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PSYCHOLOGICAL ANALYSIS AND THEORY OF HEARING.

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During the last few years rapid additions have been made to our knowledge of auditory sensations, their relations, and their causes. And the natural product of the new facts has come forth in various extensions and modifications of previous theories. One of the most striking features of these is the eagerness with which inspiration is borrowed variation[1]. The only obvious variant in articular sensation is 'position,' which we may take to be its ordinal attribute. Extensity behaves here as in muscular sensation. It is implicit in the varying bulk, mass, or volume of the sensations from the joints, small and large. In organic sensations we find that obscurity and want of variation is fairly general, but with a little willingness all the attributes can easily be identified.

Two psychological problems are raised by the sensations of this group. The first is that of the presence and cause of the obscurity of an attribute. In these cases it may be referred readily enough to the absence of variation in the physiological correlate. Thus the organs of the articular sense are stimulated at all times by forces of practically constant intensity, while the organs of the muscular sense, if there are enough of them in each muscle to form a small system there, are all stimulated at once, not one at one moment and then another.

The second problem is that of compound sensations. In all the senses of this group we have good reason to suppose that many receptors are stimulated at once, as there are many in each muscle, round each joint, and in each proprioceptive mechanism of the body. This fact confirms the psychological conclusion that must be drawn from the mass, bulk, or volume that is inseparable from the corresponding sensations. Apparently we never get in isolation a single 'spot' of articular, muscular, or organic sensation, but only a mass or area of it. Area is familiar to us in cutaneous sensation and there is every reason to suppose that area consists there of a large number of neighbouring minimal (as we get them) sensations, which fuse to a continuous whole in virtue of the extensity that each possesses. There is in area no accentuation or discrimination of orders, unless by means of concomitant differences in the variable attributes of intensity, as also sometimes of quality (vision)[2].

This theory of compound sensations may be extended to apply to the minimal sensation from the 'spot' of cutaneous sense, which may well be supposed to be only minimal for us, because the units of the receptor organs or their neural attachments make a smaller particulation of sensation impossible, and not because the 'spot' sensation does not really consist of still smaller psychological particles fusing into a tiny area in which no orders can be discriminated[3].

[1] Cf. This *Journal*, 1911, IV. 159.

[2] Cf. my paper on "The psychology of visual motion," This *Journal*, 1912, V. 32 ff.

[3] Cf. the psychological theory of intensity offered by F. Brentano, *Untersuchungen zur Sinnespsychologie*, Leipzig, 1907, 53 ff.

The third group of senses raises all the difficulties and problems already mentioned and some new ones. The senses included are vision, hearing and smell. We find in the matter of attributes special cases of obscurity, difficulty, and complexity. The obscurity of intensity in vision raises a general problem regarding the primacy of intensity as an attribute and regarding its place amongst experiences, if it be secondary or derived[1]. The manifold and continuous variation of colour quality, as surveyed in the colour figure, raises anew the problem of compound sensations. Both of these appear again in the study of auditory sensations, which adds as a third the problem of a non-spatial, systemic order and continuity. In this paper I shall confine my attention to the problems of hearing.

II. *Views concerning the quality of sounds.*

The feature of auditory sensation which till very lately has been generally classed as quality is pitch. As it varies continuously from lowest to highest tones, the number of qualities equals the number of distinguishable pitches. The smallest number of primary qualities to which this series of continuous variations can be reduced is two, of which each occupies one end of the phenomenal series. Such a reduction which treats the series of tones as the analogue of the series of visual brightnesses, was suggested by Mach[2]. But it is rejected by Stumpf and is not generally admitted. Each distinguishable tone is rather considered to be a simple elementary sensation, requiring, as in Helmholtz's view, a specific sense-organ. The number of these is therefore very great. Ewald has pointed out that the assumption of this correlation between a vast series of qualities and a vast series of sense-organs puts an enormous strain upon our conception of the biological evolution of hearing[3].

Brentano[4] thought to remedy the deficiency of Mach's theory, while retaining its postulate of tonal primaries, by extending the analogy with vision. He therefore recognised in hearing a series of 'saturated elements'—the tones that lie within an octave. The repetition of octaves

[1] Cf. F. Brentano, *op. cit.*, and the physiological theory of intensity proposed by C. S. Myers, This *Journal*, 1913, VI. 137 ff.

[2] *Beiträge zur Analyse der Empfindungen*, Jena, 1886, 122. In this edition Mach uses the analogy of the series leading from red to yellow. This is in principle the same as the series leading from black to white.

[3] J. R. Ewald, *Arch. f. d. ges. Physiol.* 1899, LXXVI. 155.

[4] *Op. cit.* 101 ff.

was to be explained by reference to variation of the admixture of the brightness components of sound. But this theory, like Mach's, could not hope to be accepted on its own merits. For, although it explains various matters well enough, as any such theory may, it does not add enough to our insight and to our knowledge to make itself compelling, and it is not founded upon a mass of observations, as is that lately propounded by Révész and von Liebermann[1]. Their theory is practically identical with that of Brentano, except that what he calls difference of brightness, they call difference of *Höhe*[2]. But the observations made by von Liebermann on his own auditory sensations, as modified by a chronic paracusis, allow these authors to go far beyond the range of Brentano's views and make their theory much more convincing. For they have established the independent variation and recognition of these two aspects of tone. Their observations seem to me to be thoroughly consistent with all the known phenomena of hearing and therefore *prima facie* correct. Only their classification and theory of these aspects of tone appear to me to be impeachable and misleading. Their distinction explains much, of course; but as its basis in fact seems to be correct, proper classification and proper deductions therefrom should enable their theory to explain still more.

It is to be noted that Max Meyer[3], whom Révész also quotes[4], distinguishes the same two aspects of tones, but he calls them by reverse names. Révész's quality is his pitch and Révész's height is his quality.

In the hands of Köhler[5] and more especially of Jaensch[6], Stumpf's generally accepted views have taken a different line of development. The chief influence here has been the observation of the resemblance between pure tones and vowels and also in the case of Jaensch consideration of the nature of the relations between the stimuli of tones, vowels and noises. The pure vowel sounds are for Köhler the sole qualities of hearing. For Jaensch they are only the qualities of the sense of noise. He recurs to a modified form of the analogy between vision and hearing in identifying vowels and brightnesses as less differentiated sub-senses. The stimulus for the former is a rate of

[1] Cf. the works quoted below under their names.

[2] This is the ordinary German word for our 'pitch'; for the sake of clearness, however, I shall translate it by its wider meaning—'height.'

[3] "On the attributes of sensations," *Psychol. Rev.* IX. 83, esp. 95 ff.

[4] *Zur Grundlegung der Tonpsychologie*, Leipzig, 1913, 42.

[5] *Ztsch. f. Psychol.* 1910, LIV. 241 ff. and 1911, LVIII. 59 ff.

[6] *Ztsch. f. Sinnesphysiol.* 1913, XLVII. 219 ff.

vibration which varies irregularly but not too extensively about a certain average, while the stimulus for the latter, although not necessarily irregular, is at least in the normal, light-adapted eye usually provided by certain pairs of lights or any mixture of these pairs. The more differentiated and presumably later developments—tone and positive colour—are evoked by steady rates of vibration and are likewise to be identified. The relation between tones and vowels, like those between colours and brightnesses, varies, sometimes being closer than at other times. Thus, presumably, we should have to admit a whole spectrum of qualities in the tones, and a series of qualities in the vowels—as it were octaves of brightness. Why there are different qualities in this series is not explained. And as Jaensch distinguishes vowels from noises, I can see no reason why we should not admit three sub-senses in hearing instead of merely two[1].

The analysis into sub-senses is hardly attempted at all by Révész[2]. The only indication of it we get is the suggestion that the height of tones would need two psychophysical processes, while quality, as the evidence of binaural mixture of small pitch differences seems to show[3], would also probably require two. If we add to these the differences of vocality which Révész recognises[4], still more processes will be required and we shall quickly exhaust the resources suggested by even the most complex of visual theories.

The enormous influence of the analogy with vision upon these theories is obvious. I shall now proceed to summarize the facts and observations included in them and to interpret them according to my own theory of the attributes of sound stated above. It will then be evident which of all theories is the more systematic, that is free of difficulties and full of promise.

[1] Cf. *op. cit.* 240, 255, "Weichen die Schwingungszahlen der einzelnen Elemente eines Kurvenzuges allzu stark von einander ab, so wird aus dem Vokal ein Geräusch....Das undifferenzierte Geräusch ist somit die eine Klippe, welche bei der Herstellung eines Vokales vermieden werden muss; die andere Klippe ist der Ton." If there are only two cliffs, there must be a valley between them, namely vowels. Or are there really three cliffs?

[2] *Zur Grundlegung der Tonpsychologie*, 41 ff.

[3] *Nachr. d. Gesell. Wiss. Göttingen*, Math.-phys. Kl. 1912, 676 ff.

[4] *Zur Grundlegung der Tonpsychologie*, 89, "Es gehört eben jeder Schwingungszahl eine Qualität, eine Höhe, und eine Vokale zu."

III. *The relations of tones, vowels, and noises.*

There are pure tones and there are compound tones. The latter consist of many simultaneous tones which harmonize more or less with one another and with their fundamental component. If a tone does not last longer than the time of two vibrations, it is heard as a noise. Many tones of neighbouring pitch or generally inharmonic in relationship, sounded together, form a noise. The pitch of single noises is not very evident. But many noises contain distinguishable tones, and if noises are sounded one after another, their pitch becomes easily noticeable. Thus we obtain the propositions: (what is presumably) tone is sometimes heard as noise; some noises consist of (what are presumably) tones; some noises contain tones. The net result of these is the proposition: many (phenomenal) noises are, or consist of, (real) tones. In reliance upon this, it has been usually inferred that all noises consist of tones or are tones of very indefinite pitch or are not yet tones, so that there is no special sense of noise[1]. This conclusion seemed to be confirmed by the rather vague and conflicting statements made about the tonal nature of vowel sounds. The synthesis of sounds by experimental means did not admit of a direct examination of the proposition: are there any unanalyzable noises of indefinite pitch?

This experimental question has been answered by Jaensch by the use of a selenium cell placed in the circuit of a telephone and illuminated by an arc lamp whose light was varied by the revolution of an obstructing disc. The edge of this disc was cut out so that the variations in the length of its radius corresponded with the variations in height of any desired vibratory curve, pendular, periodic, or irregular. The results of these experiments are most acceptable. A constant rate of vibration produces a tone. The same average rate of vibration produces a vowel-like tone, if the mean variation from the average is still small. As the mean variation increases, the sound passes gradually into a vowel, then it takes on a noisy character, and when the mean variation is great enough, it may finally appear as pure noise. The average rates of vibration of the vowels *m, u, o, a, e, i*, and *s* are approximately octaves of one another[2]. These vowels do resemble certain tones, as Köhler maintains[3]. But they are not to be identified with them, as he proposes. The resemblance is close only between the lowest and highest

[1] Cf. C. S. Myers, *Textbook of Experimental Psychology*, 2nd ed., Cambridge, 1911 25 f.

[2] Jaensch, *op. cit.* 234 ff.

