infection, which probably contributed towards the fatal termination of the case.

Œdema of the Larynx, associated with Chronic Nephritis.—E. RUTTIN.

A woman, aged 49, fell ill with a sore throat on *March 20th*, 1933, and on *April 7th* had a further relapse with fever, hoarseness and joint pains for which she was treated for seven weeks in Hospital.

In *August* subcutaneous hæmorrhages occurred in the arms and legs. On *December 13th*, 1933, she was admitted to the Medical Wards, where examination revealed a chronic nephritis. On *January 26th*, 1934, she was first seen by the Exhibitor complaining of hoarseness. An œdematous condition was found in the left ventricular band and the corresponding arytenoid area but the remainder of the larynx and epiglottis were quite normal in appearance. The glands in the right side of the neck were swollen and were later incised on account of suppuration. The condition of the larynx had proved unresponsive to all treatment. On *February 19th*, 1934, she died.

The *post mortem* examination revealed septicæmia, chronic nephritis, general œdema, etc. The Exhibitor reported the case as representing an unusual localized laryngeal lesion, doubtless due to a nephritis, which latter was presumably the result of sepsis beginning with a sore throat.

ROYAL SOCIETY OF MEDICINE—SECTION OF OTOLOGY

May 3rd, 1935

President—E. A. PETERS, F.R.C.S.

The Treatment of Otosclerotic and Similiar Types of Deafness by the Local Application of Thyroxine.

The opening paper read by MR. A. GRAY is published in this issue of the *Journal of Laryngology and Otology*, Vol. L, No. 10, pp. 729-751.

DISCUSSION

THE PRESIDENT said that otosclerosis had been the greatest bugbear of the specialty. He had been struck by the fact that in the drums of the patients exhibited there were no signs of inflammation. The treatment should be used extensively in these cases.

MR. ALEXANDER TWEEDIE said he would like to have Dr. Gray's opinion as to the way in which the thyroxine acted. Did it depend

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for its action on actually reaching the auditory nerve? Would it be convenient for Dr. Gray to express the hearing values in terms of cycles or double vibrations.

MR. J. F. O'MALLEY said that twenty years ago he had conceived the idea that in otosclerosis there was a fixation of the stapes, and that the changes found were related to those in rheumatoid arthritis. Acting on that idea he had tried treatment by local applications of heat. He had made an asbestos-lined box, designed to include the whole mastoid and ear, the remainder of the head being shut off from the heating apparatus. He had tried this in twenty cases extending over a year. The idea was not far removed from that of Dr. Gray's, as the local heating caused hyperæmia, just as injection of thyroxine did. Associated with this hyperæmia was a vasomotor reaction which, no doubt, influenced function. When localized action was produced anywhere in the body there was a corresponding increase in vasomotor activity for the time being. The line of treatment which Dr. Gray had adopted was an exceedingly interesting sequence to the conclusions at which he had arrived as to the underlying deficiency in cases of otosclerosis. It provided otologists with a new hope.

DR. GEORGE CATHCART said that Dr. Gray had arrived at the same conclusions as Zünd-Burguet had arrived at more than twenty years ago. Zünd-Burguet believed that it was essential to obtain local hyperæmia, and he invented the electrophonoide method which appeared to give the same results as Dr. Gray's thyroxine injections. The hyperæmia of the ear (presumably of the whole internal ear) could easily be seen by inspecting the drum; again the secretion of wax was restored, and in 70% of the cases the tinnitus ceased. The tinnitus in two cases had ceased completely and permanently after the second treatment. The chief difference in the two methods of treatment appeared to be that the Zünd-Burguet method took a longer time to carry out; a week must elapse before one could tell whether improvement was likely and another week before improvement of a more or less permanent nature could be expected.

The results of Dr. Gray's treatment did not compare favourably with those obtainable from the Zünd-Burguet method. Dr. Gray did not take into account the voice. He only mentioned the whisper and seemed to imply that if hearing for the whisper improved, hearing for the voice improved *pari passu*, but that had not been his (Dr. Cathcart's) experience. Perhaps Dr. Gray's method of whispering was different from his. He (Dr. Cathcart) used the method described in his book which was one of constant strength. This was obtained by taking a breath and forcing it out again, using only the residual air for the whisper. Not one of Dr. Gray's patients shown to-day could hear that kind of whisper, and therefore he (Dr. Cathcart) had no means of judging if improvement had been

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produced. Case VI was an exceptionally good lip-reader, and with his eyes shut he could not hear his (Dr. Cathcart's) ordinary conversational voice at a distance of 4 ft., and yet he could hear Dr. Gray's whisper at 18 in. Cases III, IV and V stated that they could not hear ordinary conversation across the dinner table, therefore the results could not be regarded as particularly good. It would be interesting to know if a combination of the two methods would bring about a better result, and Dr. Gray had kindly promised to allow the speaker to try the Zünd-Burguet method on cases which did not respond to his (Dr. Gray's) method. It would be valuable to ascertain how long the results of Dr. Gray's method remained good.

MR. F. C. ORMEROD wished to ask whether the use of distilled water in itself produced any effect in this procedure. He thought it must produce some osmotic changes. Did distilled water assist in the hyperæmia, or would more hyperæmia follow if he used normal saline as the solvent of the thyroxine ?

Mr. T. B. Jobson asked whether Dr. Gray considered it necessary to adopt special methods of sterilizing the external auditory meatus before applying the injection.

MR. MILL asked whether injection of distilled water alone had any effect on controls.

DR. GRAY (in reply) said that the effect of the injection was a dilatation of the blood-vessels throughout the organ; it was not probable that it had acted by any direct effect on the cochlear nerve. If there were a proper strength and a proper osmotic tension it might then pass through the membrane of the round window; but he doubted whether, as used in the cases shown, it went through that membrane.

Local treatment by heat produced exactly the effect which was desired, because it caused dilatation of the blood-vessels, but it was difficult to produce heat in the ear, and, moreover, the effect would not last very long. One of his patients had said, three or four days after the injection, that his ear began to feel " warm inside ".

