a table on which the dog is placed. The animal is secured by its collar to a transom beam above the table and its paws fixed in string loops. (As a rule the dogs become so accustomed to this procedure, that they run as quickly as possible, and of their own accord, into the inner room, jump unaided on to the table and lift each paw in turn to be placed in the string loop.)

Seeing that we are dealing with the formation of salivary reflexes, all the dogs had a permanent fistula of the parotid duct. The record of the conditioned salivary flow was carried out in the following way: a small celluloid ball, provided with an upper and a lower tube, is attached to the parotid fistula by means of a so-called Mendelejev mask. The upper tube leads to the outer room entering a cistern with electrolytic fluid (solution Na_2SO_4).

As soon as the parotid gland commences to secrete saliva into the celluloid ball, the electrolytic fluid by means of air transmission moves into a horizontal tube connected with the cistern and ending in a hollow platinum electrode. This electrode is placed above a similar hollow platinum electrode and a current of air passes continually through the lower electrode; the suction exerted is regulated and unvarying. So soon as a drop of liquid appears on the upper electrode, it is sucked away by the air current and makes a contact at the same moment. In this way each drop can be recorded. Owing to the current of air, the drops are excessively small. By bringing the electrodes closer together the size of the drops can be made of 0·01 c.c. and even of 0·005 c.c. each. All the following experiments were made with 0·01 c.c. drops.

Out through the lower tube, leading from the celluloid ball, the accumulated saliva is collected into a jar hanging underneath the dog; this is produced by diminishing the pressure in the jar. (All this, naturally, is done from the outer room.) During the experiment the lower tube closes automatically, so that ball and jar are disconnected. All connec-

tion between investigator and dog are therefore of a purely mechanical nature.

The conditioned reflex is obtained in the following manner. The dog stands quietly on the table, the experimenter presses one of the small rubber balls, bringing into action the stimulus desired—in my case an auditory stimulus. After a few seconds (three to five seconds) the conditioned stimulus is stopped and an unconditioned stimulus is introduced. In my experiments a plate with a measured dose of biscuit powder was moved forward automatically. When the dog has finished eating, the plate is withdrawn, hidden behind a screen and automatically refilled with a fresh portion of powder.

In order to test the pitch discrimination of the dog a sound of a particular pitch was accompanied by the unconditioned stimulus, and the unconditioned stimulus was omitted with all sounds of any other pitch. The first note had best be referred to as an "active" one and the others as "inactive" sounds.

Each dog puts in from about one to two hours' work per diem. Intervals between stimuli vary from five to thirty-five minutes.

After several combinations of the active tone with the unconditioned stimulus, a test of the reflexes is made. In other words the auditory stimulus, instead of lasting five seconds only, is prolonged for any time desired—30 seconds and even over a whole minute. I, personally, have accepted 30 seconds as my time unit in testing the force of the reflex. The active note is of course strengthened by food after the test time is over, while the inactive ones are not. Each dog is subjected to 8–10 stimuli per diem. The inactive notes were sounded in different order, sometimes in the beginning, sometimes in the middle and occasionally at the end of the experiment, sometimes two or three times in the day, sometimes only once. My dogs had a whole series of other not auditory conditioned reflexes and I usually mixed them during the experiment inducing now and then a cutaneous or visual conditioned stimulus.

Criticism of Johnson's experiments.

Johnson formed motor conditioned reflexes. The response in his case was not due to the work of an individual group of muscles, but was based on the joint activity of practically the whole body. I have already dwelt on the inconveniences of studying conditioned reflexes on the muscular system. Johnson himself has drawn attention to the essentially negative side of the motor reflexes investigated on the free animal, that is the dependence of reflexes on the position of the dog during the

stimulus. In my experiments there was no danger of such a difficulty as the dog was kept in the same position all through the experiment.

The essential argument against Johnson's methods is that in his investigations both the notes he wanted to make discriminate were accompanied by a food stimulus, although in each case the food was administered in a different place. This is not permissible from the standpoint of conditioned reflexes; it is imperative that the active notes should acquire absolutely opposite qualities from the inactive ones. In any case food is far too powerful a stimulus to be counter-balanced by any change in the place where the food was administered.

