THE PLACE OF STIMULATION IN THE COCHLEA VERSUS FREQUENCY AS A DIRECT DETERMINER OF PITCH

BY JOSEPH PETERSON

University of Utah, Salt Lake City, Utah

Our problem is closely related to that of the specific energies of the fibers of the cochlear branch of the auditory nerve. Two types of theory, however, have attributed the pitch of any tone directly to the place of stimulation in the cochlea. The first of these types is represented, for example, by the resonance theory of Helmholtz and by the non-resonance theory of Hurst. Both of these theories associate any given pitch with a somewhat constant section of the cochlea. The second type of the *place* theory, so to designate it, is represented by the non-resonance theory of Emile ter Kuile, which holds that pitch is conditioned by the stimulation of a certain area or extent in the cochlea measured from the outer or fenestra end. The further the impulses extend into the cochlea the greater the extent of stimulation and therefore the lower the pitch of the sensed tone.

Of theories making pitch directly dependent upon frequency of stimulation Max Meyer's non-resonance theory is probably the best example. Some resonance theories also fall under this head in part or entirely; for example, that of Ebbinghaus belongs here.[1] A number of resonance theories are based in part on the resonance idea and in part on that of frequency. Such obvious inconsistencies come to resonance theories devised to support the view of Koenig, that certain tones result from rapid beats of interference.

It seems to me that sufficient experimental and easily verifiable facts are now known to reduce the confusion in this

[1] A theory that should be in a third class by itself is that of Ewald, also a resonance theory, assuming pitch to be determined by the distance of separation of the supposed stationary waves on the membrane in the cochlea.

field and to redefine more specifically our problem. Certain of the 'supplementary' resonance theories, more or less inconsistent with themselves, are no longer necessary to 'explain' any facts known. Experimental evidence has steadily gone against them. In the first place modified resonance theories of the type of Koenig's and Ebbinghaus', devised to account for beats of wide intervals, for 'beat tones,' phase-changing 'middle tones,' other kinds of alleged phase-changing tones, interruption and variation tones have no good reason for their existence. The brilliant experiments conducted by Koenig, Hermann, Dennert, and others, have lost their convincing effect against analysis of tones on the principle of Ohm's law, defended by Helmholtz. More recent experiments, conducted chiefly by K. L. Schaefer and O. Abraham, have shown that the interruption, variation, and the so-called phase-changing tones can be reinforced by physical resonators, and are therefore after all based upon sinus-form vibrations. The origin of these periodicities therefore becomes a physical problem, and has been explained mathematically. The phase-changing 'middle tone' of Hermann is now generally discredited.[1]

The question of the beating of wide intervals is evidently not so easily disposed of, inasmuch as it seems to involve in a measure the matter of tonal fusions and probably the much discussed problem of dissonance. Without trespassing too much upon unsettled problems we are probably safe in saying that the extreme view taken by Koenig on this matter is unnecessary. It is well known now that a beating of difference tones as well as of upper partials makes the generating tones seem to beat. My own experience makes it doubtful to me that the real source of the beating can be singled out with certainty, so that the generators will not themselves seem to beat. Lord Kelvin pointed out that the beats of an imperfectly tuned chord, *e. g.*, 3 : 4 : 5, could sometimes be heard after the primary tones themselves had become inaudible.[2] This beating has been

[1] See my 'Combination Tones and Other Related Auditory Phenomena,' PSYCHOLOGICAL REVIEW SUPPLEMENT, No. 39, p. 73.

[2] *Proc. Royal Soc. of Edin.*, IX., 1878, p. 602.

shown to be due to interfering difference tones.[1] Thus wholly inaudible combination tones may make the generators seem to beat very perceptibly. However we may finally explain dissonance, it does not at all seem necessary now to assume any direct physiological interference of the primary tones of wide, beating intervals. Wundt, who has held to the Ohm's law formulation, has explained certain beats of wide intervals as due to direct stimulation by both primary tones of all the fibers of the auditory nerve. Other writers, as Cross and Goodwin, have attributed them in part to some similar condition produced in the cortex itself. Such suppositions may prove to be useful in some instances and consistent with facts showing that one can experience phase-differences in vibrations falling upon the two ears and that one thereby locates the source of the sound on the side of the advance phase; they do not yet, however, seem to be necessary.