[3] *Op. cit.* esp. LVIII. 91 ff.

vowels and the corresponding tones; in the middle of the scale, from below the vowel *o* to above the vowel *e*, tone and vowel are easily distinguishable. Jaensch, therefore, ascribes vowels to a separate and older sense of noise, of which he supposes them to be primary qualities. Average rates of vibration that are greater than that of any vowel and less than that of the next higher one, form a series of mixed vowel sounds, which show a decreasing resemblance to the lower vowel and an increasing resemblance to the next higher vowel, as the average pitch rises. The changes from pure vowel to pure vowel thus obtained are parallel with the changes encountered as we pass from red to yellow, etc.[1]

Two statements of great psychological importance are involved in these views: (1) that hearing contains two psychologically independent sub-senses—tone and noise; (2) that *u*, *o*, *a*, and the rest are pure vowels, forming a series of qualities in the sense of G. E. Müller[2].

The following objections have to be urged against the distinction of two sub-senses. It is supported by nothing more than analogy, and, at its best, that analogy is the analogy of stimuli, not of experiences. The stimuli of vowels and noises are irregular, those of tones are regular; the stimuli of colours are regular, those of neutral greys irregular. But it is to be noted that, while the former vary round an average, the latter go in pairs—those of the complementary colours. It is known that the sub-senses of vision exist independently; but there is no evidence that the sense of noise can exist without the sense of tone. And if there were such evidence, it would not be clear of ambiguity; for noises are not only, *ex hypothesi*, excited by tones, as brightnesses are excited by colours, but noises, when given, *ex hypothesi*, alone, resemble tones. It is true, as Jaensch propounds[3], that each positive colour has an affinity to a neutral brightness, but it is not true, as his diagram suggests, that each neutral brightness has a resemblance to a colour, *quâ* colour; whereas each tone resembles (or according to Köhler is) a vowel, and on Jaensch's analogy must resemble it, because it excites it, and also each vowel resembles (or according to Köhler is) a tone or has the pitch of a tone, as Jaensch[4] has shown experimentally, and as all those who have attempted to find the component tones of vowels have observed. Moreover increase of intensity of light modifies a colour in

[1] Köhler, *op. cit.* LVIII. 99; Jaensch, *op. cit.* 258 f.

[2] *I.e.* "Eine Reihe von Empfindungen, in welcher sich die Qualität geradläufig [d. h. in konstanter Richtung vor sich gehend] und stetig ändert," *Ztsch. f. Psychol.* 1896, x. 33 ff.

[3] *Op. cit.* 264 ff.

[4] *Op. cit.* 288.

the direction of greater neutral brightness, but increase of the intensity of sound does not bring a pure tone nearer to either vowel or noise. The sound-figure given by Jaensch[1] should be made tri-dimensional to suit the double parallel between the similarity of low and high tones to vowels and the varying similarity of different hues of different brightness to neutral greys, just as the tri-dimensional colour figure does. But this cannot be done for tone. The analogy of vision and hearing, tempting though it be, is both incomplete and misleading.

In any case, what sort of statements do the facts precisely warrant? The stimuli used show that, as we pass from tone through vowel to noise, the average rate of vibration remains constant, but its mean variation increases; in other words, the pitch wobbles; not markedly and noticeably, but none the less truly. We become less able to indicate the pitch by singing, or by naming it. But we can approximately find it (with a little circumspection and comparison) the more easily, the more it is isolated from accompanying (not from successive) tones, vowels or noises[2]. I see no reason to depart from the substance of the formulations regarding the relations of tones and noises referred to above. A noise may then be said to be a simple sound whose pitch is not yet audible or a complex sound of many pitches which make each other indistinguishable to the unaided attention. Or, to put it more briefly: tones, vowels, and noises vary from one another in respect of (decreasing) definiteness of their predominant pitch. Later on we shall see more how this statement can be founded and confirmed by both physiological and psychological theory.

The following objections have to be urged against the view that the series of vowels *m, u, o, a, e, i, s* and *ch* form a series of qualities in the sense of G. E. Müller.

It has long been an accepted view that the tones within the octave are not a mixture of the two end-tones of the octave. If the octave relationship and such others are ignored or suppressed, as for example by running in a single chromatic series through several octaves, all the tones passed seem to form part of a single series. If we suppressed the connexion between the octaves, as it holds in the slightly indefinite pitches of vowels, by somewhat similar means, should we not seem to pass through a single series of vowel sounds, in which there are no

[1] *Op. cit.* 265.

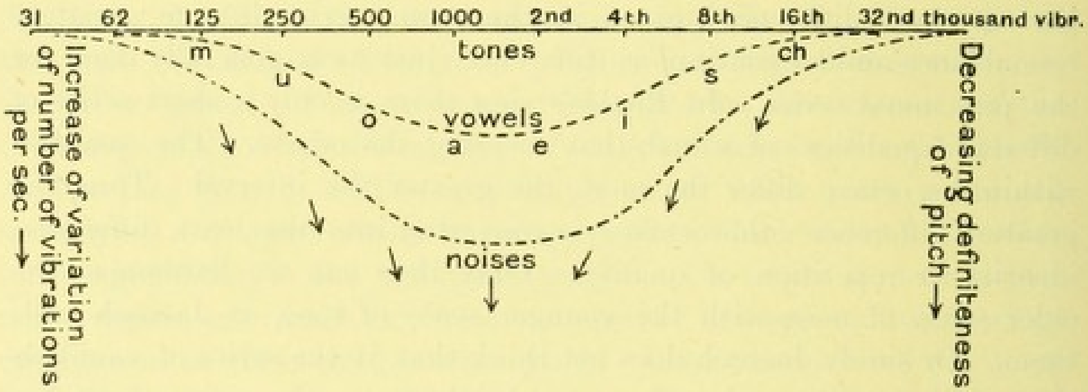
[2] Cf. Jaensch, *op. cit.* 288. A vowel produced apart from a fundamental tone has a pitch. "Die Tonhöhe erscheint hierbei im allgemeinen um so weniger deutlich, je grösser die mittlere Variation der Schwingungszahlen ist."

turning points? Amongst pure noises, the existence of which, as we have seen, Jaensch, like everyone else, must admit, no octave connexion of any kind is to be found. How then does it come to be present in the parallel series formed in the course of his experiments—pure tones, tone-vowels, and vowels, if not simply in virtue of the character which the stimuli suggest—the more or less precise average value of the vibratory frequency, the more or less definite pitch of the tones and vowels? The purity of the members of the vowel series would therefore by presumption rest upon the general octave relationship of sounds and not upon changes of quality in Müller's sense.

If the series of vowels consists of greatest differences whose stimuli differ by approximately an octave, we must not forget that the series of tones also contains an octave relationship of the greatest resemblance. It is not easy to reconcile this opposition, unless by the conversion of the 'greatest difference' given in the vowel series into a 'greatest resemblance amidst continuous difference,' just as is generally done for the pure tonal series. In Révész's view there is only a short series of different 'qualities' of sound, that covering the octave. The qualities within the octave differ the more, the greater the interval. Thus the greatest difference—the octave—is converted into the least difference, identity or repetition of quality. Only thus can we harmonize the older sense of noise with the younger sense of tone, as Jaensch calls them. Or surely Jaensch does not think that, if the series of vowels is the parallel to the series of neutral brightnesses, the series of pitches can be the parallel to the series of positive colours in any psycho-physical sense? For if it were, we should have to see in the series of pitches a uni-dimensional series of qualities; or else we should have to split the bi-dimensional or tri-dimensional manifold into several uni-dimensional series, as is done in vision. In either case it would be difficult to find pure primary qualities. Even for Révész the method he suggests of deriving all the inter-octave qualities from two qualities is excluded; for his end qualities are greatest similars as well as greatest differences. If he wishes for greatest differences, he must seek them in tones that are just short of an octave apart. But to admit these as greatest differences would compel him to suppose that the greatest differences of all are next neighbours in the tonal series. These minutest of differences do not, of course, act as greatest differences; a slight mistuning of the octave is hardly noticeable. And then again no tones in any part of the octave give themselves out to be purer than any others.

If the vowels of the series are to be taken as greatest differences, the fact that their pitch, in spite of its indefiniteness, is fixed round about an absolute point, becomes important. For the octave relation seems to hold only between the pure vowels, not between impure vowels which differ by approximately an octave. If this be so, it becomes difficult to see why the octave relation should be perfectly relative amongst tones, as it is. To postulate different senses, as Jaensch does, is too easy a way out of the difficulty. For both vowels and noises have a pitch, indefinite though it be.

We must therefore conclude that the series of vowels is a single linear series. If so, it is hardly necessary to bring the relations between tones, vowels, and noises into schematic form. But it might be done thus:



The only variant is pitch; it is higher or lower, and it is more or less definite. We have not far to seek for an analogous case of variation in definiteness of order, in the specific form of localisation; it is found in the differences between the epicritic, the protopathic, and the deep systems of cutaneous sensations. Besides a very acceptable physiological basis for increasing indefiniteness can be suggested in hearing.

The only question which remains is to explain why the vowels form a series of absolute octaves. When there is no known reason, any theory formed solely to explain this fact, as Jaensch's and Köhler's are, is as good as any other. Whether we explain octaves by the use of *ad hoc* qualities or not, we still have to explain why in pure tones the octaves are thoroughly relative, whereas for vowels they start from an absolute basis. If this absolute basis be denied, the whole distinction of pure vowels collapses. It is therefore quite as good a suggestion to suppose that the mouth for some reason or other chooses to form such a cavity for some one vowel that it gives a certain average tone, and that the other vowel sounds are chosen, owing to the otherwise and already existing octave relationship, in relation to this primary vowel.

Our conclusion, then, thus far would be that there are in sounds differences of pitch of greater or less definiteness; that the only qualities involved in these differences are those of pitch, if these can be classed as qualities, or, if they are classed otherwise, as I believe they must be, that there are no differences of quality in sound at all; that there is only one auditory quality, namely sound.

IV. *The distinction of two aspects of tone within pitch.*

Various distinctions of this kind have been made. The most familiar recognises the variation of volume or voluminousness which accompanies change of pitch[1]. I think this distinction is also the most correct, and I have therefore adopted it[2]. Low tones are bulky and massive, high tones are thin, sharp and wiry. Sound offers in this respect a parallel to the bulk and volume of the second class of sensations, the obscure group. And we have good reason to extend to them the theory of compound sensations which applies to the articular, muscular, and organic sensations[3].

Max Meyer and Révész also distinguish two aspects within pitch. The former calls them pitch (corresponding to the place in the musical scale) and quality (Révész's 'height'). Révész calls the differences of pitch within the octave 'quality' and an accompanying distinguishable difference he calls 'height.' I accept the distinction intended without the least hesitation, but it seems to me more correct[4] to class the differences intended as differences of 'pitch' and of 'volume.' Pitch, as I mean it, is Révész's quality, such differences as are given within the octave and are named by the letters *c*, *d*, *e*, *f*, etc. But I would not confine the range of these differences to those of a single octave, supposing, as Révész does, that the most similar qualities of different octaves are identical; I would recognise a regular variation of pitch from lowest to highest tones. Volume, as I intend it, is the difference intended by Révész's *Höhe* (height), a difference which changes with pitch throughout the whole range of tones, and which can be separately distinguished from pitch or order, and which can be compared with other degrees of volume. Révész and v. Liebermann have added very much to our knowledge of these properties of tones by

[1] Cf. Stumpf, *op. cit.* I. 207 ff., II. 56 ff., 537 ff.