Double feeding was still more complicated by Johnson as if the dog failed to find the food and looked for it in the wrong place it was not fed at all and this for both sounds. The main objection to double feeding is what Johnson calls the "position habit," which he describes in the following words: "...every animal which I have used in discriminating tests...quickly forms the habit of reacting in a certain rhythm, or in a regular order as by taking a certain position, regardless of the stimulus presented." In all probability in Johnson's experiments the auditory stimulus merely represents one of a series of stimuli brought to bear on the animal's brain, instead of being the only one. The dog did not make a conditioned reflex to the sound, but to some other stimulus or even it adopted the way of rhythmic choice. In his preliminary tests, Johnson got his dogs to differentiate between sounds of 256 and 389 vibrations per second respectively, but as soon as he repeated the experiments with a separation of the dog from the experimenter by a wall, he was absolutely incapable of discerning any traces of pitch discrimination in his dogs at all. Despite 505 tests the dog showed no sign of pitch discrimination, a conclusive proof that the reflexes which appeared in the first instance were not due to any sound effect, but merely to some other stimulus arising from the experimenter himself. Johnson tried to avoid the negative side of his preliminary experiments by employing what he calls a "stimulus cage." The animal was enclosed in a "stimulus cage"; when a certain note was sounded it was released and allowed to choose the direction down a series of corridors leading right and left. At a given sound the animal gained access to the right-hand corridor, whilst the corridor to the left was blocked—and *vice versa* at another sound. The blocking of the alternate passage was effected by means of a metal grating let into the floor through which an electric current was passed, so that the dog received a shock if it took the wrong turning.

From the standpoint of the Pavlov school everything in this method seems to have been done to hinder the achievement of a successful discrimination test. Double feeding was not abolished, the sounding of given notes was accompanied by a far stronger stimulus, viz. that of releasing the animal from the cage, and strong extra stimuli were present. As a matter of fact Johnson's experiments gave negative results; as soon as the dog was released, it made for the right or for the left independently of the sounds. And finally when the dog was released without the accompaniment of any sound at all, it still ran down the corridors in quest of food. It is perfectly clear that in all these tests the reflexes were not conditioned by sound, but by the far greater stimulus produced by the animal's release from the cage. At the same time the dog was given unlimited scope for the development of rhythmic and other reflexes, and the motor reflexes Johnson investigated must be practically called by an unscientific word—"voluntary."

Being unsuccessful in obtaining evidence of a pitch discrimination, Johnson resorted to further auditory stimuli. In all his previous experiments he had made use of resonant tuning forks, but he now replaced them with a far more powerful stimulus in the shape of ordinary electric buzzers of different pitch and timbre. In the previous experiments the tuning forks were placed next to each other and at some considerable distance from the stimulus cage. Now each buzzer was hung over its own particular food receptacle. When the right-hand buzzer sounded the dog was supposed to run to the right and *vice versa*. The terrifying electric grating was removed. The dogs soon differentiated the origin of the sounds and invariably ran in the correct direction for their food, guided by the sound of the buzzers. On deciding after this whether the reflex was conditioned by the difference in the pitch of the sound or by the place it originated from, it appeared to be the latter, as by changing the position of the buzzers, the dogs ran straight for the new place of origin of the sound and not in the previous direction where the given buzzer originally was.

From his experiments Johnson draws the conclusion that dogs are unable to discriminate pure sounds by their pitch and can only recognise noises and blurred sounds and these mainly by the place whence they originated.

The production of pure sounds.

In criticising the usual methods of sound production, Johnson proved that they did not give an even pitch or a clear tone, but that the sounds produced vary in force and composition owing to the large

number of extra tones, and in studying the pitch discrimination he had to exclude all other qualities in the sounds he used. In other words he had to equalize both sounds in regard to force, timbre and source of origin, leaving only the difference in the number of vibrations produced by each. Having dismissed as impractical for the purpose all ordinary wind instruments, bottle whistles, organ pipes and mouth organs, Stern variators, etc., he decided on a system of tandem driven tuning-forks, primarily adopted by Helmholtz for pure sound production. The first tuning-fork, placed 100 feet from the experimenting room, was set in motion by an electro-magnet and the ensuing vibrations were transmitted through another electro-magnet to a second tuning-fork. A condenser was situated in the circuit. The second tuning-fork was placed in the experimenting room and was provided with a resonator. The condenser in the circuit was supposed to eliminate the noise of sparking.