He did not know how the improvement with regard to the tinnitus was to be explained by the treatment, except on the lines of general improvement in the hearing. Case II was an instance of striking improvement in this respect. The patient had previously been having a terrible buzzing in the ears, and that had now almost gone ; it had disappeared also in Cases IV and V.

He had never seen any very satisfactory results from the Zünd-Burguet method of treatment. As to hearing whispers, all whispers were different ; one could only estimate the intensity of one's own whisper; the watch and the audiometer constituted better standards. All the patients seemed to be pleased with the result, and that was the best test. All tests which involved the attention of a patient introduced the possibility of fallacy. It was a striking tribute to the

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success of the treatment that knocks and rings at the door, church bells, etc., previously unheard, were now heard again.

With regard to the use of distilled water because of its effect on osmotic pressure, he was unable to answer the question. If distilled water produced any effect on the hearing it was difficult to see how that effect could last for several weeks.

Before injecting the thyroxine he mopped the meatus out with spirit. It must be remembered that aniline itself was a powerful antiseptic, and was able to penetrate the skin.

He had no reason to suppose that distilled water alone would improve the hearing, whereas he thought that thyroxine would, because of his former experience with it as applied to the nose. The injection of distilled water could not be expected to produce improved hearing four days later and continue that improvement for weeks, and also cause a freer secretion of wax in the external auditory meatus.

Some Practical and Theoretical Points in Labyrinthology

By Dr. Gösta Dohlman (Lund)

Abstract.—The work of Steinhausen in Greifswald on the function of the cupula (*Pflüger's Archiv.*, 1933, Band 232, Heft 4) is of extreme value in the conception of the physiological mechanism of the labyrinth. Extension of this work by measurement of the exact relationship between the movements of the endolymph in the canals and the bending of the cupula, together with demonstrations by means of pictures, etc.

Comparison of the three methods of examination of the peripheral labyrinth. The time of the post-rotational nystagmus, the duration of the nystagmus after applying a constant pressure to a fistula, the time during which the caloric nystagmus continues after changing position of the head from Brüning's optimum to minimum position—all are comparable quantities and are at times of about the same length, and all seem to be due to the elasticity of the cupula.

The galvanic reaction: why it does not give practical results: the theory and technique of chronaxiemetrie: a new method of obtaining an exact estimation of the chronaxie of the vestibular nerve and the possibility of the practical use of this method.

Demonstration of apparatus for determining the vestibular chronaxie, and of instruments for the exact registration of the eye-movements during nystagmus.

In 1874 Mach-Breuer, and Crum-Brown made the suggestion that fluid currents of the endolymph were the essential stimulus for the epithelium of the crista ampullaris. This was the first step to a more physical conception of the function of the labyrinth, as distinguished from an earlier physiological point of view, based more on theory than supported by facts.

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In the sixty years that have passed since then, an enormous amount of work has been done in investigating the mechanism of the labyrinthine functions. This work has been well justified since it is obviously important for us to know, with detailed certainty, the physiological functioning of the labyrinth, in order to decide as to the value and scope of our methods for its examination. And we must also have a firm physiological basis, if we are some day to be able to distinguish the functions of the peripheral organ from those of the central nuclei.

When the story of the endolymph-current was advanced, it was thought—and it is still sometimes thought—that during a head-turn or a multiple rotation, the endolymph-current in the semi-circular canals went on as long as the vestibular reactions lasted. But the labyrinth is a very small organ with minute capillary canals, so that there was great difficulty in obtaining an acceptable correspondence between the current-theory of relatively large and lasting fluid movements and their physical unlikelihood. This brought about fresh—physically, still less likely—hypotheses, e.g. those presuming a pressure effect to be the stimulating agency (Biehl, Wittmaack).

When Gaede calculated physically what amount of endolymph-movement was possible, he proved that so powerful a stimulus as ten revolutions in twenty seconds cannot produce a fluid current for longer than about $\frac{1}{10}$ second. Direct observations on models made by Maier and Lion proved that movement of the fluid took place during and after rotation, but they were of too short duration for measurement by direct observation. The difficulty that remained was how to explain the mechanism of a nystagmus lasting as long as from fifteen to forty-five seconds caused by a movement in the endolymph during only parts of a second. Bárány tried to explain this as a result of central reactions, Maier and Lion as dependent on chemical processes; others avoided the question. But until such a main point in our conception of the function of the labyrinth has been brought into correspondence with known facts, we are building very insecurely in our attempt to distinguish the central and peripheral factors during stimulation.

First, turning to the peripheral organ, we must ask what mechanism transmits the stimulus from the fluid-current to the neuro-epithelium. From an illustration in any histological description of the labyrinth (Fig. 1) we get the impression that the cupula from the crista extends into the lumen of the ampulla at most perhaps half its section. With a current from the canal into the ampulla after a rotation of ten revolutions in twenty seconds, there is, according to Gaede's calculations, a movement of the endolymph in the canal of at most $\frac{1}{2}$ mm. In the ampulla, as a result of its ten-times greater width, there can occur a movement of, at most, $\frac{1}{20}$ mm.

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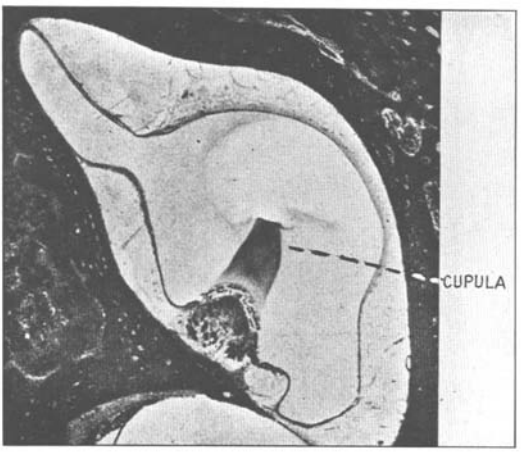


Fig. 1.

