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STUDIES

IN

AUDITORY AND VISUAL SPACE PERCEPTION.

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STUDIES IN AUDITORY AND VISUAL SPACE PERCEPTION

BY

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PREFACE.

The several essays here brought together appear at this time as the regular publication demanded of the incumbent of the Kellogg University Fellowship of Amherst College at the expiration of his official term. As the table of contents shows, they purport to be contributions to a particular field of experimental psychology. Whatever unity pervades the essays is determined rather by the general identity of subject-matter than by the continuous application of any single principle of interpretation. Still the attentive reader will note that the principles expressly defended in the first essay, though not explicitly reaffirmed later, are nowhere violated by the discussions contained in the essays that follow. The general theoretical position may then be defined as nativistic, the nativism being of that moderate and elastic form which acknowledges the large and all-important rôle played by an organizing and systematizing experience. To determine the details of the particular experiences under which some of our visual and auditory spatial perceptions, illusory or otherwise, appear, has been everywhere the incitement to these investigations.

To Professor Münsterberg I wish to express here my deep gratitude for first arousing my interest in these matters and for giving me many valued suggestions. Most deeply too am I indebted to the numerous persons who from time to time have kindly acted as subjects in the experiments, especial thanks being due to Mr. Cobb, Mr. E. T. Esty and Mr. Towne who by their careful and long continued assistance have rendered me invaluable service.

Northampton, Mass.,
June, 1901.

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THE LOCALIZATION OF SOUND.

THE attempt of the following pages is, first, to make a systematic presentation of all important facts and theories connected with the localization of sound, and, second, to develop a satisfactory theory for the formulation of all facts, old and new, that shall have been brought together. An historical survey will always precede the communication of any original material, and the endeavor will be made to show as clearly as possible how the new lines of experimentation grew out of previous discussions and already existing problems. The original experiments to be described were begun in 1892 and 1893 in the Harvard Psychological Laboratory at the instigation and under the direction of Prof. Münsterberg. Certain portions of the investigation were then brought to completion. Further experimentation along supplementary lines was carried on at Amherst College in 1897 and 1898. It will be seen that this paper embodies much of the matter reported in the *Psychological Review* for 1894, among the Studies from the Harvard Psychological Laboratory. At the same time this is in no sense a mere reproduction of what was there written. Not only is much new material here introduced, but in addition the general standpoint is considerably different. Points of divergence are perhaps especially marked in connection with the theoretical attitude towards the subject. A noteworthy difference of interpretation will also be remarked for the facts relating to dizziness reported in Chapter 6.

A bibliography practically complete to date has been appended, and the numbers in parentheses scattered throughout the paper refer to this.

INTRODUCTION

AND GENERAL ORIENTATION.

HOWEVER justifiable may be the procedure of the theorizer upon Space Perception to speak as if there were but a single, unitary space to which his reflections were directed, for the psychologist who is attempting to analyze the matter thoroughly there must be assuredly a full recognition of the existence of "spaces." Now among the various spaces that have been overtly recognized—visual, tactual, etc.—it is only too evident that an *auditory* space has not been generally included. Any acknowledgment of its independent claims has indeed been seldom made. To be sure we may safely presume that no one has ever ventured to champion an auditory space of coördinate importance and development with either the visual or the tactual space, for nothing is more patent than that our auditory spatial experiences are far less manifold and far less comprehensive than those in the other domains mentioned. But to deny these experiences altogether is not only hazardous but in open contradiction to fact. And consequently, as we shall see presently, a repudiation of auditory space has necessarily been accompanied by a particular interpretation of those auditory experiences, numerous enough in everyday life, which are seemingly cast in a space form belonging to them in their own right.

It seems indeed plausible to expect the existence in theory of a genuine auditory space—to however subordinate a position it may be relegated—and in view of this one cannot suppress an exclamation of surprise at finding so widespread a denial to hearing of all independent participation in a world of space. And yet if one name Stumpf, James and Münsterberg—and these are not equally explicit in the statements of their belief—one will almost have exhausted the list of those who have been bold enough to stand out for an auditory space.[1] While over against these stand the opponents of the doctrine in overwhelming numbers. Nor are the opponents wholly of a single school. They count as their adherents followers of the nativistic no less than of the empiristic and genetic trends. In fact, to be committed in reference to one of these general modes of interpretation does not by any means imply a particular belief in reference to auditory space. For obviously its existence or non-existence must first be settled to the satisfaction of a given thinker before he need apply to it the particular theory of space perception that has given satisfaction in the other departments of sensation. And yet, on the other hand, a justifiable rule of method should surely require that all the seemingly independent spatial experiences in the domain of hearing should be carefully and thoroughly scrutinized in order then and only then to assume a position in reference to the question of an independent auditory space. And having come to a decision in this particular, the data gained in this field should certainly be massed with those already possessed in the visual, tactual and motor fields before

[1] Compare also Sully (67).

venturing upon the elaboration of any general theory of the perception of space *überhaupt*. This is unmistakably the only procedure that is methodologically correct. But aside from this, were it not that an unwonted prejudice had powerfully hindered the fullest and freest investigation and discussion, it would seem that more than has actually been the case would have searched the field in the hope of possibly bringing to light some facts which should prove decisive from the point of view of theory. For the facts of visual and tactual space perception seem capable of different interpretations, each of which to the mind of him who champions it appears to be eminently satisfactory and to do full justice to the facts at hand. Such being the case, and an auditory space by virtue of its material divergence from the common characteristics of the other spaces, seeming to promise something new for reflection, it is really astounding that its independence has not been more critically and searchingly examined and that a more considerable array of facts has not been marshalled long since. In our own opinion the contributions made by auditory space to the general theory are of decisive moment. We may anticipate somewhat the outcome of our theoretical discussion by stating here that certain facts, which have hitherto failed to receive the attention which they certainly merit, will be appealed to as telling unmistakably for the nativistic interpretation. What these facts are will be seen in due time. They are mentioned here only as indicating the confidence of the writer that an examination of the possibilities of auditory space is by no means a vain task.

In preparation for this task, we have now, *first*, to gain some comprehension of the widespread rejection of an auditory space, together with the explanation adopted for all those seemingly independent spatial determinations made whenever sounds are localized: and, *second*, we have to seek the reasons for this rejection, and thus at the same time reach a clear conception of the nature and scope of our undertaking.

I.

To the majority of those who have written about the matter it has seemed thoroughly self-evident that sounds possess *in themselves* no spatial attributes. In themselves they are neither "here" nor "there," neither "massive" nor "piercing," and the former just as little as the latter. Their seeming endowment with spatial characteristics is due to experience, through the aid of which there has been a systematic *borrowing* from the visual and tactual fields. That is, the ear is regarded solely as a *qualitative* organ (as indeed it is *par excellence* with its more than 10,000 discriminable qualities), the various qualitative differences due to varying positions of any sounding body becoming associated more or less intimately with the *visual*, or *tactual*, impressions of the sounding body. Thus these visual and tactual impressions being joined in experience to the appropriate auditory qualities, *carry along with them their own spatial characteristics* and thus give to the natively unlocalized sensations of sound the semblance of being possessed of spatial characteristics in their own right. Accordingly localizations of sounding bodies not seen or touched are virtually *estimates* or *inferences*.

Berkeley held the above view in a sufficiently definite manner (49). Wishing to show that "outness" and distance are to the mind not objects of sight, he appeals confidently to hearing for a clear analogy the admission of which he takes for granted. The different distances of the approaching coach, he says, are perceived by the variations of the noise, and by this Berkeley means that these variations of noise are associated with corresponding amounts of locomotion, in terms of which we think of distance. But, he continues, "I do not, nevertheless, say I hear distance," the non-spatial character of sound is too obvious to everybody for that mistake to be made.

Similarly *Hartley* (51) would seem to reduce all auditory localizations to associations with other experiences. Distance is associated with intensity, and direction with less clearly assignable indications. "We judge of the position of the speaker, or sounding body, by the eye, or by some other method independent of the ear. And thus, if from some mistaken presumption a voice, or sound, shall be deemed to come from a quarter different from the true place of it, we shall continue in that error from the strength of that mistaken presumption."

Still more explicit is *James Mill* (57), and clear as always. Localizations of sound are obvious cases of indissoluble association; there is nothing "but the close association of the sounds with the ideas [visual?] of the objects." "I believe," he says, "not only that I hear the sound of a man's voice, but that I hear it behind me, or before me; on my right hand, or on my left; at this distance or at that. The indisputable fact, in the meantime, is that I hear only a modification of sound, and *that the position and distance, which I believe I hear, are nothing but ideas of other senses, closely associated with those modifications of sound.*"[1] This is clearly shown, as Hartley long before had suggested, by the phenomena of ventriloquism. For here "a man acquires the art of forming that peculiar modification of sound, which would come from this or that position, different from the position he is in; in other words, the sound which is associated, not with the idea of the position he is in, but that of another position. The sound is heard, the association takes place, we cannot help believing that the sound proceeds from a certain place, though we know . . . that it proceeds from another."

Again, *Spencer* (64) expresses in unambiguous language the same opinion as his predecessors. Hearing is put upon the same plane as smell, for "sensations of sound do not of themselves yield the consciousness of space." They are to be regarded as "not having in themselves any space-implications." And this seems to Spencer so self-evident that he boldly writes: "No one will allege that sound, as an affection of consciousness, has any space-attributes. And even those who have little considered such questions will admit that our knowledge of sound as coming from this or that point in space, is a knowledge gained by experience—is a knowledge not *given* along with the sound but *inferred* from certain modifications of the sound. When being deluded by a ventriloquist and led to draw wrong inferences, or when, respecting the whereabouts of a humming gnat at night we can draw no inference, we get clear

[1] Italics not in the original.

proof that primarily sound is known as pure sensation." [!]

Bain (47) keeps up the traditional opinion, displaying by his language a woeful lack of acquaintance with the facts of the case. Distance is spoken of as "a purely intellectual sensation," and the opinion is approved that "the knowledge of it is owing to a process of reasoning applied to the [auditory] sensation." Then he complacently adds: "We can readily judge whether a voice be before or behind, right or left, up or down; but if we were to stand opposite to a row of persons, at a distance, say, of ten feet, we should not be able, I apprehend, to say which one emitted a sound."

Other writers both in England and America may be referred to briefly. *Lloyd Morgan* (59) affirms that localization of sound is not much better than that of taste and smell. *Ladd* (54) says: "The surrounding space is not a construction, either immediate or indirect, of the ear," and "It is by tactual and muscular exercises, and practice gained by such exercises, that our auditory sensation-complexes are localized in a space already constructed by activity of eye, muscles, and skin."—And *Titchener* (68) writes: "auditory sensations, which have no spatial attribute, contribute nothing directly to our ideas of space." And referring to the same sensations again, he speaks of the "impossibility of their spatial arrangement."

Among the German writers too expressions of the same general tenor as the above are to be found. *Lotze* (56) showed a fondness for speaking of tones as "ortlos," and this to him obvious character of auditory impressions is seized upon for purposes of illustration, mere sensational qualities in the fields of sight and touch stripped of their positional determinants being compared to tones. That is to say, sensations of sound have no "local signs."

Wundt (70), while fully admitting the possibility of making localizations of sound on the basis of many factors that enter into auditory experience (see p. 50), expressly and emphatically repudiates the notion of an independent auditory space. The determinations of auditory distance and direction, he says, presuppose always the space perceptions acquired through sight and touch. "The existence of a special auditory space independent of visual and tactual sensations qualitatively and spatially considered, is a fiction which is contradicted by the immediate witness of every sort of spatial localization." The only problem of auditory space perception is, therefore, to show how sounds become localized in the already existent space of sight and touch.

Ziehen (71) also, though allowing that auditory impressions are projected into space, and though discussing briefly the conditions of this projection, comes to the conclusion that in the case of sounds "there is wanting those direct spatial relations which are to be found in the tactual and, in the highest degree, in the visual experiences. Hearing is, in fact, no space-perceiving sense. It is, briefly characterized, purely qualitative."

Lipps (55) implies that the notion, from which we cannot rid ourselves, that we actually hear tones in the distance or out of the distance, is like the notion that we immediately *see* things *hard* or *soft*. Distance is no possible *object* of hearing. But each distance, or each direction, of sound has a particular character which is the sign for the position—in visual or tactual terms, of course —of the sound-producing object.

And, finally, *Külpe* (53), though differing from all the foregoing in that he is a professed nativist, denies to sound the possession of spatial attributes. In the first place "*extension* belongs only to the visual and cutaneous sensations. If we speak of the 'extension' of tones, scents, or tastes, we are either using the term allegorically . . . or employing it in a secondary sense." And, secondly, "when we 'localize' a sound heard, we do not make it spatial: we merely determine the part of space which is the seat of its visible source or condition." Hence the localization of sounds "is *mediate* only: it consists in a reproduction of originally spatial sensations, of movements indicative of the place of origin of the given contents, or of judgments directly expressive of their local determinations."[1] This is, plainly, a *transferred* localization, and the business of any theoretical elaboration of the matter is to make clear the various incentives to the reproduction of the "originally spatial sensations" which alone make auditory localizations possible.

Among the French, we may mention *Egger* and *Lechalas.* The former (11) discredits the attempts to make the ear a space-perceiving organ. For the ear cannot *juxtapose* two impressions, and this is needful for the perception of space. The ear gives *successions* while the eye gives *positions.* Hence the ear is the organ for apprehending time, as the eye is that for perceiving space. Lechalas too (20) claims that one cannot speak of an

[1] Titchener's translation.

auditory space without an abuse of language, for the fundamental condition for the development of such a space is lacking, viz., the correspondence between points of the object and separately excitable points of the auditory organ. Two points objectively vibrating are not capable of representation by two distinct stimulations of the ear.

Clearly the holding of such opinions as those above rehearsed is not calculated to stimulate much investigation into the possibilities of auditory space perception. But having now gained to some extent an appreciation of the prevailing attitude towards auditory space, let us endeavor to discover somewhat more in detail the grounds for this attitude.[1]

II.

As already stated, these adverse opinions have not belonged exclusively to any one class of theorists. They have been as stoutly maintained by champions of nativism as by the adherents of the empiristic and genetic theories. Yet the *grounds* in each case for the opinion held have probably differed.

(*a*) The first reason to be mentioned—and this has probably been chiefly decisive for the nativists—is *that the ear possesses no spread-out surface such that an arrangement of stimulations upon it may represent the spatial relations of the external world.* The ear, that is, possesses nothing comparable to the retina, the articular surfaces or the skin. These can be stimulated *partially* and *seri-*

[1] All references to current works on physiology have been omitted, since these simply reproduce opinions originally expressed elsewhere.

ally. And coexisting spatial relations may be impressed upon them *simultaneously*, so that the total excitation of any one of these surfaces at any moment virtually presents (at least in the case of retinal and cutaneous surfaces) a map of the outer excitations. Thus positions may be compared and variously related. Linear extents, forms and magnitudes may be sensed, and incentives be aroused to careful measurements of them. But the ear is inexorably doomed to *total* stimulation, and that too by *successive* stimuli. As to the former, it is as if the eye were compelled to have its field of vision always completely filled at any one moment with an unvarying extent of color; or as if the skin were to be stimulated only by total immersions in homogeneous liquids of uniform temperature. Plainly these experiences would never become broken up into parts. There might perhaps arise the notion of crude extension, but it would be an extension without subdivisions, without points of reference, in short without differences of any sort. Such, it is argued, is the case with the ear. Being forever subjected to total excitation, there is no possibility of its attaining independently that sort of development which a truly space-perceiving organ must possess.[1] But, further,—and this amounts to much the same as the foregoing,—the ear can receive only *successive* impressions. (We shall find later that this is not altogether true in the unqualified way implied, but at any rate this is the argument put for-

[1] To be sure the basilar membrane may be partially stimulated, but in all probability by differences in *pitch* only. Positional differences would not find representation by differences in its vibration. And yet the partial stimulation that is possible to it causes Ziehen to wonder that the notes of the octave are not projected to different points of space.

ward.) And thus at best the ear is reduced to the perception of *positions*, only one position of course being apprehended in any one moment. Auditory *objects*, in the ordinary sense, are thus impossible from the very nature of the case. Positions must be simultaneously seized to arouse the idea of coexistent points belonging together and constituting an extended object. But even the perceiving of auditory position seems dubious when one recalls the total stimulation mentioned above. The successive character of the impressions would serve, seemingly, to give fresh auditory qualities and beyond that to accomplish nothing.

(*b*) The second reason why an auditory space has been denied would seem to be *that the ear is unprovided with a muscular apparatus of such a complexity as to enable it to focus itself*, as it were, *upon sounding bodies lying in various directions from it.* That is, there is no possibility of having a widely varying set of movements of the auditory organ which may correspond to the various positions of this organ necessary for most distinct hearing. And thus there is manifestly lacking that possibility of forming closely welded associations between the sensations of hearing, as pure auditory qualities, and accompanying motor sensations, which associations shall give spatial character to the sounds either after the fashion dear to the heart of the English empiricist, or by the more genetic method of Wundt where fusion and not mere association is appealed to. Perhaps hearing *is* a spatial sense for those creatures whose pinnæ are movable, and it may be that man has forfeited not a little by having reached a point of development where the pinnæ are fixed. This supposed need of accompanying motor sensations has been well expressed in a general way by Delboeuf in the paper where he claims that only senses endowed with movement are spatial senses.[1] His fanciful supposition in reference to smell applies equally well to hearing. Suppose an animal, he says, with an antenna on the end of which is the organ of smell, and suppose in addition that the olfactory stimulus be momentary and not continuous as it now is. Under these conditions, Delboeuf maintains, the animal by moving his antenna about in every possible direction would be able to localize odoriferous flowers, for example, and would be able to adapt himself to them as if seeing them with his nose. Translated into hearing this means: supply the ear with the possibility of multifarious movements and hearing becomes a spatial sense. Meanwhile, in the absence of such possible movements, an auditory space is a mere fiction.—It is to be wondered at that movements of the whole head have not been considered satisfactory substitutes for more direct movements of the sense organ. The reason is presumably that such movements could hardly be related exclusively to hearing. They would necessarily affect vision too in part, and possibly also smell. Still, as we shall see later, Münsterberg has made the attempt to connect these head-movements unequivocally with the ear.

(*c*) One more reason has been adduced, but upon very slender grounds. *Heller* (52), discussing the space per-

[1] Pourquoi nos sensations visuelles sont-elles étendues? *Revue philosophique*, IV. (1877), 167.

ceptions of the blind, concludes that an independent auditory space is an illusion, because if developed before touch *it would be a space of positions without objects*, and consequently tactual objects later experienced could not be localized in this auditory space after the manner in which the visual impressions of those restored to sight are at first localized in the already existing tactual space. But there seems to be a gross misconception here, for the positions perceived by the ear would by no means be *pure* positions, without objects. *Auditory qualities* are the "objects" localized. And if Heller further means that at any moment of attempted localization of tactual impressions in this field of auditory qualities there might be a complete absence of all varieties of sound, one could retort simply that the case would be analogous to that of localizing tactual sensations in the visual field *while the eyes were closed.* As the remedy here would be to open the eyes and thus create visual impressions, so there it would doubtless be necessary to make some sort of sound at the point of the skin touched that the tactual impression should be assigned its proper place within the system of auditory "objects."

If the reasons that have prevailed for the rejection of an auditory space have been clearly presented, we should now see very distinctly just what limitations are to be imposed upon our coming investigation. It is at once evident that we shall not be able to divide the subject, as is usual in the other sense departments, into the *perception of extension* and the *perception of position.* By the very nature of the ear auditory *extensities* are excluded from the realm of possibilities. For the ear there are no lines or surfaces of varying magnitude, curvature and direction. Accordingly we shall meet here none of the problems that usually cluster so thickly about these particular perceptions. *Our attention must be turned exclusively to the perception of* POSITION, and hence our undertaking has been appropriately entitled "The Localization of Sound." And since in localization there are the two determinations of *direction* and *distance* to be made, the treatment of the subject will fall naturally into two corresponding divisions. After reviewing the literature of the subject and after reporting the results of our own researches, we shall have finally to discuss the *theory* of the perception of auditory space, in order that, if possible, we may come to a full conviction of the latter's independence.

PART I.

THE LOCALIZATION OF SOUND IN RESPECT TO ITS DIRECTION.

CHAPTER 1.

GENERAL RESULTS AND THEORIES.

A. HISTORICAL.

THE theoretical problem relative to the reality of an auditory space has rarely concerned those who have made experimental investigations in this field. Investigators have been occupied rather with the indisputable *facts* of sound localization and with their psychophysical explanation, leaving to others the interpretation, in terms of psychological theory, of the material thus gained. Records of observations and comments in the experimental field are to be found only within the last half century. Earlier reflections upon the general problem of auditory space perception were made without reference to any systematically ascertained or definitely formulated facts.

WEBER, 1848–1851.

The first recorded experiments in the field were made by Weber. In 1848 (46, A) he communicated to the Saxon Academy of Sciences the observation that the tickings of two watches, held simultaneously one at each ear, can be located each at its appropriate ear. Three years later a further communication (46, B) contained the conclusion, reached by him on the basis of experiments, that our power of localizing sounds rests upon two factors connected respectively with the drum and the shell of the ear.

If, says Weber, one dips the head under water in such wise that air remains in the passages of the ear, it is still possible to tell clearly whether the sound comes from the left or the right. If however the ear passages are carefully filled with water before plunging in the head this localization is no longer possible. . . . The difference in these two cases can be explained only on the basis of sensations in the drum of the ear. For when air remains in the passages the drum can act as usual, but when the passages are filled with water the drum is thrown out of function. It is the presence or absence of sensations from the drum then that conditions our localization of direction. For this localization cannot be grounded in the sensations of the auditory nerve, since this is equally affected in both of the above cases.

But, once more, Weber says, although we are able when under water to distinguish right from left in the cases where the ear passages remain filled with air, we cannot discriminate any finer directions than these. In ordinary hearing, however, we can perceive whether a sound comes from above or below, from in front or behind. This must depend upon the *outer ear*, which cannot act when the head is under water. That this is so is seen at once from the fact that if the shell of the ear be bound flat upon the head and the hands be held in front of the ears and palms backwards, in such a way as to form an ear-shell in reversed position, sounds from in front are supposed to come from behind, those from above from below, and vice versa in each case, the eyes of course being closed. Why the outer ear should be influential, whether through peculiar sensations that it mediates, or through its manner ot directing the sound waves as they enter the ear, Weber does not discuss.

SCHMIEDEKAM, 1869.

Schmiedekam (35), in 1869, claimed on the basis of his experiments and in direct contradiction to Weber, that even when allowing air to remain in the passages of the ear, sounds can no longer be localized.

RAYLEIGH, 1875-1877.

The next recorded experiments are those made by Lord Rayleigh (32, A) in 1875 and 1877. Speaking of our power to localize sounds, he says: "It has long been conjectured that the explanation turns upon the combined use of both ears" (by which he evidently refers to the distribution of sound intensities to the two ears), and the purpose of his experiments was to clear the ground somewhat in preparation for a satisfactory theory. Such a theory he does not propound, but his arguments are concerned mostly with the "conjecture" in reference to the "intensity-theory."[1] We give a brief account of his methods and results.

[1] We shall find it convenient to designate by this term that theory which bases our power to perceive the direction of a sound upon the fact that the ratio of the two intensities received by the two ears varies with the position of the sounding body. The expression "intensity-theory" will always signify this.

"The observer stationed, with his eyes closed, in the middle of a lawn on a still evening, was asked to point with the hand in the direction of voices addressed to him by five or six assistants, who continually shifted their position." Care was taken to prevent any accessory noises which should suggest the position of the person speaking, and "the uniform result was that the direction of a human voice used in anything like a natural manner could be told with certainty from a single word, or even vowel, to within a few degrees." . . . "But with other sounds the result was different. If the source was on the right or the left of the observer, its position could be told approximately, but it was uncertain whether, for example, a low whistle was in front or behind." To test the matter still further a tuning fork was used, and to avoid giving any aid by means of accessory sounds, two forks were always struck, only one being brought before its resonator. When one of the two was either directly behind or directly in front of the observer, "it was impossible for him to tell which fork was sounding, and if asked to say one or the other, he felt that he was only guessing." Rayleigh's first result is, then, *that, with the exception of the human voice, sounds given immediately in front or behind cannot be successfully located.* The superior position occupied by the human voice appears to him "to depend on the compound character of the sound in a way that it is not easy to understand, and for which the second ear would be of no advantage." That is, there must be some factor other than the mere co-operation of the two ears that must be appealed to for the explanation of the fact ascertained.