We have long sought a method for obtaining pure sounds in our laboratories and have made use of an apparatus similar to the one employed by Johnson. I must, however, own that all the instruments, the tandem driven tuning-forks inclusive, failed to achieve the desired results. To begin with, it is quite impossible to eliminate the traces of the noise of sparking by means of a condenser placed in the circuit. Distance and the resonator added to the second fork could not help it. None of Johnson's sounds were therefore pure.

To obtain really pure sounds I had to resort to a rather difficult system, the idea of which based on sinusoidal current was given by Thaddeus Cahill(17). The sinusoidal current was obtained by means of electric resonances.

The apparatus constructed for my investigations consists of a synchronous motor, driven by an alternating current from the Central Power Station at Petrograd. The shaft of the synchronous motor is provided with several pinions each with a different number of teeth. Every pinion has its own electro-magnet. The secondary circuit of the electro-magnet in each pinion is connected with an induction coil in the experimenting room. A condenser is placed in the secondary circuit and both condenser and coil (in the experimenting room) are carefully adjusted with a view to obtaining the maximum effect of electric resonance. Consequently, in the apparatus described, the condenser introduced a certain capacity which at the given periodicity of the current compensated the self induction. This is the only method by which a true sinusoidal can be obtained. If an electro-motive force is

applied to a system in which self-induction is not compensated, the current will fail to react immediately to its full limit. One result would be that the sinusoidal curve of the current would either advance or recede from the sinusoidal curve of the electromotive force. In this case the maximum of the current would not coincide with the maximum of the electromotive force. In the case when the capacity of the circuit at a given periodicity compensates the self-induction the difference between the phases of the current and of the electromotive force will be equal to zero. Thus is the resonance obtained. The secondary induction coil in the experimenting room is adjustable, so that the induced current can be subjected to desired variations in force. The secondary coil is connected with the sound generator in the room where the dog is placed.

A telephone is used as sound generator, it was built in the guise of a resonator and tuned to the requisite pitch; the resonating diaphragm formed the bottom of the resonator. An undesirable feature of this apparatus was a certain dependence on the irregular working of the Central Power Station. The degree of this irregularity was ascertained by means of sound interference with a corresponding tuning-fork. The aberration of sound seldom rose to 1 p.c. and the correction of this insignificant value was made by automatic rotation of the stator of the synchronous motor.

The pinion-bearing shaft is provided with a fly-wheel filled with mercury; this fly-wheel completely absorbs the pendulous vibrations of the speed of the motor. All the switches for the primary current from the accumulators and for the intermittent currents supplying the telephones were mercurial.

A maximum purity of tone is thus guaranteed. According to desire the force of the sounds could be varied over broad limits. The direction of the sounds could be made to coincide by altering the position of the telephone. The timbre was absent as in all pure sounds. The absolute height of the sound could be gauged by a simple calculation; the synchronous motor gives 21·25 revolutions per second, the number of teeth on the cog wheels is equal to 30, 32, 34, 40 and 50 respectively. By multiplying the corresponding number of teeth by 21·25 we obtain the number of fluctuations of the sinusoidal wave and of the periodicity of the sound waves. The absolute height of the sounds was equal to 637·5; 680; 722·5; 850 and 1062·5 vibrations per second respectively.