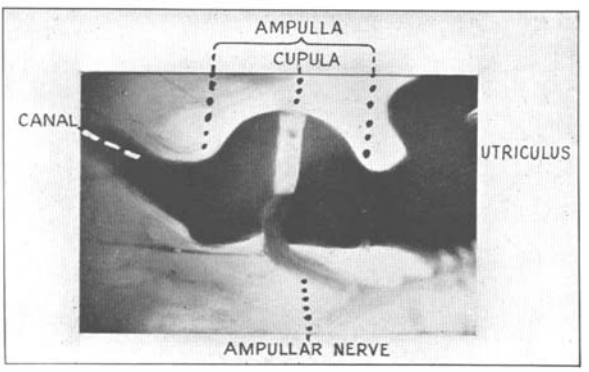


Fig. 2.

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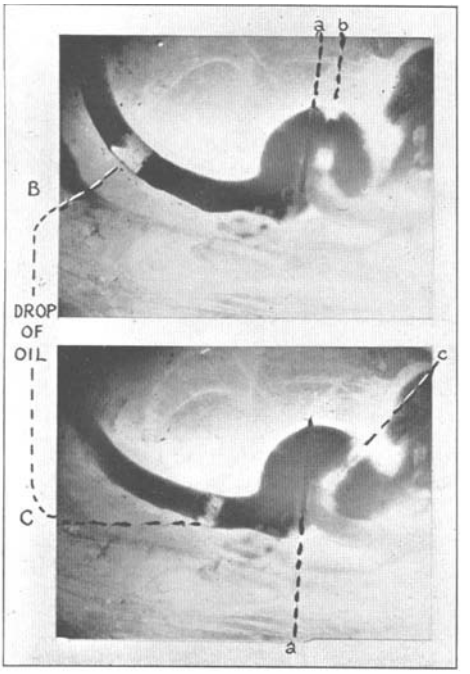


FIG. 3.

a. Piece of hair showing the initial direction of the cupula.
b. Initial position of the cupula.
B. Initial position of the drop of oil in the canal.
c. Forced position of the cupula due to the movement of endolymph.
C. Second position of the drop of oil due to the movement of endolymph.

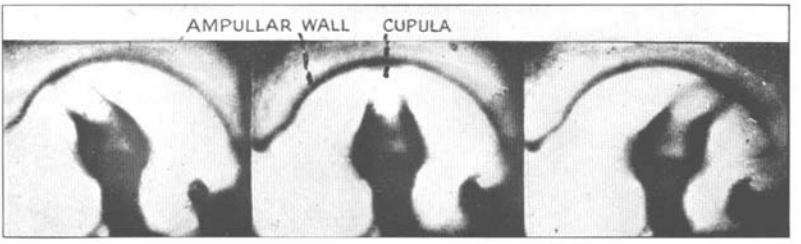


FIG. 4.

Through the courtesy of Professor Steinhausen one of his films was shown at the meeting. The above pictures (Figs. 3 and 4) are taken from this film, and show the different positions of the cupula during vestibular reactions.

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Naturally, from this, one gains the impression that the current from the canal would lose itself like a streamlet in the great lake of the ampulla, without any noteworthy effect on the insignificant cupula. Therefore we must ask: (1) Is this picture of the cupula correct? (2) Is the relation between the sections of the canal and the ampulla really as large as 1 : 10?

The first question is answered by the investigations of the function and morphology of the cupula, published during recent years by Professor Steinhausen of Greifswald, who showed that by introducing a contrast, e.g. Indian ink, into the endolymph, the cupula could be made visible in unstained dead, or even living, animals. This can be demonstrated from one of my preparations made from a fish, according to the methods described by Steinhausen (Fig. 2).

As seen in this way, the cupula actually fills the ampulla from the crista to the opposite walls. Every movement of fluid in the canal must then unconditionally cause a movement of the cupula. By this means we get a corresponding stimulus in the neuro-epithelium, probably in the same way as we get it through the disturbance of other similarly built sense-organs, e.g. in the skin. From this we have plain evidence (1) That endolymph-movements caused by rotation of caloric stimulation can occur in the canal, as previous investigations have shown; and (2) that they can displace the cupula. By experiments on living fishes Steinhausen has shown that the bending of the cupula gives vestibular eye-movements, and thus really forms the essential means of stimulus for the neuro-epithelium. By this the Mach-Breuer and Crum-Brown hypothesis is corroborated in principle, and the function of the semicircular canals explained.

The second question is the relation between the cross-sections of the canal and the ampulla. The canal cross-section, as I have just indicated, is only one-tenth of the cross-section through the ampulla, according to the measurements of Biner Wulf and others. From this we conclude that the movement of fluid in the canal must be ten times as great as the endolymph-movement or cupula-movement in the ampulla can be. The difficulty is, then, in seeing how an endolymph current large enough to stimulate the neuro-epithelium in the ampulla can take place in these fine capillary tubes of the canals. By Steinhausen's method, however, we can see to what extent the cupula follows the movements of the endolymph. From a little opening in the wall of the membraneous canal, the lumen of the canal and the ampulla is filled with the black fluid from Indian ink introduced into the canal, but it stops at the cupula, which then appears as a transparent formation stretching from the crista to the opposite ampullar wall. With a glass capillary I have introduced a little drop of oil into the canal (Fig. 3). The drop

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lies like a transparent pearl occupying the canal lumen. In this way the fluid movements in the canal and the displacement of the cupula can be followed together. As is apparent from the figures, an endolymph displacement of from 1 to 2 mm. causes an angular change of position of about 40° in the cupula.

Again, we know that a fluid movement in capillary canals like those of the labyrinth, after a rotation, cannot last longer than the fraction of a second, because the inner friction, the viscosity of the fluid, in these minute capillaries prevents a longer-lasting movement. And yet after rotation we have a nystagmus and other vestibular phenomena of stimulation that last about half a minute.

By his experiments on living animals Steinhausen has shown to what this is due. Two of his results are of importance here: (1) That a brief displacement of the cupula occasions only a deviation of the eyes, but a longer-lasting displacement causes a nystagmus. (2) That after displacement—for example during or after rotation—the cupula slowly resumes its initial position through its own elasticity, and at the same time the nystagmus gradually decreases and stops. This displacement and elastic return of the cupula explains why a post-rotatory stimulation lasts longer than the stimulating endolymph current, which only can bring about the cupular displacement.