[2] This *Journal*, 1911, IV. 143 ff.

[3] Cf. This *Journal*, 1913, VI. 242 f.

[4] For reasons see This *Journal*, 1911, IV. 143 ff.

showing in such detail that pitch and volume (or, as they say, quality and height), are separately variable, to some extent in normal cases, and so completely in such an unusual case as v. Liebermann's. But I do not think that their theory of the limitation of qualitative differences to those that lie within the compass of an octave, contributes towards the explanation of the octave recurrences of tones. It introduces more difficulties than it removes. It is possible and it is better to place the octave recurrences as a special feature of tone to be explained by reference to the nature of the smallest complexes in which tonal sensations are given to us. To establish this view it will be necessary to make a somewhat detailed study of the facts reported by v. Liebermann and Révész[1].

(*a*) *The hearing of single tones.* These were heard from a certain point of the scale onwards as a more or less constant pseudotone. The nature of the disturbance varied from time to time, often during one day, and sometimes even during a single observation, then giving rise to a gliding tone. But the main features of the disturbance were constant enough. As I take it, the disturbance consisted in some abnormal process affecting the basilar membrane or the contiguous parts involved in the reception of sounds; in its milder forms this made certain points or short extents within a larger extent of the receptors more sensitive to stimulation than the other parts of this extent; at the same time the effect produced upon the receptor itself was less intense than usual. In its severer forms a part of this region might be totally unreceptive. An analysis of the table given by von Liebermann and Révész[2] shows the following state. In general the region from about e^2 to $d\sharp^4$ or f^4 is affected. On 20th and 27th April, 1907, there is in that region only one sensitive area, $g\sharp^3$ (as such and *quâ* g^2, g^4), with sometimes a semitone higher and lower owing to assimilation or the halving of differences. On 17th October there are two almost equally sensitive points, $f\sharp^3$ and c^4 (also *quâ* f^2 and $f\sharp^3$, or c^3 and c^4); of these f is affected by tones above f and below c in scale, while other tones

[1] The papers by these authors, Paul von Liebermann and Géza Révész, are: (1) "Ueber Orthosymphonie," *Ztsch. f. Psychol.* 1908, XLVIII. 259 ff.; (2) "Experimentelle Beiträge zur Orthosymphonie und zum Falschhören," *ibid.* 1912, LXIII. 286 ff.; (3) "Ueber eine besondere Form des Falschhörens in tiefen Lagen," *ibid.* 325 ff.; (4) "Ueber binaurale Tonmischung," *Nachr. d. Gesell. Wiss. Göttingen*, Math.-phys. Kl. 1912, 676 ff. These will be cited in the following pages as L. and R. 1, 2, 3, 4. Under the name of Géza Révész alone there have appeared: (1) "Nachweis, dass in der sog. Tonhöhe zwei voneinander unabhängige Eigenschaften zu unterscheiden sind," *Nachr. d. Gesell. Wiss. Göttingen*, Math.-phys. Kl. 1912, 247 ff.; (2) *Zur Grundlegung der Tonpsychologie*, Leipzig, 1913.

[2] L. and R. 1, 263.

affect c; the region from $c\sharp^4$ to e^4 was dead. On October 29th the more sensitive point was g^3 (also *quâ* g^2, g^4), with some assimilation to a^3; c^4 was also sensitive. The same holds true of November 5th, only the dead region is now slightly alive. In these last thrice there is assimilation to tones lower than the most sensitive one only if the assimilating tone is on the lower edge of the affected area. The paracoustical region is not, as von Liebermann and Révész suggest[1], broken by normal points, but only by points where the objective tone and the pseudotone coincide. The hypothesis of greater sensitivity fully explains the normality.

At a later date (1911) the state of the ear was of the same character, the most sensitive point was about $g\sharp$, but the area around it down to e and up to b was also sensitive. The extent of this sensitive area and the more sensitive points in it fluctuated from time to time. There was also for a time a sensitive $c\sharp$ and d. Generally also tones lower than the lower end affect the low end of this area and higher tones the higher end[2]. This is the usual state of things and seems to occur when there is only one sensitive area with a most sensitive point.

It is important to notice that long-lasting tones, such as those of the violin and the flageolet, and also intense tones produce correct tones, while the brief tones of the piano produce pseudo-tones[3]. This bears out the idea of sensitive points; a full and proper stimulation is still able to affect the normal point.

(*b*) *Orthosymphony*. Here we have a suggestion for a theory of the phenomena called by von Liebermann and Révész 'orthosymphony,' *i.e.* that when two tones were sounded at once[4], the interval was always correctly judged. To get the correct interval, however, it was necessary to take the chord as a whole without analysis. If the observer listened to get the single tones, he heard them as pseudotones. von Liebermann and Révész therefore supposed at first that both pseudotones were then really given. But later they believed and argued that this was not so; for "in dem Augenblick, wo das Heraushören gelang, wurde die dem richtigen Intervall eigentümliche Konsonanz nicht im geringsten verändert; die Verschmelzungsstufe blieb dieselbe, hinsichtlich des Intervallurteiles trat aber Verwirrung ein, da die Vp. naturgemäss nicht im stande war, ein Urteil zu geben über ein Intervall, das bei der Zerlegung

[1] L. and R. 1, 265. [2] L. and R. 2, 308 (table). [3] L. and R. 1, 264, 274.

[4] Or successively (arpeggios), *i.e.* physiologically and psychologically overlapping and therefore partially simultaneous, see L. and R. 2, 298. Also in melodies with accompaniments, *op. cit.* 299.

andere Komponenten lieferte, als nach dem Gesamteindruck zu erwarten war[1]." But these and all the other statements of the paper only show that the degree of fusion, not the pitches of the fusing tones, remained constant[2]. In fact everything points to the idea that in the *Gesamteindruck* other pitches were there than in the analysed impression or in the single tone. If one maintains that in the *Gesamteindruck* the false tones, though not heard, were really there, one might as well assert that when an intense tone or a continuous tone of the violin or flageolet evokes a correct tone, the pseudotone is really there, though it seems to be absent[3]. There can be no doubt that the pseudotone appeared in chords only when it was supported by the attention[4], but that synthetically observed chords and intense and continuous tones, unsupported by the attention, were heard true. The abnormal sensitivity of the ear suppressed the normal sensitivity only when the latter was unsupported or briefly stimulated[5].

(*c*) *Theory of the preceding.* It is not true that according to Ewald's theory or to any other we should have to say: the total impression is independent of the form of the sound picture[6] or of the actual effect of the stimulus on the receiving bodies; but we should have to qualify these theories by an addition to the effect that the *pitch* of a tone does not conform solely to the conditions laid down by the stimulus, either for single tones or for chords; doubtless it bears, as *e.g.* in Helmholtz's theory[7], a specially close relation to the stimulative conditions; but this relation need not always be unmodifiable and final; the general expression, which includes it, will rather refer to the point of the basilar membrane that is stimulated most effectively, whether because of its own greater sensitivity, or because of the greater intensity of the stimulus, or because of the mutual support of several stimuli

[1] L. and R. 1, 270.

[2] Cf. L. and R. 2, 298.

[3] From a certain aspect of the theory which I shall later develop, both of these statements are true of the real components of tone, but for the present and from the phenomenal aspect both are surely false.

[4] Cf. L. and R. 1, 270. Pseudotone given first and attended to kept its pitch even when a second tone was sounded.

[5] Certain familiar facts of touch form a curious parallel to orthosymphony. In certain disturbances of the peripheral nerves of touch a single touch is very badly localised, whereas simultaneous points are discriminated from one another better than usual.

[6] L. and R. 1, 271.

[7] Helmholtz, *Sensations of Tone*, transl. A. J. Ellis, 3rd ed., London, 1895, 144, fig. 52. Cf. also the extensions suggested by A. A. Gray, *J. of Laryngology, Rhinol. and Otology*, 1905, xx. No. 6.

acting against the effects of greater sensitivity[1], or because of the heightened sensitivity that attention gives to an already abnormally sensitive point, acting against the effect of mutually supporting stimuli. These various forces, which sometimes concur and sometimes oppose one another, suffice to explain all occasional exceptions to orthosymphony, *e.g.* that melodies given with accompaniments are not affected by the attention[2], and others.

Such a modification retains for the theory of hearing what is for very many reasons absolutely indispensable, namely a basis for the modification of analysis by means of the attention[3]. The pitch of a tone thus depends on the point of the basilar membrane that is for any or all reasons most effectively stimulated. The fusion of tones and the varying volumes of them upon which fusion rests, must therefore depend on the extents of the basilar membrane that are stimulated[4]. It is perfectly consonant with this view that the occurrence of beats is dependent, as in normal hearing, on the objective pitch, *i.e.* on the way in which the basilar membrane is affected by the stimuli, apart from greater sensitivity, attention, etc.

On the other hand the theory of illusion proposed by von Liebermann and Révész is not acceptable. After all an illusion of presentation is only 'illusory' by comparison with the objective stimuli. We cannot suppose that such an illusion belies and covers phenomena as well as realities; for that is what the authors' explanation really means. The impression of correction, they think, is a phenomenon which displaces another phenomenon, which, however, is not phenomenal at the moment. If musical experience can have the associative effects here ascribed to it, why does von Liebermann not hear the correct (imaged) pitch along with the (sensed) pseudotone? We should expect as much from what we know of visual images. And if it be said that tonal images do not mix themselves into tonal sensations, then the whole explanation given

[1] Orthosymphony seems to be better with strong tones. See L. and R. 2, 292, 310. Cf. what is noted above, that intense single tones give no pseudotone.

[2] L. and R. 2, 299.

[3] L. and R. report no gliding tone (*Gleitton*) at the moment in which the pseudotone is heard in the chord by the analytic attention. Nor should we then expect any; on my hypothesis no change takes place on the basilar membrane at this moment. But when the pseudotone changed during a single observation and a *Gleitton* was heard (see L. and R. 2, 306) we may suppose that the basilar membrane actually shifted its point of maximum yield to the stimulus.

[4] A theory of hearing which relies on the statement of the preceding sentence alone must appear to be arbitrary. Why should the effects of all the sub-maximally stimulated resonators be suppressed?

by the authors is invalid. It is therefore best to suppose that in all cases the pitch heard is phenomenally given, not reproduced[1]. This view is supported by the fact that the presence of the pseudotone in a chord seems to depend as much on the intensity of the components as upon attention[2]. So also, of course, the other aspect of tone-volume, or, as von Liebermann and Révész call it, *Höhe*,—is phenomenally given, not reproduced, in spite of the fact that in this unusual case pitch and volume vary independently.