The investigations concerning the purity of the sounds are based on the so-called "sound shadow" phenomenon, when a reflected sound wave on meeting the direct wave forms nodes of interference. If in

these nodes some instrument for sound reception is placed a so-called "sound shadow" (number of sound vibrations—zero) is proved in the case of all pure sounds. If the sound is not quite pure all the extra tones which have crept into the fundamental wave will now assert themselves. When estimating the purity of sounds produced by Stern variators (set in motion by a flow of air under constant pressure), by organ pipes and by the tandem driven tuning-forks, all noises and overtones asserted themselves in the sound shadows. In Johnson's apparatus in the spot where the fundamental sound disappeared, the noise and the tone of the first tuning-fork were distinctly audible. The sounds used in my experiments proved a perfect sound shadow: the human ear could no longer distinguish a single sound. The tests were repeated a great number of times and by many people. There was also made a test with a quite objective method by means of a soap film resonator and with the same positive result.

Experimental results.

Four dogs were used for the experiments; all of them had already served in the laboratory for a long time and displayed much zeal in their work. They ran into the experimenting room and jumped unaided on to the table, behaved with great calm during the experiments and secreted no saliva at all in the intervals between stimuli.

The first series of experiments on all the four dogs was to make a simple conditioned reflex to a pure sound of 637·5 vibrations per second. Two of my dogs had already this reflex previously to my experiments so that I had merely to restore it; the other two were unused to sound reflexes and I had to establish their reflexes from the very beginning.

I have previously explained how the conditioned reflexes are made: a note is sounded for 5 seconds and then after a lapse of 2–3 seconds the unconditioned stimulus is induced. To test the strength of the new reflex the sound is prolonged for 30 seconds and the quantity of saliva secreted during this pause serves to determine the strength of the reflex.

The process of re-establishing the conditioned reflexes in the first two dogs is shown in Tables I and II, where all the data referring to the 30 seconds tests will be found. My predecessor who had worked with Dogs 1 and 2 had established conditioned reflexes on sounds of 637·5 and 1062·5 vibrations per second. To begin with I strengthened both of these reflexes. As both animals had been resting for over four months their reflexes had greatly diminished.

As the dogs had a whole series of conditioned reflexes from other

RE-ESTABLISHING OF THE CONDITIONED REFLEXES TO SOUNDS OF 637·5 AND 1062·5 VIBRATIONS.

TABLE I. Dog 1.

	637·5 vibrations			1062·5 vibrations		
Working days	No. of combinations with the unconditioned stimulus	Strength of reflex in 30 seconds in drops	Latent period	No. of combinations with unconditioned stimulus	Strength of reflex in 30 seconds in drops	Latent period
1	1	17	15 sec.	1	20	13 sec.
4	8	30	10 „	7	25	9 „
5	12	40	5 „	11	45	4 „
7	16	62	2 „	17	65	2 „
8	20	60	3 „	22	63	2 „
10	25	66	1 „	27	61	2 „
15	37	60	2 „	35	59	3 „

TABLE II. Dog 2.

1	1	20	11 sec.	1	35	8 sec.
2	5	40	4 „	6	45	3 „
5	11	85	2 „	14	90	1 „
25	84	85	2 „	80	87	2 „
27	90	84	2 „	86	85	2 „

receptors which were also tested each working day only 3 to 5 combinations were reserved for auditory stimuli.

It is obvious from these two tables that the reflexes were re-established rapidly. The strength of the reflex in Dog 1 varied from 60 to 70 drops of saliva in 30 seconds and from 80 to 90 drops in Dog 2. Tables III and IV show the calculations of the reflexes formed on a pure note of 637·5 vibrations per second in Dogs 3 and 4, both of which had no sound reflexes before.

ESTABLISHING OF CONDITIONED REFLEXES TO A SOUND OF 637·5 VIBRATIONS PER SECOND.

TABLE III. Dog. 3.

Working days	Number of combinations with the unconditioned stimulus	Strength of reflex in 30 seconds in drops	Latent period
1	1	0	—
3	9	18	15
5	15	30	4
11	31	65	2
14	41	64	3
17	51	69	2

Tables III and IV prove that the reflex reached its maximum strength at about the 30th combination, *i.e.* not so rapidly as in the tests with Dogs 1 and 2 which had merely to re-establish their reflex.

TABLE IV. Dog 4.