Which mechanism occasions the movement of the endolymph during and after rotation? The only thing that can affect the endolymph during a rotation is the inertia of the fluid, acting during increase or decrease of rotational speed, i.e. an acceleration or retardation of the movement—but not rotation at even speed. Although during a brief turn of the head there is no post-rotational nystagmus, even if the alteration in speed is considerably greater with an ordinary quick turn of the head than with our usual experimental stimuli. This must be due to the fact that in turning the head we get a swift deviation of the cupula, and thereby a deviation of the eyes; and as a result of the retardation we must get a movement of the cupula in the opposite direction, and a deviation of the eyes also in this same direction. By this means the cupula and the eyes return to their starting point. But if we continue a rotation for a longer period, as in the usual rotation test, we have to imagine the reaction in the following way:—

The cupula deviates during, perhaps, the first, or possibly also the second, revolution as long as the rotation is accelerated. On reaching a constant speed the rotation surely no longer affects the fluid or the cupula in any way. The deviation of the cupula is now, however, gradually diminished by its elasticity; and after about half a minute it has returned to the original position. It is to be expected that the nystagmus *during rotation* should cease when the

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cupula resumes its normal position, and it does, as Buys showed by nystagmographic registration several years ago.

When rotation stops we again have, as a result of the effect of retardation on the endolymph, a deviation of the cupula, this time in the opposite direction. A post-rotational nystagmus occurs, which lasts as long as the cupula needs to return once more, through its elasticity, to its starting position. This explains why a post-rotatory nystagmus does not occur until after a longer continued rotation. And it explains why we must have a rotational time of about twenty seconds to obtain the longest nystagmus for any rotational speed. For the cupula has by that time regained its initial position by its elasticity, so that it might be ready for the maximal displacement in the opposite direction through the inertia of the endolymph when the rotation ceases. So the nystagmus *during* rotation, like the *post-rotational* nystagmus, is a consequence of the deviation of the cupula ; and its duration depends on the time the cupula requires to reassume its normal position.

From this it will be expected that as Buys, Grahe, Maier, Lion, and others have shown, the nystagmus should have the same duration both during and after the rotation.

We may conclude from this that the rotation test cannot directly gauge the sensitiveness of the vestibular apparatus, or measure its capacity for reaction. The time for the post-rotatory nystagmus that we measure in the rotation test is primarily related to the elasticity of the cupula. This by no means directly corresponds to the susceptibility of the end organ or the central nuclei.

But these deviations of the cupula and its elastic return are characteristic not only of the rotatory stimulus. In the nystagmus by pressure, as in the fistula test, the following is found : If a constant pressure is applied to the fistula, of e.g. the horizontal canal, the effect will be that the membranous canal is more or less compressed, the endolymph displaced, and a deviation of the cupula is obtained. The nystagmus then lasts about twenty seconds, as Nylén has already shown. So we have once again the time which the cupula needs to reassume its initial position, by its elasticity.

We can obtain a similar and comparable reaction with caloric stimulation also. If a sufficiently strong caloric stimulus be applied with the head in the optimum position—i.e. with the horizontal canal in a vertical position—the maximal response is obtained after about one minute. If the head be then raised, so that the horizontal canal returns to a horizontal position, the nystagmus continues for a further period of about half a minute, or approximately as long as the nystagmus in the same subject after a rotation test.

What is the reason for this ? According to several investigators there should be a fundamental difference between the stimulating

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mechanism of the rotational reaction and that of the caloric reaction. On the one hand, there is the powerful but brief nystagmus after a stimulus as that after a rotation, which physically appeared to be a strong stimulus. On the other hand, as the result of an apparently weak physical stimulus—the caloric test—a nystagmus occurs lasting as long as two or three minutes, This has given rise to hypotheses attempting to explain the caloric reaction in another way. Bartels, for instance, thought that cooling paralysed the neuro-epithelium of the labyrinth and that warmth stimulated it. Kobrak has tried to explain the caloric reaction as a vascular reflex. By measuring comparatively the threshold for the caloric and the rotatory nystagmus, however, I have previously obtained a physically quantitative agreement between stimulus and effect on the assumption that these two stimuli acted in the same way on the neuro-epithelium. The difference in the physical changes in the canal during rotation and caloric stimulation would then be a question of duration of the physical changes in the labyrinth rather than of difference in the influence on the neuro-epithelium.

After a rotation the cupula suddenly deviates from its normal position and then by its elasticity returns thither. During caloric stimulation, on the other hand, the canal wall cools with gradually increasing alteration in the temperature. In conformity with this, the weight of the endolymph increases during cooling in that part of the horizontal canal which is most lateral and most exposed to temperature change. The change in weight has a tendency to occasion a fluid displacement. But this is first checked by the elasticity of the cupula. No fluid movement can occur because the cupula prevents the endolymph from escaping as it extends across the ampulla. A deviation of the cupula can occur only to the extent that the weight-change in the endolymph balances the elasticity of the cupula. So the mechanism might be comparable to that of a spring-balance, the elasticity of the spring corresponding to that of the cupula, and the endolymph weight-increase corresponding to weights laid successively on the scale. As long as the weights are in the scale the elastic tension of the spring is balanced.

The difference between the two forms of stimulation—rotational and caloric—is therefore, that by rotation we stimulate *both* labyrinths, suddenly ; by the caloric test we stimulate *only one* labyrinth, with cumulative force during a relatively long time.