(*d*) *Pitch the more precise basis of judgment.* All the statements made by these writers about the different relations of pitch (quality) and of volume (*Höhe*) to absolute judgments in terms of tonal names and to judgments in terms of names of intervals may be accepted as important additions to our knowledge. Evidently the preciser basis of the arrangement of tones and of the naming of them and of their differences (intervals) is pitch; but differences of volume also offer a basis for these judgments, although it is less efficient and exact. This agrees entirely with my proposal to class pitch with local sign and position as a kind of 'order,' to class distances and tone intervals in one group of experiences and to refer them to a common foundation in differences in the attribute of order[3]. Our judgments regarding tones and intervals are best when based on this attribute of pitch. But even when the pitch is distorted so as to be unreliable, the same judgments can be got from the volumes of tones by abstraction from the order most prominent in that volume. This may be compared with the fact that we can compare lengths of line as such without any comparison or superposition of their points, although the latter method is much the more precise. We have then to suppose that the ear provides us with a single series of orders; pitch is judged by the most prominent order; interval by the 'form[4]' constituted by the prominent orders; volume by the line or mass of orders stimulated at all; pitches and intervals can therefore be compared and fixed to some extent by means of volumes alone.

(*e*) *von Liebermann's deep symphonic pseudotone.* The hypothesis I have indicated can also be applied to the facts observed by von Liebermann[5] relating to the raising of a deep tone, *e.g.* C_1 by a fifth when

1 "Der orthosymphonische Zusammenhang erscheint, wenn der Korrektionseindruck da ist, normal in jeder Beziehung," L. and R. 2, 299. Then it *is* normal.

2 "Auch wenn der Zweiklang unmittelbar nach dem Anschlage orthosymphonisch erscheint, so kommt doch mit fortschreitendem Abklingen der Pseudoton immer mehr zur Geltung," L. and R. 2, 310.

3 This *Journal*, 1911, IV. 143 ff., 172 ff., 179 f.

4 Cf. below, 38 ff.

5 L. and R. 3, 325 ff.

played after its octave C. The affected region extended from E_1 to B_1, for only within this area was displacement difficult or impossible[1]. There the objective stimulus and the abnormal sensitivity of the ear coincided. Or else the region B_2—$D_1\sharp$ was less sensitive than usual. What the physical cause of these peculiar changes of pitch was, it is not easy to imagine. But it is abundantly clear that it was not central, but peripheral. For the phenomenon was not subject to von Liebermann's choice: it was compulsory. The five reasons given in the text[2] for a central basis are: (1) that the true tone appeared on insistence upon it; (2) that the idea of a comparative C turned C_1 into G_1, although only C_1 was sounded; (3) that the disposition of C_1 to become G_1 varied from time to time; (4) that the occurrence of C_1 or G_1 sometimes depended upon the will and attention; and (5) that only the pitch of the tone was affected, not the intensity, timbre, or volume. But these reasons are not cogent. For in the main abnormality shown by this subject, which can hardly be supposed to be of 'central' origin, insistent attention also affected the phenomena in orthosymphony, and the disposition to abnormality changed. The second reason given may be supposed to be due to the effect of attention coming to the support of the greater sensitivity of the ear. Without the help of the attention the normal disposition for single tones is stronger than the abnormal[3]. The physical basis of the abnormality may be to some extent concerned with the effect of the momentary intermingling of the stimulations which takes place when a deep tone follows its octave. But it must remain obscure on any theory. As the phenomena are the opposite of orthosymphony, it might be thought to be due to blunting of one region, not to increase of sensitivity of the other—E_1—B_1.

(*f*) *Binaural mixture.* The hypothesis accepted by me can also be extended to the facts of binaural mixture observed by these writers[4]. It is well known that the ears of many persons, if tested singly, are found to be of different pitch; for the same objective stimulus they render two tones of slightly different pitch. But when both ears are acting at once, no diplacusis occurs[5]. In von Liebermann this pitch difference occurred in a higher degree than usual. Each uniaural tone seemed to stand away from the binaural tone in opposite directions. If the uniaural tones were of equal intensity, the binaural tone was that tone which is normally evoked by the average of the numbers of vibrations which normally evoke such uniaural tones as were heard. If the uniaural

[1] *Op. cit.* 327. [2] *Op. cit.* 331 f. [3] *Op. cit.* 332.
[4] L. and R 4, 676 ff. [5] Cf. Stumpf, *op. cit.* II. 320 ff.

tones were of different intensity, the pitch of the binaural tone approximated correspondingly to the pitch of the stronger tone[1]. Von Liebermann and Révész see in these facts evidence of a mixture of tonal qualities. The comparison is just, provided of course it is correct to classify pitches as tonal qualities. They take pains to explain why tonal mixtures within a single ear do not occur, viz. because uniaural tonal qualities occur in different 'heights.' But if this be a 'reason,' do we not then lose all touch with the analogy of vision? We do indeed; for at the next moment we read that 'heights' are (not, as it were, differences of brightness, as we read elsewhere[2], but) as it were corresponding points of the two ears; so that when the same 'height' occurs in both ears, the qualities mix. And if these observations are evidence of tonal mixture, then the resulting mid-tone must be supposed to resemble the two mixing tones. Then, as Révész suggests elsewhere, the whole variety of tonal 'qualities'—the scale between octaves—will also be reducible to two end qualities. But it has commonly been denied that *d* resembles *c* and *e*, as an orange-red resembles both red and orange.

Theory of its occurrence. A sufficient explanation of these binaural 'mixtures' can be found in a much simpler hypothesis. For it does not involve the admission of uniaural mixtures, and it demands neither the presence of phenomena which do not occur, nor the assumption of phenomenal illusions of presentation. It is agreed that the pitch of a tone is determined by the point of the basilar membrane that is most intensely stimulated. This maximum of stimulation is surrounded on both sides by a region of decreasing intensity of excitation. Now in binaural hearing, as the objective stimulus for both ears is the same, the length of the basilar membrane affected will remain the same, and therefore the volume of the tone in each ear will be the same. We have only to suppose, what is highly probable,—that all the points or pitches of the one basilar membrane are connected with those of the other physiologically, and the phenomena observed by von Liebermann and Révész will immediately follow. For then a superposition of the stimulations, *i.e.* of their intensities, would follow[3]. If the pitch displacement of the two ears is small (in von Liebermann's case it was never greater than a semitone[4]), the resultant order or pitch will

[1] Cf. also Révész, *Zur Grundlegung der Tonpsychologie*, 63. Stumpf noticed something like this, *op. cit.* II. 326 f.

[2] Révész, *Zur Grundlegung der Tonpsychologie*, 41 f.

[3] Cf. L. and R. 4, 680. The binaural tone is stronger than either uniaural tone.

[4] Révész, *Zur Grundlegung der Tonpsychologie*, 64.

naturally fall where there is the greatest resultant stimulation; as the intensity from the other ear increases, the maximum point will move over gradually to the pitch of the other ear. The physiological hypothesis here involved is already familiar in Bernstein's theory[1]. It is, in fact, merely an application of Bernstein's hypothesis to the facts of hearing. But it would not at all follow that a similar mixture should hold for uniaural tones. For there the superposition of intensities is affected by a number of other physical disturbances which give rise to such things as beats[2]. We suppose, that is to say, for uniaural tones that there are really two different stimuli, two rates of vibration, whereas in this binaural case the stimulus remains the same; only the way it affects each ear differs, because each ear differs. It is at least evident that the mixture theory of von Liebermann and Révész does not necessarily apply to the whole series of their qualities at all, and the analogy with vision must thus far break down. Besides their whole theory of the segregation of miscible qualities by means of the attached 'heights' must fall to the ground, unless they can show why 'quality' is segregated with 'height' at all. And the doing of that is excluded by the very use of the category of quality. Similarly Révész cannot show why in von Liebermann certain qualities have dropped out, while others remain, and why different stimuli can excite the sole remaining pitch. Will he recur, like Jaensch, to the assumption of differences of age amongst his qualities? The use of the category of quality blocks all advance here. Nor can Révész explain why certain qualities drop out only in certain octaves. If the lost qualities are really lost, they should be lost altogether.

In certain cases, where perhaps the pitch difference of the two ears is greater and where one ear has been mistuned in an irregular way[3],

[1] Cf. in W. Nagel's *Handbuch der Physiol. des Menschen*, T. Thunberg, "Physiol. der Druck-, Temp.- u. Schmerzempf." 1905, 720 ff. Cf. A. A. Gray, *op. cit.*, who refers to the discrimination of tactual points for an analogy to the discrimination of tones on the single basilar membrane.

[2] But under certain circumstances a better mixture can be got than results from the simultaneous action of two neighbouring tones, viz. by 'mixing' many neighbouring tones. Cf. S. Baley, *Ztsch. f. Psychol.* 1913, LXVII. 271 ff. It need hardly be said that Baley's results do not favour Helmholtz's theory as against Ewald's or mine.

[3] I have recently had occasion to observe this in myself in degrees varying from a semi-tone of difference to a just noticeable deterioration of the timbre of sounds. In the former case the flat pseudotone is located in or opposite the left ear and it has an unpleasantly metallic timbre; melodies and voices of the appropriate pitch are heard double, but the false ones are devoid of any proper tonality. In the latter case tones that are usually full and round sound slightly flat and rather metallic.

both tones are heard at once, as if they were presented to a single ear, except that no beats occur[1]. A well trained observer will then be able to attend to the tones of each ear separately. Without this attention the general effect will still be that of a great dissonance, since the superposition of the maxima of the two ears will produce a resultant containing either two maxima or a line of maximal stimulation, *i.e.* the effect produced in one ear when dissonances arise from the simultaneous occurrence of neighbouring tones. At the same time the volumes of the two tones will not quite coincide, whereas in orthosymphony they do, thus giving a degree of fusion that is not consistent with the pseudo-tonal components that appear upon attentive analysis. There should be rather narrow limits to the possibility of cases like that of von Liebermann, where there is an even passage from the maximum of one ear to that of the other. A change in the resonators of one ear, whereby they are lengthened or shortened and so respond maximally to lower or higher tones than usual, forms a sort of auditory squint.

(*g*) *Vocality.* Révész attempts to show that vocality is a third aspect of sound alongside quality and height[2]. His chief argument is the case of N. N., who could only hear as far as e^2, and yet could recognise the vowel *a* and even *e*, *i.e.* (according to Jaensch) average rates of vibration of 1000 (c^3) and 2000 (c^4) per sec. If this observation is correct as it stands, vocality would become not only an aspect of sound, but a component of it, a kind of subsense. The observation must therefore be treated with great reserve. Besides an average rate of vibration with a fair variation might very well be heard when no tone could be heard. In the same way, we hear vowels without being able to sing their pitch, especially when they are given on a fundamental tone, as the vowels were given to N. N. (on c^0). A vowel is a complex and irregular impression which might well be effective when a regular impression was ineffective.

(*h*) *Interval.* Révész has devoted a considerable part of his book —*Zur Grundlegung der Tonpsychologie*—to a discussion of interval. Interval, like the pitch of a tone, may be judged either promptly or by slow laborious effort. The former is based in both cases on differences of quality, or, as I call it, pitch; the other rests upon the vaguer *Höhe* or volume[3]. The former Révész calls interval, the latter distance.

[1] Cf. Stumpf, *op. cit.* II. 460; L. and R. 1, 269, 274.

[2] *Zur Grundlegung der Tonpsychologie*, 84 ff.