But there are special difficulties when one comes to the consideration of the intensity-theory,—difficulties encountered when one considers the slight "sound-shadow" cast by the human head in the case of a lateral sound. That is, the difference in intensity for the two ears,—differences demanded by the intensity-theory,—are hardly great enough to be perceived. This is to be roughly seen by closing one ear and then turning slowly through 360° while listening to the sound of a tuning fork. The intensity perceived at the point where the open ear is turned towards the sound is hardly different from that where the open ear is turned away. On the supposition that the head is a perfect sphere, the anterior pole of which represents the ear directed towards the sound and the posterior pole the more distant ear, Rayleigh calculates the intensities that each pole would receive.

$$\text{Let } A = \frac{\text{circumference of sphere}}{\text{wave-length of sound}}.$$

	Intensity at Anterior Pole.	Intensity at Posterior Pole.
Then, $A = 2$	0.690	0.318
$A = 1$	0.503	0.285
$A = \frac{1}{2}$	0.294	0.260

For $A = \frac{1}{4}$ the difference of intensities amounts to only about one per cent., a difference too small to be recognized. Yet considering the length of sound-waves that issue from a man's mouth as 8 ft., a still more minute difference of intensity must be recognized if sounds be located in the way that the intensity-theory claims. On the basis of these considerations it might be expected "that the facility of distinguishing a lateral sound would diminish when the sound is grave." But no difference in point of facility could be discovered between forks of 128 and 256 vibrations respectively. Still, although sound-shadows in general may not be produced, there may take place a "diffraction" of the sound-waves in such a way as to cause that the partial tones of different pitch will arrive at the two ears with very different intensities. The perception of direction will then be mediated not so much by differences of crude intensity as by differences in quality or timbre. In a similar way a sound from behind differs from the same sound heard in front not so much in mere intensity as in its general quality.[1]

In a second communication made about a year later (32, B), Rayleigh finds, however, certain facts that make more or less strongly for the intensity hypothesis. If the latter be true, there must be certain points of confusion, as must appear from the following reflection. "To the right of the observer and probably nearly in the line of the ears, there must be one direction in which the ratio of the intensity of the sound as heard by the right ear to the intensity as heard by the left has a maximum value greater than unity. For sounds coming from directions in front of this the ratio of intensities has a less and less value, approaching unity as its limit, when the sound is immediately in front. In like manner, for directions intermediate between the direction of maximum

[1] Rayleigh's original article has not been accessible to the writer, and the last paragraph contains matter not hinted at in the summary in *Nature* but referred to by S. P. Thompson in B. 386 and 390 and C. 411.

ratio and that immediately behind the observer, the ratio of intensities varies continually between the same maximum value and unity. Accordingly, for every direction in front there must be a corresponding direction behind for which the ratio of intensities has the same value; and the two directions could not be distinguished in the case of a pure tone." To test the matter a tuning fork was used, and an observer facing north "made mistakes between forks bearing approximately N. E. and S. E., though he could distinguish without a moment's hesitation forks bearing east and west." The accompanying figure puts this in graphic form.

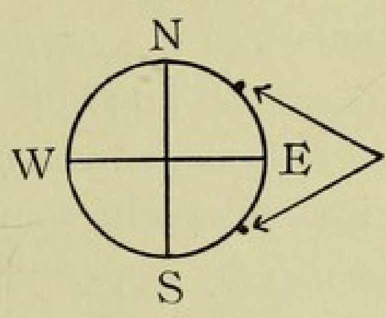


Fig. 1.

Rayleigh's purpose seems to have been primarily to communicate facts, not to discuss theories, and consequently these results are not commented upon.

The outcome of Rayleigh's experiments we may summarize as follows. They show clearly:

1. The existence of a considerable general ability to locate the direction of sounds, particularly in the case of noises and the human voice.

2. The liability to confuse the position of sounds immediately in front and immediately behind.

3. The liability to confuse positions more or less symmetrically placed to the front and to the rear of the line of the ears.

POLITZER, 1876.

This observer (29), while studying disturbances of localization in cases of defective hearing, noted that localization in one-sided deafness is faulty to the extent that sounds are located towards the side of the perfect ear, judgments becoming more and more uncertain as the sound approaches the deaf ear. In normal cases he remarked the especially faulty localization in the median plane. "The hypothesis," he says,[1] "that this deception of judgment rests upon a diseased condition of the semicircular canals, as was formerly supposed, is supported neither by experiment nor by pathological observation."

von Kries and Auerbach, 1877.

These authors (17, A) still further uphold the intensity theory. The comparison of intensities seems to them the most plausible and most probable means for localizing sounds. To be sure there are difficulties to be encountered, for (*a*) points in the median plane (that plane which symmetrically divides the head into lateral halves) will have no distinguishing mark, since from any point in this plane the intensities that reach the two ears will be equal; and (*b*) there are many points where the relative intensities are the same, which points are thus liable to confusion.

Then, too, these authors find the general process of localizing a sound a very complex one. For them it involves three steps: (1) An estimate of the intensity received by each ear. (2) A judgment as to the relation of these two intensities. (3) A conclusion from this as to the position of the sound source.

Ground for assuming such a real *comparison* of intensities the authors claim to find in experiments ar-

[1] Quoted by Bloch (5), p. 7.

ranged to determine the time required for distinguishing from which side a sound comes. The time was found to increase as the sound approached the median plane. Or, letting the angle of divergence be that angle formed by passing lines from the root of the nose to sounding bodies placed on either side of the median plane and at equal distances from it, the discrimination time was found to increase as the angle of divergence decreased. The following table reproduces their results:

Observer.	Angles of Divergence.			
	120°	35°	26°	11°
A.	.015 sec.	0.32	0.35	0.53
B.	.032	0.43	0.54	0.77

The sound used was the crackle of the electric spark. These results seem to show at least that relative intensities enter into the phenomena of localization. To the median plane difficulty mentioned above von Kries returns some years later.

Tarchanoff, 1878. Urbantschitsch, 1881.

These two writers, experimenting with two like sounds given simultaneously one to each ear, found that when the sounds where of equal intensity the resultant was located in the median plane. When, however, one sound was more intense than the other, the resulting localization was on that side of the median plane where the more intense sound lay (41 and 45, A).

Laborde, 1881.

Some definite suggestions in regard to the possible part played by the semicircular canals in the localization of sounds were given by Laborde (19) in 1881. After speaking of the usual well-known phenomena of head movements produced by artificial stimulation of the exposed canals, he says: "Remplaçons l'impression artificielle due à la lésion expérimentale par une des impressions naturelles que le sens spécial dont il s'agit (sens auditif) est destiné à recevoir dans son exercise normal, impression de bruit ou, en general, d'onde sonore, et nous aboutissons exactement au même mécanisme physiologique de phenomène; en sorte que la destination fonctionelle des canaux semi-circulaires se trouve ramenée, en dernière analyse, à celle d'un appareil sensitivo-moteur annexé et approprié à un sens spécial, le sens de l'audition." That is, it is plausible to suppose that the phenomena attending artificial stimulation of the canals serve simply to reveal their real and proper functions, viz., to reflexly produce movements of the head and even of the whole body in response to the normal stimulus, the auditory impression. This definite connection of the semicircular canals with localizing movements of the head we shall find more minutely discussed in immediately succeeding writers.

SYLVANUS P. THOMPSON, 1879 AND 1882.

In 1879 Thompson (42, B) reports experiments made with his "pseudophone," an instrument, he says, which does for hearing what the pseudoscope does for sight. It consists simply of two flat rings bound one to each ear much as the receivers are bound to the head of a telephone operator. The center of the ring allows the free passage of sound-waves to the drum of the ear, and the outer side of the ring is provided with an adjustable metallic flap fastened to a rotating collar. The flap thus takes the place of the pinna, and its position and amount of reflecting surface can be varied in a manner unknown to the subject. These artificial pinnæ may now be set in any desired position and the resulting effect upon the localization of sounds be studied. If now one flap be set advantageously for the collecting of sound-waves and the other disadvantageously, the subject believes that the sound lies too far to the side where the greater intensity is produced. Setting the flaps toward the rear produces the front-back illusion already mentioned. Such illusions come about best with a loud-ticking clock, the human voice, a whistle or a sharp click ; they are hardest to produce with a tuning fork. This points to the supposition of Rayleigh (see p. 28) in reference to partial tones.

In 1882 (42, C), Thompson examines all extant theories of sound-localization with which he is acquainted, and concludes : " Judgments as to the direction of sound are based, in general, upon the sensations of different intensity in the two ears, but the perceived difference of intensity upon which a judgment is based is not usually the difference in intensity in the lowest or fundamental tone of the compound sound (or " clang "), but upon the difference in intensity of the individual tone or tones of the clang for which the intensity-difference has the greatest effective result on the quality of the sound." On this basis, " it is completely open to doubt whether a pure simple tone heard in one ear could suggest any direction at all."

GELLÉ, 1886.

Dr. Gellé (12) reports certain observations made upon a subject of Charcot, from which he concludes that the sensibility of the outer surfaces of the ear, extending to the meatus and drum-membrane, is necessary for the localization of direction. This subject was troubled with general anæsthesia of the skin, as well as with tremblings and troubles of equilibration and walking. Sight, hearing and intelligence had remained intact. The anæsthesia was so great that neither ice nor sprays of ether in the auditory canal caused any sensation. Nor was any sensation, either of contact or of pain, caused by touching or pricking the tympana or the walls of the meatus. Localization of sound was simply impossible. Standing behind him and directing him to close the eyes, Dr. Gellé held a watch at some centimeters from either ear. The subject heard the ticking but *could not tell on which side the watch was.*

It is not absolutely necessary to conclude from this that sensations of touch are indispensable for localization. For the motor troubles with which the patient was afflicted may well have indicated a much more deep-seated disturbance—presumably in the discriminative and associative mechanisms of the ear—than the facts stated disclose.

PREYER AND ARNHEIM, 1887.

With Preyer (30) and Arnheim (1 ½) we find the semicircular canal theory of sound-localization developed to its extreme form. "When one reflects," says Preyer, "that fishes, in which the cochlea is wanting, not only hear, but in spite of the lack of an external ear-passage recognize the direction of a sound, the conjecture seems justified that it is the semicircular canals that make this possible, the stimulus being transmitted to them through the tissues of the head." On the theory of these writers the semicircular canals furnish to any sound the "space feeling" which constitutes its particular direction. To give this "space feeling" is the special function of the nerves of the ampullæ, and the various combinations of stimulation that the three ampullæ on either side can receive correspond to the manifold directions in which a sounding body may lie. That is, sound-waves from a particular direction stimulate the ampullæ nerves in a way characteristic to that direction. All ampullæ upon one side may be stimulated alike, two may be affected more strongly than a third, or one alone may receive a predominant stimulus. With any combination of stimulus-effects upon one side goes a definite combination upon the other side. And thus, once more, the varying directions of sound are mediated by the various combinations in which the specific energies of the six ampullæ nerves may be aroused. Of course sound-intensities are influential here in exactly the same manner as for those who hold the intensity-theory. The unequal intensities in the two ears cause that sort of stimulation of the semicircular canals that determines whether a sound shall be placed upon the right or left. Or, equal intensities in the two ears determine the sound to the median plane. Distribution of intensities is effective, only this distribution first makes possible a spatial judgment through the medium of the ampullæ nerves.

Such in brief is the semicircular canal theory of sound localization as developed by Preyer. It is to be noted that this is the first of the recorded theories that pays any heed to the really fundamental problem of space perception as applied to sound; how, namely, the sensations of sound get space characteristics anyway. Granting original spatial characteristics of an indefinite sort as inherent in the sensations of sound, the intensity hypothesis might, perhaps, show how these became arranged in proper order so as to become practically available. It could never show, however, without accessory hypotheses, how the sensations of sound, regarded as mere qualities, became possessed of spatial attributes in general. That is, the intensity hypothesis attempts only to show how sensations of sound become spatially arranged within the already existing visual or tactual space. Of an independently existing auditory space they do not for a moment conceive. Just this defect the hypothesis of Preyer attempts to remove. To mediate auditory spatial characteristics is the specific energy of the ampullæ nerves, and since with every quality of sound there is a particular definite stimulation of these nerves, every sound has attached to it not only a spatial attribute in general, but a spatial attribute expressive of a single definite direction.

Manifestly it is difficult, if not impossible, to arrange experiments which shall decide in favor of this theory as opposed to the theory of intensities. For as we saw above the distribution of intensities is primarily as effective here as there. Consequently one cannot say that the experimental results of either Preyer or Arnheim are in any way decisive or capable of only one interpretation.

Their method and results are briefly as follows: On the head of the subject was placed a "sound helmet," a sort of cap made of wire and fitting the head closely, from which projected "pointers" several inches in length. These pointers were arranged symmetrically over the surface of the helmet and gave in all twenty-six different directions. Sounds were given at the extremities of these pointers and the subject was required to give his judgment of the position of any sound in terms of the positional designations agreed upon. Thus the positions of greatest certainty and success were found, and also the misplacements to which one is most liable.

Eighty experiments were made with each direction and 29.4 per cent. of all sounds were correctly judged. Many misplacements were made, but these were always either within the median plane, or within the regions lying to the right or left of this. That is (*a*) no sound from the right or left was ever located on the opposite side. And (*b*) no good careful observer ever located in the median plane a sound coming from the left or right, and none located a sound from the median plane in the regions at the right or left of it. Very interesting and instructive too are the facts that the misplacements made in the median plane were almost completely duplicated by analogous misplacements in both of the lateral hemispheres; and that these misplacements were to points from which a similar distribution of intensities would be made to the two ears.[1]

The errors made were, however, the authors think, just such as might be expected on the basis of their

[1] For a full confirmation of these results see Matsumoto (25).

theory. For it must be admitted that sound-waves from different directions may produce approximately equal effects upon the ampullæ nerves, in respect, that is, to the manner in which the stimulation is distributed to them. Preyer finds that the theoretical considerations as to the points of similar stimulation are confirmed by the confusions appearing in the experimental results.

Münsterberg, 1889.

The first genuine recognition of an independent auditory space, and the first adequate formulation of the psychological problem of sound-localization is due to Professor Münsterberg (26). Starting from the indisputable fact that we do localize impressions of sound, and urging that we have no more right to deny the independence of an auditory space than we have to say that impressions of touch first acquire spatial significance by borrowing the same from sensations of sight, Professor Münsterberg endeavors to show the psychophysical processes involved in the localization of sound. He calls attention at first to two sets of facts hitherto separated and regarded as standing in mutual contradiction. (1) In the first place, Preyer has shown the more or less complete parallelism between the differences in the stimulations of the semicircular canals and the ability to recognize the directions of sounds. This has been done at least to the extent of showing that wide differences in stimulation are accompanied with the greatest accuracy in localizing, and further that similarities of stimulation are attended by corresponding confusions and misplacements.

(2) In the second place one must take fully into account the oft-observed and well-known phenomena consequent upon artificial cuttings and excitations of the semicircular canals of pigeons, rabbits, etc. From out these somewhat complex and varied phenomena one fact stands sure, viz., *the reflex execution of head movements in the plane of the stimulated canal.*

These two sets of facts then must be brought into relation and harmonized. The line of thought is not far to seek. The two facts are not antagonistic; they are the two sides of one and the same process. The effect of any sound impression is not only to arouse the qualitative sensation of tone or noise, but at the same time to so excite the semicircular canals as to call forth reflex movements of the head of such sort as to bring the sounding body into the direct line of sight. These movements must of course reveal themselves through muscular sensations, *and it is just this union of particular muscle-sensations with the sensation of sound that constitutes what we mean by the "localization of sound."* The manifold movements of the head, rendered possible by the variations in stimulation of the canals, produce that complete three-fold system of movement-sensations which form the foundation of our auditory space, in the same way that sensations from the movements of the eyes and limbs form the foundations of our visual and tactual spaces respectively. And it is the placing in this system of movement-sensations of any particular movement-sensation, called forth by a definite reflex turning of the head, that *is* the localizing of that sound which excited the head movements concerned. When the movements of the head are actually carried out they are, as remarked, of such a nature as to bring the sounding body into the direct line of sight, or in other terms into the region of most distinct hearing. The movements may, however, not take place in any actual case. The mere *impulse* to movement, that is, may take the place of the movement itself, and this is what really happens in the majority of cases in normal adult life. In this case, however, the localization is none the less sure, for the impulse can associatively reproduce the movement-sensations formerly connected in experience with the actually executed movement.

With this theory of sound-localization in view, the next task was to test it, if possible, through experimental observations. Noting the fact that previous investigators had concerned themselves preponderatingly with the question as to what directions are correctly recognized and what confused, Professor Münsterberg proposed to examine rather the delicacy of auditory space perception in respect to direction. The subject was seated in the center of a circle of one meter radius, and the sound, given at successive points on the circumference of this circle, was the whir, twice repeated, made by turning backwards the stem of a stem-winding watch. The problem was: starting with any point on the circumference of this circle, how many degrees of separation are necessary in order that two sounds may be just noticeably different in direction? Regarding 0° as immediately in front and 180° as immediately behind (both consequently lying in the median plane of the head), and 90° and 270° as points half way between these two and lying respectively at the right and left of the observer, and expressing the angular separation of the sounds in linear terms, the following table will present the experimentally ascertained variations in delicacy as one passes from 0° to 180°. The figures given in the right hand column are valid for both sides of the circumference.

0°	1.5	cm.
22°.5	2.5	"
45°	5.5	"
67°.5	6.0	"
90°	7.5	"
112°.5	8.	"
135°	8.5	"
157°.5	8.5	"
180°.5	10.	"

That is, on the basis of these figures the delicacy of localization steadily decreases from the point in front to the point behind. But this, says Professor Münsterberg, is exactly what one must expect if the localization of sound is based upon sensations of movement. For, on this theory, the delicacy of localization is precisely the delicacy with which one may distinguish these sensations of movement that enter into the complex known as sound-localization. And if this localization is accomplished by head-movements, actual or not, the resulting sensations of strain, actual or associatively revived, must be more and more intense as the sound passes from 0° backwards. As an immediate consequence, therefore, of Weber's law and in exact conformity to it, the further one goes in passing from 0° to 180° the greater must be the separation of two sounds for the apprehension of a direction-difference, since the more intense the sensation of strain the greater must be the sensational increment in order that a difference of intensity be noticed. This perfect agreement between the theoretical demands and the experimental results seems strongly confirmatory of the theory which the experiments set out to test.

von Kries, 1890. Breuer, 1891. Titchener, 1891.

In the two years immediately following the publication of the paper we have just outlined, two serious objections were advanced against the semicircular canal theory, von Kries (17, B), writing in 1890, called attention to the fact that two simultaneous sounds of different quality can be located each in its own place. If, for example, a whistle be given at one ear and a noise at the other, they are both correctly localized. And, further, tone and noise behind, or both in front, or tone behind and noise in front were, in his experiments, correctly placed. Only when the tone was given in front and the noise behind was there a confusion, to the extent of judging both behind.

If now, says von Kries, the localization of sounds rests upon the aroused impulses to movement, there should be in the case of qualitatively different simultaneous sounds, either a fusion of the two impulses into one, or a confusion of such a kind that the sound upon the right, *e. g.*, should now and then be associated with the impulse towards the left, and *vice versa*. The facts mentioned show, however, that this is not necessarily true, and von Kries is compelled, therefore, to look askance at the Münsterberg theory.

Breuer (7), though interested primarily in the physiological problem, also brings objections against both Preyer and Münsterberg. Preyer, as we have seen (p. 34), has supported himself upon the fact that fishes can locate the direction of sound though without cochlea and without external opening to the ear. On the basis of his experiments, however, Breuer thinks it questionable whether fishes really hear. The state of the matter seems rather to be that they perceive vibrations by the sense of *touch* rather than by the sense of hearing. But in the second place, Breuer states clearly the physical difficulties that confront the theory in question. How shall sounds from various directions produce those varied stimulations of the canals that the theories both of Preyer and of Münsterberg demand? All the waves of sound from no matter what direction pass in nearly the same manner through the external passage to the drum of the ear, and from that point they certainly pass in the same manner through the auditory ossicles on the way to the labyrinth. How is it possible then that at this point they are so distributed to the canals as to represent the particular direction from which they originally came? Only one way seems possible, viz: by conduction of the sound through the bones of the skull. But not only do the upholders of the theory in question not make this hypothesis, but their experimental results show that such conduction is of little or no moment, since by closed ears the ability to localize is strongly diminished. Nor, further, can these authors show why waves from a sound moving in the horizontal plane should more readily stimulate the horizontal canal than either of the vertical canals.

Professor Münsterberg had investigated the degree of delicacy for the transverse and median planes as well as for the horizontal. In a brief review of his results Titchener (43) calls attention to the fact that the degree of delicacy varies astoundingly along the different arcs *at the points of intersection of these arcs.* Thus, at the point in front, where horizontal and median planes intersect, the respective delicacies are 1.5 cm. and 4.5 cm. and 3 cm.; at the sides, where horizontal and transverse planes intersect, they are respectively 7.5 cm. and 2.5 cm.; above, where median and transverse planes cut, 1.5 cm. and 3 cm. respectively. These differences, Titchener says, "can hardly be explained by variations in sensations of strain," whereas they become readily comprehensible on the basis of the intensity theory and the influence of the ear shell.

Bloch, 1893.

Suspicion being cast upon a theory against which such strong objections as those we have just noted could be raised, Bloch (5) set himself to a renewed examination of the whole problem. The point of approach was determined by the previous experiments of Münsterberg. For the latter the question of delicacy of localization was of supreme moment, and on the theory that auditory localization rests upon accompanying sensations of movement, a decrease of delicacy by an increase of the sound's distance from the anterior point was a necessary postulate. It concerned Bloch, therefore, first of all to make a most careful examination of the question of delicacy. The experimental conditions were in all essentials identical with those of Münsterberg. But the results given in the following table show a striking deviation. As before, 0° is directly in front, and 90° opposite the right ear. The column on the right gives the separation of any two sounds necessary for the recognition of a change in direction.

Horizontal Plane.		Delicacy of Localization.	
0°	.... 8 cm.	180°	....18 cm.
22°.5	....18 "	202°.5	....25 "
45°	....28 "	225°	....40 "
67°.5	....45 "	247°.5	....45 "
90°	....55 "	270°	....60 "
112°.5	....40 "	292°.5	....30 "
135°	....30 "	315°	....22 "
157°.5	....20 "	337°.5	....12 "

That is, in marked divergence from the results of Münsterberg, whose single subject appears to have displayed an almost inexplicable idiosyncrasy, we find here no steady diminution of sensibility from 0° to 180°, maximum and minimum points respectively, but on the contrary a rapid diminution towards 90° and 270° where the minimum sensibility is reached, and from these points a rise in sensibility to a second maximum at 180°. There are thus two maximal and two minimal points. The delicacy at 180° is less than that at 0°, but still considerably greater than that at either 90° or 270°. To make certain that these results possessed more than individual validity, Bloch carefully repeated them upon two other subjects, and upon the same subject with a sound of essentially different character. The results were everywhere the same. *The keenness of localization was always the greatest before and behind and least at the sides.*[1]

How now shall these results be explained? Without hesitation Bloch rejects the semicircular canal theory *in toto* and attaches himself to the intensity theory. With this the facts observed can be explained and harmonized, the facts, namely, that the positions of greatest delicacy are at points where the sound, if moved, must issue from the median plane; that the points of least delicacy are where, by reason of the ear's structure, a wide region must be passed over before a difference in relative intensities may be noted; and, lastly, that the parts of the field lying in front of the transverse plane are, in point of delicacy, symmetrical with those behind it, a condition of things demanded by the intensity-theory, as long before pointed out by its adherents.

Bloch then proceeds to supplement the intensity-theory by appealing to *the accessory influence exerted by the shell of the ear.* This influence he sees in two directions: in the determination of points of delicacy in monaural localization, and in the rendering possible of any local discriminations whatever in the median plane. These matters will be spoken of in their appropriate places.

We no longer need, then, says Bloch, to appeal to the semicircular canals to explain the facts of sound localization. "The comparison of the sound-intensities per-

[1] These facts are graphically presented in Fig. 11, p. 108.

ceived by the two ears and the fine variations of clang conditioned by the position and form of the ear-shell, is for us the approved means for this end." Movements of the head may indeed assist greatly in the process of learning to interpret these differences in intensity and clang, as well as in forming the necessary associations between them and the corresponding visual impressions, but theirs is by no means the leading rôle.

In concluding this summary, attention should be called to the fact that Bloch speaks of a "comparison of intensities." Whether this is meant in the sense of von Kries (p. 30), or is loosely used as an equivalent for the mere fact of *relative* intensities at varying points, cannot be definitely decided from the text. This is a point of obscurity that touches the details of a *theory* of localization, but it in no way affects the validity of the *facts* adduced.[1]

STUMPF, 1873–1890.

In his attitude towards space-perception in general, Stumpf is an outspoken and thorough-going nativist. Auditory impressions possess spatial attributes as do the impressions of sight and touch. This must be premised to comprehend the position taken here.