So that if we bring about a deviation of the cupula by altering the endolymph weight by caloric action, and then alter the position of the canal so that it lies horizontally and can therefore no longer affect the cupula, the cupula will be able by its own elasticity to resume its initial position in the same way as during post-rotatory stimulation. The post-rotatory and what I should like to call this post-caloric nystagmus, are presumably comparable,

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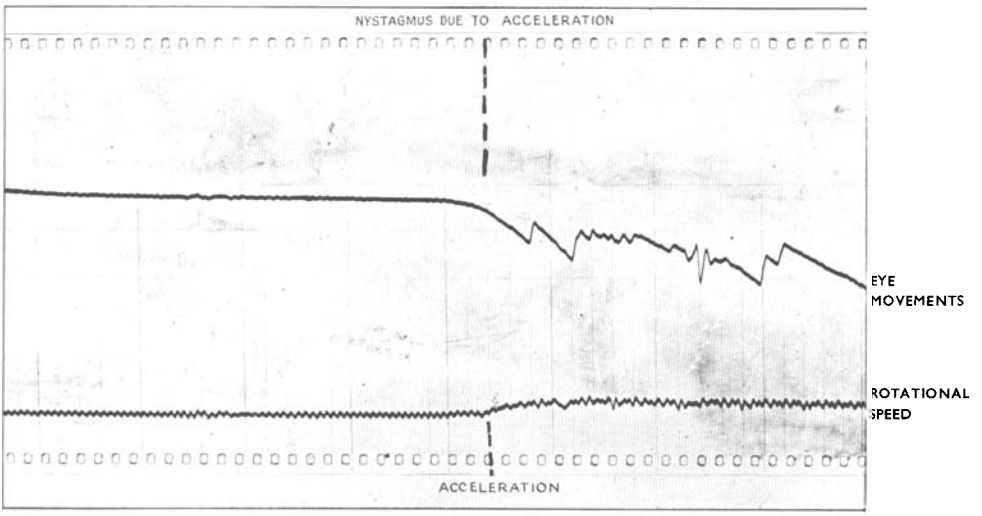


Fig. 5.

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except that the post-rotatory affects both labyrinths and the post-caloric only the stimulated side.

When we investigate the functional capacity of the eye, the sensitiveness of the skin, or the sensitiveness of the cochlea to sound-waves, we do not select the strongest light, the strongest skin-stimulus, or the strongest tone, to see how far perception can continue, as we do when using the rotation test. We cause the greatest deviation of the cupula and measure the time till the nystagmus has stopped. The physiologically appropriate way would be to apply the least possible stimulus that can produce a reaction, and from this infer the reactive capacity of the organ. So it ought to be with our current examinations of the labyrinth. Slight and brief turns of the head causing slight and brief deviations of the cupula are the normal, adequate stimuli for the vestibular apparatus. Powerful, long reactions are unphysiological stimulations. They cause dizziness and subjective discomfort which do not accompany the normal function of the organ. Therefore I think we should get more reliable results if we tried to fix the normal stimulability of the organ, and the threshold for its sensitiveness, as far as possible within physiological limits for the strength of the stimulus. Remembering the fact that only speed-alterations during rotation can affect the vestibular apparatus, I think it would be most appropriate to measure the sensitiveness of the labyrinth by slight increases and decreases during an otherwise constant rotational speed. In that way it would be possible to get the response of slight stimulations and estimate their effect. I have tried to find a method of comparing these accelerations and retardations with the vestibular eye-movements received. Through nystagmographic registrations of the eye-movements and electrical registrations of the speed—and especially of the speed changes of the rotation-chair—I have been able to compare both the response in the form of the vestibular nystagmus and the stimulus-strength, i.e. the increase or decrease in rotational speed (Fig. 5).

Now if we examine the various methods of caloric stimulation, we find the following: There is Brüning's method of syringing till the nystagmus appears. There is Kobrak's method with small graded quantities of water, where the time of latency and the course of the reaction are estimated. Or there is the method of syringing recently suggested by de Kleijn and Versteegh, in which small quantities of water of varying temperature are employed until just that quantity and temperature that occasion a reaction are obtained. All these methods indicate a degree of heat that is claimed to be a quantitative measure of the stimulability of the labyrinth. But I think this is not the case.

With the caloric reaction we can compare both sides of the same subject. From one-sided deviations we can draw certain conclusions

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about pathological changes, but we cannot compare the reactions of two subjects, and conclude, from the deviations, anything about the peripheral or central function of the vestibular apparatus. I believe that I can throw some light on this known but less noticed fact, in the following way. Together with one of my assistants, Dr. Kaplan, I have measured the temperature changes of the labyrinth-wall during caloric reaction in radically operated patients with good labyrinthine function, and I found that a raising or lowering of temperature of only from $\frac{1}{10}$ to $\frac{2}{10}$ of a degree centigrade was needed to obtain a nystagmus in one direction or the other. The temperature measured in the operation-cavity was always somewhat lower than the rectal temperature (about 1.5° F.). It is interesting to note that we then actually obtain a warm-water nystagmus when syringing with water colder than the rectal temperature. But there is certainly not a strongly increased sensitiveness of the labyrinth in these cases even if we get reaction at such slight temperature-changes, rather the contrary. In an ear which has not been operated on, however, we have a barrier of bone and cavities containing air. These take most of the applied warmth or cold, not measurably and regularly, but uncontrollably and irregularly. So that we are primarily measuring the warmth-absorption of the surrounding bone, not the reactive capacity of the labyrinth, or the relation between these values.

In the previously mentioned rotatory stimulus we can, of course, measure the value of the stimulating moment in the peripheral organ through the acceleration and retardation. But the response, the nystagmus, is dependent on two moments, in rotatory as well as in caloric stimulation.

The second moment is the central reaction to the stimulation of the end-organ. Thus the problem is, to what extent the response in the form of vestibulary eye-movements is dependent on central conditions.

We know that a vestibular nystagmus can be altered by fixation, conscious eye-movements, and other cortical and subcortical movements. We have, for example, modifications of the nystagmus after a previous stimulus which have been interpreted as a fatiguing of the centre. A post-rotatory or caloric stimulus does not elicit the same response on repetition. Here I think it necessary to distinguish between the slow phase of the nystagmus and the quick component. I have stimulated human fistulas and experimentally made fistulas in animals, and I have stimulated the labyrinth by the galvanic current, all for lengthy periods, but have not been able to demonstrate any decrease of response. These responses, however, referred only to parts of the slow phase, when the stimuli were so brief that the quick movement of a nystagmus had no time to develop. The consequence of this is that we must suppose the slow phase not

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to be easily fatigued. Suppose, however, that we study the changes in the quick phase under similar conditions. We find, as Quix, Spiegel and Lorente de Nó have indicated, that with a weak caloric stimulus a nystagmus can be obtained almost indefinitely. But with stronger stimulation the powerful caloric nystagmus is soon changed to slight, quick, oscillatory eye-movements and the whole nystagmus-apparatus is disordered.