No consistent physiological explanation has yet been given of how the ear could, on the resonance principle, experience *un*analytically the general form of the complex physical wave. Ebbinghaus recognized that such a view contradicts that of the specific energies of the nerve fibers and therefore gave up specific energy and made his well-known modification of the Helmholtzian theory. One may be surprised, however, that scientific men have taken Ebbinghaus seriously at all. To explain how beats may be experienced directly from the form of the objective complex wave Ebbinghaus assumes that the fibers do not have a high degree of elasticity and that they can therefore continue their movements no considerable time after the cessation of the objective impulse.[2] Such fibers he assures us will take up vibrations that are not strictly pendular form (336). On the other hand, and in direct opposition to this, his second assumption, he makes another, 'not without a certain feeling of reserve' (339 ff.)! This third assumption is his well-known doctrine of partial vibration of these same 'inelastic fibers' by means of the formation on them of nodal

[1] Peterson, *op. cit.*, pp. 111 ff.
[2] Grundzüge, 1905, 336–338.

points. Not inferquently does he assume partial vibrations of a high degree. This assumption is to account for the beating of wide intervals and the production of difference tones, when these beats become rapid enough. It also serves him in a rather ingenious explanation of tonal fusions, on the basis of coincident partial vibration of certain fibers. To make plausible this view of partial vibration of so small fibers Ebbinghaus points out that the heavily laden fibers of the contra octave of the piano may give forth 'on the slightest impact their twelfth, yea even their fourteenth, partial tones as a most splendid after-klang of the tone' (340). The contradiction in these assumptions, first of a low degree of elasticity and then of a high degree, is so obvious and so serious as entirely to discredit Ebbinghaus' modification of the Ohm-Helmholtzian theory.

It should be said here that Ebbinghaus' so-called intertone (Zwischenton), which he could not explain on the Helmholtzian theory, does not require for its explanation so extreme a modification of the specific energy resonance hypothesis as he supposed. I do not myself experience anything that can really be called a tone, without greatly extending the meaning of the term. All other experimentalists whom I have consulted on this matter agree with me on this point. I hear rather a beating mass involving more or less clearly the primaries, according to the extent of separation of the latter, and seeming to cover the space, so to speak, between them. If either of the primaries is suddenly stopped the beating complex at once makes a jump to the exact pitch and distinctness of the remaining tone.

Subjective combination tones are now with much plausibility attributed to periodicities arising in the liquids of the inner ear *directly from the primaries* on the principle of superposition of their vibrations, an explanation not dependent—as is that of K. L. Schaefer—on any asymmetrical vibration of the inner membranes though not denying that those membranes *may* play some part in the production of combination tones. *All combination tones are therefore in their origin probably objective to the sensory end organs of hearing.* This position is strengthened by the establishment of the fact that 'subjective' summation tones actually exist as this explanation requires. In 1907 I proved that such summation tones (some of which were audible) are not difference tones of upper partials as had been maintained by several investigators. This proof rested on counting the beats of summation tones with slightly mistuned auxiliary forks. Krueger and Meyer had already come to the same conclusion on other grounds. On the question of these summation tones, because it was crucial, certain writers had fluctuated not a little.[1]

In connection with vibrations involving phase-changes we find an interesting test of any resonance theory. Early experimenters who supposed that they produced phase-changing vibrations discredited the Helmholtzian view of the resonance hypothesis because they found that they were able to experience such vibrations as tones. Since the reinforcement of such tones by physical resonators has been accomplished this objection has lost its force. In a positive way it has been found[2] that real phase-changing vibrations, when the changes follow closely upon one another, do obliterate the tone. In addition to these facts it has long been known that high tones probably reach their maximum intensity earlier at the beginning of periodic stimulation and certainly decline more rapidly at its close than do low tones. This is all favorable to the view, if it does not actually prove it as Helmholtz urged, that different parts of the ear respond to different periodicities.

Of non-resonance theories that of Hurst assumes specific energies but fails to account for most secondary phenomena of hearing. Emile ter Kuile's theory, also in a measure a place theory, not only fails to explain the origin of most of the subjective combination tones, but is also contradicted by such pathological phenomena as tonal islands. The last-named phenomena, if their existence can be established by careful reports of a few cases thoroughly studied, seem to be fatal to Meyer's theory, the most completely

[1] Peterson, *op. cit.*

[2] *E. g.*, by Exner and Pollak, *Zeitschr. f. Psychol.*, XXXII., 1903, 305–332.

developed probably of all non-resonance theories. The writer has been assured by Dr. G. E. Shambaugh that cases of tonal islands are not infrequent among pathological phenomena of hearing.