It is incontestable, Stumpf says (66, A and B), that we form spatial judgments in regard to sounds. We at

[1] Any who may care to examine the merits of a secondary line of argumentation in its positive and negative aspects, as developed respectively by Münsterberg and Bloch, the former resting upon an interpretation of the interesting phenomenon reported by Urbantschitsch, against which interpretation the latter brings forward apparently unanswerable objections, are hereby referred to the articles concerned. (Münsterberg, pp. 210–212; Bloch, pp. 13, 27, 58–59.)

least locate them, although we cannot speak of "auditory extents" in the sense of experiencing sound images or sound forms. The question then is: Do these spatial judgments "rest upon the spatial attributes in the sensations of tone, immanent in them in much the same way as intensity and quality, or do they rest upon other and merely accompanying sensations out of whose union with the sensations of tone the spatial apprehension of the latter is, through some psychological process, developed?" The reply is here the same as for sight and touch, "the first and indispensable foundation for the spatial apprehension of sensations of tone lies in them themselves, in a moment immanent in them." However this immanent moment may be further characterized, it is at least the capital stock upon which the wealth of auditory spatial experience is built up. In particular it is due to this *that we can distinguish the right from the left ear.* The fact of the possession of this ability is of fundamental import for our author.

Not only can one distinguish on which side of the head a sound is given, or tell which of two sounds continues if one on each side be given and one be stopped, but also one can correctly locate two tones of different pitch or quality sounding simultaneously one on each side. How can one explain these well-known facts? The attempt has been made to explain them on the basis of *differences of intensity.* But how shall these differences in themselves signify anything in reference to which side receives the greater intensity? A turning of the head may indeed be followed by a stronger stimulation of the right ear (*e. g.*), but how know that it is the right ear that is being stimulated unless that ear has something about it that first makes it distinguishable from the left.

Again, an explanation has been proposed in terms of *touch-sensations in the ear-drum*. But there are here the same difficulties as just noted, together with others. For irrespective of the facts that such touch-sensations could only be located as at the right or left on the ground of some more basal distinction between the ears, and further that in pathological cases ringings and buzzings in the ear are correctly located without any possible concurrence of touch sensations from the ear-drum; there is the deeper difficulty similar to that noted by von Kries above (p. 42) that no guarantee exists for the correct association of the auditory and tactual sensations. If a tone *c* upon the right and a second, *e*, upon the left produce respectively the tactual sensations *α* and *β*, what can assure us that *c* is to become associated with *α* and *e* with *β*? Original spatial differences in the tones being disregarded the associations *cβ* and *eα* are as conceivable as the reverse.

Only then by means of a spatial moment possessed by the tone in addition to its quality and intensity can such judgments be made as are made. The tone heard by the left ear differs thus from that heard by the right, and thus determinations of right and left are possible. In certain cases indeed these differences may disappear and the two spatial moments of right and left coincide. Such seems to be the case when two simultaneous sounds are located within the head.

Stumpf produces no original experimental evidence to reinforce these theoretical considerations. He only endeavors to show how, out of the original differences posited, we build up our ability to localize sound. Auditory space has undergone little independent development because of its small practical value. The principal business of experience has been to establish associations between these auditory spatial differences and the various factors in the visual and tactual fields on the basis of which we interpret our auditory experiences.

WUNDT, 1893.

Wundt (70) recognizes only an indirect localization, a localization, that is, made in the visual or tactual space on the basis of certain factors connected with the auditory impressions. Of leading importance among these factors he regards the tactual impressions from the shell and drum of the ear, to which are probably added the sensations from the tensors of the tympanum. Of secondary importance is the distribution of intensities to the two ears.

MATSUMOTO, 1897.

Dr. Matsumoto (25) has carried out a seemingly careful and painstaking research in reference to several matters connected with the localization of sound. With some minor modifications the experiments that concern us in this section were made almost exclusively along lines previously followed by Preyer, and by Münsterberg and Pierce. The results obtained by these investigators were fully corroborated. The *intensity-theory* is made everywhere the basis for experimentation and explanation. Appropriate references to the article will be made in other connections. Only one result need be noted here. This relates to the delicacy of localization.

On the basis of the intensity-theory which says that the localization of direction is based upon the differences of the intensities that reach the two ears, one should expect that in accordance with Weber's law the delicacy of localization would be greatest at points where the differences of the two intensities are minimal, and least where these are maximal. Now these differences are least at 0° and 180°. Consequently the addition of a relatively small increment to the intensity differences should be perceptible at these points. Or in other words a relatively small movement of a sound from these points should be apparent to perception. By placing two telephones one on each side, opposite the ears, the resultant sound could be made to appear anywhere from in front to a point at either side by means of a change of intensity of one of the sounds. This change of intensity was regulated by a sliding inductorium. Then for any degree of this intensity—that is, for any position of the resultant sound—there was found the increment of intensity necessary to produce an apparent change in the direction of the sound. The result was wholly as expected. For sounds at the side a greater change of intensity was needed than for sounds in front or behind. Thus delicacy in the localization of direction is fundamentally the delicacy for apprehending intensity-differences.

SCRIPTURE, 1897.

In close connection with the preceding article, Scripture (36) has attempted to give a mathematical expression to the phenomena of sound-localization in so far as they depend upon relative intensities. Such an expression, however, has little, if any, value for psychology.

This completes our historical survey so far as that phase of the matter is concerned which we have proposed to treat in this section. The facts and theories reviewed may be conveniently summarized as follows.

Facts.

We may safely accept the following as reliable.

(*a*) A general ability to locate noises and the human voice better than tones.

(*b*) A universal ability in normal individuals to say whether a sound comes from the right or left.

(*c*) The ability on the part of all good observers to distinguish between sounds in the median plane and those lying at either side of it.

(*d*) The tendency to make certain confusions, the most pronounced being between sounds from points lying symmetrically with respect to the transverse plane.

(*e*) The ability to separately locate each of two simultaneously given and qualitatively different sounds, one on each side.

(*f*) The existence in front and behind of two points of greatest delicacy of localization, and at the sides of two points of least delicacy of localization.

Theories.

The power to localize sounds in respect to their direction is variously supposed to be accomplished on the basis of:

(*a*) The tactual sensations in the shell and drum of the ear. (Weber *et al.*)

(*b*) The varying distributions of sound-intensities to the ears. (The "intensity-theory.") (Rayleigh, von Kries *et al.*)

(*c*) Processes connected with the semicircular canals.

1. Effects of stimulation not precisely defined.
2. "Space-feelings" mediated by the nerves of the ampullæ. (Preyer.)
3. Movement sensations from actual or intended movements of the head reflexly called forth by stimulation of the canals. (Münsterberg.)

(The "semicircular canal theory.")

(*d*) Original spatial differences in the sensations of the two ears. (Stumpf.)

(*e*) The above factors in various combinations.

B. EXPERIMENTAL.

Description of Apparatus.

The apparatus used in the Harvard Laboratory in all experiments in the perception of direction consisted of a graduated circular rim 1 m. in diameter, which rested horizontally upon supports and could be adjusted to any desired height; and of arcs of the same curvature as the rim, which could be adjusted in the median, transverse, or any other desired plane. (Fig. 2.)

The subject sat at the center of the horizontal circle in such a way that the planes of the median and transverse arcs intersected at the middle point of the line joining the ear-drums, and, further, in such a way that the entrance to the ear-passage was at the same height as the source of sound.

The sounds employed were given by telephones.

These received the secondary current of a small induction coil the rustle of which was inaudible to the observer, and their sounding could be controlled as to

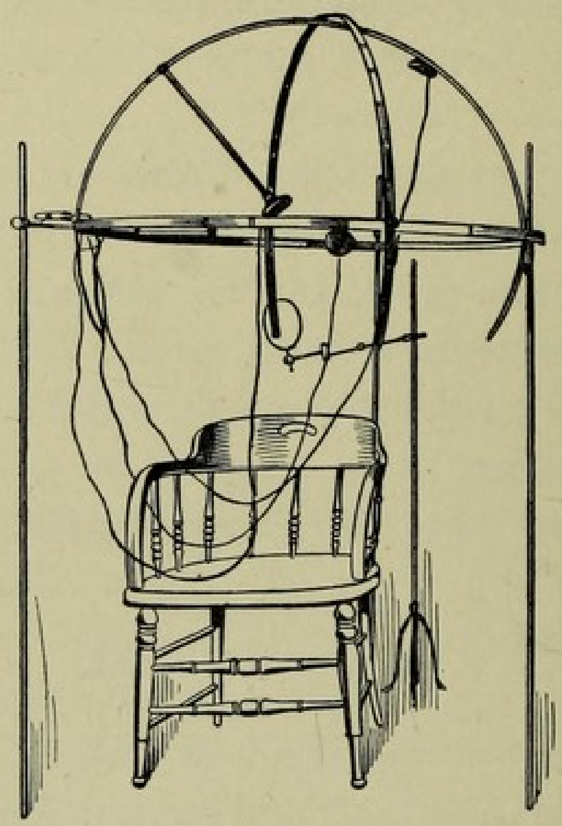


Fig. 2.

intensity and duration respectively by a resistance box and a key inserted in the circuit.

In the Amherst experiments a modification of the above apparatus was adopted, in order to obviate to the greatest possible extent all accessory noises and all suggestions arising from the known or suspected position of the operator. (Fig. 3.) This modification consisted in making the whole apparatus capable of rotation about the seated subject. To this end the horizontal circle with its two arcs was suspended at the point of intersection of the latter from a light horizontal beam resting upon supports. Several inches below this rotating circle was placed a similar circle resting fixedly upon supports. This latter circle was graduated to degrees and served as a means of orientation for the subject as well as for designating the position at any moment of a sounding body upon the rotating circle. As sounding body a telephone was used here also and this was placed in a cradle-like rest fastened to the rim. By means of this form of apparatus, great ease of experimenting was secured, and, as remarked above, the influence of suggestion was reduced to a minimum. Obviously the Amherst form of apparatus, while particularly adapted

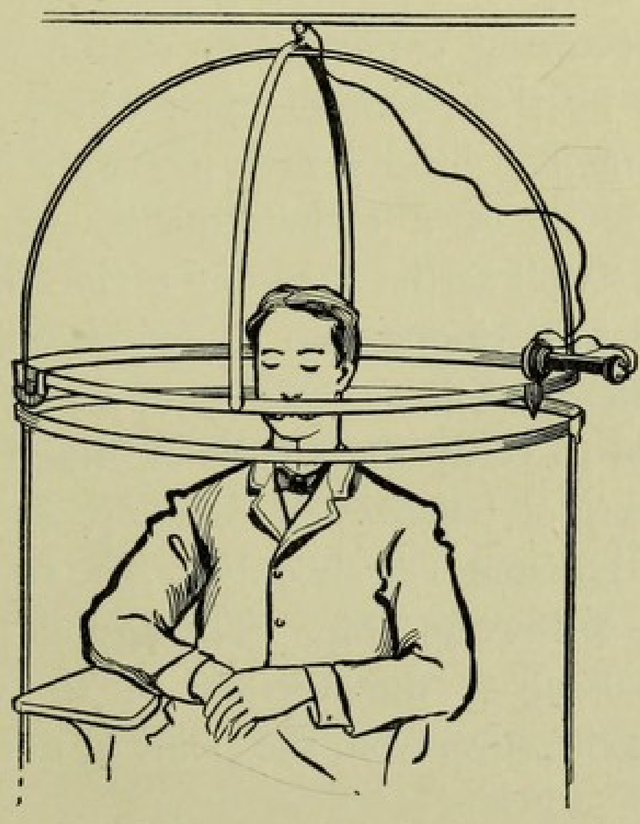


Fig. 3.

to the use of a single sounding body, also permitted the easy manipulation of two sounds by a single operator.

The description of the experiments will fall under several headings, according to the number or character of the sounds used.

Positions on the horizontal circle are always designated as follows:

0° = in front. 90° = opposite right ear.
180° = behind. 270° = " left "

I. One Sound.

With one exception no systematic experiments along the lines heretofore described were made with a single sound. In one way or another, however, results were constantly occurring confirmatory of the various statements made by those writers upon whom we have reported. Thus, *e. g.*, the greater certainty in the localization of sounds of a certain quality was repeatedly manifested, as well as the accuracy of localization in respect to certain positions, and the chronic inaccuracy in respect to others. The one exception mentioned referred to an attempt to verify the results of Bloch in reference to the delicacy of localization. (See p. 45.) This was done for the horizontal circle only. The results are presented in the following table. The figures given denote degrees, and each is the average of six experiments, three in each direction from the point in question. One subject, B.

TABLE I.

	0°	45°	90°	135°	180°	225°	270°	315°
Least perceptible movement.	2.5	2.5	8.76	6	3.3	6.15	8.49	2.65

Thus the results of Bloch for the horizontal circle are compietely confirmed.

Since only relative values were sought in the above verification, the method of procedure was to move the sound until the subject was aware of the direction in which movement was taking place. In following out this method it often happened at 90° and 270°, and less frequently at 45° and 315°, that, while a change of position soon became noticed, the *direction* of this change was apprehended as opposite to the real. This fact points to the same sort of confusion emphasized by Rayleigh and theoretically deduced by others as a necessary consequence of the intensity theory. The changed ratio of intensities consequent upon a movement of the sounding body is often such as not necessarily to be given one and only one interpretation in terms of perception. The perceived direction of movement is therefore not always to be predicted. From among the various directions theoretically possible from a physical point of view, certain circumstances will usually determine which direction shall be actually perceived. The expectation of the subject is often the deciding factor. This expectation may be the result of a recent localization which makes one direction of the attention easier than another, or it may be due to some suggestion inadvertently given. And when once a given direction of movement has been perceived, false though it may be, localizations of adjacent positions will the more readily fall into line with the direction first perceived. This determining influence both of suggestion and of an antecedent localization will appear still more clearly on later pages. What we shall come to know as "the confusion of quadrants" is in all essentials the same phenomenon as the above. And as there a given sound may be perceived in one quadrant to-day and in another to-morrow, so here the perceived direction of motion may not be the same in two successive sittings. All this is in entire accord with the observation of Preyer (p. 37).

II. Two Sounds.

Relatively speaking, the literature of sound-localization has had little to say in reference to two simultaneous sounds. Aside from their use in studying intracranial localization (chap. 5), they have been mentioned by Weber (46, A) and von Kries (17, B).[1] But the latter were both concerned in keeping the sounds qualitatively distinct. Our own concern, on the contrary, was to procure complete fusion of the two sounds as in the experiments on intracranial localization. Only the desire here was to produce a resultant sound that should be invariably located *outside the head*. A perfectly satisfactory resultant sound was secured by the use of two telephones of practically identical quality. When made to sound as above described (p. 53), their fusion was complete. The exteriority of the sound was readily brought about by placing the telephones on the rim of the horizontal circle. At this distance from the head there was no trace of any tendency to locate within the head. On the contrary, the resultant sound seemed ordinarily to be at about the same distance as the rim of the horizontal circle.

[1] See also Matsumoto, loc. cit.

There was thus obtained the possibility of creating a subjective auditory field with no objective spatial equivalent. For the complete blending of the two objective sounds gave rise to a phantom-sound with perfectly definite characteristics, and the highest possible diversity of positions could be given this phantom-sound by the suitable arrangement of the telephones upon the rim. The proposed attempt in these experiments was accordingly to correlate with every possible arrangement of the telephones the subjective localizations of the corresponding phantom-sound. The hope was entertained that the results thus obtained might furnish new data for the further resolution not only of the particular problem of auditory space perception but also of the problem of space perception in general. Let us turn to a report of the results themselves. Five subjects regularly participated in the experiments now to be described, while two others were in a condition of good training and were occasionally used, and five others still were used in one or two series each for purposes of verification. All subjects were in a state of good training when made use of for recorded experiments. From the trained subjects groups of two, three or four were selected for special sections of the general experimentation. The method of giving the judgment was by stating the supposed position of the sound in terms of degrees upon the arcs. So good was the practice of the observers that this method could be relied upon entirely. If uncertain, they were allowed to open the eyes and designate the position by pointing. Frequently too they were asked to do this as a means of controlling the accuracy of the verbal designations.

Another way of giving judgments, viz., by opening the eyes and marking a point upon a diagram of the apparatus, was used in a few later experiments. The duration of the sound was approximately one second.

(A) Two Telephones on the Horizontal Circle.

(*a*) *One on each side of the Median Plane.*

1. Symmetrically placed.—Here all subjective localizations were at either 0° or 180°, that is, at particular points in the median plane. The discussion of the conditions that determined which of these positions should be given must be deferred until we treat the median plane localizations more in detail.[1]

2. Placed in all possible combinations of position, at intervals of 20°, or in other cases of 22½°. Number of experiments 477 ; number of subjects 4.

The limitations of the localizing power, and the entire lack of constancy of localization on the part of different subjects, are shown everywhere throughout this series. It is highly probable that any table of usual localizations for a given pair of sounds would be valid for the individual alone from whose observations the table was compiled. The differences in the shape of ear and head, as well as the differences in the hair, beard, etc., are so marked and are such important factors in determining any particular localization of sounds within a few feet of the head that no one individual's special localizations can

[1] It should be remarked at the outset that the phantom-sound seemed usually to be in the horizontal plane, whenever the objective sounds were there. This may have been due partly to suggestion. Occasionally, however, the sound seemed to come from a point above or below the plane, as one would naturally expect.

be considered a standard for those of another. In the description of results, therefore, there will be no attempt to establish a system of subjective localizations corresponding invariably to certain objective combinations and valid for all observers. Such a task would be both hopeless and useless. Several general statements, however, can be made. (*a*) *For any given point in either of the two quadrants upon one side of the median plane a point may be found in EACH of the two quadrants on the opposite side which in combination with the first will give a localization in the median plane at 0° or 180°.* For example, a sound at 45° will give a subjective sound at 0° or 180° not only with its symmetrical at 315°, but also with a sound in the third quadrant (180°–270°). The following tables taken from the records of nine different observers will make this clear.[1]

TABLE II.

COMBINATIONS GIVING SUBJECTIVE SOUND AT 0°.

From 1st and 3d Quadrants.						From 2d and 4th Quadrants.			
		Observers.							
		P.	W	M.	B				
22½°	25°	45°	45°	45°	45°	110°	112½°	135°	150°
247½°	225°	220°	230°	245°	255°	290°	337½°	337½°	350°

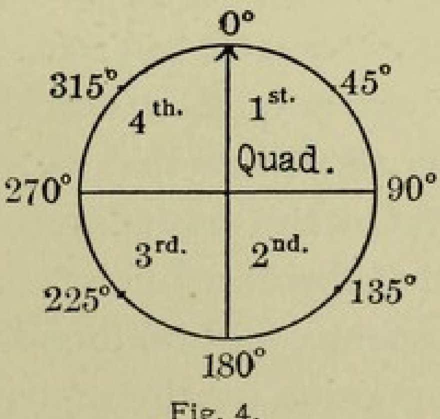


Fig. 4.

[1] The reading of these and subsequent tables may perhaps be facilitated in many cases by the constant presence in the field of view of some such reference-circle as the following.

Table III.

Combinations Giving Subjective Sound at 180°.

From 1st and 3d Quadrants.			From 2d and 4th Quadrants.			
45° 230°	45° 235°	67½° 247½°	110° 290°	112½° 315°	135° 330°	150° 350°

The particular combinations here given are of course valid only for the observers from whose records they were taken. That some such combination, however, can be found always is beyond question, and the statement at the head of the section may be regarded as universally true.

There is an interesting corollary to the above. If one of the telephones be made to remain constantly at 90° or 270°, the second telephone may be moved through a region of considerable magnitude, *the subjective sound remaining meanwhile constantly in the median plane.* Thus if one telephone remain at 90° the other may be moved, in the case of B., from 255° to 285°; and, in the case of P., from 220° to 320°, without causing the subjective sound to issue from the median plane. Or, if the resting telephone be at 270°, the other may be moved, in the case of B., from 70° to 105°; and in the case of P., from 70° to 120°. During this process of movement, there may to be sure be a shifting of the sound from 0° to 180°, but that as we shall see later is to be expected.

On the basis of the foregoing, it is highly probable that for every point in any quadrant a region of some extent could be found, both in its symmetrical and in its opposite quadrant, within the limits of which any objective sound would combine with the fixed sound to give a subjective localization in the median plane.

(β) But we may state a more general principle, of which the foregoing is only a special case. *For any given point in either of the two quadrants upon one side of the median plane, a point may be found in each of the two quadrants on the opposite side which in combination with the first will give the same subjective localization.*

TABLE IV.

COMBINATIONS YIELDING THE SAME SUBJECTIVE LOCALIZATION.

SUBJECTIVE LOCALIZATION	OBSERVER B.	SUBJECTIVE LOCALIZATION.	OBSERVER P.
10°	50° with { 230°, 350°	10°	30° with { 230°, 350°
25°	100° with { 210°, 310°	15°	45° with { 247½°, 337½°
40°	120° with { 260°, 320°	20°	50° with { 210°, 350°
			110° with { 210°, 310°
340°	10° with { 250°, 290°	340°	10° with { 230°, 310°

This table will serve to illustrate the principle enunciated. It is noteworthy that the combinations reported were not found by special search but occurred in the course of general experimentation.

(γ) Of a similar purport to the facts noted in (α) and (β) are the two further facts *that the same individual may at different times locate a given combination in two different quadrants;* and *that different individuals may locate the same combination in different quadrants.* In illustration of these facts the following tables of localizations may be given.

TABLE V.

THE SAME SUBJECT AT DIFFERENT TIMES.

OBJECTIVE COMBINATION	SUBJECTIVE LOCALIZATION.
0°–225°	190° and 350°
0°–247½°	230° and 300°
112½°–180°	30° and 130°
135°–180°	30° and 150°
180°–337½°	200° and 340°

TABLE VI.

SAME COMBINATION WITH DIFFERENT INDIVIDUALS.

OBJECTIVE COMBINATION.	SUBJECTIVE LOCALIZATION.		
	OBSERVER B.	OBSERVER P.	OBSERVER S.
10° –190°		355°	185°
10° –230°	220°	340°	
10° –290°	340°	330°	190°
30° –210°	20°	5°	175°
50° –230°	10°	30°	175°
90° –190°	60°	120°	165°
90° –210°	150°	40°	160° or 40°
90° –250°	160°	15°	175°
90° –350°	20°	35°	170°
112½°–135°	15°	170°	
150° –250°	200°	340°	185°
150° –330°		350°	190°
170° –290°	250°	320°	

These figures represent not conjectural judgments but localizations that for the most part are sharp and definite. I say "for the most part," because there was occasionally a doubt expressed in reference to the quadrant from which the sound seemed to come. That is, while according to the first impression the sound might seem to issue from the 2d quadrant, before its cessation it would jump to a point in the 1st quadrant. Subjects were always advised to indicate any uncertainties, and when such were known the sound was repeated. Thus the recorded judgments may be taken to stand for definite subjective localizations. It is obvious, therefore, that the differences shown in the above tables, both those between individuals and those for the same individual at different times, cannot be attributed to vague feelings of position helped out by conjecture, but *must be due to the fundamental confusion between points in anterior and posterior quadrants*, of which the confusion between 0° and 180° is but a limiting case. Thus in Table V. the combination 0°–225° gives 190° and 350°. The first may be understood if one suppose the sound at 0° to have passed into its equivalent at 180°, while the localization at 350° becomes comprehensible by supposing 225° to have been represented in the combination by a sound from an equivalent position in the 4th quadrant.

Similarly the last localization in the same table shows a confusion in the opposite direction. 180°–337½° is placed at 200° and 340°. Placing at 200° would be accounted for by a shifting of the sound at 337½° to a point in the 3d quadrant; and placing at 340° is readily explained by a shifting of 180° to 0°.

TABLE VII.

N. B.—THE SIGN (=) MEANS "PLACED AT."

0° SHIFTED TO 180°.	180° SHIFTED TO 0°.
0° [180]–247½° = 230°	22½°–180° [0°] = 20°
0° [180]–157½° = 170°	45° –180° [0°] = 30°
	67½°–180° [0°] = 40°
	90° –180° [0°] = 50°

In the same way it might be shown that the differences exhibited in Table VI. are to be reduced to the presence or absence of this confusion of quadrants. Instead of endeavoring to show this in detail, let me append a small table of localizations where undoubted confusions are to be seen. (Table VII., p. 65.)