Working days	Number of combinations	Strength of reflex in 30 seconds in drops	Latent period
1	1	0	—
3	10	6	18
7	20	20	8
10	30	60	2
13	40	62	1
16	50	59	2

When the reflex could be considered to have reached a determined and unvarying strength, I undertook to work out the differentiation between the sound of 637·5 and the sound of 1062·5 vibrations per second. As previously explained, the differentiation was worked out in the following manner: whilst the active sound of 637·5 vibrations per second had been constantly strengthened by unconditioned stimuli, the sound which had to be made inactive, *i.e.* the one of 1062·5 vibrations received no strengthening at all. The process of establishing this rather rough differentiation is shown in Tables V and VI.

DIFFERENTIATION OF TONE OF 1062·5 VIBRATIONS.

TABLE V. Dog 1.

Working days	Number of tests	Strength of reflex in 30 seconds	After discharge in drops	After discharge in seconds	Latent period
1	1	67	154	203	1
4	8	26	66	115	2
5	9	22	50	90	3
8	13	11	33	50	2
9	15	8	25	45	2
12	19	5	14	20	15
13	20	0	18	20	35
15	23	0	0	0	—
16	24	0	0	0	—
18	25	0	0	0	—

TABLE VI. Dog 3.

1	1	51	163	115	3
3	3	30	95	110	6
5	5	18	53	60	7
6	6	0	25	45	10
7	8	3	28	40	10
11	13	0	2	30	38
12	14	0	0	0	—
20	25	0	0	0	—

Table V shows the process by which the differentiation was obtained. No saliva was ever secreted during subsequent experiments with the 1062·5 vibration sound.

Table VI shows that Dog 3 developed the same differentiation far

more rapidly than Dog 1. The same was observed in Dog 4 when compared with Dog 2. The second animal obtained zero at the 28th attempt, Dog 4 at the 9th. I am not giving a corresponding table for Dogs 2 and 4 as it would be merely repetition.

If Tables V and VI are compared we see that there is no great difference between both animals in respect to their first reaction to the sound that was to be made inactive, which is of an important significance. Dog 1 had had previously a positive conditioned reflex to this sound and had now to convert it to a negative one. Dog 3 had simply to establish a negative reaction. No doubt the strong reflex to the sound in Dog 1 is to be explained by the previously established conditioned reflex. This explanation cannot be put forward for the same reaction in Dog 3 to a sound which was never used. The explanation is to be sought in the phenomenon of irradiation. Irradiation of excitation and inhibition is very common in conditioned reflex and was studied in many series of experiments and in respect to all the analysers of the body. (In conditioned reflex terminology analyser is called the effector with the part of the cortex it leads to.) The irradiation of the inhibitory process could be seen in my experiments, for the initial positive reflex considerably diminished the whole time a new sound discrimination was developing (sometimes over 25–30 p.c.). But as soon as the new discrimination is established, the initial reflex soon attains its normal value. The wave of irradiation can now be studied in its time relation and the laws of the cortical irradiation-process of conditioned reflexes seem to be firmly established.

The rough differentiation between 637·5 and 1062·5 sound variations obtained, I undertook to work out a more subtle one, using the sound of 850 vibrations per second as inactive. I shall merely quote the figures referring to Dog 1 and indicate the deviations from these figures as observed in the other dogs.

DIFFERENTIATION OF TONE OF 850 VIBRATIONS.

TABLE VII. Dog 1.

Working days	Number of tests	Strength of reflex in 30 seconds	After discharge in drops	After discharge in seconds	Latent period
1	1	12	25	40	7
2	3	4	11	20	15
3	4	0	8	20	37
4	6	0	0	0	—
5	7	0	0	0	—
15	20	0	0	0	—

The last differentiation was obtained far more rapidly than in the first one, for zero was obtained at the 6th sounding of the inactive note. Dog 2 worked out the differentiation at the 12th test, Dog 3 at the 9th, and Dog 4 at the 4th attempt.

Once having established a differentiation of a sound of 850 vibrations per second, I proceeded to a differentiation of a sound of 722·5 vibrations, *i.e.* of less than a full tone's difference from the active sound. I shall once again confine myself exclusively to data in connection with Dog 1 which are given in Table VIII.