[3] In connexion with this distinction Stumpf's note on the possibility of such a thing is of interest. *Op. cit.* II. 336.

There is, as he shows, a great difference between the intervals formed by successive tones and those formed by simultaneous tones. The latter are much harder to analyse; very often the tones have to be attended to successively. Differences of 'height,' *i.e.* distances, recede very much in chords. And even the pitches of tones and the 'intervals' based on them are not easy to hear then. Spatial symbolism, *e.g.* deeper, higher, is only applicable to successive intervals.

Further theoretical indications. These and other facts mentioned by Révész are immediately explicable on the hypotheses I would retain for the physiology of hearing. For if pitch depends on the more intensely stimulated region of the basilar membrane, it is obvious that two pitches together will immediately offer great difficulties, especially when both are given equally intensely. They will tend to suppress one another or to detract from one another's 'point.' Only if the attention is turned to them successively, supporting first one and making it more intense and then the other, will the analysis be easy[1]. Orthosymphony, even on the illusional theory given by von Liebermann and Révész, may itself be partly accounted for by this means, although the fact that the two tones cooperated and so stimulated the basilar membrane quite as they normally do, is of primary importance. In chords, moreover, the extents of the basilar membrane excited by the components of the total sound wave must often be largely coincident, so that there must be a large amount of identity amongst the orders of the elements of auditory sense of which they consist, and they will therefore in many cases fuse very well. The more exact the coincidence of the parts of the basilar membrane that are stimulated, the more will the maximal region of the higher tone add itself evenly and regularly to the maximal region of the lower, extending it and it may be giving it a slightly different form. Thus tones which fuse according to volume will tend to fuse according to pitch and two tones will then tend to be taken for one. Only when the maximal points of the fusing tones are close together, will they interfere with each other markedly and give a broad line of maximal stimulation with oscillations due to the differences in the number of vibrations per second. They will be heard as increasing dissonances, the nearer they are together and the nearer they come to an even superposition without reaching that entirely. Some chords will therefore be easy to analyse, others will be difficult. But whatever the ease or difficulty of analysis may be, each chord will be a characteristic complex of tonal orders and

[1] Cf. Stumpf's "Mit dem Ohre singen," *op. cit.* II. 291 ff.

so will be recognisable as a unitary whole, or at least distinguishable from other complexes, whatever the stage may be at which the analytical training of the individual has reached. The same hypothesis offers an explanation for the fact that musical people often fail to notice the presence of difference-tones and overtones. They notice generally only what their attention is trained to pick out by the successive analysis of the attention. All the rest goes into the sound picture; it does not remain ineffective; it makes a difference to the total pitch picture; but it does not excite analysis until the attention is turned by special means towards the difference-tones and partials[1]. The facts of the separate discovery of difference-tones, combination-tones, and overtones is an obvious proof of this[2].

This view also explains why in a complex tone or in a single chord the fundamental tone is the most easily apprehended. It forms the greatest extent of stimulation, its maximum is more widely spread, and also more intense as a maximum than that of any other tone in the chord, unless it be a much higher and very intense tone. When successive chords, however, are given as accompaniment to a melody, it is the melody which is noticed most easily, *i.e.* oftenest by musical and unmusical observers alike. For the melody is commonly the part that moves most connectedly and most effectively[3]. This motion draws the attention most to itself, just as it does in vision, where also of stationary points of light the most intense is most noticeable.

As regards the ambiguity of intervals formed by 'qualities' divorced from their usual 'height,' the facts recounted by Révész[4] only confirm the distinction between 'quality' (pitch) and 'height' (volume). Any and every theory of these facts has still to find a reason why the ambiguity of an interval given by hypothetically pure and unattached

[1] Cf. Stumpf, *op. cit.* II. 232.

[2] It must be evident that since the fusion of tones is not a property of the elementary sensation, but only of complexes of two or more sensations, we must give a psychological theory of it first (cf. Stumpf, *op. cit.* II. 211 ff.) and use that to formulate a physiological basis for it. Both these kinds of theory, of course, will refer primarily to the corresponding properties of the most elementary auditory sensations we actually get. These, however, must be analysed into complexes of hypothetically ultimate elements of auditory sensation, which differ from those of other senses only in the matter of quality. The structure of the complexes which form our simplest auditory sensations, must explain all the special peculiarities of sound.

[3] Cf. Stumpf, *op. cit.* II. 337 ff.; also 393, "Bei aufeinanderfolgenden Zusammenklängen macht das Ganze scheinbar die Bewegung der in den grössten Schritten bewegten Stimme mit."

[4] *Zur Grundlegung der Tonpsychologie*, 113 ff.

'qualities,' *e.g.* *c*—*e*, was perhaps oftenest determined to that of a rising third, why it was sometimes unresolved and therefore arbitrary, and why it was sometimes determined to that of a falling minor sixth. Révész's facts nowhere prove the absolute independence of 'quality' and 'height,' but only the relative independence of them[1]. Nor can he raise more than a presumption in favour of identifying the 'qualities' of a tone and its octave; for in spite of their similarity and approximation to identity, there can be no doubt that the uppermost quality of an octave comes next after that of the tone that just precedes it, when we play up the scale; it does not leave the latter aside to begin afresh, as we should have to admit, if we suppose with Révész that the tone *c* and the tone just less than c^1 are maximal differences from which all intervening tones are probably derived by mixture. Besides, Révész's theory of the absolute independence and ambiguity of quality does not do justice to the fact that we can begin an octave on any quality whatsoever. Also if 'qualities' were miscible, we should for that reason have to suppose that the maximal differences within an octave were all those qualities which are separated by a little more than the tritone interval[2].

Moreover the facts suffice in no way to show, as Révész maintains[3], that an interval has a direction only because it is normally, *i.e.* habitually, associated with a difference of 'height.' Here we encounter again the misleading influence of the analogy with vision. If 'height' has direction, it must also include some basis of 'position'; by what means then is Révész going to show how his 'qualities' have got linked up normally with differences of position? And if he attributes some rudiment of position to his 'qualities' also, he will find it hard to show why there is only one range of them, while there is a much larger range of 'heights' and therefore of positional differences, capable of using up the octave over and over again. Of course there is a problem involved in the facts. But that problem is to explain the special differentiation of the ends of the series of pitches, not by any means to explain that the ends are distinguished at all and that there are two directions of change within the series. Positions and directions are distinguished in touch, vision, and articular sensation; the last of these forms the best analogy to sound, as it presents so often a single series. But in sound we call the one end high and the direction towards that end 'rising,' the other end

[1] Or, more strictly, of volume and the most pronounced 'order' within that volume.

[2] Cf. Stumpf, *op. cit.* II. 201 ff.; Révész, *Zur Grundlegung der Tonpsychologie*, 134.

[3] *Tonpsychologie*, 116 ff.

low and falling. This special distinction, I take it, is one that could well be explained by the help of the normal accompaniment of differences of volume. For these latter differences form a series with obviously different ends, the one having first a physiological, and then a physical limit[1] of smallness, the other hardly a limit of largeness. The occasional ambiguity of the direction of intervals is more easily explained by the peculiar conflict they entered into in von Liebermann's case with the direction of volume-differences. Other cases may obviously be put down to the confusion of scale relations, *e.g.* thirds, tenths, etc. Besides, of course, Révész's appeal to habit belies the very kind of discovery he derives from von Liebermann's observations, viz. that the 'habit' of the normal correlation of 'quality' and 'height' can be dissolved and observed in dissolution.

After all, the conviction must force itself upon one that the octave relationship is not to be explained by a presumption of identity based on the presence of great similarity, but by reference to the fundamental properties of all sensation in relation to the simplest complexes in which auditory sensations are given to us, *i.e.* by relation to coincidences of volume. It is only such a theory that can explain the absolute relativity of scale relationships, which is, nevertheless, accompanied throughout a large part of the auditory range by the discrimination of practically absolutely equal differences of pitch. The decrease of clearness of pitch at the lower end may be readily explained by the greater diffusion of the region of maximal stimulation, and the analogous nature of the upper limit by the difficulty the soft basilar membrane presents towards fine stimulatory differentiation as well as the want of a sufficient number of sensitive spots to receive the fine differences of position which successive tones imply. But as the volumes are extents, there might still be a fair differentiation of tones in that respect; although it is obvious that both pitch and volume differences must deteriorate together in the highest regions. Still as the extent has always a greater basis than pitch, the former might somewhat outlast the latter in high tones[2].

As for all the problems, "die gar nichts Problematisches haben, so lange man Intervall mit Distanz identifiziert," namely the problems of transposition and inversion, it must now be clear that they arise only

[1] Thus I dissent from the view of Révész and others that the *Höhenreihe* is 'prinzipiell unendlich,' *Zur Grundlegung der Tonpsychologie*, 87.

[2] Cf. Stumpf, *op. cit.* II. 57, "Wo man noch den bestimmten Eindruck hat, dass ein Ton spitzer ist als ein anderer, den man doch seiner Qualität nach nicht mehr davon unterscheiden würde."

for those who identify pitch with quality and volume with something quite heterogeneous—height. If the relation between pitch and volume I have suggested is adopted, it becomes clear at once that transposition and inversion and the like are only matters of form, which present no other problem than do any matters of form, *e.g.* those of vision and touch. We recognise a form more or less in any part of the visual field and more or less in any relation to the frontal plane, *e.g.* laid on its side, or turned upside down. And hearing offers us parallels to practically all the familiar processes of change of form, namely motion[1], speed and all their relations to emotional life as signs of the activity of experience in general.

V. *The nature of the system of hearing.*

The result of the preceding discussion of later researches on hearing is to confirm on all notable issues the formula I proposed for auditory sensation on the basis of a general study of the common attributes of sensation. I need not again proclaim any of the principles upon which I found a study of the senses that is both systematic and special. In fact they must occur to anyone who sets himself the task of such a study, confident that it must succeed.

We must therefore admit that the sense of hearing brings us only one quality, differences of intensity, of pitch (which falls under the generic head of 'order'), of volume, and of temporal attributes. These differences of volume introduce the problem of compound sensations and we solve that problem by postulating primary atoms of audition, which we practically never experience in isolation. We get them only in masses, in which a small region of orders is more or less definitely emphasized by means of intensity. From this psychological analysis and theory we can proceed to formulate the hypothetical nature of parallel physiological processes and to explain all the special phenomena of sound. Only known and commonly accepted physiological hypotheses need hereby be invoked. The psychological construction, on the contrary, is largely new; but that is no sign of its error, but only of the failure of previous efforts to find the correct line of analysis. Nothing else was wanting; for the psychological hypotheses involved in it are in other spheres of sensation the most trivial and familiar. No one could doubt that areal experiences of colour are obtained physiologically by the simultaneous stimulation of a large number of neighbouring sense-

[1] Cf. This *Journal*, 1911, IV. 169 ff.

organs and psychologically by the fusion of a large number of elementary (*i.e.* smallest known to us) sensations of colour, differing similarly in order from one another and fusing in virtue of their extensity to an area. We have only to add the hypothesis that in hearing we never get sensations more elementary than such areas or extents. To turn from the clear daylight of such a view to the darkness of qualitative differences is to nourish a passion for ignorance and scepticism. On the qualitative line of analysis the time when we shall reach some understanding of the sense of hearing is indeed far distant.