It was long ago found, moreover, that destruction of the base of the cochlea, either by pathological conditions or by operation, produces deafness for high tones and that injury to the other end of the cochlea results in deafness for low tones.[1] While these facts, on account of certain difficulties in applying the test, have been questioned, we have nothing to refute them and strong reasons to expect just such results. Recent experiments by Kalischer[2] contradict these facts, but Kalischer's experiments are in many respects unsatisfactory as evidence against specific energies.

Many of the above considerations, as well as others that could be pointed out, seem to require a specific energy theory of hearing. The only reasonably successful specific energy theory thus far put forth is the one based on resonance and strongly advocated by Helmholtz. The Helmholtzian theory as first stated has of course proved inadequate in many respects. The requirements of such a theory now are simpler than formerly, since it has been found that all tones heard—except Ebbinghaus' intertone, if it really can be called a tone—are attributable to periodicities coming to the organ of Corti as objective. Some of these periodicities are secondary, being derived from the primaries, and so occasion the so-called secondary phenomena of hearing. Some such derived periodicities arise externally to the ear and some very probably in the liquids of the cochlea; but they all apparently come about on the principle of superposition of the primary vibrations, above referred to, according to a well-known physical law.[3]

We seem to be at a juncture, then, where physiological theories of hearing have to do, so far as the *existence* of all perceived tones is concerned, principally with the primary phenomena of tonal analysis. If Ewald's view accounts for

[1] See reference in Nagel's *Physiologie des Menschen*, III., 1905, 564–565.

[2] *Arch. f. Physiol.*, 1909, 303–322.

[3] Peterson, *op. cit.*, 103–105.

these phenomena better than does the specific energy hypothesis we should accept it as a better theory. But as yet Ewald has no small degree of difficulty with primary analysis. Meyer's theory obviously has little basis for its existence other than his objection to the assumption of resonators in the ear, if the origin of combination tones suggested above be accepted, and Meyer himself accepts the possibility of such origin.[1] We are in need of some good crucial tests—less doubtful in interpretation than those of Kalischer and of others before him—as to whether tones of different pitch are associated essentially with different and definite sections of cochlear stimulation. A careful post-mortem study of the cochlea of a person who had been deaf to certain tones would be most satisfactory. In the meantime a theory like that of Helmholtz, not taking specific energies too strictly, however, seems to be most helpful.

While it is yet uncertain whether there are structures in the cochlea that can respond sympathetically to tones, the resonance hypothesis has doubtless been strengthened by the demonstration by means of Ewald's *camera acoustica* that a thin rubber membrane .55 mm. by 8.5 mm. suspended in a liquid can be made to respond sympathetically to tones ranging within a compass of six octaves. Ewald of course uses these facts to support his own theory, which, however, does not seem to have any advantages over the specific energy resonance theory, and fails on several points which the latter meets successfully. Ewald's explanation of tonal islands is far from satisfactory; as is also his assumption that pitch is determined by the distance of separation between stationary waves on the membrane. In case of several tones experienced simultaneously so that waves overlap, which distances shall determine the pitch? If a vibration with a strong second partial be given, why should it not be located an octave too high? If the tone is not 'placed' by

[1] He so admitted in his discussion of this paper in the Cleveland meeting. In my paper there I stated that Meyer's theory rests its case on the intensity relations of combination tones and that it has thus far failed to meet the facts on this point as well as the Helmholtzian theory. Professor Meyer, however, informs me that his objection to the assumption of resonators in the ear is really the basis of his theory.

the nearest regular distances between waves, why on hearing a pure tone should one not also hear a tone an octave, or as many octaves as you please, below the given tone? One, as a matter of fact, not only does not but *can* not do this.

We shall now consider three objections to the Helmholtzian theory, objections which have had some weight and some of which we have not yet mentioned in this paper. The first is that based on the intensity relations of combination tones. We shall touch it very briefly. It has been pointed out that combination tones do not frequently have the intensity that we should expect them to have. Hermann pointed out the fact that the tones $c^2 : e^2$ sounding gently give a second difference tone, g^1, which has much greater intensity than the first. He regarded this tone g^1 as belonging to a *second* order, and therefore as being dependent upon the first difference tone. Other critics have proceeded on the same idea. This view, however, is erroneous and inconsistent as I have elsewhere shown.[1] Helmholtz himself at one time assumed various orders of combination tones, in this sense, but Bosanquet pointed out as early as 1881 that this assumption is not necessary.[2] On the principle of superposition of vibrations suggested by Helmholtz in 'Sensations of Tone,' p. 412, all the combination tones spring directly from the primaries, so that the higher orders are not dependent on the lower.[3] The fact that the higher order combination tones may occasionally be more intense than the one of the first order, is not surprising. Nothing could be more natural, since some of these tones occur at a pitch more frequently heard and are therefore mediated by a section of the cochlea more commonly used than that mediating some of the first order combination tones. Again, two or more difference tones frequently coincide and thereby reinforce each other, as Krueger has urged; while some such tones are weakened by others lying below them. Intermediate difference tones are always weak, partly, we should think, because they are obscured by the lower primary tone. When

[1] *Op. cit.*, 23–25.