DIFFERENTIATION OF TONE OF 722·5 VIBRATIONS.

TABLE VIII. Dog 1.

Working days	Number of tests	Strength of reflex in 30 seconds	After discharge in drops	After discharge in seconds	Latent period
1	1	22	81	95	20
2	3	20	48	75	18
3	5	8	24	60	23
4	6	0	20	55	45
5	7	0	5	40	50
6	8	0	0	0	—
10	16	0	0	0	—

Dog 2 attained full differentiation at the 17th test, Dog 3 at the 6th, and Dog 4 at the 5th. These tests alone are sufficient to prove that dogs are able to discriminate pitch.

All my subsequent experiments were carried out with a view to proving that the experiments are really based on pitch discrimination and not on differences in other properties of the sounds used, which could not be fully eliminated. For this I altered the strength of the reflex in various ways, alternatively weakening and strengthening the active and inactive sounds. In varying the strength of the active sound a corresponding change was observed in the strength of the reflex. The difference in the reflex by dealing with an exceptionally strong and very weak sound rarely exceeded 25 p.c. The latent period was slightly longer in the weak sounds.

The strengthening of the inactive sounds resulted in traces of a very slight secretion (3–7 drops) but only in the first test, whilst subsequent tests registered zero. The weakening of the inactive sounds did not result in any secretion. An objection may be made that if the inactive sounds were prolonged over 30 seconds the reflex might appear. In the course of my experiments I prolonged many times the inactive tone to 60–120–180 or more seconds but the sound continued to remain inactive. Fig. 1 and Fig. 2 show conditioned reflexes of 30 and 60 seconds duration.

As both sounds were received through different telephones, it is clear that they varied in the place of their origin. The instruments were really separated from each other by one inch only and it seemed little probable that the dog could discriminate the sounds by such a small

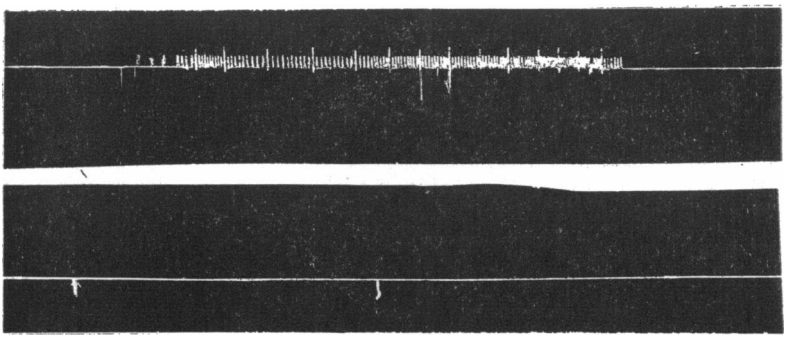

FIG. 1. Dog 1. *Upper tracing* shows the conditioned reflex to a tone of 637·5 vibrations. The tone lasted 30 seconds—begun at the first and ended at the second downward mark. The third mark shows the beginning of the unconditioned stimulus. Each mark upwards = 1 drop = 0·01 c.c. Each bigger mark = to each tenth drop. Reflex = 68 drops.

Lower tracing shows a full discrimination of the inactive tone of 680 vibrations lasting 30 seconds.

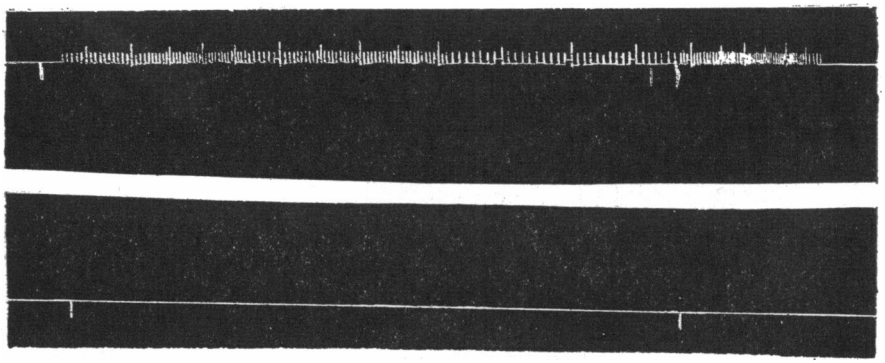

FIG. 2. Dog 1. Same as in Fig. 1, but both tones continued for 60 seconds. Full discrimination: active tone 637·5 vibrations—128 drops; inactive tone 680 vibrations-note.