Only one point need again be emphasized[1]. The failure to appreciate it has been a common barrier to progress. Pitch and volume constantly urge their true nature upon our attention, but it has been called 'quasi-spatial' and so the truth has been ignored and suppressed[2]. But in fact far from being quasi-spatial, they are non-spatial. They are simply systemic, *i.e.* such orders and such continuousness as will with sufficient variation of order constitute a sensory system. The practically most important correlation of differences of systemic order in most senses happens to link them to the spatial differences of material things, both really and cognitively, both as a matter of fact and as a matter for our knowledge. So we call the tactual, the visual and other systems spatial. In the sense of hearing order differences are linked to spatial differences of matter only in fact; cognitively they are not so. But they form none the less a continuous system of positions. If we talk of this system as quasi-spatial, we should talk not only of the perceptual, but also of our conceptual systems as quasi-spatial. But that would be quite misleading. They are no more necessarily cognitively spatial than is any system of numbers. They are simply ordinal. Temporal differences of position are also ordinal. I distinguish this order from the other by calling the former temporal and the latter systemic[3].

VI. *Physiological theory of hearing.*

A physiological theory of hearing must fulfil three sets of requirements: (1) it must account for all the observed facts of hearing, and it must correspond adequately to the result of the general analysis and systematisation of auditory experiences; (2) it must be compatible with what is known or probable regarding the physical nature of the basilar membrane, etc.; (3) it must show how the sense of hearing has, or

[1] Cf. This *Journal*, 1911, IV. 142.
[2] Cf. Stumpf, *op. cit.* II. 55 ff., 58 f., etc.
[3] Cf. This *Journal*, 1913, VI. 241.

could have, developed, both physically and psychically, from its lower forms to the relatively advanced form in which we find it. Of the chief theories of hearing, Helmholtz's satisfies the first two of these conditions as well as might be, but as commonly understood, it does not satisfy the third at all. The general distribution of the functions of hearing over the basilar membrane which it assumes, appears to be the correct one, and it also correctly admits only one (at least only one most intensely stimulated) resonator for each tonal sensation. Ewald's theory, on the contrary, posits a whole row of similar physical processes for each single tonal sensation, and finds great difficulty in conceiving such connexions between the parts of the basilar membrane involved in the various sound-pictures and the 'centres' for each tone as will afford our actual awareness of tones. Ewald's theory is thus untrue to experience; we ought to hear for each sound picture a series of identical tones, but we do not. And it is as impossible to imagine how the connexions Ewald postulates, can have developed, as it is to imagine how a series of resonators can be placed in the ear and how the different 'qualities' (pitches) of tones came to be linked to them properly on the common psychological interpretation of Helmholtz's theory[1]. I wish now to indicate the kind of theory that would combine in it the advantages of the chief theories of hearing hitherto advanced and avoid the errors into which they have fallen. That is just what a theory that is guided by the correct lines of psychological analysis should be able to do. These lines have been found by the guidance of a properly planned analysis of the psychological facts of hearing and this again is inspired by a general consideration of the ways and means of attaining a complete systematisation of all sensory experience, at least of its simpler forms[2].

[1] Cf. This *Journal*, 1913, VI. 254; also J. R. Ewald, *Arch. f. d. ges. Physiol.* 1899, LXXVI. 181 ff.

[2] Even so predominantly physiological a theory as that of Helmholtz has been able, as it ought, to suggest the question, whether "die Töne uns auch flächenhaft ausgebreitet und angeordnet im Bewusstsein erscheinen" (Stumpf, *op. cit.* II. 101). It is interesting to read Stumpf's note to these words, in which he reports Waitz's objections to supposing that tones are isolated by special organs in the ear. Stumpf says: "Derselbe Punkt schien mir früher bedenklich; doch könnte man, meinte ich, vielleicht noch die Hilfsannahme versuchen, dass die Töne zwar wirklich einen verschiedenen Ort in der Empfindung hätten, aber jeder immer denselben, wodurch der Tonraum ebenso bedeutungslos für unser Bewusstsein würde, als wenn er gar nicht existierte," etc. It can only be the confusing notion that *Ausbreitung* and *Anordnung* must always be spatial, instead of space being a kind of *Ausbreitung* and *Anordnung*, that thus forces Stumpf to make an hypothesis to conceal what is surely present.

As Helmholtz supposes, different parts of the basilar membrane subserve the reception of different tones; the higher whorls (towards the apex) of the cochlea give low tones and the tone evoked becomes the higher, the further we pass towards the basis of the cochlea and towards the root of the basilar membrane in the wall of the saccule. It is utterly inconceivable that, as Ewald's experiment suggests, the whole basilar membrane should be involved in the reception of every tone. Ewald's experiment is certainly very ingenious and instructive. But its main value is the proof it gives that the small fraction of his membrane[1] that forms the unit of any sound picture is the actual and sufficient physical response to an enormously greater wave-length. Any other value the experiment might have had, is annulled by its result —the sound picture—and by the fact that the artificial membrane used was straight and of equal breadth along its whole length[2]. Ewald seems to have felt it necessary to modify his theory to meet these considerations; for he suggests that under normal conditions the whole of the basilar membrane would be used only for low tones and that the higher a tone, the more it would confine itself to the narrower end of the membrane[3]. As regards the tone-picture, however few the number of repetitions it may contain, Ewald seems also to have felt the force of the psychological objection, that we do not for each rate of vibration hear a row of identical tones corresponding to the number of standing waves; for he sketches an hypothesis[4] which shall circumvent the necessity for this conclusion. But such an hypothesis is developmentally more impossible than is the correlation of a row of resonators with a row of pitches or 'qualities' on Helmholtz's view. Ewald himself points out this weakness in Helmholtz's theory[5].

The psychological analysis of hearing postulates not only that different parts of the basilar membrane shall subserve the reception of different tones, but that only one unitary part of it shall subserve the reception of each regular rate of vibration. This postulate is confirmed by the result of ter Kuile's investigation, who holds that for tones of different pitch a variable length of the basilar membrane is bulged out, beginning from the base and proceeding towards the apex of the cochlea. This view would give a good physiological basis for differences of volume, but it is hardly satisfactory in its explanation of pitch; for receptors

[1] Cf. *Arch. f. d. ges. Physiol.* 1903, XCIII. 489 f.

[2] Cf. A. A. Gray, *op. cit.* p. 16 of offprint.

[3] *Op. cit.* 1899, LXXVI. 184 f.

[4] *Loc. cit.* LXXVI. 156 ff., 183 ff.

[5] *Loc. cit.* 155.

that stand at the limiting points of the extent stimulated[1], cannot well be stimulated sufficiently for the purposes of the sharp determination of sensory differences. In sound, as in vision and touch, there will hardly be at the edges of a stimulated extent or area the most rapid change of stimulation, from positive to nothing, or from positive to negative; the change should be gradual in sound. For that reason it seems better to transfer the points of stimulation with Helmholtz to the middle of the extent of stimulation, which decreases in both directions towards the sacculus and towards the hamulus[2]. And if ter Kuile's conception, according to which any tone excites twice as long an extent of the basilar membrane[3] (always starting from its base at the saccule) as does the octave of that tone, could be combined with Helmholtz's, according to which a certain series of resonators is stimulated for any tone, the amplitude of resonance rising from *nil* at two points towards a maximum between them[4], it seems to me we should then have a perfectly sufficient basis for a full physical parallel to all the phenomena of sound. To the minimal complexes of auditory sensation which we experience, we should have corresponding minimal physical complexes. Multiplication of these complexes would give on both sides similar secondary features of tone. Fusion, especially, would receive an explanation on this view, as it gets on no other except ter Kuile's and Ewald's. In ter Kuile's theory, however, the task to be fulfilled by the explanation is overdone; for it becomes impossible to see why any but tones of great pitch differences should be distinguishable at all.

I have not yet found in the literature any explanation of the peculiar changes of curvature that are seen in the basilar membrane from its basis to the apex of the cochlea. These cannot be fortuitous, for they are almost exactly the same in all animals, no matter what degree of development their cochlea may present[5].

High tones would then be evoked by stimulation of the bases of the basilar membrane, while decrease of pitch would carry the extent of the basilar membrane stimulated further and further towards the apex of

[1] *Arch. f. d. ges. Physiol.* 1900, LXXIX. 500 ff. It should also be noted that this limiting point is supposed to *move* forwards over the extent stimulated once in each tonal period. This oscillation of pitch is hardly reconcilable with the phenomena of hearing.

[2] It is quite consistent with this that the injury caused to the basilar membrane by a long continued intense tone affects not a point, but a short region of it.

[3] *Op. cit.* 201.

[4] This also agrees with Ewald's theory, in so far as a single unit of a tone picture is concerned.

[5] Cf. A. Gray, *The Labyrinth of Animals*, London, 1907, I.; 1908, II.

the cochlea. Lowest tones would probably affect the whole extent of the basilar membrane. But the region of the basilar membrane concerned in the determination of the pitch of the lowest tones would lie approximately at the centre of this extent. It would agree with this view that, as Ewald implies[1], dogs become deaf to lowest tones only when the upper end of the basilar membrane is considerably shortened. If the hearing of low tones is to be affected by mutilation of the basal end of the membrane, the whole of the first whorl must be destroyed[2]. It is a familiar fact that the differential threshold of pitch does not conform to Weber's law, any more than does the discrimination of points on the skin by the method of successive stimulation. The necessary 'increment' increases slowly with the rise of pitch. I have not been able to ascertain whether the nerve supply to the organs on the basilar membrane decreases from the basis towards the middle or is regularly distributed. Probably the silence of Retzius and other authorities on this point is in favour of the presumption of regularity of distribution. In any case we must suppose that the size of the differential increment of pitch is determined by the nature of the spread of excitation round the maximum and the influence of this upon the displacement necessary to give a noticeable difference. The displacement would be about the same relative amount throughout the scale, the same fraction of a vibration per second. Otherwise we should expect the discrimination of pitch to increase proportionately to the extent of the basilar membrane affected by a tone. Stumpf thinks[3] that the distance, as distinguished from the interval, between two tones increases up to the third accented octave. The nerve supply may possibly be richer near the bases of the cochlea than in the upper whorls; or our notion of the distance between tones may be affected by the number of differences we can distinguish between two bounding tones. Apart from the standardisation of distances which is given by consonance and by the relations of form between tones upon which consonance rests, it seems possible that we should take the finer subdivision we get in the higher octaves for greater distance. We do so in using the fingers or the tongue to appreciate distance; the former are to some extent standardised with other and less discriminating parts of the skin and with vision, but the tongue can hardly be said to be so.

[1] *Arch. f. d. ges. Physiol.* 1899, LXXVI. 179.

[2] It is quite possible that the remainder might after a time develop a response to high tones. Cf. Ewald, *loc. cit.*

[3] *Op. cit.* II. 405.