3. The two sounds placed symmetrically, the intensity of one gradually increased by causing it to move along a graduated radius towards the head of the subject. Number of experiments, 312. Two subjects.—The two extreme positions of the sound in this case are, first, in the median plane, either at 0° or 180°, and, last, at a point approximately that occupied by the nearer sound when the latter has approached to within about nine inches of the head. The interesting feature of this series lies in the consideration of the region traversed by the subjective point as it passes between these extremes. These movements are typified by the following taken from the records of subject B. 90°–270° was located at 0°, and when the intensity of the sound at 270° was increased, the subjective sound moved from 0° (360°) to 270° *through the 4th quadrant.* On the following day the same combination was located at 180°, and, the same change of intensity ensuing as before, the subjective sound moved from 180° to 270° *through the 3d quadrant.* Again 45°–315° was located at 0°, and as the intensity of the sound at 45° was increased, the subjective sound moved towards 90° *through the 1st quadrant,* while on a later date the same combination was placed at 180° and moved from there to 90° *through the second quadrant.* On still another date the same combination was located at 0°, and when the intensity of the sound at 315° was increased, the subjective sound was now in the *4th* and

now in the *3d* quadrant, until finally it settled in the former near 290° and moved from there to 270°.

Aside from the general quadrant confusion with which we are already familiar, what should be emphasized here is *the influence of the first localization.* If that be at 0° the attention is more or less predisposed to that direction and the movement perceived is usually continuous from that point. If, however, the primary localization be at 180°, the subsequent movement will be determined by that fact.

(*b*.)

Both sounds on the same side of the median plane, in all possible combinations, with intervals of 22½°. Number of experiments, 216. Three subjects.—The results of this series exhibit once more in a somewhat new form the confusion between the anterior and the posterior quadrants.

TABLE VIII.

Objective Combination.	Subjective Localization.	
	Observer B.	Observer H.
0° - 45°	150°[180°-135(?)]	20°
0° - 67½°	160°[180°-140(?)]	25°
0° - 90°	130°[180°- 90°]	80°
22½°- 90°	120°[150°(?)-90°]	40°
45° -135°	100°	45°[45° - 45°]
67½°-180°	45°[67½°- 0°]	60°[67½°- 0°]
180° -247½°	200°	335°[0° -300°(?)]
180° -270°	200°	310°[0° -270°]
180° -337½°	200°[180°-225°(?)]	350°[0° -337½°]
225° -315°	230°[225°-240°(?)]	310°[300°(?)-315°]
247½°-337½°	200°[247½°-190°(?)]	320°[292½°(?)-337½]
270° -315°	240°[270° -225°(?)]	300°

When both sounds are in a forward quadrant, the resultant may be located in the rear quadrant on the same side, and *vice versa*. For when the two sounds are in different quadrants, one observer may locate in one quadrant and another in another. In all cases the one prominent fact is the existence in the forward and rear quadrants of pairs of points so related that it is practically indifferent from which one of them the objective sound issues. The above table presents this in definite cases. The square brackets contain the conjectured equivalents of the actual sounds.

The confusion of points in the anterior and posterior quadrants is too obvious from the above table to demand any extended comment. In fact it is in the highest degree probable, although the range of our experiments is not sufficiently great to state this with detailed verification, that if one component of the sound remain fixed in any quadrant, while the second be made to assume all possible positions in the *other* quadrant on the same side of the median plane, the same subjective localizations will be secured as would result from appropriate placings of the second sound in the *same* quadrant with the other (fixed) sound.

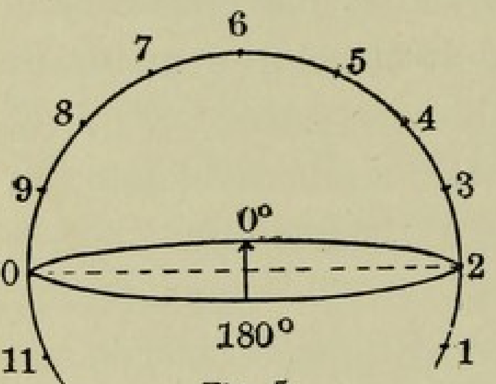


Fig. 5.

(B) Two Telephones on the Transverse Arc.

Number of trained subjects, two. Number of experiments, 110.

In these experiments only that position of the transverse plane was investigated that is represented in the above figure (Fig. 5). The eleven positions were at intervals of $22\frac{1}{2}°$, and all possible combinations of these were made. Some of the subjective localizations here are of peculiar interest.

In the first place we find no such simple locating within the plane concerned as was the case for the horizontal circle. There are of course some localizations in the plane, but *more than half of the total number of sounds given are placed without the plane.* Let us look briefly at the details. *Sounds placed symmetrically with respect to the median plane were, with a single unimportant exception, located at 180° on the horizontal circle.* It was of course to be expected that such sounds would be located at some point in the median plane, for each ear received the same intensity of sound. And it may be that the particular point taken in the median plane—180°—was determined by habits of localizing established in the course of the previous series. Still it is worthy of note that the procedure was entirely with knowledge, and that the subjects were consequently expecting sounds in the transverse plane. For this reason an expression of surprise always accompanied the first localization on the horizontal circle.

Other combinations also of sounds, one on each side of the median plane, were given the same subjective location as the symmetricals. Twenty of these in all were placed at 180°. Such combinations are, *e. g.*, 1–9, 2–8, 3–11, 4–10 (following the positional designations of Fig. 5).

Three times such a combination was located above the head (2–7, 3–10, 4–7), but, with these exceptions, *not* *a single combination of this kind was located on the transverse arc.* The localizations given are of the highest interest. They are brought together in the following table.

TABLE IX.

OBJECTIVE COMBINATION.	SUBJECTIVE LOCALIZATION.
1– 8	150° on horizontal circle.
3– 7	140° " " "
3– 8	160° " " "
5– 9	230° " " "
5–10	240° " " "
3–11	230° but 30° below horizontal plane.
4–11	250° " 10° " " "
5–11	250° " 30° " " "
5– 8	105° " 10° above " "

In this table we find splendid instances of just what the intensity-theory would lead one to expect. For on the basis of this theory, not only should such confusion-points exist as we found in considering the horizontal circle, but others also of a much more general sort. Consider the median and transverse arcs, together with the circumference of the horizontal circle as lying upon the surface of a sphere, at the center of which the head is placed. Let this be thought of as divided by the median plane into right and left hemispheres. Now upon the surface of either of these hemispheres *an innumerable number of points must exist from which a sound would give the same relative intensities to the two ears.* The locus of these points for any given relation of intensities must naturally be a very irregular curve depending principally upon the form of the head and the characteristics of the outer ear. It is completely within the line of expectation then that while experimenting with sounds in the transverse plane pairs of points should be found, whose subjective equivalent is the same as that of points lying in the horizontal, or other, plane. Practice in localizing in the horizontal plane may have had much to do with the particular placings in the examples given, but the main fact to be emphasized is *that they were not in the transverse plane.* A glance at the table shows that all the localizations are in the 2d and 3d quadrants, according as the greater intensities come from the sound upon the right or left. It is of course clear that the higher a sound is upon the transverse arc the less will be the intensity received by the ear upon that side.

It remains only to state that when the two sounds were upon the same side of the transverse arc the resultants were usually placed at points of varying height upon this arc. Still out of the 30 possible combinations here, 9 were located out of the transverse plane.[1]

III. THREE SOUNDS, ONE IN EACH OF THE THREE PLANES.

Number of experiments, 334. Two subjects.—Apart from those median-plane-localizations resulting from certain definite combinations (see Chap. 2), no results were obtained which seem capable of general formulation. Individual differences were very marked here.

In pausing for a moment to reflect upon the experimental material thus far adduced, we see that we have been persistently met at every turn by the fact that the

[1] With the above experimental results from the use of two sounds compare the statement of Matsumoto drawn from experiments of a somewhat more general character. He says: "Two component sounds of equal intensity at the same or different levels will give in combination a localization at the middle point between the two points at which the components are placed." (Loc. cit., p. 50.)

manner in which the sound-intensities are distributed to the two ears is a paramount factor in determining the localization of sounds. The use to be made of this fact in developing a theory of auditory space perception can first be seen when our discussion has advanced much beyond its present position. In the meantime the fact is to be carefully noted and verifications are to be everywhere looked for. None of the other theories mentioned in our historical survey seems to have met with any positive confirmation. If semicircular canals play any part in the matter of localization, they certainly perform their part very inefficiently and show themselves liable to a wide range of illusion. Nor, in the second place, has anything been found to suggest that the sensations of the two ears are originally different. Further, tactual sensations in the shell and drum of the ear appear also to play at most only the obscurest part possible in localization. That they do not coöperate in determining the side from which a sound comes has indeed not been shown. But such results as we have seen in sections *a* and *b*, particularly in the second division of the former, may well be regarded as creating a strong presumption against their existence. For if they were present in any passable grade of intensity it would be difficult indeed to understand how the same objective sounds could be assigned widely varying subjective positions at different times, and how, on the other hand, widely different objective sounds could be located at the same subjective spot. Yet it is exactly this that we found above to be true. Just these vagaries of localization, however, that we have seen in such abundance, are the immediate consequences of the intensity-hypothesis. At this stage of the matter we may accordingly feel some justification in expressing ourselves as well disposed towards this hypothesis and somewhat suspicious of the applicability of the others mentioned.

IV. Two Sounds of Different Quality.

Attention has already been called (Weber, von Kries, Stumpf) to the fact that when two qualitatively different sounds are given one on each side the observer can correctly designate the side from which either sound comes.[1] Whether, however, this ability is great enough to distinguish not only the side *in general* from which either sound may come, but also *the particular and more detailed directions* within these lateral regions, does not appear from the reports referred to above. It was, therefore, for the purpose of contributing somewhat to a knowledge of the accuracy of this sort of localization that a series of experiments was carried out. The sounds employed were a telephone and an electric bell in the first series, and two bells in the second series. In both cases the sounds occurred with strict simultaneity. All possible combinations were made, with one sound on each side, of points on the horizontal circle separated by 45°. Too few experiments (154 in the first case and 92 in the second) were made to justify the formulation of any binding laws, but several interesting tendencies were noticed with all the four subjects engaged. Essentially the same results were obtained in the two series, and since they are more definite and striking where telephone and bell were used, these only will be reported.[2]

[1] Urbantschitsch also mentions this fact. Pflüger's Archiv., 24 (1881), 582.

[2] See chap. 2 for the results where both sounds were in the median plane.

1. The quadrant confusions were here again in evidence, the sound affected being usually that from the telephone.

TABLE X.

OBJECTIVE SOUND.	TELEPHONE SOUND PLACED AT.	BELL PLACED AT.
Telephone 135°, Bell 270°	50° and 60°	Correct.
T. 135°, B. 0°	50°	"
T. 225°, B. 45°	300°	"
T. 135°, B. 315°	45°	"
T. 135°, B. 180°	45°	"
T. 135°, B. 225°	45° and 50°	"
T. 225°, B. 180°	300°	"
T. 180°, B. 315°	0°	245°

2. Though every combination given was correctly localized at least once by one or more of the four subjects, there were continually manifestations of tendencies to misplacements which do not occur with a single sound. There was, namely, a tendency to misplace sounds in the direction of 90° on the right and 270° on the left. That is, sounds from the forward quadrants tended to be thrown back towards the transverse plane, and those from the rear quadrants to be thrown forwards towards this plane. This tendency was somewhat more marked in the latter case than in the former. The following tables contain the results obtained. (Tables XI. and XII.)

TABLE XI.

OBJECTIVE POSITION.		TELEPHONE.	BELL.
135°	Correctly located.	6 times.	16 times.
	Located 25°–45° forward of 135°.	6 "	3 "
	Located to rearward of 135°.	1 "	1 "
225°	Correctly located.	10 "	12 "
	Located 25°–45° forward of 225°.	7 "	
	Located 5°–45° " " "		9 "
	Located to rearward of 225°.	0 "	0 "

TABLE XII.

OBJECTIVE POSITION.		TELEPHONE.	BELL.
45°	Correctly located.	14 times.	12 times.
	Located forward of 45°.	0 "	0 "
	Located at 90°.	4 "	2 "
315°	Correctly located.	12 "	8 "
	Located forward of 315° (at 325°).	1 "	0 "
	Located 15° and 45° to rearward.	2 "	
	Located 15°, 35° and 45° (4) to rearward.		6 "

Insufficient as these experiments are for the elaboration of any absolute doctrine, they yet display a preponderating tendency to make misplacements, when such occur, in the direction of the line passing through the two drum-membranes of the ears, that is, a tendency under these circumstances to locate towards the point of greatest absolute intensity for the ear concerned. The explanation for this fact may be sought presumably in the consideration of the "dynamogenic" influence exerted by one sound upon the sensations of the opposite ear. Urbantschitsch has called attention[1] to this mutual influence of the two stimulations received one by each ear. If, *e. g.*, a person somewhat deaf cannot hear the

[1] See 45, B. Also Le Roux (21).

ticking of a watch held near one ear, the stimulation of the opposite ear by means of a tuning fork will so act upon the auditory apparatus (central and peripheral) that the ticking becomes audible. Or, in general, if two non-fusing sounds are given one to each ear, the intensity and fulness of either is increased by the presence of the other. In the results under consideration, this seems to have been exactly the case. The intensity of either sound was increased by reason of the fact that the two were sounding together, and increased principally, if not wholly, for the ear on its side. The relative intensities of either sound for the two ears were accordingly changed in such a way that a tendency always existed to locate the sound from either side at its point of highest intensity, that is, at the point directly opposite either ear, at 90° or at 270°. Consider, for example, that the telephone is upon the right and at about 45°. The presence of a bell sounding upon the left increases the intensity of the telephone sound *for the right ear*, so that, the intensity of this sound remaining the same for the left ear, the distribution of intensities has become such as it would be if a telephone were sounding alone at 90°. Hence the illusory subjective localization. This interpretation is strengthened by the consideration of further results, to which we now turn.

3. In addition to the false localizations reported in Tables XI. and XII., there occurred a sort of misplacement that is of wholly unusual nature in sound-localization, namely *a misplacement out of the median plane.* The testimony of all writers is uniformly to the effect that, in the cases of a single sound coming from the

median plane, good observers never make misplacements into the regions lying at the right and left of this plane. That is, median plane sounds are always recognized as such when there is but one sound. In our experiments with *two* sounds, however, (both where bell and telephone, and two bells were used), there were twelve cases where one of the sounds was falsely located 20°–50° from the 0° or 180° point, and *always to the side opposite that on which the second sound was situated.* This result again becomes intelligible by reflecting on the mutual reinforcing of two sounds as described above. The sound situated in the lateral region so influenced the character of the sensation of the opposite ear that the latter's share of the intensity from the sound in the median plane became subjectively increased. And the only possible adjustment, in spatial terms, to this one-sided increase of intensity was to locate the sound out of the median plane—the plane of equal intensities—to a point nearer the affected ear. Here again, then, the alteration of the distribution of intensities produces an accompanying change in the localization of the sound from which the intensities come.

But if it be true that the intensive stimulation of one ear is increased by the simultaneous stimulation of the other, why were not *all* sounds in the rear and forward quadrants pushed towards the transverse plane, and why did not misplacements occur in *all* cases where one of the two sounds was in the median plane? For the fully satisfactory answer of these questions there seems to be no sufficient material in reference to the mutual influence of sounds. One consideration, however, is at least enlightening. Urbantschitsch remarks that the effect produced was in some cases exactly the opposite of that described above. That is, the intensity of a sound upon one side was sometimes *weakened* by the simultaneous presence of a second sound upon the other side. In conformity to this we found also that in a few cases (*six* out of a possible sixty) one sound was misplaced from the position of greatest unilateral intensity (90° or 270°) from 10° to 45° to the front or rear. This would seem to indicate a lessened intensity upon the side in question, and consequently such misplacements cannot be looked upon as furnishing evidence antagonistic to the position we have taken, but simply as illustrating the spatial counterpart of the occasional intensity-effect just noted.

But if a sound may sometimes weaken, though usually strengthening, the sensation received by the opposite ear, it is not unnatural to suppose that the mutual influence may sometimes also fail altogether. At least it may fail to produce marked enough results to become evident under the form of a particular false localization of sound. Manifestly there is need of a much wider range of experiments here to warrant any dogmatic conclusions. Still the conclusion suggested has no small degree of plausibility, and it has the distinct advantage of making intelligible the curious localizations that we have found in the use of two non-fusing sounds.[1]

[1] In reporting the experiments referred to above Urbantschitsch says (B, p. 282): "Ein auf das eine Ohr einwirkender schwacher Ton erfährt *bei vielen Versuchspersonen* eine deutliche Verstärkung, sobald dem andern Ohre gleichzeitig ein stärkerer Stimmgabelton zugeleitet wird etc." The italics are mine, but the insertion of the phrase italicized would seem to show that the reinforcement mentioned was by no means universal.

CHAPTER 2.

LOCALIZATIONS IN THE MEDIAN PLANE.

A. HISTORICAL.

RAYLEIGH, 1876. PREYER AND ARNHEIM, 1887.

IN the writings already referred to in the previous chapter, these writers have commented briefly upon the practically universal tendency to confuse sounds coming from directly in front and directly behind. Rayleigh found that the position of a tone was more readily confused with its opposite, than that of a noise or of the human voice. Preyer (30) and Arnheim (1 ½) found false localizations occurring at each of the positions in the median plane that they investigated,[1] but at the same time they discovered more or less ability to make correct

[1] Preyer, for example, loc. cit., p. 599, gives the following confusions as occurring (see Fig. 6):

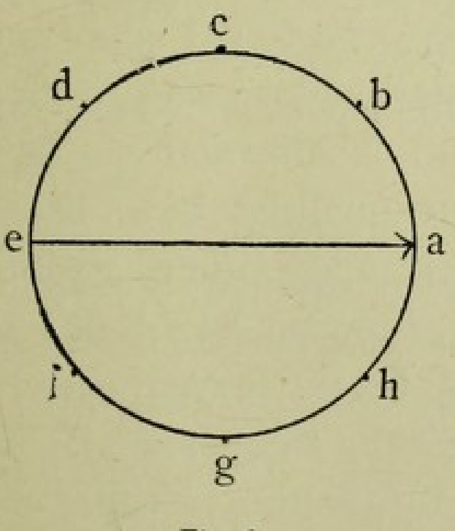


Fig. 6.

a	confused with		b, c, h, f, e.
b	"	"	c, h, d, f, h.
c	"	"	d, e, h, f.
d	"	"	e, f, h, g.
e	"	"	f, g, h.
f	"	"	g, h.
g	"	"	h.

localizations. Thus sounds lying directly in front and behind in what we have called the horizontal plane were at times correctly located, but there was a constant tendency to misplace them to points somewhat above the horizontal, whereas sounds actually placed at these latter points were correctly localized. This tendency to elevate a sound to another point in the median plane was seen also in the case of sounds coming from directions below the horizontal.

VON KRIES, 1890.

Von Kries (17, B) started from the consideration that if sound localization is based upon a comparison of intensities, it would seem that a sound within the median plane could be localized only to the extent of saying that it was within that plane, and nowhere else. The particular position within that plane, however, could not, it would seem, be determined with any accuracy whatever. Notwithstanding this, investigators have found some ability to recognize positions within the plane, and there must therefore be additional factors operative here in the formation of judgments. That is, localizations in the median plane must be "mediate" and based upon modifications of quality which the sound undergoes as it moves from place to place. Successful localization can occur, accordingly, only as the various qualities have become attached by experience to their appropriate positions. This association of quality and position von Kries suspects must have come about in the experiments of Preyer, since he constantly used the same sound. Our author proposes therefore to obviate the possible effect of experience by altering from experiment to experiment the quality, the intensity and the distance of the sound employed. Various sound-qualities were readily obtained by using telephones, whistles, closed and open reed pipes, and coins or wooden disks to be snapped together.

The method of procedure was to select some pair of positions from those indicated in the accompanying figure (Fig. 7), the duty of the subject being simply to state from which of the two known positions the sound seemed to come. The results may be summarized as follows:

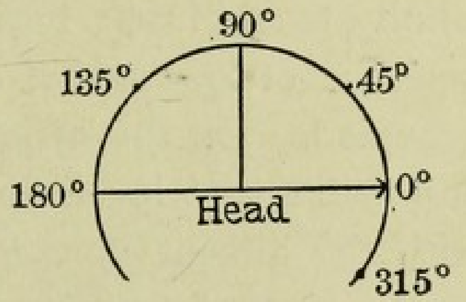


Fig. 7.

1. There is a fairly good ability to localize sounds within the median plane, even under the above adopted conditions of changing quality and intensity. This ability increases with practice.

2. Along with this ability there goes, however, an extraordinary uncertainty in reference to certain positions under certain circumstances.

3. There are individual tendencies to locate at particular points. These tendencies may change from day to day.

4. Certain qualities of sound are more readily located than others, but there is no tendency to place particular kinds of sound in particular directions.

5. The ability to distinguish front and behind (0° and 180°) is not great. To eliminate the possible effects of practice, 22 subjects were asked to give a few judgments apiece. Out of 111 experiments 47 localizations were

correct. One subject only never made an error, though in two special series of 30 and 32 experiments respectively he refused three times to give a judgment.

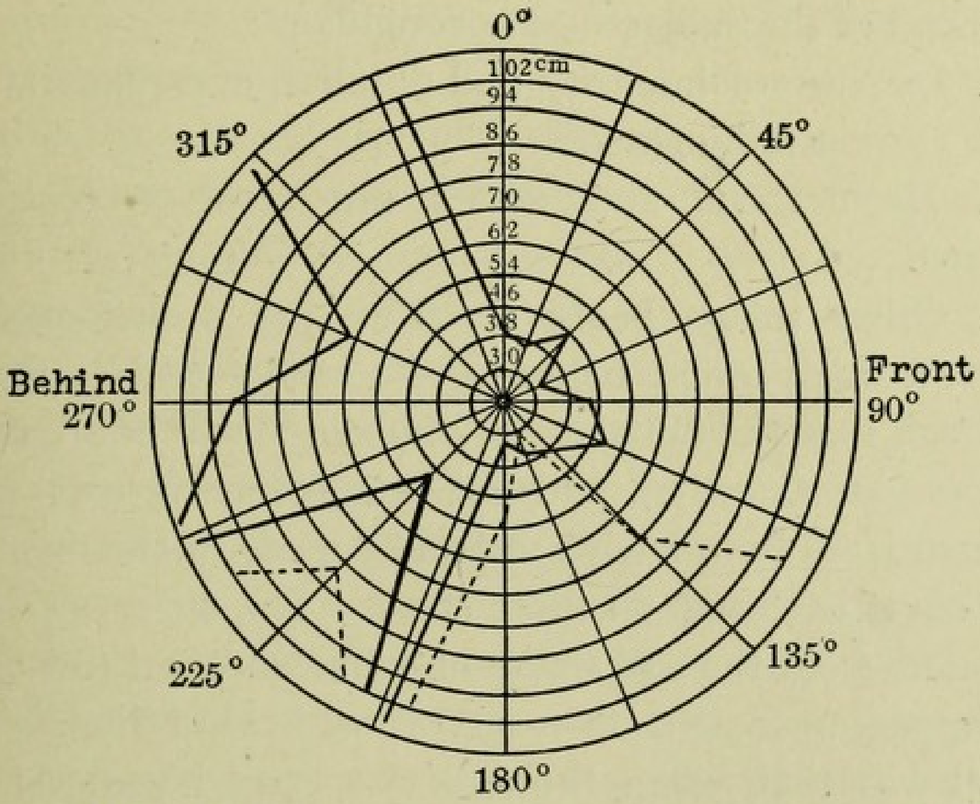
It was never found that a weaker sound was with preference placed behind and a stronger in front.

With the theoretical interpretation of these results von Kries finds difficulty. The ability to localize is too good under the conditions imposed by him to make the theory wholly tenable that localizations are based upon the experience of qualitative differences due to the positions occupied. On the other hand the striking uncertainty and inability to localize occasionally manifested, together with the almost universal tendency to make misplacements of 180° from front to back and *vice versa*, would seem to preclude such a physiological theory as that of Preyer (p. 35), since it is hardly conceivable that in these cases the physiological mechanism should do its work so badly. On the whole, nevertheless, it seems most plausible to suppose that localizations in the median plane are made possible by perceived differences of positional quality, though one may be no more conscious of these differences than in the case of vision where differences in visual quality mediate differences of distance in the visual field.

6. When a noise and a tone were given simultaneously, either both together at 0° or 180°, or one at each point, there was a fair success in giving each its proper location. The greatest difficulty arose when the noise was behind and the tone in front. Here both were placed behind. In several cases the tone was misplaced to the position of the noise.

BLOCH, 1893.

Bloch's experimental determination of the delicacy of localization within the median plane is graphically given in the accompanying diagram. (See Fig. 8.)



Median Plane. ——— Binaural.

Fig. 8. - - - - - - Monaural.

The heavy line in this, as in Figs. 11, 12 and 14, represents the locus of the various grades of delicacy, found as described on pp. 40-41.

Now since, very obviously, there can be no changes of relative intensity in this plane, the peculiarities of the above figure must rest wholly upon the form of the shell of the ear. The striking influence of the latter can be immediately seen by noting the utter helplessness in the posterior half of this plane and the relatively great ability in the anterior half where the influence of the shell can be felt.