[2] *Phil. Mag.*, 5th series, XI., 420–436 and 492–506.

[3] It will be noted that Helmholtz suggested several origins of combination tones, some of which are contradictory while others really involve the same principle.

these things are considered the Helmholtzian theory is probably not so unsatisfactory in this regard as has been supposed. While many difficulties of course still remain with respect to this matter of intensity, we should not be unmindful of the difficulties of applying mathematics to matters of this kind. At any rate, no other theory has yet met the intensity difficulties as satisfactorily as the specific energy resonance theory.

The other two points are more simple in their nature, but have given considerable difficulty to the theory under consideration. In 1876 A. M. Mayer reported[1] the fact that tones of considerable intensity, when heard by themselves, may be completely obliterated by lower loud tones; while intense higher tones cannot obliterate lower ones though the latter are very weak. "These results," says Lord Rayleigh, "which are not difficult to verify, involve a serious deduction from the universality of Ohm's law, and must have an important bearing upon other unsettled questions relating to audition."[2]

This objection has for years not been answered satisfactorily by any defender of the specific energy resonance theory. Recently I noticed that the phenomenon is more pronounced for some positions of the sounding forks than for others, when they are held near the ear. In certain relations of the forks the longer vibrations seem to interfere physically with the shorter ones in the outer ear. For occasional positions the higher tone does not disappear at all, though the lower one is very loud. The habit of disregarding upper partials, moreover, seems to make the obliteration more easy and complete in the case of consonant than of dissonant tones. To test whether the obliteration is not due to some sort of physical interference in the middle ear, I applied the stem of the forks to the skull. Under such conditions, it was found, obliteration even of weak tones is very slight or impossible. There can be no doubt that in this case both vibration-series reach either ear in common. If the stem of the higher pitched fork is first applied to the head and then

[1] *Phil. Mag.*, II., 500.

[2] *Theory of Sound*, II., 1896, 445.

that of the lower fork, sounding loudly, when the first has become almost inaudible, one still hears the higher tone very distinctly, and can mark its gradual disappearance. If at this moment the experimenter quickly removes the lower fork, the upper tone does not reappear, showing that its disappearance was really not due to the lower intense tone. The conclusion is inevitable, that the interference of the higher by the lower tone is largely physical, and therefore external to the cochlea. The damping possibly occurs mostly in the ossicles of the middle ear. The phenomenon is consequently not an obstacle to the Helmholtzian theory of tonal analysis.

Our third point relates to the perception of the direction of sounds by means of phase differences, a fact which Lord Rayleigh regards as proof that tonal analysis is central and not peripheral in the sense that Helmholtz and others have assumed. It has unquestionably been established that we locate lateral tones of a period below about 128 vibrations—or *c*—largely on the basis of the phase-differences of the waves impinging upon the two ears, the ear on the near side getting the wave in an advance phase. This phenomenon has been tested under most carefully controlled conditions by Lord Rayleigh and others.[1] By holding, one to each ear, the resonance boxes of two tuning forks which are beating very slowly, so that they alternate in presenting the advance phase of a practically single vibration-series, one finds by a simple method that the sound shifts from side to side, as Lord Rayleigh has reported. The experiment is simple and convincing, though the proof of the perception of phase-differences rests upon tests of a more complex nature. If the tones, in this illustrative experiment, are not too loud, it is impossible with the keenest attention to either box to hear a tone on that side continuously. When the sound has completely left one side, it instantly returns on removal of the fork at the opposite side. When the tones are very loud or of unequal intensity other factors interfere.

The perception of phase-differences does not in the writer's opinion disprove the Helmholtzian resonance hyopthesis.

[1] *Phil. Mag.*, since 1907.

If the elasticity of the resonating structures in the cochlea, whatever they are, is slight, as Helmholtz always maintained, I see no reason why, from some cortical region, note cannot be taken of a slight disparity in phase, so to speak, of the neural impulses from the two ears. Any other theory, even the one favored by Lord Rayleigh—that of Rutherford—if it can be called a theory, must posit an effect in consciousness that is based upon a lack of simultaneity with which the individual impulses from the two ears arrive at some common center.