A physiological theory of hearing of this kind would easily satisfy all the demands of developmental theory. It would do this so far as the development of the receiving membrane is concerned, as easily as does Ewald's theory, and it would do so for the relations of the receiving membrane to the sensory centre, as Ewald's never could. For no new and inexplicable attribute of auditory sensation enters to upset the derivation it makes possible. We need only postulate a membrane of texture and length just sufficient to receive auditory stimulations, no matter how imperfectly, and connected with this a primitive auditory receptor, evoking a sensation of a certain quality[1] of variable intensity, having a certain 'order' aspect and extensity. A multiplication of these elements and an accompanying extension of the membrane will mean a variation of 'order' and a fusion of these orders in virtue of their common extensity or continuitiveness. This multiplication is already admitted in other senses and its biological advantage would be immediately patent in the modification of experience which would result from it alone. The sense of tone would, therefore, as Stumpf suggests[2], develop from above (high pitches) downwards (to lower pitches). Hand in hand with this development by multiplication would go a development by refinement of texture of the membrane and of the receptors, so that the experience would gradually approximate to that of pure tone, as the lengths of the radial fibres, etc., were systematically adjusted to one another in virtue of the advantages which variation towards that system would make patent through experience. The whole development can be seen at a glance from this point of view[3]. And there is not the least disparity between the peripheral and the central processes of complication or between both of these and the complication of the accompanying experience.

Another advantage of this theory is the ready explanation it offers of the *general nature of hearing*; complex sounds of all kinds first appear blended into a unity[4], which may afterwards be analysed. But this analysis never approximates to the kind of separation we find between different patches of colour in the visual field. That we can only explain by supposing that the cause of fusion is physiological and is not

[1] Ewald admits that on his theory there is only one "specific energy in Mach's sense" in hearing. *Arch. f. d. ges. Physiol.* 1899, LXXVI. 181.

[2] *Op. cit.* I. 339 ff., II. 218.

[3] For indications of the process of standardisation, whereby intervals, including the octave, are standardised throughout a large part of the musical scale and whereby pitches come to possess an 'absolute' identity and name, see below, 40 ff.

[4] Cf. Stumpf, *op. cit.* II. 77.

removable[1]. I suppose that the extent of the basilar membrane stimulated by every tone begins at the base near the oval window and extends towards the apex of the cochlea. The extent of basilar membrane involved by a higher tone will therefore always coincide with a part of that involved by a lower tone. Only in the case of a large difference of pitch will the maximal point of the higher tone stand well away from that of the lower and so be readily noticeable.

We should expect to be able to deduce the relative degrees of *fusion* shown in different intervals from a consideration of the nature of the supposed coincidence of extents. Thus it is evident that the extent involved by the octave of a tone will be the part reaching from the base of the basilar membrane to the maximal point of the lower tone. In the interval of the fifth the maximal point of stimulation of the higher tone and the end-point of the extent of the basilar membrane it involves will severally lie on either side of the maximal point of the lower tone and away from it by one-sixth of the whole extent involved by this lower tone. The corresponding relations for the fourth are one-eighth towards the base, one-quarter towards the apex. The next degree of fusion applies, according to Stumpf[2], to four intervals (4 : 5, 5 : 6, 3 : 5, 5 : 8); the corresponding relations for these are: one-tenth and three-tenths, one-twelfth and one-third, one-fifth and one-tenth, three-sixteenths and one-eighth. The relations for the tritone are three-fourteenths and one-fourteenth, and for the second and seventh one-eighteenth and seven-eighteenths, seven-thirtieths and one-thirtieth. Thus we see that only for the octave and the fifth is there complete balance in relation to the maximal point of the lower tone. For the fourth the balance is uneven, but it is only as two to one, and the denominators are still small. For all other intervals the denominators are larger and the balance is usually less. These facts seem to claim some significance alongside the oscillations of the stimulations which must accompany dissonances.

Beyond the interval of the octave, however, these relations must cease to be of much significance. And there is no doubt that fusion is then less[3], although the same harmonic affinities are maintained by indirect means. I mean by that, not overtones or other secondary accompaniments of chords, but the system of tonality that has grown up on the basis of the greatest and primary fusions.

[1] *Op. cit.* II. 211 ff.

[2] *Op. cit.* II. 135; cf. W. Kemp, *Arch. f. d. ges. Psychol.* 1913, XXIX. 162 f.

[3] Cf. Kemp, *op. cit.* 162 f.

It is most natural that the grades of *sensuous pleasantness* of intervals should differ greatly from their grades of fusion[1]. For the former we have to appeal to other features of the tonal complex than its fusional aspect, namely its variety, a certain tension of parts not reaching as far as discord and the suitable footing for subjective activity that arises therefrom. But I shall not attempt to construct the physiological or psychological basis of all the phenomena of fusion. That is a task for special investigation. I wish merely to indicate the lines my general analysis would extend to meet the facts. I can see in the facts nothing which would constitute a serious objection to my analysis. On the contrary the facts rather seem to invite the application of it.

The relation of *attention* to analysis also receives a place in my theory. As Stumpf says: "Several simultaneous tones can be sensed and rough differences between them can be noticed without ado; finer differences only after practice and other favourable circumstances[2]." The aid given to attention by sounding one or more of the components of a chord before the whole chord, consists in the fact that the attention is thereby directed to that particular point in the tonal order which is to appear in the chord; the tone heard is thereby intensified, for its entrance is made easier and quicker. As the attention passes from one component to another, each is intensified and heard specially[3]. But we can only thus intensify what is already a relative maximum in relation to the just surrounding degrees of excitation. We cannot, as Stumpf[4] thinks we should be able to do on Helmholtz's view regarding the spread of sympathetic resonance on the basilar membrane, intensify by the attention any less than maximal points that are in no proper sense relative maxima. At the same time this does not prejudice the possibility that the pitch of low tones, for which the relative increase of resonance up to the maximum is very gradual, should be rather indeterminate.

The attention cannot without effort attend to all the differences that are given[5]; it must be trained, just as for the perception of the niceties of visual form; and it can be trained in different directions,—for analysis of the objective components of chords, for overtones and difference-tones, for accompanying significant noises, etc.; and the effect of imperfect training may fade out in time. Yet whatever the degree

[1] *Op. cit.* 192 f., 249 ff.

[2] *Op. cit.* II. 85.

[3] Cf. Stumpf, *op. cit.* II. 314, "dass mit dem gleichzeitigen Heraushören (zweier Obertöne) nicht, wie mit dem einzelner Obertöne eine Verstärkung verbunden ist. Verstärken kann man immer nur einen auf einmal."

[4] *Op. cit.* II. 113 f.

[5] For conditions affecting analysis see Stumpf, *op. cit.* II. 328 ff.

of training may be, all given differences are there and may be familiar in bulk without any analysis. Thus persons who make no analysis of sounds so as to single out their components one by one, may still recognise voices, noises, vowels, etc., simply by a comparison and distinction of these sounds in bulk. Limits may be set to analysis in any person by the nature of his basilar membranes; only a fine membrane will make very pure tones and a high power of analysis possible; in a coarser membrane total sound complexes, though tending in character more towards noise, will still be distinguishable and a certain amount of analysis will be easily attained. For noises are less constant and precise stimulations. So the powers of the musical and of the unmusical are all of the same nature and origin. Unmusical people are compelled to recognise sounds by a 'hear and say' method, as it were; those who have absolute ear may recognise most chords also in this way, while they will still spell out unfamiliar ones.

The greater ease with which sounds rich in *overtones* can be recognised[1] is just an instance of the greater ease of recognition of the complex as against the simple whole; it is also acquired on the 'hear and say' method. It is not through making the fundamental more precise that the overtones make the fixation of the pitch of the fundamental more precise. But the whole sound complex is made more precise and more easily recognisable by the addition of overtones. For the number of distinguishable differences in an octave increases greatly, as we pass from one octave to the next higher (up to a certain point); these finer differences must, therefore, react through the sound complex as a whole upon the distinction of that complex, when it is treated, as it usually is, as equivalent to the fundamental tone it contains.

But analysis can be carried only up to a certain point; it can never separate a tone from a complex in which it is given, so as to make it in all respects appear exactly as it does when it is given alone. Only in the matter of pitch, apart from quality and the temporal attributes which may be neglected, is there true equivalence. The pitch of the component, except perhaps in the most special cases[2], is identical with that of the isolated tone. The seeming perfection of tonal analysis rests entirely upon this fact. Intensity and volume, on the contrary, must be very much affected. There can be no true analysis of the intensity[3] of a component, except on the basis of the difference between

[1] Cf. Stumpf, *op. cit.* II. 351.

[2] *Op. cit.* 397 ff. These, however, are not deemed true cases of change of pitch.

[3] Cf. Stumpf, *op. cit.* II. 420, "Die gleichzeitigen Tonempfindungen oder besser die gleichzeitigen Erregungen des Nervus acusticus tun sich gegenseitig einen Abbruch."

the maximum, to which it owes its distinction, and the surrounding level, in so far as that is more or less appreciable. Analysis of volumes, such as would give the definition of volume found in the isolated tone, is also impossible. Component volumes can be detected only unitarily and, as we have supposed, through the secondary aspect of fusion. This is a general effect; analysis may be necessary to ascertain the nature of particular fusions in a complex chord; but we do not first, or at all, separate x and y from one another in respect of volume in order to notice their fusion. We attend to the x—y part of the complex in order to attend best to the x—y fusion, and that may be helped, of course, by first attending to the pitches x and y, so as to direct attention better to the x—y part of the fusion[1]. It is also obvious that in the process of analysis the unity of the timbre of a component will be largely sacrificed; the component will be heard without its timbre, unless special circumstances, such as the unitary movement of a timbre provided by the concert of several different musical instruments, make it specially easy to attend to a unit of timbre[2].

Special conditions will attach to the way in which either of two tones covers or obliterates the other, when both are given together. Stumpf's conclusion[3] "that the higher tone must possess a greater excess of intensity if it is to cover the lower one than conversely," seems to be deducible from consideration of the extents of the basilar membrane involved. For the extent of the higher tone will always fall within that of the lower, so that the amplitude of oscillation it produces will add itself to that of the lower tone. If the higher is to suppress the lower its maximum must lie so near to that of the lower and its intensity must be so strong that, when it is added to that of the lower, the relative increase of the maximum of the latter over its neighbours will no longer be enough to give a noticeable difference. The lower tone, on the contrary, has the advantage of being the only clearly defined one; it begins at the base of the basilar membrane and ends clear of all the higher tones. The others only help to colour it, to modify its form, so that it naturally usurps to itself the chief attention. If a higher tone is to be heard, it must not only make a bigger relative maximum (building already on a considerable amplitude) than it would in isolation; but it must be strong enough to call the attention away

[1] Cf. Kemp, *op. cit.* 214 ff., Summary, 235.

[2] For observations of the condition of tones in more or less stationary complexes, cf. W. Köhler, *Ztsch. f. Psychol.* 1913, LXIV. 100 ff.

[3] *Op. cit.* II. 228, cf. also from 219 onwards.

from the lower tone. This predominance of the lower tone in unchanging tonal complexes is probably the natural basis of Külpe's law of consonance, "dass bei gleichen Verschmelzungsgrade der einen Akkord zusammensetzenden Intervalle der grössere Verschmelzungsgrad des am tiefsten liegenden Intervalls für die höhere Verschmelzung des Ganzen den Ausschlag giebt[1]." That the special fusional relations of tones will modify these general rules is obvious. But fusional relations, as I have supposed, are to be considered merely as special cases of these rules[2]. "In a resting chord the whole seems to have the pitch of the deepest tone, even when this is not also the strongest[3]."