In a later portion of his monograph (5, pp. 52–6) Bloch endeavors to determine more clearly than heretofore the conditions that underlie the front-back confusion. He employs high tones and the sharp click of a ratchet, and gives the sounds at distances of 0.6 m.–2.4 m. from the head of the subject. He concludes:

1. Increase of intensity and duration of sounds facilitates correct judgments.

2. High tones and noises are most successfully located.

3. Other things being equal we are accustomed to locate a weak sound behind and a loud sound in front.[1]

Bloch sees the influence of the outer ear in all these results (see Chap. 4), and this conjecture is somewhat substantiated by a few experiments made in the open air in a court enclosed on three sides. The observer stood 5 meters from the end wall and pebbles were throw upon the stone pavement in front of or behind him. The result was that, when the face was turned towards the wall, the legitimate influence of the pinnæ was merely increased and the localizations were mostly correct. When, on the contrary, the back was turned to the wall, sounds coming from behind were apt to be falsely located in front, since now the reflection of sound-waves by the wall produced an unwonted intensity in the sound.

B. EXPERIMENTAL.

I. *Two or more sounds used in connection with the apparatus described in Chap. 1.*

[1] Compare Matsumoto, loc. cit., p. 51.

1. The front-back localization.—Though von Kries and Bloch have both investigated to some extent the circumstances under which this well-known confusion occurs, the determination of the exact conditions attending it seems not yet to have been made. It was with the hope of contributing something to the resolution of the difficulties in the way of an explanation of this matter that the experiments in this section were made.

(*a*) When two fusing telephone-sounds are given at 0° and 180° the general tendency seems to be to place the combination at 0°. 10 out of 14 such combinations (71%) occurring at random in the midst of other series were located at 0°. But individual differences are very marked in this matter, and in a way that seems almost inexplicable. Thus subject C. located this combination invariably at 0°. In a series of nearly 200 experiments, with the intensities of both telephones varying in every possible fashion, not a single localization was made at 180°. There were, naturally, changes in apparent distance, but no change in direction.

On the other hand, subject S. nearly always located this combination behind, and that, too, under the condition of changing intensity mentioned in the case of C. But if the forward sound were much increased, confusion and uncertainty followed, and, when the forward telephone was held at the end of the subject's nose, the phantom-sound was located at 0°, but with a feeling of confused indefiniteness.

With others, however, who seemed to have no such curious predilection for either direction, an increase of the intensity of either sound was sure to be followed by a localization at 0°. In our records of experiments not especially directed to the investigation of median plane localization, we find six cases where the 0°–180° combination was given with one sound more intense than the other. In all these cases the localization was at 0°.

(*b*) When two non-fusing sounds, as a bell and a telephone, or two bells, are given simultaneously at 0° and 180°, there seems to be *a greater tendency to misplace from 180° to 0° than in the contrary direction.* Our experiments are few in number, but the results given in the following table seem significant:

TABLE XIII.

Objective Position.	Number of Experiments.	Both Correctly Located.	Both at 0°.	Both at 180°.
Telephone–Bell				
0°–180°	4	4	0	0
180°– 0°	6	3	3	0
Bell–Bell				
0°–180°	8	4	4	0

There would appear, therefore, to be some ground for accepting the statement of Bloch that we are prone to locate the weaker sound behind and the louder in front. For, first, the *general* localization of the fused resultant of the 0°–180° combination is at 0°, both when the sounds are of equal and of unequal intensity. But in either case the combination-sound is louder than a single sound in either position. And, second, when two non-fusing sounds are used, the false localizations are apparently more apt to be those made at 0°, and in this case it would seem not far wrong to suppose a mutual reinforcement of the sounds after the manner described by Urbantschitsch, in consequence of which the increased intensity at 180° causes the subjective localization at 0°.

2. Two telephones (sounds fusing) symmetrically placed one on each side of the horizontal rim at intervals of 20° or 22½°. Number of experiments, 587. Six subjects.—Naturally the resultant here will in every case be placed in the median plane, and, as a matter of fact, the localizations were almost without exception in the direction either of 0° or 180° in the horizontal plane. Very rarely the phantom-sound seemed a few degrees above or below the plane. The particular question to be answered here is obviously in regard to the conditions that determine whether the localization shall be at 0° or at 180°. (*a*) In the first place, there are marked individual differences, some having a decided preference for one position, others for the other. Thus, subject R., on two different dates separated by an interval of three weeks, located all symmetrical sounds at 180°. Even the combination 5°–355° was thus located, the subject remarking, however, that the resultant was in this case nearer the head. On the other hand, certain other subjects were relatively more inclined to locate at 0°. Still it is significant to note that *no subject was ever found in the course of some 500 experiments who persisted in localizing all symmetrical combinations at 0°.* Again, the same individual differs at different times, and on the same or consecutive days the same objective combination will be placed with equal certainty now at 0°, now at 180°. (*b*) Secondly, there is usually a point at which the phantom-sound seems to shift from front to back, or *vice*

versa. Thus, subject B. usually located at 0° all pairs of sounds from 10°–350° to 80°–280°; from here to 150°–210° the sound was located now in front and now behind, while all symmetricals back of this were placed at 180°. S. vibrated irregularly from 0° to 180° for the region between 10°–350° and 60°–300°, while back of the latter point all sounds were placed at 180°. Two other subjects located at 0° up to 80°–280°, back of which they vacillated backwards and forwards up to 160°–200°. Two others still wavered in the region between 60°–300° and 110°–250°, while all sounds in front of this were placed at 0° and all behind it at 180°. The general deduction is, accordingly, that all symmetrical sounds in front of 60°–300° are most likely to be placed at 0°, and those back of 110°–250° at 180°. And this state of affairs is precisely what we ought to expect if we are to allow any decisive influence to the pinna or to the intensity of the resultant sound. As to the influence of the pinna, no direct correlation was made between its form and inclination and the points at which for a given individual the symmetrical combinations began on the one hand to be located less surely at 0° and on the other hand definitely at 180°. But it seems by no means unwarranted to suppose that such correlation exists. Subjective changes of a more or less marked character, both in the total intensity of a sound and in the intensity of its components must certainly occur at certain points. And, since we have already found reason to suppose that weaker sounds are more likely to be located behind, we can see why in the present case a position was reached sooner or later back of which no combination was ever for any observer located in front.[1] As to direct evidence for the influence of intensity we have two series of facts: (1) In any set of experiments, widely separated as well as closely adjacent positions were of course given consecutively, to avoid any particular habit of localization that might otherwise arise. When now such widely separated positions were given consecutively, the subject frequently remarked one of two things, either that the two phantom-sounds differed in intensity or that they differed in distance from the head. The greater intensity or the diminished distance were noticed for sounds coming from in front of the transverse plane, the reverse for sounds behind it. Thus subject S., who on a certain date located at 180° all symmetrical combinations given in irregular order at intervals of 10°, remarked, in reference to every combination coming from in front of the transverse plane, either that it was loud or that it was nearer, while, for the eight combinations coming from behind this point, it was twice said that the sound was weaker and twice that it was more distant. (2) Any change of intensity, brought about by using the hollowed hand as a reversed ear-shell or by making the sound strictly instantaneous by means of pendulum and mercury contact in place of the regular key, was attended by a change in the habitual localization. Thus, for subject P., all sounds back of the transverse plane were located at 0° when their intensity was increased by the presence of the hands held palms to the ears. When

[1] Compare p. 69, where it is reported that sounds coming from symmetrical positions on the *transverse* arc were always located at 180°, and never at 0°. As far as the pinna is concerned, the conditions here and in the rear portions of the horizontal circle are nearly identical

the sound was instantaneous (and diminution of duration is equivalent to diminution of intensity), both B. and P., who ordinarily placed at 0° all symmetricals as far as 80°–280°, localized the one all at 180°, the other all but three at 180°.

We seem justified then in concluding once more that, at least in the case of sounds of known intensity, the louder is more likely to be placed in front and the fainter behind. And that this results from experience, developed on the basis of the usual influence of the pinna, is assuredly a most plausible supposition, especially if one recall the opinion of Rayleigh (see p. 28) that the partial tones of sounds coming from behind may be obstructed and kept from reaching the ear.

3. Two telephones (sounds fusing) on the median arc in all possible combinations at intervals of 22½°.

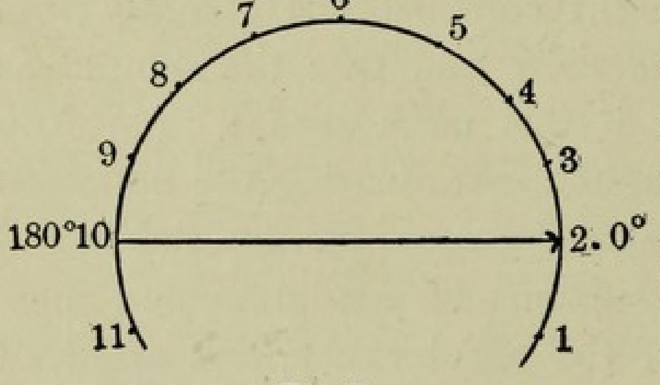


Fig. 9.
Number of subjects, 4. Number of experiments, 220.

The only noteworthy feature of these experiments is that out of a total of 220 localizations 130 were in front as compared with 80 behind, 10 being directly above or uncertain. That is, 59% were placed in front as compared with 36% placed behind. This result can have but one meaning, viz., that a greater number of misplacements occurred from the rear to the forward position than in the reverse direction. A particular case of this kind of misplacement we have already met in considering the 0°–180° combination (see p. 85). The same explanation must hold here as there: the greater intensity of the combined sound, together with the possible reinforcement of one by the other in certain positions, sufficed to turn the balance of localization to the front, where sounds are normally clearer and sharper.

4. Three telephones (sounds fusing), one opposite each ear on the horizontal rim, the third at 0° or 180°.—With the third sound at 180°, the resultant was always placed at 180°. With the third sound at 0°, there was no constancy of localization either for different observers or for the same observer at different times. Sometimes the localization would in succeeding moments vibrate without apparent reason from 0° to 180° and back again. Increasing the intensity of the third sound by approaching it from 0° to the head had no influence in one case where the localization was 180°. In another case, however, when the telephone had been brought half way to the head, the sound changed from "180° loud" to "0°."

The experimental results reported in this section are, it is clear, not without importance for the resolution of the question which we set out to answer. To be sure nothing has been learned in regard to the ability to locate single sounds in various parts of the median plane, for only double sounds have been used throughout. But on the other hand evidence has been thrust upon us from every quarter showing that *there is a widespread and clearly manifest tendency to subjectively locate a relatively loud sound in front and a relatively weak sound behind.* There are, to be sure, marked individual differences, and one may frequently be obliged to appeal to *suggestion* to explain persistent tendencies to locate in a particular spot; but on the whole the position assumed above seems to be well grounded in fact.

II. Education in Median Plane Localization.

While the experiments thus far described in this chapter have, as we believe, contributed some new material for the better understanding of the conditions underlying the usual confusions in median plane localizations, they have obviously furnished no data for the further comprehension of those factors that subserve *successful localizations*, when such occur. The only theoretical material here is the conjecture of von Kries, (p. 80), that such localizations are mediated, not by variations in the distribution of intensities to the two ears—since such variations cannot exist—but by differences in quality through which a sound passes as it moves from point to point in the plane. Our problem was therefore to determine experimentally whether such qualitative differences do actually exist, and, if so, whether subjects can be so trained that accurate and certain localizations can be made on the basis of these differences. Some confidence in the successful outcome of such training might well be gained from a consideration of Preyer's observation that for localization in the general field a subject in good practice gave 36 per cent. of correct judgments as against 18 per cent. given by an inexperienced observer, and that too when no special effort was made to utilize the effects of practice.

For the purposes of our experiments it seemed best to choose for investigation those positions which come very little within the range of normal experience, and which consequently are most readily and most universally confused. Such positions are to be found *close to the head and upper part of the body* in the median plane. Eleven points were selected as indicated in the accompanying figure (Fig. 10), position 10 being, however, omitted because of the obvious difficulties attending any attempt to reach it by any sound-producing instrument.

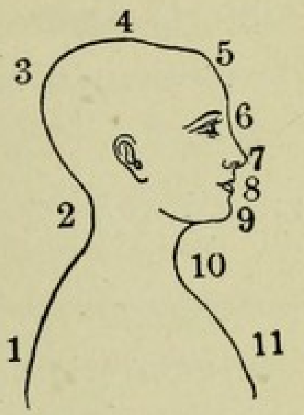


Fig. 10.

Nearly every one is acquainted with the utter inability to locate sounds given in these positions, notably perhaps those given in front of the face or below the chin. In fact the ludicrous mistakes made in any attempt at such localization has given rise in certain quarters to a parlor amusement, which consists in the endeavor on the part of a blind-folded subject to locate the sharp click made by coins snapped together in the positions indicated in the diagram. Since in such cases the localizations, though erroneous, are for the most part perfectly definite for the person experimented upon, the question naturally arises whether any universal tendency can be discovered to locate such sounds as the above preponderatingly in particular positions. The following table contains the results of experiments made for the purpose of answering this question. Twelve chance subjects were taken, and since only one series was given in each case, and since no one of the subjects had ever received any special practice in the localization of sounds, it would seem that any natural tendency should appear, if such exists. The sound used was, as before, that given by a telephone. The ten positions were given in irregular order, and the subjects knew the general character of the objective positions.

Table XIV.

Localizations Close to Head, Without Practice. Twelve Subjects. One Series Each.

Position of Sound.											Doubtful or on the eyes.	Omitted.
Objective.	Subjective.											
	1	2	3	4	5	6	7	8	9	11		
1	4	4				2				1	1	
2		6			1	3	2					
3	1		4		3	1	2				1	
4		2	3	1	2	2	1				1	
5		3	2	1	2	3	1					
6		2	1	1		4	2				1	1
7		3	1			1	3	1				3
8		4			1	2	2	3				
9		4				1	4	1	1	1		
11		6				1	3	1	1			
Totals.	5	34	11	3	9	20	20	6	2	2	4	4

The column at the left contains the objective positions of the sounds. The remaining columns contain the number of times that any sound indicated at the left was placed at the points indicated at the head of the columns.

We thus reach the curious result that position 2 (behind the neck) is *for these twelve subjects* both the position of greatest accuracy of localization and the point to which the largest number of sounds are misplaced (6 out of 12 sounds being rightly placed there, and a total of 34 localizations being made there). That these results are to be regarded as possessing universal validity is, however, not to be thought of for a moment. In a general way they may, to be sure, indicate a more or less pronounced partiality for certain localizations, but at the same time they can indicate this only in a general way by reason of the fact that a whole series of localizations may be determined by the *first* localization given. Thus B., out of a series of nine, made 7 localizations at position 7, where the first was made; and S., similarly, made 6 localizations at position 6. The influence of suggestion is so great that subjects must never have seen such experiments performed upon another, if a condition of experimental naïveté is desired. Such being the case, then, only an immense number of observations could suffice to eliminate the factor of suggestion and reveal a preference, if such there be, for any position or positions.

But to come to the matter of training. Three subjects took part in the experiments, C., E. and the writer. C. and E. were both careful observers and well trained in psychological experimentation. The sound was at first given by a ratchet or telegraph snapper, but after some days these were mostly replaced by the telephone. The sound-source was in each case at the end of a rod, the other end of which was held in the hand, and thus by simple devices the sound could be given without danger of revealing its position through the rustle of the garments.

The general method of procedure was this. After sufficient preliminary testing to obtain some knowledge of the individual peculiarities, a few positions of relatively wide separation were chosen (*e. g.*, 1, 4, 11) and the subject attempted to note the qualitative differences that existed, associating each quality, if possible, with its appropriate position. If the positions could not be readily recognized, this succession of sounds was repeated until the subject became confident that he could correctly discriminate them, after which this confidence was tested by giving the sounds in a different and unknown order, or by giving some one sound from the three. If the subject found himself master of these positions, others less widely separated were taken and the same sort of procedure continued as before. Thus the procedure was wholly *with knowledge*, and the subject was practically director of the experimentation, since his requests were always heeded in reference to repetitions, duration of sound or length of interval. This training and testing lasted usually from 15 to 30 minutes several times a week and was spread over the period from Sept. 20 to Dec. 21, 1897. The number of training periods for C., E. and P. were respectively 21, 29 and 26.

It is needless to follow day by day the varying fortunes and discouragements encountered. In anticipation of final results, however, let it be said here that the general outcome of the process of training was very gratifying. Absolute perfection in localizing was not striven for. But sufficient ability was obtained to warrant the thought that complete perfection is by no means beyond the reach of time and patience. Let us pass in brief review the results obtained and the theoretical inferences that seem to emerge from these.

Each Position has its Distinctive Qualitative Mark.

As little as one would suppose this to be wholly true, after seeing the wretched confusions made by any observer at the first attempt, a slight experience showed clearly that a direct comparison of any two positions revealed at once the differences in the character of the sounds. These differences were of course not uniformly great, and a later table containing results gathered during the training process, will give a fairly correct measure of the magnitude of these differences, the positions most subject to confusion being plainly those whose distinctive marks are most similar. After three or four sittings all positions could be qualitatively differentiated (though, naturally, not yet correctly *located*) with the exception of 3 and 4.[1] Presupposing that differences existed between these points, an effort was made to assist their discrimination. For example, positions 2 and 5 being clearly distinguished as to quality, the sound was slowly moved from these points towards 3 and 4 respectively, the differences between these points being thus accentuated. Or, the sound was given several inches away from the head and directly out from 3 and 4, and the differences being clear under these conditions, the sound was given alternately at the two points while being moved in gradually towards the head. Finally, not later than the seventeenth sitting, these two positions could be qualitatively separated when given in immediate succession, although as yet their correct localization when given in connection with other positions was by no means possible.

[1] That a wide region of qualitative similarity exists at 3 such that a change of direction can first be noted when the sound has come well over into the territory of 4, is to be seen by referring to Bloch's figure on p. 83. Our own results, soon to be given, seem, however, to show the existence of somewhat greater differences than are there implied.

Whatever, then, the degree of ability to localize may be, this much is sure, viz., *that each position has its own distinctive quality, which may be looked upon as its local sign.*

Ability to Localize Definitely and Correctly Under the Conditions Here Imposed Can Be Acquired by Experience and Training.

Let no one be under the misapprehension that in these experiments there was first a discrimination of qualitative differences and then first an attempt to forcibly associate these with their appropriate positions as represented in visual, tactual or motor terms. Getting qualitative differences and learning to make correct localizations proceeded hand in hand from the very outset, although the latter process was naturally longer and more tedious than the former.

Two varieties of naïve localizations must be sharply distinguished, (*a*) the vague and indefinite, no single position seeming to claim the sound more than another, and (*b*) the perfectly definite and certain which is nevertheless false. The existence of these two varieties gives rise at once to two questions:

1. Can one by practice and training reduce indefinite localizations to those that are both definite and correct?

2. Can one by practice and training change localizations that are definite but false to those that are definite and correct?

We think that a positive answer may be given to both these questions.

1. Indefinite localizations are relatively infrequent, occurring perhaps most often in connection with positions

3, 4 and 11, and these may quickly be reduced to definiteness, though the localization may not at first be correct. The problem of indefinite, scattered sounds passes over then, after a few sittings, into that of sounds more or less widely misplaced, but still located with a feeling of entire subjective certainty.

2. The major part of our training was directed to this transformation of false into correct localizations. Two cases meet us here. The transformation was either effected directly after a brief period of practice, or it was brought about only by passing through an intermediate or transition stage. For example, positions 2 and 8 are confused, most frequently in the sense that 8 is located at 2, rather than the reverse. But the confusion soon disappears after repeatedly giving the sound successively at these two points. The clear discrimination of the two qualities of sound, and the development of a definite "space-feeling" for 8 as distinct from that for 2 arise almost simultaneously. And now the perception of the sound at 8 (or 2) is as *immediate* as when before falsely localized. On the following day, to be sure, the same confusion may occur, but a single direct comparison of the two positions suffices to produce correct localizations for some time to come. This illustrates the first case. The second case is more complex. The best illustration is drawn from the consideration of positions 3 and 4. As has been mentioned, these presented great difficulty. The experience of subject P. may be described. After some days of trouble with these positions it was found that 3 had a well-marked quality of its own but that it was located at 5, several inches from the head.

In the same way 4 had its own quality but was placed near 1. For some days it was possible to recognize the positions 3 and 4 only by the "5 feeling" and the "1 feeling" which they respectively possessed. That is, the localization was in this case strictly *mediate*, and consciously so. The localization was a matter of *judgment*, and not of immediate perception. It became an interesting question, accordingly, whether the real "space feelings" belonging to 3 and 4 could ever be made to replace the false-definite feelings that now attached to them. By dint of much careful attention and much moving of the sound after the manner described on page 97, a moment arrived, often suddenly, when 3 and 4 were by immediate perception located at their proper positions and with a feeling of perfect subjective certainty. The experience just described was paralleled, for these or other positions, in the case of the other two subjects. Methods similar to those above outlined were in each case employed, with the additional feature that if the positions confused were those in front of the face, the eyes of the subject were often opened, that the vivid visual perception of the sounding body might facilitate the coming of the correct localization. All subjects, therefore, succeeded in drawing sounds from false positions by means of transitional localizations. To be sure, the original necessity of judging certain localizations usually recurred more or less persistently, but for so long a time as the experiments continued these recurrences became less and less difficult to overcome.

The principal results of the process of training, to which these three persons were subjected, we have now before us. No attempt was made to secure a lasting perfection in localizing. The *possibility* of such perfection was all that we cared to demonstrate. Consequently experimentation was stopped as soon as each position

TABLE XV.

Objective Position.	Subjective Localization.		
	First Series.	Last Series.	After Long Interval.
Subject C.	Sept. 20, 1897.	Dec. 21, 1897.	May 19, 1898.
1	3	Correct	Correct
2	3	"	"
3	4	"	"
4	Correct	"	"
5	4	"	"
6	5	7	"
7	2	Correct	8
8	5	"	9
9	3	11	2
11	3	Correct	Correct
Subject E.	Sept. 20, 1897.	Dec. 22, 1897.	June 15, 1898.
1	2	Correct	Correct
2	1	"	"
3	Correct	"	"
4	"	"	3 or 11
5	4	4	4
6	5	8	6, 8 or 11
7	8	Correct	Correct
8	Correct	6	"
9	11	7	8
11	Correct	9	2
Subject P.	Sept. 20, 1897.	Dec. 23, 1897.	May 19, 1898.
1	Correct	Correct	Correct
2	"	"	"
3	4	"	4
4	3	"	Correct
5	Correct	"	"
6	5	"	"
7	6	"	"
8	9	"	"
9	11	"	"
11	Correct	"	2

was correctly localized often enough and under sufficiently clear conditions to show that the localization was a matter not of chance but of genuine and subjectively certain space perception. So much was accomplished for each subject, and the possibility of an ultimate perfection, if training were continued, seemed demonstrated beyond a doubt.

Tables of figures can show very little here, for a test series usually contains errors that rarely, or never, occur in the regular experiments. Still it may be interesting to compare the localizations of the first day of inexperience with those of the last day of training, and with those given after an interval of some months. The table is self-explanatory. (See Table XV.)

We append also the summary of all localizations made in test series given to the three subjects in the whole course of the experimentation. The numbers in the

TABLE XVI.

Objective Position.	Subjective Localizations										Uncertain, Omitted or at Intermediate Positions.
	1	2	3	4	5	6	7	8	9	11	
1	53	3	10	1							3
2	3	53	2	2	2	3		1			2
3	2	3	41	16	2			1			4
4	1		26	37	2						3
5		2	5	15	42	4					1
6		3	2	3	21	25	7	1	2		4
7	1	3	6		4	20	25	7	1		2
8		4	4	2	4	3	12	28	7	2	2
9		9	4	1	1	4	7	9	25	6	2
11	3	4	6	6	3	2	1		5	36	3
Totals.	63	84	106	83	81	61	52	47	40	44	26

Grand total.. 687

body of the table indicate the number of times that sounds occupying the objective positions indicated at the left were placed at the points designated by the numbers at the heads of the columns. (See Table XVI.)

No detailed comments on this table are necessary. Let it be remarked only that, as may be seen, the totals reveal a preponderance of correct localizations for each position, while the indicated directions of most frequent misplacement are presumably valid only for these particular conditions of experimentation.

Theoretical Inferences.

We may mention here the following theoretical inferences, postponing until later the discussion of their significance for the general theory of sound-localization.

1. Ability to localize sounds in unusual positions is the product of experience.

2. Such localization depends upon the power to qualitatively distinguish the local differences of the sound sensations.

3. The effect of practice is wholly central, consisting in the sharpening of the attention for auditory differences.

4. If the exigencies of life required, an ability to locate sounds perfectly within the median plane could be acquired.

5. The local signs of the median plane would seem not to be "complex," in the Wundtian sense, nor to contain any quality, motor or otherwise, beyond what is purely auditory.