Compared with this we have another change in the caloric nystagmus as well as in the nystagmus during and after rotation. After the completed stimulus we have a weak nystagmus in the contrary direction to that previously stimulated. We get this not only with stimulation of the peripheral organ, but also after a galvanic stimulus —which acts centrally beyond the labyrinth—and even after the visual action which produces optokinetic or " railway " nystagmus. All this seems to me to show that the central modification of stimulability during a nystagmus is to be attributed rather to the mechanism that controls the quick phase than to that which influences the slow phase of the nystagmus.

The rotatory, caloric, and pressure stimuli give a stimulation of the neuro-epithelium in the end-organ. The only one of our investigation methods that acts centrally beyond the labyrinth is the galvanic stimulus. This has not become practically significant, because it gives us no information about the functional state of the peripheral labyrinth, or about the nerve or the central nuclei. If we obtain a galvanic nystagmus at 2, or 4, or 8 ma., we learn nothing of the stimulability of the vestibular apparatus, which in all cases may be equally great, or of that of the peripheral end-organ, as well as the nerve, or the central nuclei. The electrical conductibility of the surrounding tissue may vary so much on account of anatomical differences, in the same way as the warmth-absorbing capacity of the tissues varies in the caloric reaction.

The galvanic current acts centrally beyond the peripheral end-organ. From earlier experiments I thought I might conclude that the current acted in the vestibular ganglion, but from later experiments, and those of others (Huizinga), I think it can be asserted that not even the ganglion is needed for a reaction to occur.

But if we in practical work may abandon the galvanic reaction in its present form as a method of testing the vestibular apparatus, we agree with the change that has occurred in other branches of electro-diagnostics.

Bourgignon, Lapique and others considered that they had found a more correct value for the stimulability of a nerve, theoretically and practically, by introducing as the measure of that stimulability the time during which a certain electric current needs to act for a nerve stimulus to occur. This time-value, which has been called " chronaxie ", is usually measured in thousandths of a second.

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Practically, chronaxie-measurement is carried out in this way: The lowest current-strength, i.e. one which can just twitch the muscle or stimulate the nerve on test—is tried out. Then the current-strength is doubled and the time of stimulation decreased until the threshold is reached, and this is considered to give the required value for the nerve's stimulability.

Bourgignon has also tried to fix the chronaxie of the vestibular nerve. He has used the vestibular disequilibrium as an indicator of the reaction.

It is plain that the mass and slowness of the body as a whole, and similar irrelevances, must have a most disturbing effect here, and must render the reaction unsuitable as an indicator for a mechanism so subtle and sensitive as the vestibular reflex. The shortest reflex path is between the vestibular apparatus and the eye muscles. It is also, as is well known, the most sensitive, and the least susceptible of influence by the subject himself.

To be able, however, to use the eye-movements as an indicator in fixing the chronaxie, a nystagmus cannot be utilized. It is the deviation of the eyes which begins the slow phase of a nystagmus that interests us. The quick phase in nystagmus is too irregular in its appearance and has, therefore, no significance in this connection.

A vestibular eye-movement, though slight, occurs even with very weak stimuli. If a galvanic nystagmus is obtained at from 3 to 4 ma., most frequently even at less than 1 ma., eye-movements can be observed at the switching on-and-off of the current. If the electrically stimulated eye-movements are registered nystagmographically, it is possible to measure the response objectively and to establish its relation to the stimulation-time.

Now, change in current-strength per time-unit forms the strongest stimulus for all other known nerves, and so the faradic current with its continual fluctuations stimulates more readily and during all the time the current is applied, but the galvanic current stimulates only at the points when the current is being switched on or off, or otherwise alters in current density.

With vestibular stimulation, it has generally been considered that things were the other way round. There the galvanic current, the constant current, has been the stimulant and the faradic current, alternating current, considered incapable of stimulation. But this is not correct. If the chronaxie value of twenty-thousandths of a second found by Bourgignon were correct, it would mean that a stimulus lasting one-fiftieth of a second could give a vestibular effect. If, in other words, the faradic current has a frequency that gives fifty stimulations per second, it can occasion a stimulation-effect. This is so, even with a considerably higher frequency, but the response is not a nystagmus; it is, for every alteration in

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current-strength, a slight and insignificant eye-movement. These eye-movements are so extremely slight and so frequent that they are not observable by the naked eye, but they can be registered photographically to a frequency of over 150 changes in a second.

We have, however, yet to determine whether this effect on the eye-movements is due to stimulation by changes in the strength of the current at its opening and closing; or to the stimulus usual for the vestibular nerve by the passage of the galvanic current through the nerve, presuming that the current has been passing long enough. The reversal of the eye-movements with the current-direction favours the latter notion.

To solve this problem I made an apparatus with which I could vary the length of the stimulus, and the change in current strength in order to distinguish whether the vestibular nerve reacts only for the passage of the constant current, or also, transiently, as the strength of the current is varied—for instance at the opening and closing of the circuit. Until we know that, we cannot know what phase of the current is the significant stimulus, but I hope that by this method we shall be able to discover these details.

During the era inaugurated by Bárány's important works on the vestibular apparatus we were enabled to arrive at an adequate diagnosis of the diseases of the peripheral labyrinth. Further, intensive work in otoneurology has given us a faithful picture, in many respects, of the central connections and their function. From this we have been able to diagnose a number of changes affecting the vestibular nuclei or their connections. Often, however, our methods are insufficient to decide whether a central or a peripheral lesion is in question. This is apparent from the many suggestions made for the improvement, refinement and alteration of our investigation technique, which indicate that such methods, unfortunately, are not yet altogether satisfactory.

In future, by a more exact interpretation of the results of stimulation of the peripheral organ and by the help in elucidating the function of the central paths, which may be afforded to us by the method of electric stimulation, we shall, I hope, evolve a better technique for the differentiation of peripheral and central lesions of the vestibular apparatus.