An important deduction regarding the facts of pathological hearing can also be made. The excitation of the basilar membrane that is produced by a tone that is the octave of another tone, just reaches the point of maximal stimulation of the latter tone. Any tone higher than the octave will not reach this maximal point and so will not include it within its extent. On the other hand the excitation of every tone lower than a given tone will include the maximal point of the latter within its extent. Now we may suppose that every diplacusis or pseudotone that is due to an affection of the basilar membrane arises because within the total extent excited by a tone there is, besides the point of maximal objective stimulation which gives the pitch of the objective tone, a sensitive point that is for some reason more sensitive than usual and so gives a pseudotone. It would then follow that the objective tone may be, according to circumstances and cases, any tone that is lower than the pseudotone, but never more than an octave higher than the pseudotone, if indeed it ever quite reaches the octave[4].

It is now easy to apply this theory of hearing to the exceptional facts gathered by von Liebermann and Révész. In a certain sense there is contained in any normal tone every pseudotone that can ever take its place, so long as the physical stimulus remains the same[5]. Pseudotones, it is clear, may be caused by any such alteration of the basilar membrane as changes the point of maximal stimulation, whether this process represent an increase of sensitivity somewhere, or a loss of sensitivity somewhere, or both, or a more or less regular change in the physical nature of the resonators of the basilar membrane. The pseudo-pitch is

[1] See Kemp, *op. cit.* 207.

[2] Cf. Stumpf, *op. cit.* II. 234.

[3] Stumpf, *op. cit.* II. 384, 407.

[4] According to Stumpf, *op. cit.* I. 277, mistuning in diplacusis to more than one-third probably rests upon illusion.

[5] Cf. above, p. 15.

not really, but merely phenomenally, new; it is always contained in the normal tone, but it is then strictly subordinated to the normally most prominent pitch, *each tone being a regular system of sounds.* Noises are also sounds, but they are not the kind of system of sound that constitutes tone; they are not regular in system, but irregular; the irregularity may be of many kinds, just as noises are. We can hardly expect to be able to classify noises in any exhaustive manner. My analysis shows that it is scarcely a matter of great interest to do so. Theoretical interest centres chiefly on the question of the pitch of noises. Their pitch, however definite it be, will always differ from that of a tone by reason of the absence of the tonal system in which the pitch stands. But the very irregularity that surrounds the pitch must help to hide it. It will not predominate in a system of pitches, but it will merely be one of many simultaneous, or very rapidly changing pitches. Some degree of determination of the bulk of these will always be possible; we shall hear whether it is a low or a high sound, and in each case approximately how low or high. No more is surely needed. We do not ask the theorist who discusses the localisation of visual sensation to find the local sign of the sunbeam dancing on the water.

With regard to the appreciation of intervals, the recent results obtained by Catharina v. Maltzew[1] are of importance. She has shown (1) that the judgment of successive intervals is not based upon their conversion into fused simultaneous intervals; the judgment is not based upon that form of the fusional relationships of tones. Nor is it (2) based upon 'distance'; for, as Stumpf observed, distance and interval do not vary together[2]; descending intervals are harder to judge than ascending, whereas both are alike in the matter of distance; and the introspections of Frl. v. Maltzew's observers show that the 'distance' between tones is a very unreliable basis of judgment, unless perhaps for grosser differences. She points out further (3) that 'distances' are gradually variable quantities, whereas interval varies qualitatively. The fact that in successive intervals we pass from one tone to another, leads her to recognise a special qualitatively peculiar experience of 'passage' in every interval and to assume that 'this content' it is that makes us speak of 'one and the same interval' (the pair of tones being of a certain relative frequency)[3].

These three points call for some comment. (1) It may readily be

[1] "Das Erkennen sukzessiv gegebener musikalischer Intervalle in den äusseren Tonregionen," *Ztsch. f. Psychol.* 1913, LXIV. 161 ff.

[2] Cf. above, p. 31 f.

[3] *Op. cit.* 197.

admitted that fusion in the sense of the simultaneous representation of the successively presented tones had nothing to do with the judgments given by the observers. But it has not been shown that such relations of tones as account for their fusion when simultaneous, do not form the ultimate means of the standardisation of 'distances' that converts them into 'intervals[1].' (2) It may also be admitted that intervals in this strict sense are not judged by reference to 'distances,' and yet it has not been shown that distances are not always involved and included in intervals. Intervals are distances that have somehow been standardised. (3) The time is past when qualitative differences can be established by a mere assertion, unless we are to use the term 'qualitative' in the loose sense of 'distinctive' or 'peculiar.' The bare assertion, therefore, carries no weight and may be dismissed. Moreover, on any theory, some explanation of the relation between 'distance' and 'interval' is called for, and this explanation will do much to settle what the psychological nature of the essential experience of interval is. I should not hesitate to assert that a proper classification of any 'peculiar' experience can only be given with the help of a systematic theory of the relations and connexions of the members of the large group of experiences to which it belongs.

I am not acquainted with any analysis, except my own, which suggests a ready explanation of the distance between tones, their interval, and the relation between these two things. Tonal distance, as I take it, is the parallel of visual or tactual distance. It is the crude, primitive 'mode,' the first and simplest of those that are founded on ordinal differences. Interval is, in general, a 'musical' term, a notion of the developed, discriminating, systematized, tonal consciousness[2]. Its nature is best defined by its simplest relation to distance; it is in the first place distance standardised. I do not mean to say that it is recognised or named by reference to distance, or that a judgment of interval is up to some point *eo ipso* a judgment based upon tonal distances; but it is a judgment which ultimately affects or concretes distances by standardising them. Frl. v. Maltzew's theory at the most merely points more correctly to the direction in which we shall find the means by which this standardisation is accomplished. It must be procured by the identification of something that is identical in intervals in spite of the differences of distance. She asserts—and I think it should now be

[1] Cf. above, p. 31 f.

[2] Cf. This *Journal*, 1911, IV. 180, "In talking of interval in the primitive sense we cannot mean consonant, dissonant or 'tonal' intervals."

clear that her view is merely assumption or assertion—that the basis of identification is the identity of the 'step' or 'passage.' But she does not show why the step from x to y is different from that from y to x. She does nothing to explain by reference to the familiar facts of other senses what the step or passage from one tone to another is. We learn much from her as to the connexions of similarity and familiarity amongst intervals and as to their relation to the memory. But that does not illuminate the problem as to their nature.

My analysis can only welcome, and be welcomed by, the results of Frl. v. Maltzew's careful study. It would point to the identical relations of form constituted by successive intervals as the basis of the standardisation of distances, and therefore as the true constituents of intervals. Simultaneous intervals are also marked by peculiarities of form, which, though they differ from those of successive intervals, are derived from one and the same source. I have already pointed to various reasons which will make simultaneous interval a very different matter from successive interval. The greatest difference lies in the fact that in simultaneous interval two 'systems' of sound are summed to make a new system, with a definite form, containing two special maxima, various less prominent maxima due to difference tones, etc., and in the case of many dissonances various other oscillating features. These features appear in successive intervals at most only for an instant at the onset of the second tone, if the time interval between the two tones is less than a certain amount, and they must be much blurred and obscured. Successive interval must be marked, as Frl. v. Maltzew assumes, by the special features of a passage from one system of sound to another. But here each system is given separately. The form of the whole unity of the two systems of sound is therefore not that of a combined system, but of a succession. I do not see how on any theory this complex of form (successive) could be turned into the other combined (simultaneous) unit by means of representation, unless the other simultaneous complex had been identified as containing the pitches given in the successive form, and the two had been correlated and were reproducible through memory. We might as well expect the untrained eye to draw the sum of two sine curves without calculation. Confusion of thought about the relation between simultaneous and successive intervals has resulted from want of clearness regarding the voluminous nature of sounds and the nature of the analysis of sounds made by the ear. As I have shown above (page 36) by reference to observation and by deduction from my analysis, only pitches can be analysed perfectly in simultaneous intervals, whereas intensities and volumes can only under special circumstances be separately gauged. But in successive intervals pitch, intensity, and volume all stand out clearly and separately in the successive 'systems' of sounds. The difficulty of holding, of recalling, and of singing certain of these intervals is the difficulty of passing from one sound system to another; it is clear that the presentation of the interval must largely determine the standpoint of the observer within the total combined system. The inversion of an interval means a great change of form unity. Identity of interval means identity of the form unity which the two successive systems form.

This unity may, of course, contain various secondary features which might serve as a basis for its recognition. Frl. v. Maltzew does not refer to any of them; they may, indeed, be wanting in any number of cases. She has shown the various means used by her observers to retain an interval and to make it clearer for recognition, such as transference to lower octaves, movements of the larynx, etc. All these aids presuppose the recognition of the peculiar form unity of successive interval. Only in the case of inference from recognition of absolute pitches is the form unity ignored. Direct and immediate estimation of interval[1] is the product of much previous memory work: correct classification presupposes processes of comparison of a more or less direct kind, which, as Frl. v. Maltzew has shown, still come into operation where intervals have to be judged in unfamiliar regions of sound. At the extremes of the tonal scale the form unities of intervals become very large and very small, and only by reference to the distortion of forms which for obscure physiological reasons then ensues, can we expect to explain the slight falsification of pitch which is found in the extremes of the tonal scale[2]. This normal false hearing does not necessarily imply a falsification of auditory distances, but only in the first place a disturbance of the usual processes for the standardisation of intervals and for the naming of tones which thereby becomes possible.

If both successive and simultaneous intervals involve attention to

[1] As also of pitch, of course, for the naming of pitches must be a result of the standardising of intervals. Thus is the primitive ordinal difference between tones converted into a musical system. It must now be clear how superfluous such an hypothesis as that of the 'counting cell' (*Zählzelle*) is. Musical evolution, moreover, does not require the assumption of any rapid development of the sense-organ of hearing, but only the construction of varied and thorough systems, based upon such standardisation, of greater and greater complexity.

[2] Cf. Maltzew, *op. cit.* 216 ff.

matters of tonal form, it becomes clear that tonality is the highest expression of the maintenance of continuity of form. Some basis for continuity of attention must be given, if complex sequences of sounds are to be apprehended rapidly. The more complex the nature of simple intervals is shown to be, the more necessary does this support to the attention appear. It may indeed be true that unity and proportion of form are made more noticeable by the increase of the complexity of the masses of sounds and the tonal forms they create ; just as the unity and proportion of visual schemes are more effective in the larger visual works of art than in the simple parallelograms, triangles, and crosses of experimental analysis. But even so the attention of the observer must be guided in both of these arts and the more so perhaps in the successive structures of music. Tonality, or the introduction of a general scheme or system of intervals, needs no further justification in general than this. Only when we turn to the details of the schemes of tonality we actually find before us, do we need to enquire into the particular causes that have led to the formation of each particular scheme. The outlook that must guide these enquiries is clearly indicated in the psychological analysis and theory of hearing I have advocated.

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