6. Nothing has been found to indicate that learned localizations are products gained by uniting auditory qualities and visual positions. Improvements in auditory space perception due to practice appear in every way to be as fundamentally independent of visual factors as are similar improvements in the tactual field.

CHAPTER 3.

THE MONAURAL LOCALIZATION OF DIRECTION.

In this chapter the single attempt will be made to present the most important facts known in reference to the monaural apprehension of direction. Of new matter we have almost nothing to add.

It has been a generally accepted principle among physiologists and others that *binaural* hearing is an essential prerequisite of the perception of direction; that whatever else monaural hearing may accomplish, it is not adequate to perform the usual spatial determinations of hearing.[1] For common opinion the case is much the same as for sight, with certain differences. Each of these senses with double organs abrogates a part of its space-perceiving power with the loss of one sense organ from the pair. But while *vision* that has become monocular still perceives direction but suffers principally in the inability to make correct determinations of *distance*, the case is exactly the reverse for hearing. For when the latter has become monaural it is not the perception of distance but rather that of *direction* that has become supposedly impossible. I say 'supposedly' because we shall soon meet certain facts that seem to warn us against being too unguardedly dogmatic in this assertion. Let us examine briefly some of the published reports in this connection.

[1] For example, von Kries (17,B, p. 330) and Wundt (Grundzüge II., 94).

It will be well to separate facts of two sorts here—(*a*) those gained from laboratory experiences, the ears being normal but one of them closed, and (*b*) those gained from the testimony of persons afflicted with one-sided deafness.

(*a*) When monaural hearing is artificially produced by securely stopping one ear, there is a universally noted tendency to misplace a sound to the side of the open ear. (Preyer, Arnheim, Politzer.) An identical tendency occurred in a case that came under our own observation, where the two ears were of unequal sensibility. A sound in the median plane, *e. g.*, was nearly always judged to lie somewhat in the direction of the more acute ear. Strict monaural hearing is manifestly but an extreme case of unequal sensibility such as this case presented. Arnheim (1, pp. 8, 28, etc.) gives tables which enable one to compare the relative accuracy of localization, under the conditions of his experiments, for normal and monaural hearing.

Table XVII.

The Numbers Represent the Percentage of Correct Localizations.

	Normal.	Right Ear Closed.	Left Ear Closed.
Right-left axis	55.5	34.4	54.2
Superior-inferior axis	38 8	40 6	50.0
Anterior-posterior axis	25.0	18.7	12.5

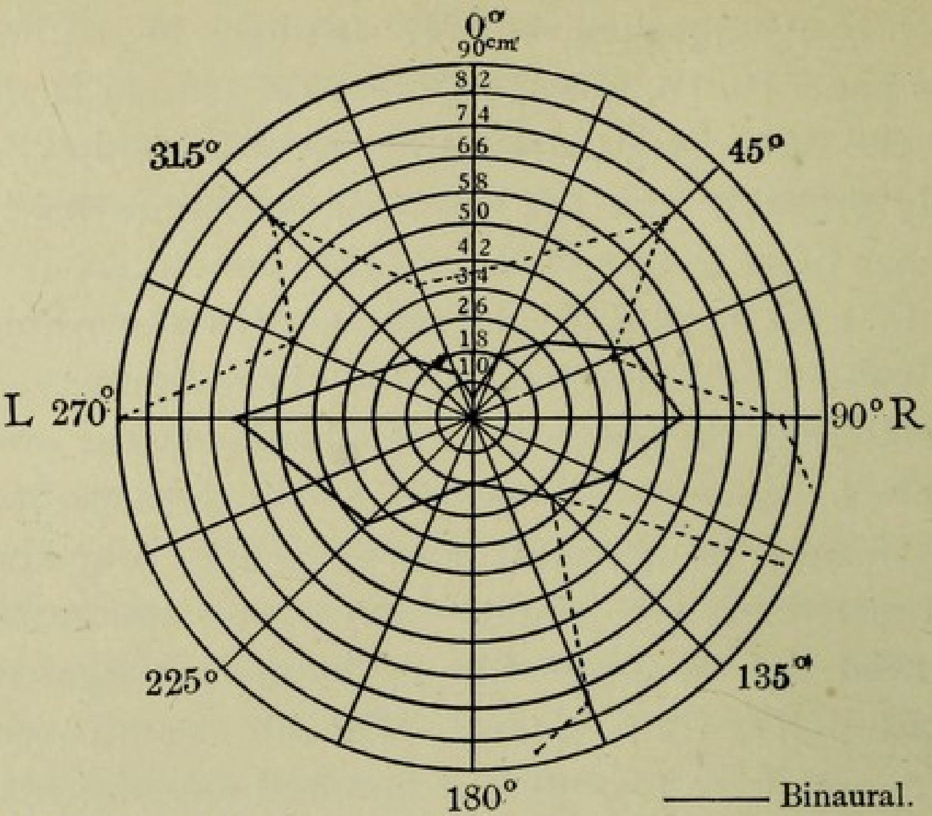
One sees from this table that sounds lying about the anterior-posterior axis suffer most in monaural hearing. The remarkable superiority of the right ear over the left seems wholly anomalous, and it stands in direct contradiction to a suggestion made by Arnheim in another part of his paper (p. 10, note), viz., that the left ear is sharper than the right because of its better blood-supply. It is highly probable that the discrepancy here is due to the presence of reflecting walls or other conditions unfavorable to experimentation on sound. On the whole, but little value should be given to the above figures.

Aside from the above general statements, the only experimental material at hand has reference to the *delicacy* of localization in monaural hearing. Following the lead of Münsterberg (the results from whose abnormal subject need not be given here), Bloch investigated this for each of the three planes. The left ear was securely closed, and from two to four hundred experiments were made in each plane. The following figures taken from Bloch will graphically present the various degrees of delicacy discovered, a direct comparison between the binaural and monaural results being also made possible.[1] (See Figs. 11 and 12, p. 108.)

As imperfect as it is, a genuine monaural localization is thus clearly demonstrated. Attention may be called to one or two points.

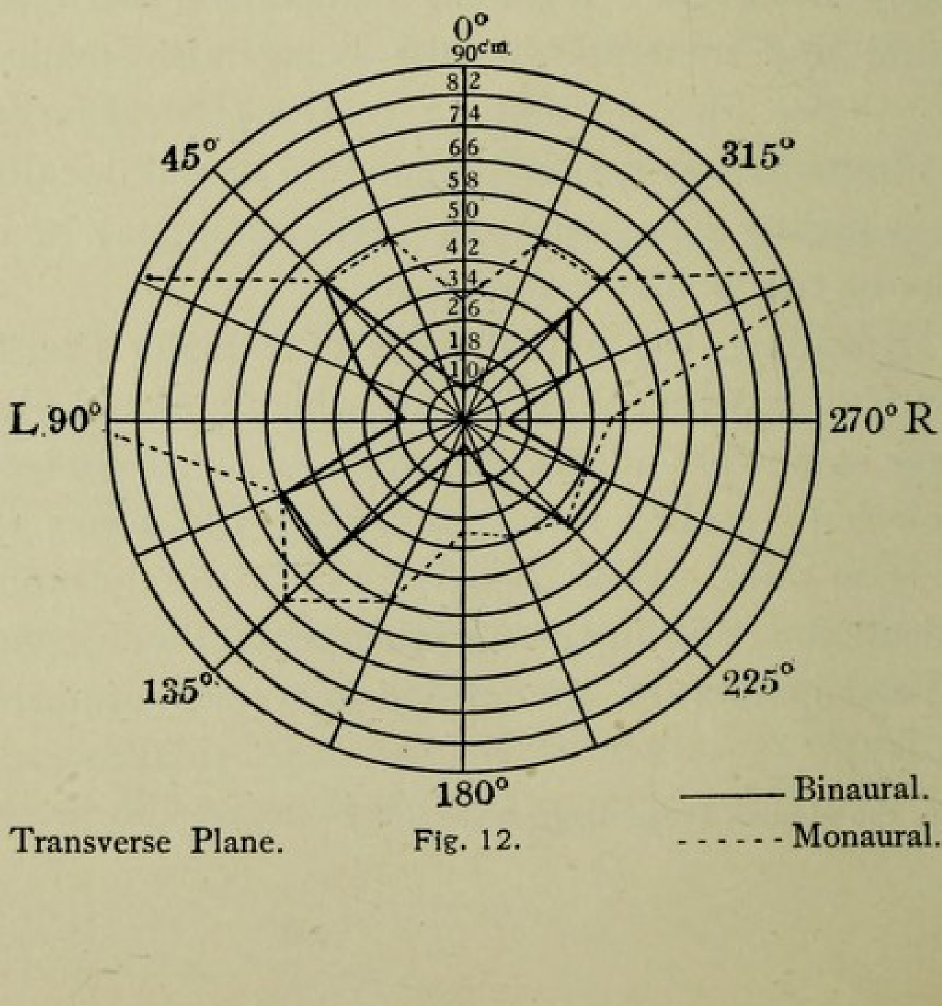
1. Recalling the experiments of Schäfer (34, C, 1), in accordance with the results of which a sound given to one ear somewhat affects the other ear, since a weakened stimulation reaches it by traversing the bones of the skull, one must speak guardedly of a purely monaural localization. Still in these experiments Bloch considers the participation of the closed ear to be improbable (5, p. 37).

[1] For the Median Plane diagram see p. 83.



—— Binaural.
- - - - - - Monaural.

Horizontal Plane. Fig. 11.



—— Binaural.
- - - - - - Monaural.

Transverse Plane. Fig. 12.

2. Monaural localization must be based on something other than the distribution of intensities to the two ears. The factors mediating this sort of localization must be the variations of quality depending upon the form and position of the ear-shell. Proof of this will be given in the following chapter.

3. Since monaural localization in the horizontal and transverse planes is reduced to other than the usual factors, the inferiority of the localization is not surprising. But since in the median plane even binaural localizations are based upon just such differences of quality as are monaurally at one's disposal, it might seem that the delicacy of monaural localization should not be strikingly inferior to that normally shown. As a matter of fact there is a point in the vicinity of 157° (Fig. 8, p. 83) where the maximum delicacies of the two sorts of localization are equal—something that does not occur at all in either of the other two planes. And in addition to this, one may turn the edge of any objection drawn from the above consideration by calling attention to the fact that the closing of one ear excludes that mutual reinforcement possible in the case of binaural impressions, and thus dulls the qualitative differences that would otherwise be sufficiently prominent for mediating differences of localization. (Loc. cit., p. 49.)

A temporary monaural hearing is experienced by most of us when lying in bed with one ear sunk in the pillow. In the case of the writer there seems to be an increased liability under these circumstances to confuse between sounds coming from the front and from behind. Thus a clock ticking some distance to the rear seems in front, and noises from the street coming through the window towards which the head is pointed seem rather to come from the direction of the feet. This misplacement of sounds appears—in the writer's experience—to be at the basis of those reversals of orientation that occur so strikingly in the hypnagogic state. The sounds by their misplacement carry everything else with them through a rotation of 180°, and however well one may know that the feet are not pointing in the direction of the window, the illusion of orientation is so complete and persistent that nothing short of some movement of the body or the raising of the head from the pillow can serve to banish it.

(*b*) Difficulties of localization in cases of partial or total one-sided deafness have been occasionally reported. *W. v. Bezold* (4) relates that he suffered several years from hardness of hearing in the left ear because of a piece of cotton that had reached the tympanum and hardened there. When this was removed he was for a time unable to locate sounds correctly. *Everything was misplaced to the side of the restored ear.* If any one called from the right he would turn to the left to see who it was, etc. It was three weeks before the keenness of the difficulty began to disappear, and six weeks before all traces vanished. The explanation is very simple here. The restored ear being for some time hypernormally sensitive, the resulting distribution of intensities was the same as if the sounding body lay more to the left than was actually the case. And, of course, the amount of the illusion was materially increased by the fact that corrections towards the left had for several years been necessary to compensate for the malfunctioning of the left ear during that period. These two causes operating together and in the same direction were adequate to produce the displacement of sound described.

G. Smith (39) gives an account of *six* cases of partial or total loss of hearing in one ear connected with inability to perceive the direction of sounds when the sounding body was invisible. In two cases there was a restoration of hearing, and the old ability to locate returned immediately. In case 3 the person learned to go in a direction opposite to what he thought correct, if he wished to reach an invisible sounding body. ("Opposite" is evidently used in a very loose sense here.) In case 1 the drum-membrane had been burst by a cannon-shot twenty years previously, and the power to correctly locate sounds had never been regained. So completely was the ability to adjust himself to the changed conditions lacking that the individual was forced to abandon his favorite sport of hunting. The direction of a continually repeated sound could be told by noticing the changes of intensity consequent upon his own movements to and fro.

In complete contrast to the last case stands that of a friend of the writer, J., who has been totally deaf in the left ear since having scarlet fever in his fourth year. Unfortunately, J. is not now accessible for purposes of experimentation, but he has kindly replied to certain questions put to him. In reply to a query about any remembered adaptation to one-sided conditions he writes: "I do not recall any difficulty in localizing sounds at the time of the disaster, nor afterward, although I recall with considerable distinctness many scenes during my illness. I think if there had been any serious difficulty in such localization, it would have led to overt confusions that could hardly have been overlooked by both myself and others, as was, I am quite confident, the case." In regard to the present he says: "I am not aware at present of greater difficulty in localizing sound than is experienced by other normal individuals. Although I am in no degree sensitive about it and never hesitate to mention the fact, I have repeatedly known persons for long periods without their suspecting my infirmity, which would tend to show it as of no great moment in my localization." To the question whether he were able to state how his localizations take place, J. replies: "I am not aware of any special machinery for localization."

J.'s testimony is of extreme interest as showing that a power to recognize the direction of sounds may exist under the conditions of total deafness in one ear. The exact degree of this power could, of course, be known only through the application of systematic experimental tests. Until such tests have been made upon some person similarly afflicted we must continue to regret the paucity of our knowledge in this interesting field.[1]

[1] Since the above words were written, Messrs. Angell and Fite have published an account of experiments that exactly meet these requirements (*Psych. Rev.*, viii. (1901), p. 225). The subject experimented upon was totally deaf in one ear, and yet the ability to locate the sound of a telephone was not markedly inferior to that of a normal person subjected to the same tests. Of course, localizations on the side of the defective ear were often extremely uncertain, but otherwise there was no serious deficiency. The suggestion offered above, to the effect that monaural localizations are based upon qualitative variations in the sound, coincides entirely with the theoretical deductions that these writers draw. And these deduc-

tions are supported not only by the introspective testimony of the subject himself, but also by the experimental evidence that "the more nearly the sounds approach pure tones the more inaccurate the localization." Thus the tones of a Galton whistle were very poorly localized, and the tones from tuning forks of 512 and 256 vibrations could not be accurately localized in a single instance. The latter tones are supposed by the writers to be practically pure. In every way, then, these systematic tests, made under the condition of absolute one-sided deafness, confirm the theoretical position maintained in the text.

In a succeeding number of the same *Review* (viii., pp. 449 ff.), these writers give several cases of one-sided deafness, of both long and short duration, in which the above conclusions are satisfactorily verified. There is also well shown the marked influence of practice—that coming from natural experiences as well as that gained in the course of an experimental series—in increasing the accuracy of localizations.

CHAPTER 4.

THE OUTER EAR IN ITS RELATION TO THE LOCALIZATION OF SOUND.

THE outer ear is commonly divided into two parts, the pinna or auricle and the auditory canal or meatus. It is with the former that we are to be especially concerned in this chapter. As in the foregoing chapter, our principal task here is to summarize the opinions expressed in technical literature in reference to the psychological importance of the pinna and meatus. These opinions may be most clearly described under three heads.

1. *The outer ear useless.*—Such is the opinion of Küpper (18, pp. 158 ff.) who looked upon the auricle as a mere appendage, insignificant and no longer having any function to perform. In much the same way Hensen (14) concludes that though perhaps not indifferent for auditory perception, the pinna plays but little part therein.

2. *The outer ear useful in that it mediates touch sensations*, which aid in the localization of direction.—This view is rarely held. It is expressed, however, by Wundt (70, p. 94) who remarks that since the pinna possesses a delicate sensitiveness to pressure (increased upon certain parts by the presence of tiny hairs), one may suppose that, in the case of rather strong impressions, the tactual sensations of the two pinnæ differ in accordance with the direction from which the sound waves come. That this view is highly improbable will be seen from what follows.

3. *The outer ear influential* either in aiding or preventing the passage of sound-waves to the meatus.—As early as 1851 Weber (see p. 24) had ascribed importance to the outer ear because, first, a much more discriminative localization is possible under normal conditions than when the head is dipped under water; and because, second, the placing of the hollowed hand palm backwards to the ear produces a reversal of the apparent direction of sounds from above and in front, or from points opposite these.

Mach, 1875 (24, C), supported partly by the conviction that localizations within the median plane are impossible, believed that little value can be attributed to the outer ear. Still, he admitted, the pinna may be looked upon as a *resonator* for high tones, and since the recognition of direction depends in part upon differences in the clang-character of a sound, the pinna may be allowed to have some influence in auditory space perception.

In 1879, Thompson published his experiments with the pseudophone. These have already been reported on p. 32. It is to be noted here that the pinnæ were not regarded as *resonators* in any sense, but simply as *collectors* of sound-waves of all sorts, one pinna being perhaps able to collect the waves of certain partial tones that never reached the second ear.

In 1882, Kessel (16, pp. 120 ff.) interestingly demonstrated the existence of that sort of influence that the pinna is here supposed to exert. One ear was closed, and the distance was determined at which a given sound could be just heard if in front, or behind or along the axis of the meatus. The greatest distance to which the sound could be removed was found to be at the side, the next greatest was in front and the least behind. If, however, the pinna was held flat upon the head, sounds from in front were heard worse and those from behind better, the reverse relation being established by holding the hollowed hand to the ear. *In neither of these two cases, nevertheless, was the distance of perceptibility at the side altered.* In a very simple way the influence of the various parts of the pinna may be roughly seen. Let a sounding body be moved past the ear in directions parallel to the median plane. If this be done distinct changes in the character of the sound may be perceived as this passes the edge of the pinna, or the edge of the tragus and antitragus.[1] On the basis of these observations Kessel distinguishes five "auditory regions," which it is unnecessary to make more precise here. This much then is clear, that a sound undergoes modifications determined by its relation to the pinna as a whole or to its various parts. We shall see presently that these modifications are spatially significant.

More recently Luzzati of Turin (23) has shown in a somewhat similar way the significance of the outer ear. A determination of the limits of the perceptibility of sound was made for eight positions in the *median* plane. The results of many observations upon 40 subjects are shown in the following figure. The sound used was the

[1] The writer finds that a striking way of illustrating the above is as follows: In a perfectly still room, pass the hand slowly by the ear in all possible directions parallel to the median plane, making a slight noise meanwhile by rubbing together the volar faces of the last joints of thumb and forefinger. The effect is most pronounced when the line of movement is as close as possible to the head.

ticking of a watch. The distances are given in centimeters.

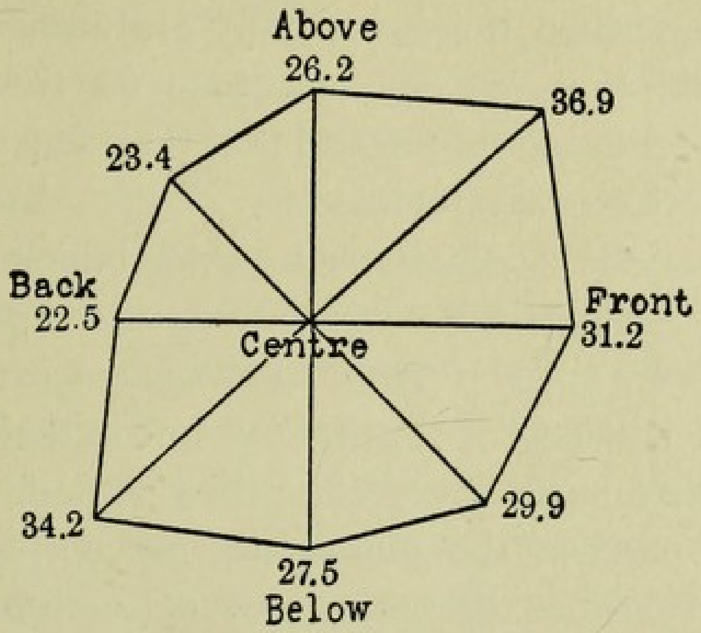


Fig. 13.

This then is the form of the field of perceptibility for weak sounds. It is, Luzzati says, a sort of "shadow of the pinna." This shadow becomes of course relatively smaller as the sound is louder and more distant. The field tends therefore to become *circular* in form as the sound becomes more distant. Once more then we see that the pinna exerts no inconsiderable effect upon the character of sounds as they reach the ear from the various directions of space.

Münsterberg (26, pp. 232–234) covered the ears with wax so as to put the pinnæ out of function, leaving only a passage to the drums open. Then the hollowed hands were held to the ears, with palms turned now forwards, now backwards. In the latter case the delicacy of localization for 180° was reduced from 10 cm. to 2 cm.; in the former case the delicacy at 0° was brought below the normal, from 1.5 cm. to 0.5 cm. It seems probable then that the pinna serves to collect and direct the sound-waves that strike upon it. And in view of the experiment last described, it seems wholly gratuitous to assume with Wundt that *tactual* sensations on the pinna are aroused by auditory stimuli and that they assist ordinarily in the perception of direction.

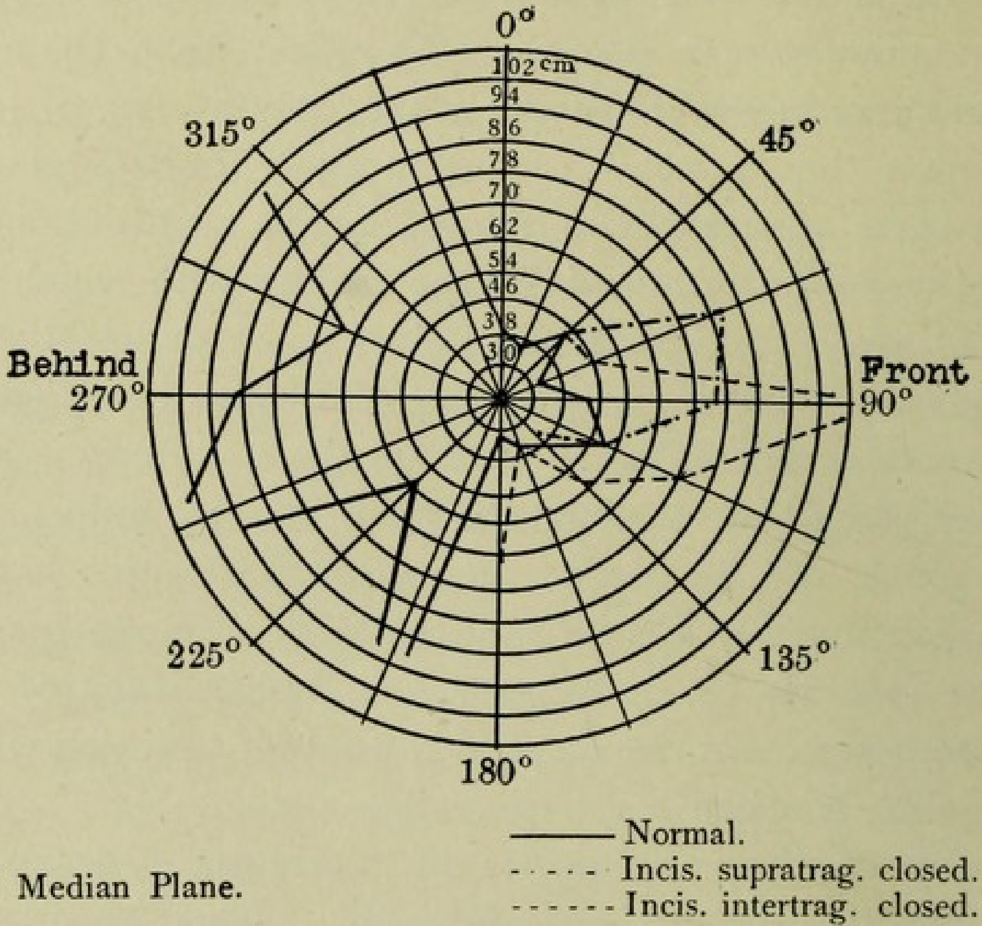
These various conjectures and beginnings of proof have been brought to a certainty by the researches of Bloch (5, pp. 35–52). He deals wholly with the variations in the delicacy of localization in the three principal planes, and attempts to correlate these variations with the prominent parts of the pinna that are correspondingly situated. Consider the monaural delicacy in the horizontal plane. (See Fig. 11, p. 108). "The sound coming from in front is taken up by the concave concha and reflected into the meatus. Not so for the sound from behind. This is rather intercepted by the pinna. Therefore a better localization in front than in the vicinity of 180°. The points by 90° and 112°.5 lie still in the shadow of the tragus, but its edge once passed, the sound reaches the meatus directly, and the difference between this point of acute hearing and the less favored points near by is shown by the sudden indentation of the monaural curve at 135°." At the rear, however, the delicacy again drops off and has no existence from here to 270°. That localizations are still possible in the anterior quadrant on the side of the closed ear may easily be explained by the consideration that waves from a sounding body one meter from the head may readily reach the ear upon the other side, being collected there by the pinna, as waves from behind could not be collected. In fact the position of the pinna forms the only ground for the enormous difference between the anterior and posterior quadrants on the left.