I have spoken of rotational, caloric, and electrical stimulation as our methods of practical examination, but that they may not embrace all experimental possibilities has been shown by some work by my collaborator and first assistant, Docent Skoog, who has been investigating the Ménière syndrome, particularly from the point of view of its production by allergens. By injecting into one of the vertebral arteries of a guinea-pig an antigen causing anaphylaxis, he has obtained a group of symptoms similar to Ménière's. For some time after injection the animal develops symptoms of vestibular

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stimulation in part referable to a disequilibration of the postural reflexes, and in part to changes in the compensatory eye-movements and the functions of the canals. That these symptoms are not referable to the peripheral end-organ can be shown by the fact that the phenomenon occurs unaltered even if a double labyrinthectomy has been performed on the animal.

The phenomenon is plainly bound up with a certain vascular territory. With injection of the same antigen centrifugally into the carotid artery, no reaction is obtained. Neither is it obtained by injection intravenously. In these cases, with the same dose only a common anaphylactic shock occurs. The phenomenon may be blocked, by injecting Indian ink a short time before the antigen is injected. The reaction corresponds neither to an ischæmia, such as we might expect after an adrenaline injection, nor to decay or damage of the vestibular connections, as in that case a contrary effect would be expected.

The interesting thing about these experiments is that, with an anaphylactic reaction like those with which we are familiar, in several parts of the body—even in man—we can produce disturbances that are comparable with Ménière's syndrome when the antigen, on certain vascular paths, reaches the vestibular connections.

Some Further Remarks on Labyrinth Tests.

By A. R. TWEEDIE (Nottingham).

DR. DOHLMAN has for about fifteen years devoted particular attention to that part of our speciality with which he has just dealt. His researches on the physiology and physics of " caloric nystagmus " were first published in 1925.[1]

This publication, which provides a summary of the work of previous investigators, contained the result of his own research, whereby he was able to establish or confirm amongst other things, that : (1) Compact bone possesses a thermic conductivity six times as great as the air in the middle ear, and thus that the bony wall and bony external semicircular canal provide the optimum route for the effect of the caloric test. (2) The comparison between the temperature of the irrigating fluid used in this test and that of the semicircular canal could be easily and accurately gauged. (3) As regards the clinical use of this test, the latent period is almost constant in the same individual, its variation being no more than four seconds. (4) The " quick " or " central " phase of vestibular nystagmus is influenced by the optical effect of fixation.

Four years later, another exhaustive article by Dr. Dohlman appeared on the result of his continued investigations towards solving the problem of the labyrinth, this time, on the galvanic

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reaction.[2] This is, in my opinion, the best account of this phenomenon yet published.

Once more Dr. Dohlman prefaced his account with an exhaustive survey of the work of previous observers, so comprehensive that, it was doubtless only by inadvertence, mention of Högyes was omitted.[3] In this report Dr. Dohlman described the rôle which he considered that the vestibular ganglion played in response to the galvanic test, and which, I believe, had previously never been suggested. His valuable and well-reasoned conclusions on this point were summarized in the statement that: The galvanic reaction depended in no way on the condition of the terminal sensory organ but on the functional integrity of the vestibular ganglion. I am sorry that he now feels obliged to modify this view, which seemed to me very helpful in the explanation of some of the responses obtained; though I must admit that I have had a case of severe herpes in which the auditory sense and caloric reactions were both totally lost on one side, whilst a normal galvanic response was retained; and, in this case of course one would be justified in assuming that there was some lesion of the vestibular ganglion. In addition, Dr. Dohlman also quoted some instances in which the caloric reaction had been obtained with no response to the galvanic stimulus; and offered, as the solution for this puzzle, the presence of an intracranial lesion.

Not content with all this effort, Dr. Dohlman published yet another paper (with Dr. Engvall) in 1930,[4] on the time-reaction in response to the caloric and galvanic tests, from which he deduced that the resulting nystagmus with the galvanic stimulus was at least twice as prompt as that obtained by the caloric test.

This sketchy résumé of Dr. Dohlman's contribution towards the solution of our problems, neither includes all his writings nor does adequate justice to those quoted, which must have represented many patient years of work; while his own hypercritical deductions will always be of inestimable value to all concerned in otology.

In submitting any of my own views on the subject, I have little further to offer since the last occasion on which I placed them before the Section.[5]

I then suggested amongst other things that in vestibular nystagmus we are dealing with very simple physiological phenomena, similar to the tickling reflex, and I have since been gratified to find that no less an observer than Lorente de Nó had just committed himself to much the same statement in a paper which I had not then seen.[6] I also said, "We know more of the physiological results of artificial stimuli than of the effects of pathological lesions" —a statement which I find is also confirmed by Lorente de Nó in a letter received from him this February, in which he writes: "The individual variations are so enormous that it is very difficult to state

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when a response begins to be pathological "—but I am glad that he was able to qualify this remark by a further statement that he was beginning " to find constancy in the results " of his tests ; and, that in a still later letter he could write yet more hopefully as to the result of his employment of the caloric test, based on the use of varying temperatures of water applied for various lengths of time, which latter factor, he says, is all-important and assessed by counting the number of nystagmic movements thus obtained.

We are still, however, faced by the lack of an accepted standard of comparison as between individuals, in whatever way we arbitrarily employ the factors of time, volume and temperature as a gauge in the caloric test, and adopt a margin of 1 to 5 ma. for the average amount to induce the head inclination, and 5 to 10 ma. for the production of nystagmus for the galvanic test, as suggested by Baldenweck and Barré.[7]

Although, however, we have this absence of accepted standard *as between individuals*, I would point out that we have in most cases a standard *for each individual*—that is to say, in most cases, there is one sound, or comparatively sound, side with which to compare the ear in question—a method which indeed is not altogether without analogy in testing reflexes in other parts of the body.