difference in origin. But still, in order to be quite sure, I determined to test it by interchanging the position of the telephones; the result was that the active sounds remained active despite the change, and the inactive ones remained likewise inactive.

Further investigations of the discrimination of pitch proving interesting, I continued my experiments with Dog 1 until a differentiation of 680 vibrations was obtained. This differentiation was reached with the same rapidity as in the preceding cases. The dog obtained a complete (see Figs. 1 and 2) differentiation at the 10th test.

Action of Extra-stimuli. All the differentiations obtained were absolutely stable and reliable. Only when affected by extra-stimulus did the differentiation become inhibited. According to data collected we have every reason to regard the differentiation obtained not as the result of an unsusceptibility worked out by the nervous system to the inactive stimulus, but as the result of true inhibition. Consequently the inactive note does not figure as an ineffective stimulus, but as an inhibitive factor. Each extra stimulus in turn inhibits the conditioned activity of the brain, superimposing itself on the process it encounters in every part of the same. If it meets with excitation it inhibits the excitatory process, if it meets with inhibition it inhibits the inhibition. In the second case the primary inhibited reflex (in my case the inactive tone) is to assert itself during the inhibition of inhibition. I will quote an example to illustrate the point. The strength of the reflex conditioned by the active sound varied from 80 to 90 drops in Dog 2. During one of the tests the noise of a heavy apparatus falling in the next room penetrated to the experimenting room. The reflex was promptly inhibited and, instead of the usual number, 30 drops only were secreted. In another case I produced the noise on purpose by adding to the active tone the sound of a beating metronome, the reflex diminished to 14 drops. On repetition, the extra-stimulus gets less effective and soon takes the character of the conditioned stimulus with which it coincides, inhibitory or excitatory. If an extra-stimulus is induced during the presentation of the inactive tone, one gets the opposite effect—the inactive tone becomes active as the inhibitory state of the brain is inhibited in its turn by the extra stimulus. This is an example: After repeated experiments with Dog 4, when the differentiation of the second sound had been firmly established, something irritated the mucous membrane of the animal's nose and the dog sneezed. Three minutes later an inactive note was sounded and instead of the zero anticipated, 28 drops of saliva were registered. No secretion was caused by the sneezing, but the irritation produced inhibited the inhibition.

In another case, when experimenting with Dog 4, a large fly flew into the room: this very slight noise was quite sufficient to inhibit the inhibition and to cause the secretion of 15 drops. If a metronome or a

simple bell is set in action during the sound of the inactive tone, one gets a still greater effect.

Dr E. A. Hannike, head of the Physico-Physiological Department of the Institute, is responsible for the construction and division of the synchronous motor for the pure sounds, of the automatic drop recorder and the physical installations.

SUMMARY.

The statement of Pavlov's school concerning fine auditory differential capacity in dogs is true also for pure tones, *i.e.* dogs have a highly developed pitch discrimination. Johnson's deductions are explained by the fallacies of his method.

Note. Johnson considers that a slight variation in the auditory stimuli might prove strong enough for a salivary reflex, though not strong enough for the voluntary muscular system. This supposition has no support in my experiments as I many times observed that the dogs, hearing the active sound, tried to reach the food plate bringing to play the entire muscular system of their bodies. The sound of the inactive note left them perfectly quiet or even gave a negative motor reaction and the dogs turned their heads from the receptacle on hearing the sound. It therefore follows that Johnson's theory concerning a partial power of pitch discrimination is incorrect. Dogs possess a full pitch discrimination power and if a suitable method is used it can be observed on muscular and salivary reflexes equally well.

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