In a similar way also one may reflect upon the monaural curve in the transverse plane (see Fig. 12, p. 108), or upon the *binaural* curves in both horizontal and transverse planes where the particular distributions of intensity are determined by the facility with which the sound-waves from particular points reach the meatus.

But the really interesting part of Bloch's investigation is in connection with the *median* plane. Here, if anywhere, would be expected the most marked influence of the pinna and its parts. In a general way this is seen clearly enough in the form of the binaural curve for this plane (see Fig. 8, p. 83), for here no delicacy of any moment is exhibited except in the anterior portion where the pinna is effective.

In particular, two points of maximum delicacy are seen to lie between 45° and 180°. Their position is such as to arouse the conjecture that the region of less delicacy between them represents the tragus, which stands at the entrance to the meatus and cuts off sound-waves coming from that quarter, while they themselves represent the incisions above and below the tragus, which incisions would readily permit the passage of incoming sound-waves. To test this conjecture Bloch stopped up the incision above the tragus (called by him the incisura supratragica) on both sides with wax and adhesive plaster, forming thus a straight edge between the tip of the tragus and the edge of the helix. The delicacy of localization was then obtained for the region between 0°

and 135°. The result is given in Fig. 14 by the mixed line.



Median Plane.

Fig. 14.

Though identical with the normal curve as far as 45°, the new curve now displays the greatly diminished delicacy for the adjacent region, the point of *least* delicacy being now at 67°.5 where before it was greatest.

Again, the incisuræ intertragicæ were in a similar way filled with wax, and the whole anterior half of the plane examined. The broken line in Fig. 14 gives the result. Though again identical with the normal curve as far as 45°, the new curve shows its peculiarities from that point on. The unexpected coincidence of the two curves at 157°.5, Bloch attributes to the fact that the manner of attaching the wax and plaster to the ear pressed the pinna somewhat against the head and thus favored the free entrance to the meatus of sound-waves from behind. In any case, however, the stopping of the passages above and below the tragus produced an effect that is patent and surprising enough.

In another experiment the whole upper part of each pinna was covered with wadding and bound firmly to the head, while the lower parts were held down by means of adhesive plaster. This placed the pinnæ practically out of function, but left the incisuræ intertragicæ wider open than usual. The result was a marked diminution in delicacy over the whole anterior half of the plane, except about 157°.5 where, evidently by reason of the above mentioned widening of the incisuræ intertragicæ, the delicacy was greater than under ordinary circumstances. It may be mentioned here that the position of the incisura intertragica was responsible for the innermost point of the monaural curve as seen in Fig. 8, p. 83.

Bloch's researches, of which the above must stand as a sufficiently detailed account, have thus placed beyond all controversy the assertion that the outer ear is of profound importance in the perception of the direction of sounds. At any rate it is of importance just in so far as its position and structure determine the ease or difficulty with which variously directed sound-waves attain the drum-membranes. We can now for the first time adequately understand certain facts which we have met here and there in the course of our investigation and which we have been obliged to impute more or less vaguely to the supposed influence of the outer ear. Thus the conjecture made on page 88, that the point of change from 0° to 180° in the localization of symmetrical sounds was determined by the general position of the pinna, may now be regarded as raised to the level of a certainty. And we can now comprehend to some extent the peculiarities of the median plane localization. In particular certain peculiar misplacements in our own experiments described in chapter 2, section II. As may be seen from Table XVI. (p. 102), positions 6, 7 and 8 were most frequently misplaced to the position next above. This becomes explicable on the basis of the considerations that have occupied us in this chapter. Sound-waves from the positions named would in part be intercepted by the tragus, but would in part also enter by the incision above the tragus, to be collected and reflected by the concha. Thus for each of these positions that condition of manner of entry of the sound-waves was in part fulfilled which was more natural for positions just above. Hence the tendency to misplacement. In a similar way may be explained the misplacements described on page 100. Finally, we can now understand the possibility, general and particular, of monaural localization.

If we are to accept the statement of Mach (p. 115) that the pinna acts to a certain extent as a resonator for high tones, we may with Bloch explain the assertion made under 2, page 84, on this basis (5, p. 53).

Lastly it may be remarked that the meatus may well have a resonance-tone of its own, the presence or absence of which would alter the quality of the sound heard and thus determine the perceived direction of its coming.[1]

[1] An interesting observation, confirmatory of the already well enough attested supposition of the influence of the pinnæ in altering the intensities of sounds from certain directions, has been made by Matsumoto. (Loc. cit., p. 63.) Two telephones were moved from 0° to 90° and 270° respectively in the horizontal plane, and during this movement the apparent *distance* of the resultant sound became steadily less. Now, as we shall see later (Part II), changes of apparent distance accompany changes in the absolute intensity of the sound. Accordingly the present decrease of distance is to be explained by the increased facility with which the absolute sound intensities were able to reach the tympana when the sounding bodies were at the sides. These changes of apparent distance should be much more marked when the sounds start from 180°. Whether this is so or not the records of Dr. Matsumoto's experiments upon this point are not explicit enough to reveal.

CHAPTER 5.

THE INTRACRANIAL LOCALIZATIONS.

Of the various phases under which the perception of auditory space presents itself, there is none more interesting from certain points of view than that exhibited by those subjective localizations termed *intracranial*, or *endocephalic*. And not only are these localizations interesting from the point of view of fact, but also, as will be seen in due time, they are of no small import for the theory of auditory space perception. The facts are roughly these. If the note of a tuning-fork be conducted through a rubber tube one end of whose Y-shaped terminal is inserted in each ear; or if two telephones with fusing sounds be held one to each ear; the resulting single sound is in each case located *within the head*. That is, there arises a subjective auditory field of the same general sort as that described above (Chap. I., 11), only in this case the phantom-sound takes up a position within the limits of the skull. Here too, just as there, neither ear acts independently in the way of perceiving at its actual place the objective sound on its side.

Purkinje (31) is said to have been the first to record this fact. In 1877, Thompson (42, A_1, p. 274),[1] while experimenting upon the production of beats by leading the beating-tones one to each ear, noticed that the beats "seemed to be taking place within the cerebellum." A

[1] Compare Plumaudon (28) and Kessel (16).

year later (42, A_2), further observations directed to this special matter were reported, and since then the different phases of the subject have been quite variously studied, without however increasing our knowledge to the extent that might be desired. Schäfer (34, A) has verified Thompson's observation in reference to the intracranial localization of beats, and (34, B) has called attention to Weber's observation that if the stem of a loudly sounding tuning fork be pressed against the skull at various points, the sound may be heard within the head. It is interesting to note that under these circumstances the closing of one ear causes the sound to come apparently from the closed ear, while stopping both ears brings the sound back within the head. A somewhat similar experience may be had by loudly singing an "oo," or an "m" (lips closed). The successive stopping of one and both ears will cause the sound to wander up first to the closed ear and then to a point within the head.

The following are perhaps the most interesting features of intracranial localization.

THE POSITION WITHIN THE HEAD.

(*a*) Individual peculiarities.—Thompson (loc. cit.) reports that the resultant of two telephones was heard "as if one had struck with a hammer the back of the skull from the inside." Subsequent observers noted various subjective positions. Thus Urbantschitsch (45, A), combining both noises and tones, and experimenting upon numerous persons, found localizations made in the head, nose and pharynx. The same individual, however, always located the sound in the same place.

The existence of these individual differences we have often confirmed by experiments of our own. The favorite intracranial positions are perhaps (1) in front, between the eyeballs, (2) behind, upon or a short distance from the occipital bone, and (3) deeply within the head, at or near the middle point of the line joining the two ear-drums.

(*b*) The localization influenced by the character of the sound.—By slightly changing the position of the sounding-body with respect to the ear, Rogdestwenski (33) obtained localizations now in the head, now in the breast and now in the abdomen. Urbantschitsch (loc. cit.) thought that each tone had its peculiar location, deep tones being in general placed at the back of the cranial cavity, high tones at the front. Thompson, at an earlier date, had found changes of pitch to be without influence, but leaving individual differences out of account, we must give weight to the statement of Urbantschitsch because of the large number of subjects upon whom he experimented.—If telephones or tuning forks are used, the apparent position of the phantom-sound seems to depend upon the coincidence or non-coincidence of the vibrations. (Thompson; Bloch, loc. cit., pp. 22 ff.) For example the latter, experimenting with telephones, found that when both vibrating plates approached the ear at the same time, the sound was most likely to be placed in the forehead. But if one plate approached the ear while that on the opposite side receded, the sound was more likely to be placed deep within the head, perhaps between the ears. Bloch has no explanation to offer for these differences. Presumably they rest upon factors that, now at least, are beyond the range of experimentation.—In our own experiments we have in several cases remarked the fact that changes in the absolute intensity of the resultant sound produce effects upon its localization. Thus with subject P. an increase of intensity caused the sound to move from the front to the center of the skull. And with subject W. a decrease of intensity caused an apparent movement of the sound from the back of the throat upwards to the occipital bone.

(*c*) The influence of unequal intensities.—If the ears of the observer be normal and the intensity of the sounds be alike on both sides, the internal localizations, like their external counterparts, are made at various points in the median plane. It was, however, early noticed that if the two ears were of unequal acuteness the subjective sound took up a position out of the median plane and on the side of the more acute ear. In the same way, if the intensity on one side was diminished, the sound was invariably located *towards the side of the greater intensity*. Thus by a suitable variation of the intensity on either side, the sound may be made to wander about within the head, passing completely to either ear if the stimulation upon the opposite side be reduced to zero. (Thompson, Urbantschitsch and Schäfer, 34, B.)

(*d*) The general conditions that determine an intracranial as opposed to an extracranial localization.—Unfortunately these conditions cannot be precisely stated. It is clear, however, *that intensity is not one of the determining factors*. Schäfer (34, B) remarked that though the removal of the two telephones to a short distance from the head caused the sound to become extracranial, the result could not be attributed to decreased intensity; for if the telephones were close to the head, the sound could be made exceedingly faint without at all affecting its intracranial character. The really conditioning factor seems rather to be *the distance of the sounding-bodies from the head.* Some attempts were made to ascertain from my own subjects the particular distance from the head at which the telephones must be in order that the sound change from outer to inner, or *vice versa.* No uniform results were obtained, even the same individual giving different testimony at different times. Out of nine subjects we may mention two. (1) Subject B. localized within the head very faint sounds when the telephones were 4 cm. from the head. At 8 cm. distance, the localization was extracranial no matter how great the intensity of the sound. And yet from the point of view of auditory stimulus the intense sound at 8 cm. was a much greater excitation than the faint sound at 4 cm. (2) We have already mentioned (p. 127) that in the case of subject W., a decrease of intensity caused an upward movement of the sound from the back of the throat to the occipital bone. If now the telephones were removed to the distance of 4 cm. from the ears, the phantom-sound moved outwards and settled upon the occipital prominence.

How these results are to be interpreted is not wholly clear. The necessary proximity of the sounding body to the head would point to the conjecture that a concomitant vibration of the soft masses within the skull is indispensable for the arising of an intracranial localization. If this were so, the individual differences in this sort of localization might be attributed to differences in the location of greatest internal vibration. The above conjecture is, however, somewhat weakened by the following observation by Schäfer. (34, C_1.) If the tone of a tuning fork be allowed to die away until no longer heard by the unaided ear, and if then a resonator be placed at one ear, the sound is heard as if issuing from the resonator. But if now the opposite ear be closed, *the sound will wander within the head towards the median plane.* Schäfer can explain this only by supposing that the sound has been conducted to the farther ear through the bones of the skull, whereupon an intracranial localization of the ordinary sort occurs. What the stopping of the ear does is simply to strengthen the vibrations of the tympanum and thus greatly heighten the effect upon this ear. If now this explanation in terms of craniotympanal conduction be true, it becomes superfluous, in this case at least, to assume any concomitant vibrations of the internal masses. Besides, these could hardly be affected by so faint a sound as is here in question. And yet the internal localization is here an undoubted fact.

On the other hand the mere absence of outer criteria is not essential for this variety of localization ; that is, the non-functioning of the external ear is not here the decisive condition. For the sounding bodies may be several centimeters from the head, and consequently all the apparent conditions for an external localization may be fulfilled, while yet the sound remains within the head. Perhaps all that we can safely say at present is that intracranial localizations occur whenever two simultaneously sounding bodies of like nature are so placed that certain portions of the inner ear, of the skull, or of the internal masses, are thrown into vibration. A more precise formulation must be left to the future.

CHAPTER 6.

AUDITORY ORIENTATION.

THE experiments to be described in this chapter were undertaken for the purpose of ascertaining whether the usual localizations of sound are modified by abnormal positions of head and eyes, or by the rather violent subjective changes accompanying dizziness. Granting that such modifications of localization would be found, the further object of the experiments was to investigate the direction and degree of uniformity of these modifications. Except in the case of dizziness, the following procedure was employed. The instrument pictured on page 55 was used, and two sets of data were obtained. In the first place, either the normal or some definite abnormal position having been assumed, the subject was required to indicate when the moving sound reached the particular point agreed upon. Successive approaches to this point were always made from opposite directions. If, for example, 0° were the position sought, the sound would be moved from the direction of 90° by stages which became shorter as the sound approached the point in question, and when a position satisfactory to the subject had been reached, the same process was repeated from the direction of 270°. Since the chief interest in this matter of orientation centers about the subjective position of the anterior-posterior axis, the greater part of the experiments were concerned with the determination of this.

In some instances, however, the right-left axis was also investigated, and, in still other cases, determinations were made for points scattered irregularly about the horizontal circle. In the second place, the sound was given at a point whose general region only was known to the subject, and the latter was required to indicate this point either in terms of degrees or by designating the location upon a diagram representing the horizontal circle with its two principal axes. These two methods, that of searching and that of simply locating, served as checks to each other, for their results should of course be harmonious.

Care was always taken to allow the subject to reach satisfaction in respect to his judgment. Consequently, any sound was repeated as often as was desired. Subjects differ greatly in this regard. Usually one or two repetitions sufficed in any case of uncertainty, but occasionally as many as five or six were necessary.

The procedure was without knowledge. The subjects knew, of course, the general nature of the undertaking, but the character of their judgments was not made known to them, at least not until sufficient evidence of uniformity had been obtained.

In some cases the subject was warned by a touch rather than by a spoken "ready," but since the end in view was to ascertain the direction rather than the amount of any errors made, the spoken warning was almost universally used.

(*a*) *Head and Eyes Normal.*

1. Anterior-posterior axis.—The general results here agree exactly with what one would expect from a con-

sideration of the delicacy of localization. Sounds given at 0° and 180° are recognized as such, but others given slightly to the right and left of these points may appear to lie in the median plane, for the differences between the two positions may not be recognizable. The determination of one's delicacy of perception for 0° and 180° will therefore be practically the same as that of the subjective position of the anterior-posterior axis, though the latter will cover a slightly wider area than the former. On the basis of our experiments we may say that *the anterior-posterior axis as subjectively apprehended may be represented by the two sectors radiating from the center of the circle towards 0° and 180°, whose arcs, bisected by these two points, shall be, respectively, 8° and 16°.* The exact size of the arc of these sectors will vary with the individual, but 8° and 16° are perhaps fair average estimates.

2. Right-left axis.—Matsumoto (25, pp. 20 and 67) has called attention to the fact that "the directions that appear to us a right and left, seem to lie slightly in front of the auditory axis." As it stands, this statement is not wholly true, for sounds coming from the rear of the auditory axis may also seem to lie exactly in it. When a sound is made to approach 90° or 270° from the direction of 0°, it is invariably stopped before it reaches the right-left axis. But when it approaches from the direction of 180°, it may be stopped on that side of the axis. Out of seven determinations of the latter sort made for 270° with three subjects, *five* were 5°–20° behind the axis, and out of the same number for 90°, *three* were 5°–20° behind. Still the averages of the seven determinations made for each position from both directions

are respectively 77° and 277°. There is thus a clear tendency to project the right-left axis forwards of its true position. It is hard to give figures which would have more than individual validity, but it may at least be considered certain that the subjective axis covers here an extent much greater than one would expect from the results of the experiments upon mere delicacy. One may perhaps fairly say *that the right-left axis as subjectively apprehended may be represented by two sectors radiating from the center of the circle towards 90° and 270°, whose arcs shall be 25°–45° in length and lie with their middle points 10°–15° in front of the auditory axis.* Within these sectors any sound may be moved about at random without seeming to depart from the right-left line.

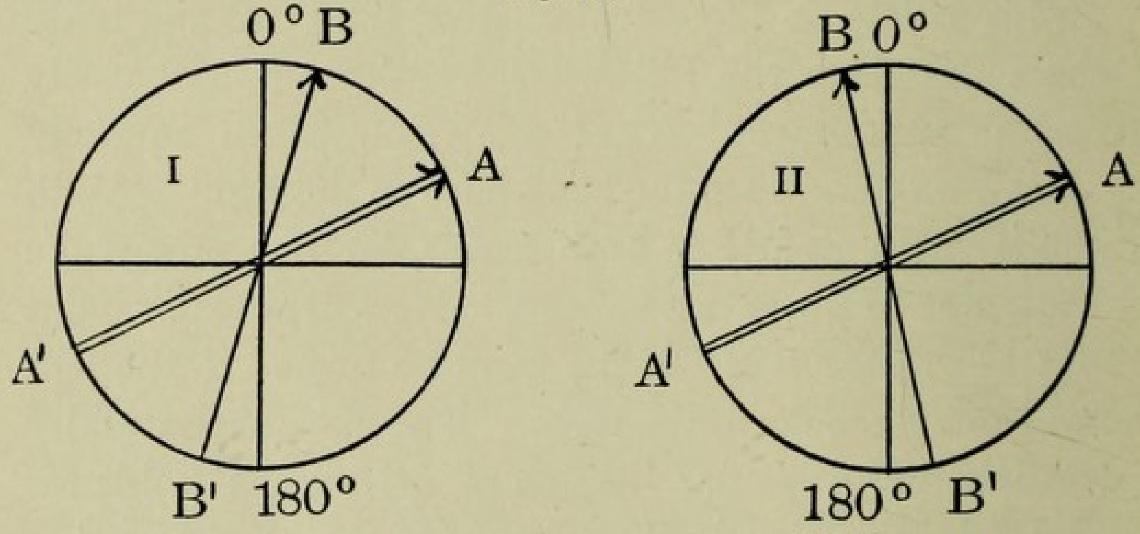
(*b*) *Head Turned to the Side.*

In his article on the orientation of the head and body Delage describes the following well-known and easily repeated experiment.[1] Let a person stand squarely against a wall and turn the head hard to the right. With a pointer held lightly in both hands, let him now make the attempt to point dead ahead. He will actually point about 15° to the left. If the head be turned to the left, he will point too far to the right. The question naturally arises whether a similar illusion of orientation holds for sound, whether, that is, the turning of the head to either side will make the subjective zero-point shift in the opposite direction. Experiments were arranged to answer this question. The method used was that described at

[1] Delage, Archives de Zoologie expérimentale, 1886, 2me ser., T. IV., pp. 535 ff. Or see Aubert: Physiologische Studien über die Orientierung, 1888, p. 18.

the opening of this chapter, the only additional feature being that a wire "pointer," lying in the median plane of the head and secured to the latter by a loop of wire, served to denote the exact amount of rotation of the head. The result of the experiments could hardly be predicted, for the fact that equality of intensities usually denotes the zero-point might overcome any tendency to the Delage illusion, and produce merely a general inability and confusion. With a single exception in 26 experiments upon 3 subjects, the subjective zero-point was chosen on the *same* side of the median plane as the point to which the head was turned. In the same way if the sound were given at 0°, it was supposed to be on the opposite side of the median plane. A diagram will make this clear.[1]

Fig. 15.



I. II.

B = subjective zero-point. *B* = supposed position of sound actually at 0°.

AA' = median plane of head.

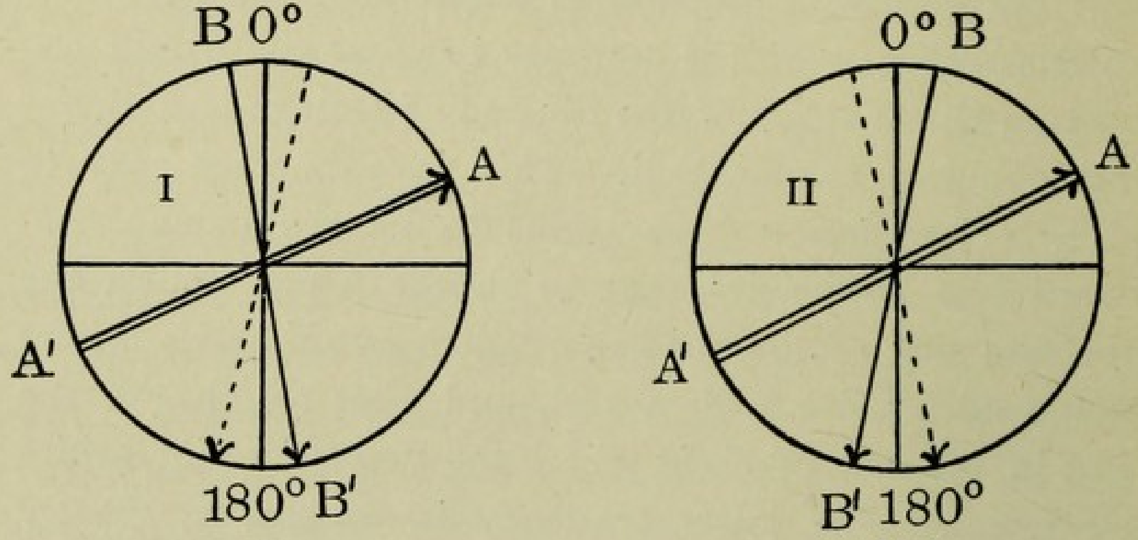
[1] In this and the following diagrams of the chapter the state of affairs represented will apply to rotations to the right. Diagrams geometrically *similar* to these would apply to rotations to the left.

Similar diagrams would represent the condition of things with the head turned to the left. Both of the above figures express the same fact under two phases—the fact, namely, that when the head is turned hard to either side *the auditory zero-point is subjectively shifted in the same direction.* This is contrary to what one would expect on the analogy of the illusion of Delage and therefore requires explanation. This appears very easy to give. The illusion here seems to be the balance between two tendencies, that to choose a position on the opposite side of the median plane approximately where one would point with a rod, and that to choose the spot in the line of sight where the two sound-intensities are equal. The latter tendency is, of course, greatly weakened by the knowledge that one is making a large correction for the abnormal position of the head, but it is still strong enough to overcome the former tendency, and thus to determine the direction of the illusion. In our experiments the amount of the illusion varied from 0° to 23° in case I., and from 0° to 45° in case II.

With respect to 180° the illusion is not quite so uniform. Although there is usually a shifting of the whole median plane in the way indicated by the lines *BB'* in the two parts of Fig. 15, it happened that two of the three observers showed a tendency to make the localizations expressed in the following diagrams (p. 136).

This discrepancy between the two positions given the median plane at the rear rests evidently upon two distinct methods of forming the localizing judgment. This may be formed (1) by referring the sound always to the *body*, the position of the latter with reference to the horizontal circle being always the most prominent thing in consciousness; or (2) the sound may be located with respect to the head, a calculation of the position of 180°

Fig. 16.



I. II.

B′ = point sometimes chosen for 180°.

B′ = supposed position (occasionally) of sound actually at 180°.