This introduces the question of asymmetry between the two sides, to which I have alluded before,[8] which Dr. Dohlman has also stressed, and which I too maintain that we are justified in utilizing fully in estimating the labyrinth condition for each patient ; since I think I may state that it exists, as regards one or more of the tests in every case of vertigo which I have examined. I was interested, in this connection, to read that such asymmetry had also been found by de Kleijn and Versteegh in the examination of most of the 100 cases of which they recently published an account,[9] cases not associated with any inflammatory lesion and constituting a class in which I consider these tests to be most valuable. That their tests were carried out with that meticulous aim at accuracy with which we have all learnt to associate the names of these observers, needs no emphasis from me, but they were restricted, one must note, to the effect of the caloric and rotation stimuli alone. It would have been of extreme interest to know the response of these patients to the galvanic test in comparison with and in addition to the results obtained from the other methods as summarized by the authors with such great care ; but de Kleijn, to whom I wrote on the subject, replied that they had not used the galvanic tests, as they found it more difficult to estimate the slight differences of irritability between the two sides—a reply at which I must confess I was rather surprised, since, of all the three tests, I have found the galvanic test to be the most adjustable, as it can be confined to the effect on one side. Also, since its application can be " continuous ", it is the easiest, for me

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at any rate, to assess as to response ; and, further, it certainly acts independently of the peripheral sensory organ. On the other hand, the rotation test must always involve some bi-lateral simultaneous stimulation—a point which Dr. Dohlman has also emphasized.

Incidentally in this paper also, as might be expected, these authors have dealt with the entry of otolithic action into such cases and they were able to separate their patients into four groups :

(1) Those in whom the vertigo was not related to any definite position of the head in space.

(2) Those in whom the vertigo and nystagmus were dependent on a definite position of the head in space.

(3) Those in whom the vertigo and nystagmus were dependent on a definite position of the head in relation to the body.

(4) Those in whom there was no sensation of rotation either in the patients themselves or in their environment, but to whom the environment seemed to be set obliquely.

This is a very valuable classification and the whole contribution will certainly rank as an important standard reference on the subject.

Although as I say, I have found the vestibular tests most useful in non-inflammatory cases, there is yet a further group where I think they may be of use, that is those subacute inflammatory conditions in which it is difficult sometimes to decide between palliative and operative treatment. This idea was suggested by Mr. Lieberman, one of the clinical assistants in the Aural Department of the Nottingham General Hospital, who is now carrying out investigations on these lines, under the ægis of the " Norman Gamble Fund " Committee. His results appear to offer some help and will be published later.

No account of the present position in labyrinthology would be complete, as Dr. Dohlman has ably indicated, without reference to the work of Steinhausen on the living pike—work that opens up vistas of promise, which as Dr. Dohlman's paper has shown, is no mere fanciful speculation, and serves as development of the early work of Flourens and Ewald, whose shades must certainly be haunting Greifswald.[10]

I would only venture to draw attention, in addition to the remarks of Dr. Dohlman, to the fact that Steinhausen also considers the rotation tests as unreliable, because the stimulus employed could not be restricted to the effect on one side, and his investigations are based on the action of mechanical and galvanic stimulation. From these experiments of his, three important points emerge: (1) That the ampullo-petal movement of the endolymph causes first a displacement of the cupula and then a deviation of the eyes to the opposite side, and (but *not unless* the stimulus is maintained) a secondary "nystagmus" to the same side ; whilst an ampullo-fugal movement, although displacing the cupula, results in no-eye movement ; (2) that the

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galvanic current causes no movement of the cupula, but (3) that a galvanic current of not more than 0·6 ma., directed from the utricle, through the ampulla to the canal, induces a deviation of the eyes to the opposite side, whilst even a current of 4 ma. in the reverse direction has no effect.

These results, at any rate, tend to confirm the importance of the primary deviation and also seem to indicate that in the galvanic test it is indeed the kathodal ear alone which is activated.

We are greatly indebted to Dr. Gösta Dohlman for the description of this work by Steinhausen and for his own investigations.

These brief remarks are intended to represent, at once, a sincere tribute to the past work of others, an indication of the present position of our knowledge, and a stimulus to further endeavour in solving the problem of the labyrinth.

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DISCUSSION

THE PRESIDENT, on behalf of himself and of the Section, thanked Dr. Dohlman for having come to this meeting to give a wonderful example of the modern phase of physiological surgery. He thought the importance of the labyrinth did not receive its due recognition in

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general medicine; for instance, the importance of toxic conditions affecting the vestibular centres, and also in everyday phenomena, such as fainting, in which the labyrinth was involved. There were three main types of mechanism in the labyrinth: the saccule and the utricle in the central division, the cochlea in the anterior section, and in the posterior division and the semi-circular canals.

Dr. Dohlman had explained the crista mechanism. He (the President) wondered whether Dr. Dohlman suggested an explanation of the mechanisms by which the cochlear and macular stimuli operated.

Sir James Dundas-Grant said he had listened to these two communications with great interest. The counsels of perfection which Dr. Dohlman had offered ought to stimulate otologists to greater accuracy but, while these new methods were being perfected, he hoped those working in the specialty might still use their simpler methods. He (the speaker) was himself responsible for having invented a cold-air apparatus, the adoption of which had considerably increased, though, after hearing such a paper as that read to-day, one felt it must be regarded as a convenient rough-and-ready method. It enabled one to obtain a very fair estimate of the degree of sensitiveness of the vestibule. The galvanic method was simple and effective, and it certainly gave a great deal of information. One placed the anode on the ear to be tested, and the kathode on the back of the neck or other indifferent spot. The patient stood with toes and heels together, and then the current was turned on until there was a postural deviation towards the anode which was unmistakable but did not cause the patient inconvenience. He had had several cases of cerebellar-pontine angle tumour in which the loss of reaction revealed by simple galvanic testing was of the greatest confirmatory value.

ABSTRACTS

EAR

An Objective Study of Auditory Fatigue. Walter Hughson and E. G. Witting (Baltimore). (*Acta Oto-Laryngologica*, xxi., 4.)

The writers point out that true objective observations in the study of auditory fatigue can be made only when consciousness is removed from the response, as in general anæsthesia.

The Wever and Bray phenomenon and its further developments offer for the first time the opportunity to study fatigue in a special sense organ in an objective manner. By this means it is possible to make quantitative measurements not only of the stimulus applied but also of the response obtained. With regard to the terms initial intensity, overload, and fatigue, used throughout the experiments the authors explain that the term initial intensity represents a value of volume loud enough to cause response in nearly all animals, but not so low as to be regarded as an actual threshold value. The

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