AA′ = median plane of head.

being made on the basis of the supposed position of the head. In localizing by the first method, the thought of the abnormal position of the head will fall relatively into the background, and, since "front" and "back" will be the most pronounced factors to be attended to, the old habit of noting the equality of intensities will gain the upper hand and the median plane will seem to take the position indicated by the dotted arrow in Fig. 16. Thus in two determinations from each direction made upon one subject who was practiced in localizing with the head at the side, the 180°-point was placed 23°–45° too far on the side of the actual median plane opposite that to which the head was turned. This subject when questioned said that he localized with reference to the body, that is, by the first method described, and it was his custom to sit with the volar faces of the finger-tips together, their points of contact lying in the median plane, in order to get the "feeling" of the true objective directions. A second subject localized confessedly by the second method, and his judgments were as shown by the lines *BB'* in Fig. 16. This subject, that is, tried to keep constantly in mind the relation between head and circle, and located by calculation. Knowing that the right ear was (in the case where the head was at the *right*) somewhat short of the 180°-point, the effort was made to get the sound a little beyond the auditory axis. But now the amount of the head's rotation to either side is always exaggerated, and by direct consequence the supposed 180°-point, the point calculated to be at the requisite distance beyond the auditory axis, *fell short of its true position.* The two varieties of the 180°-illusion agree perfectly then with the two methods of localization employed by the subjects for this point. The reason why the 0°-illusion was uniform is because the first method only was used in that case, it being much more easy to refer forward directions to the body itself than directions at the rear.

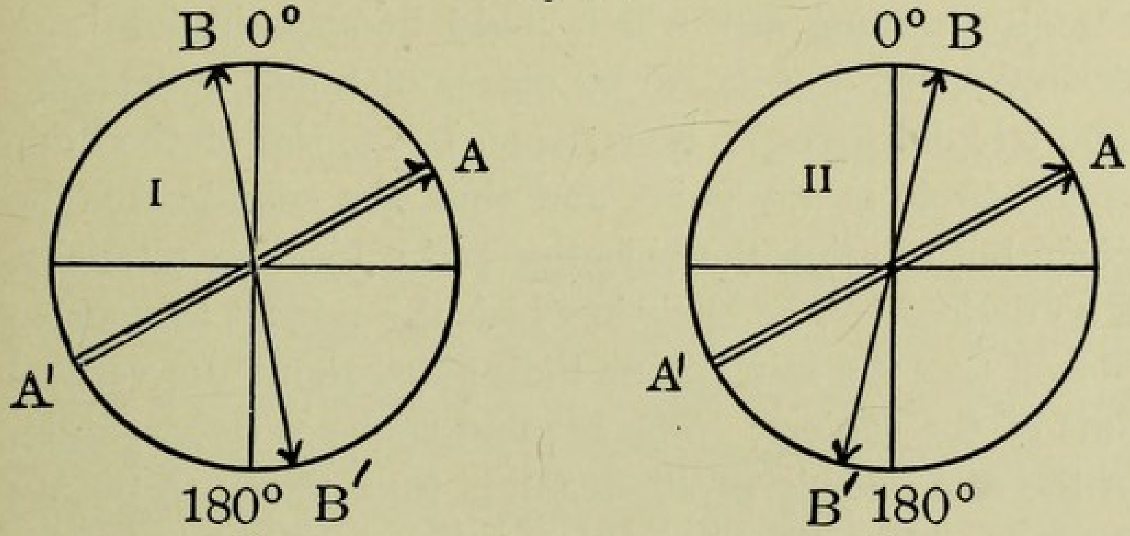
The explanation of Delage's illusion turns, as is well known, upon the overestimation of the amount of the head's rotation, this overestimation being due to the fact that the eyes have rotated about 15° farther than the head. The question therefore arises in this connection whether, if the eyes and head were kept carefully together, the illusion would be in any way modified. A few preliminary experiments along this line seem to indicate that some change would be introduced by this condition, but we cannot yet speak of its exact nature.

(*c*) *Eyes Turned to the Side.*

In this series the head was kept with its median plane coincident with the median plane of the circle and the eyes only were turned to the side. Of course there was a great tendency to turn the head also to the same side as the eyes, and any actual movement of this sort was carefully noted by means of the pointer. The eyes were rotated as far as possible to right and left, and the same procedure was followed as when the head was turned to the side. The results obtained were perfectly uniform for three subjects, and *the illusion found was exactly opposite that noted in the foregoing series.* In whichever direction the eyes were turned, the subjective median plane seemed shifted in the *opposite* direction. This is shown graphically in the following diagrams (p. 139).

The amount of apparent displacement was 2°–20°. And next in point of interest to the fact in reference to the direction of the illusion is the further implied fact that uniformity prevailed in respect to the apparent position of 180° as well as in respect to that of 0°. The front and rear portions of the median plane were alike misplaced in a direction opposite to that of the eyes. The explanation is very simple. *The abnormal position of the eyes has produced first an illusion in respect to the position of the head.* Let any one turn the eyes hard to the right and then notice how the nose seems to be pointing in a line lying also to the right of the median plane. In fact Delage found the same error in pointing when the eyes were rotated and the head kept fixed. There seem to be three planes concerned—that of the line of sight lying far to the right, the median plane of

Fig. 17.



I. — *BB'* = supposed position of the median plane.

II. — *BB'* = supposed position of sounds actually in the median plane, at 0° and 180°.

Eyes directed to *A*.

the head, and the median plane of the circle lying beyond that of the head. Consequently when the effort is made to choose the 0°-point and the 180°-point, an error is made by introducing a correction for the supposed position of the median plane.

(*d*) *Dizziness and Localizations of Sounds.*

In view of the various theories that connect the semicircular canals with the localization of sound, and in view of the undoubted relation between these canals and dizziness, a set of experiments was arranged for the purpose of studying the possibility of localizations during the state of dizziness. It was hoped to obtain some data decisive enough to enable one to take an unqualified attitude towards the semicircular-canal theory. The apparatus shown in Fig. 2 was fastened to a large horizontally rotating disk which could be brought into any desired speed of rotation by means of a belt and crank. The telephone could be fastened to the rim of the horizontal circle at any point, and thus the sound could be given both during the objective and subjective rotations, if so desired. The chair in which the subject sat was so placed that the revolutions took place about the vertical axis of the subject. The revolutions were rather rapid, at the rate namely of about ten in from fifteen to twenty seconds. Rotations in the direction of the hands of a watch are called positive, those in the reverse direction negative. The inherently disagreeable nature of the experiments precluded anything beyond the most direct examination of simple localizations in the horizontal plane.

A knowledge of the general phenomena following rotation of the body must be, to a large extent, presupposed here. We need only mention that, since the eyes remained closed during the period when sounds were given, only objective and subjective rotations of the *body* —not of the field—come in question here.

The experiments, begun in 1893 and interrupted until the winter of 1898, were all carried on in the Harvard Psychological Laboratory. The second batch were more carefully controlled than the former, and for that reason they alone will be reported here. Five subjects took part. The results to be stated are as follows:

1. Sound given during objective rotation.—Nothing very decisive can be stated here, although there is probably a tendency *to misplace a sound in the direction opposite to the rotation*. Our results were:

Number of experiments	12
Misplacements opposite to rotation	7
" in direction of rotation	4
Uncertain	1

The amounts of the misplacements cannot be given, since the subject was often unable to give anything further than the direction in which the sound must lie from a given point. When the amount of misplacement was stated, it was usually 10° or 15°.

2. Sound given during the subjective rotation, immediately upon the cessation of the objective rotation.—The results here showed a decided tendency *to misplace a sound in the direction of the rotation just given*. Our results were:

Number of experiments	16
Misplacements in direction of rotation	13
Misplacements in opposite direction	2
Correctly located	1

The amount of misplacement was ordinarily 5°–20°, and the errors occurred both for positive and negative rotations.

In attempting an explanation of these illusions of localization, we shall confine ourselves to a consideration of the misplacements made during the state of *subjective* rotation. We shall try to bring these illusions into line with others similar to them in other departments of space perception and to this end we shall appeal to two phenomena: (1) the reflex tendency to rotate the head at the beginning of the dizziness state, and (2) the nystagmus of the eyes. The direct influence of the semicircular canals may evidently be eliminated from the discussion, since no general confusion of localization was shown, such as would result from a disturbance of the normal functions of the canals, but rather a uniform misplacement in a constant direction.

1. The reflex tendencies to move the head as soon as the objective rotations cease is the simple counterpart of the head movements known to take place in the case of animals when their canals are stimulated. They do not ordinarily become manifest until some special means is used to reveal them. A series of 25 experiments was made upon various subjects for the purpose of studying the direction of the tendency. The tendency could be developed into an actual reflex movement by voluntarily straining the head against the hand at the moment when the subjective rotations began, or by holding the head turned to the side during the objective rotation and then relaxing the muscles of the neck when this rotation ceased. In both cases there was a reflex movement of the head, sometimes amounting to over 100°, *in the direction of the objective rotation.* The tendency to head-movement and the misplacement of the sound are thus in the same direction. How can the former be made to explain the latter? In the well-known and much dis-

cussed illusion resulting from a paralyzed eye-muscle any object seen appears to have been moved to the right (say) whenever the attempt is made to turn the eye in this direction. The strain not followed by movement is interpreted as a movement of the object, for the strain ordinarily means a particular position of object seen, and if an object supposed to be swept over by the eye retains its position with respect to the latter, the only interpretation possible is that the object has moved to the right. So in the case before us; the tension in the muscles of the neck is ordinarily the index of the head's position with respect to the body, and the distribution of sound intensities being of a particular sort, the sound must be misplaced in the same direction as the tendency to rotate the head. That is, the tension of the neck muscles without actual movement has been interpreted by assigning a greater angle to the auditory object. Of course there is no conscious process of inference here. It is all a matter of association and immediate perception. But the very fact that the tendency to move the head is not consciously known causes the illusion, for if it were known as such, a correction would be introduced.

2. But the above explanation is not complete and a supplementary factor in the illusion must be considered. For in the experiments made for the purpose of revealing the tendency to move the head, localizations were made *after the actual movement of the head had taken place and returned to its normal position*,—that is, after the tension in the neck muscles had been relieved and the tendency to move satisfied,—*and the same tendency to misplacement was found*. Our results were:

Number of experiments	20
Misplacements in direction of rotation	13
" opposite to rotation	6
No misplacement	1

To understand these results we must appeal to the nystagmus of the eyes. This nystagmus has two varieties of movement, one slow and in the direction of the subjective rotation, the other rapid and in the direction of the preceding objective rotation.[1] It is this latter which is effective here. Suppose that the objective rotation has been positive. The eyes are pulled strongly to the right when the subjective rotations have begun, and the state of the matter is exactly the same as is represented in diagram II. of Fig. 17. (Page 139.) *The straining of the eyes to the right causes a misplacement of the sound to the right*, and here, as there, the illusion of localization can be reduced to the same factors as are operative in the Delage illusion described above. It is probable that factors (1) and (2) coöperate to produce the illusions of localization in dizziness. If one may conjecture which of the two is more effective, one may perhaps decide in favor of the latter. Still the former cannot be wholly without influence.

[1] See, *e. g.*, Wundt: Grundzüge, II., 163–164.

CHAPTER 7.

THE LOCAL SIGNS OF DIRECTION.

It is commonly demanded of local signs that they fulfill the two conditions of *non-interchangeability* and *capacity for gradation.* In other words, the ideal local sign must not be liable to confusion with any other, and it must hold so distinct a place in a well-ordered system of signs that its spatial import shall be unmistakable. These conditions are certainly hard to secure in their perfection. But any theory of local signs that does not in some measure approach this standard is admittedly insufficient and unsatisfactory. Let us see what we can do towards elucidating a system of auditory local signs of direction.

Whatever one's opinion in reference to auditory space in general, an adequate theory of local signs must make provision for two general classes of facts : (1) the extreme liability to misplacements, and confusions of position, in short, to *illusions of localization ;* and, (2), the large ability to free oneself from this liability through the medium of practice, or, in other terms, *the non-persistent character of these illusions of auditory localization.* To provide for these two classes of facts we shall propose a composite local sign which shall contain at least two factors corresponding to the two requirements of the case.

1. The first factor which shall to some extent fulfill the conditions above specified and at the same time make clear the possibility of the numerous confusions of localization is manifestly connected with *the manner of distribution of intensities to the two ears.* Directly defined and with reference to this first factor only, the local sign of any position may be said to be *that peculiar characteristic which each sound possesses by virtue of the fact that it is heard binaurally, this fact requiring that a certain definite ratio exist between the intensities received respectively by the right and left ears, or vice versa.* Obviously this ratio is greatest for sounds lying directly opposite either ear, its value diminishing as the sound moves from this point towards the median plane where the ratio becomes unity. Detailed discussion of the grounds for assuming this factor as one of the constituents of the local sign seems needless here, since much of the foregoing text has been little else than a presentation of the manner in which distributions of intensity are effective. For the claim that such distributions are of fundamental import in localizations finds its most striking confirmation in those multifarious mistakes of localization which we have everywhere signalized. The facts, therefore, which lead us to postulate this first factor in the local sign need not be rediscussed here. We may however profitably append a tabulated summary of facts to make the matter wholly clear.

The fundamental import, for localization, of particular distributions of intensity to the two ears is shown by:

1. The general correspondence, in point of *delicacy* of localization, between positions in the horizontal plane

symmetrically placed with reference to the transverse plane.

2. Confusions of the sort mentioned by Preyer where, with a single sound in either of the lateral hemispheres, an anterior position may be judged as posterior, an inferior as superior, etc.

3. General confusions within the median plane, where the sound-intensities are equal.

4. The possibility of placing one of two fusing sounds in either of the two lateral quadrants without altering the subjective localization.

5. The different localizations of the phantom-sound by different individuals; and different localizations by the same individual at different times.

6. The movement of the phantom-sound when the intensity of one of the two objective sounds is increased.

7. The double misplacements shown in Table VIII., p. 67.

8. Localizations in the horizontal plane of the phantom-sound coming from two telephones on the transverse arc.

9. The peculiar behavior of either one of two nonfusing sounds, pointing to a reinforcement of intensity.

10. The fact that the position of an intracranial sound with reference to the median plane is determined by the relation of the two intensities.

This thought of distribution of sound intensities is not to be interpreted as meaning that it is ordinarily possible for either ear to perceive that it is being more strongly stimulated than the other. This may indeed occur under special circumstances. But all we need assume here is that each ratio of intensities finds some different *central* representation, this central "representation" coming into associative connection with an appropriate movement of eyes, head or body in a manner to be made more explicit later. (See pp. 192 ff.)

2. But manifestly this first factor does not by itself fulfill the conditions of non-interchangeability and capacity for gradation to the extent desired. The same ratio of intensities is to be found at most widely distant points, and as for gradations, they exist but are altogether ambiguous in their meaning. For the same gradation may mean, *e. g.*, a passing of the sound from an extreme lateral position in any one of numberless directions. Were the head a perfect sphere with the auditory canals at its opposite poles, and were the relative intensities of the sound the only factor at the ear's disposal, the only position of certain localization would be in the near vicinity of the auditory axis. Attempts at localizations in other positions would be simply futile. For, considering any sounding body to be upon the surface of a sphere whose center coincided with the center of the spherical head, the distribution of intensities to the two ears would be the same *for every point* in the circumference of a small circle passing through the sounding body and lying parallel to the median plane. A determination of a sound, then, to the circumference of its small circle would be possible, but not to any particular position upon this circumference.

These considerations make it very apparent that the ratio of intensities must be supplemented by a further factor before the local signs become utilizable. This second factor is manifestly to be looked for in those changes of acoustic quality which are due to the form and position both of the head itself and, more especially, of the external ear. It may be defined as *that characteristic which the sound possesses in consequence of modifications wrought by the influence of the form and position of the head and pinna.* This second factor refers then rather to the *clang-quality* than to the intensity of the sound. By its addition to the first factor, those positions, which on the basis of the latter alone are indistinguishable, become capable of separation. On this supposition the fact is explained that we are not always subject to any given auditory illusion, that is, the non-persistent character of auditory illusions is made clear. And at the same time the possibility becomes apparent of a relatively high degree of perfection in respect to the conditions of non-interchangeability and gradation. The evidence for this factor has been too prominent in the preceding text to require extended rehearsal here. Nevertheless a summary of points may be given.

The necessity, for localization, of the accessory factor of modifications of clang-quality is shown by:

1. The fact that the liability to false localizations is not invariable but may be surmounted, at least occasionally.

2. The possibility of any sort of localization within the median plane, together with the possibility of education in this respect.

3. The peculiar positions of the various degrees of median plane delicacy.

4. The usual shifting of the phantom-sound from 0° to 180° when two sounds, placed symmetrically with re-

spect to the median plane, have reached certain positions in the posterior quadrants.

5. The general possibility of monaural localizations.

The local sign of direction is accordingly composite, and contains at least these two factors. Whether other factors also enter in it is impossible at present to say. Possibly accompanying sensations from pinna, tympanum and tensor muscles may coöperate under certain circumstances to give greater definiteness to the local signs as we have supposed them to be constituted. But while no wholly decisive grounds have ever been adduced against this supposition, there are also no facts that tell unmistakably in its favor. If these additional factors exist, they simply blend with the two chief factors above discussed. In any case we have to think of the process of localization as based upon the central equivalent of a composite local sign. Unfortunately the localizations made are not always true to external reality, for at the best the differences between the local signs are too slight for exact spatial determinations, and the practical needs of life have never made necessary that sharpness of attention which might discriminate still further the differences that exist.

II.

It is incumbent upon us at this point to examine the theory of Professor Münsterberg according to which (see pp. 38 ff.) the local signs of direction are the movement-sensations—actual or remembered—that come from turning the head toward the source of sound. It will be remembered that these movements of the head are, according to the theory, evoked reflexly by excitations of the semicircular canals. Although we have seen cogent reasons[1] for a total rejection of this particular mode of producing the head-movements, it is in place here to discuss the general theory of movement-sensations as forming the content of the local signs. Professor Münsterberg's attempt was to do for the ear what Lotze had done for the eye, the reflex movements of the head and the resulting sensations being the counterparts of similar movements of the eyes and the sensations resulting therefrom. Apparently withdrawing from his first opinion, he suggested later (27, p. 472) that these movements of the head, or motor impulses representing them, might be "set free in the brain through the action of the two-sided auditory stimulus," that is, one may "suppose that the difference in the physiological excitation of the two ears conditions the motor impulses with which the head reacts physiologically to the sound." Just that sort of auditory excitation, then, which we have assumed as *constituting* the local sign is made to arouse reflexly the appropriate movements of the head, or at least to arouse impulses to such movements, the movement-sensations,—actual in the former case and remembered in the latter,—to be taken as the real local signs of direction. It behooves us therefore to examine this position and make clear our own motives for not accepting it. The full reason for this course can first become wholly apparent when we come to discuss the theory of auditory space perception in Part III. In the meantime, however, we may state

[1] See pp. 42–47.

several considerations that make for the position that we have adopted.

In the *first* place the movement-sensation theory of local signs was developed in the interests of a genetic theory of space perception, for which movements and the resulting sensations are the fundamental necessities. For us this necessity of appealing to movements in such a direct manner does not exist. We shall be obliged to make use of movements, to be sure, but their function will be secondary, not primary as for the genetic theory.[1]

In the *second* place the theory of reflex movements of the head seems always to demand absurd impossibilities. Its advocates would seem to imply that the head may be made to face any position from 0° to 180° without movement of the body, as if forsooth it were set on a swivel and could serenely rotate to any point in the horizontal circle, the only limitation imposed being that it may not pass beyond 180° in either direction. Certainly if one speak without qualification of localizations by means of impulses to head movements, it becomes hard for the imagination to construct the exact manner in which these impulses are to be carried out. The fact is that a rotation of the head beyond 60°–80° is simply impossible. After that, movements of the whole body must be made to make possible the turning of the gaze to the rear. All this is so evident that it can hardly be supposed that it has been overlooked by those holding the movement-

[1] Attention may also be called here to the fact that the phenomena of localization during dizziness, described in Chapter 6, may be so interpreted as not to be regarded as necessarily supporting a genetic theory. Such an interpretation we have attempted to give.

sensation theory, but the wording of the theory would certainly imply such an oversight.—But even within the region of possible head movement it is not always the head that executes any actual response to auditory excitation. The eyes may move to the source of sound, leaving the head almost stationary.—That is, to sum the matter up, the motor impulses, if such there be, may be to movement of the eyes, of the head, or of the whole body, according to circumstances. Surely the matter is not of such simplicity as statements of the theory would lead one to expect.—The state of affairs here is much as in the case of the eye where an analogous oversight is ordinarily made. Advocates of the Lotzian theory of local signs presuppose that the eye can so move as to bring to the fovea any excitation that falls upon the retina. Now it is absolutely impossible to thus bring to the fovea an excitation on the outlying parts of the retina *unless the head be moved as well as the eye.* If then sensations of movement are to be made the local signs of vision, these sensations must come not only from the muscles of the eye but from those also that move the head. And accordingly no such simple formulation of the matter is possible as would be implied in the statement that sensations from movements of the head subserve hearing as those from movements of the eyes subserve vision. The fact of the matter is that the former do not belong exclusively to hearing nor the latter to vision. Each may minister to the ends of either. One becomes suspicious therefore of a theory that does not recognize these difficulties, and in striving to formulate any theory of local signs that shall escape these difficulties one is brought to the necessity of searching for visual local signs among purely visual phenomena and for auditory local signs among purely auditory phenomena. This is the necessity which we have felt and which we have endeavored to follow. The association formed between the strictly auditory local signs and movements of eyes, head, or entire body we shall examine later.

In the *third* place, the theory that we are combating has endeavored to derive support from certain facts reported in reference to infants turning their heads towards the sources of sounds. These movements coming very early have been looked upon as reflexes set free by the acoustic stimulus. Now the general fact of such head movements on the part of infants is undoubted. That they first occur within the period extending from the tenth to the seventeenth week seems to be vouched for by the combined testimony of nearly all observers (69). Still the manner in which these movements are accomplished is not equally certain and the evidence is by no means conclusive in favor of the 'reflex' theory. Preyer, to be sure, remarks (61, p. 85) that in the sixteenth week the head was turned to a sound "with the certainty of a reflex movement." Darwin, however, reports (50) that the infant observed by him, though sensitive to sound very early, "was not able even when 124 days old easily to recognize whence a sound proceeded, so as to direct his eyes to the source." Perez' observation (60) is rather ambiguous, it being that at 15 months "the ear is promptly turned to the source of sound." Most instructive are the remarks of Mrs. Moore (58). Speaking of the reactions of her child to sound, she says: "No reactions whatsoever pointed to an inborn ability to localize sounds. . . . Simply to look at an object from which a sound issued was the first step towards localization. This the child did on the fourth day, in looking at a person speaking to him. As the localization of sound was not established till somewhat after the child has formed the habit of turning the eyes and head in order to see, a connection in development may have existed between the two acquirements. It is certain that the eyes had many times followed noiseless movement, and movement accompanied by sound, before the accurate localization of the direction of sound was established. . . . By the eighteenth week he could locate very well sounds coming from objects within the visual field. . . . In the twenty-seventh week, when he could sit, it was found that he could not localize sounds which came from behind him, but looked for their sources in front."

One is certainly not justified in concluding on the basis of these observations that there is an early reflex which inevitably and unerringly responds to the acoustic stimulus. It would seem rather that the process of turning the head to sounds develops by the aid of visual sensations. The eyes are turned to objects much earlier than the head is turned to sounds. All authorities agree in respect to this. Remembering then how easy it is even for the adult to form associations between sight and movement,[1] one may conjecture that all movements of the head to sounds, in so far as they display any considerable degree of accuracy, are first made possible through

[1] Think, *e. g.*, of the rapid adaptation of the arm to the new conditions imposed by attempting to touch objects seen through prismatic lenses.

associations with searching-and-finding movements of the eyes. Then little by little the auditory quality dependent upon position would come to be associated directly with the movement necessary to bring the sounding body within the field of view. This hypothesis requires only that one suppose a congenital aptitude for turning the head to the right, rather than to the left, if the sound happens to be upon the right. This much in the way of inherited tendency may perhaps be safely allowed. Experience then provides rapidly for the gradations within either lateral field. As vague as it may be, this hypothesis seems to make room for the facts of the case better than the 'reflex' hypothesis. To bring this matter into more definite form carefully planned observations should be made upon infants born blind. Such observations would unquestionably give valuable data for the question at hand.

These are some of the reasons for adopting a theory of local signs couched in terms of auditory qualities in preference to one that employs movement sensations. Further considerations we have already promised to give in Part III.

PART II.

THE LOCALIZATION OF SOUND IN RESPECT TO ITS DISTANCE.

CHAPTER 1.

I. PROPOSED THEORIES.

RELATIVELY speaking almost no experimental material is at hand in connection with this phase of auditory space perception. The crude observations of everyday life seem for the most part to have been deemed sufficient. Accepting these observations as exact enough, the theory was early propounded that the auditory perception of distance is based upon the varying intensities of the sound heard. An accurate perception would require then that the sound be a more or less familiar one, for otherwise neither the absolute intensity at a given point, nor its variations as the sounding body moved, could be interpreted with any degree of assurance. The entire English School of psychologists, in so far as they have expressed an opinion, seem to have held this view. Berkeley led off by stating in Sec. 46 of his Essay on Vision that " by variations of noise I perceive the different distances of the coach," and this opinion has been somewhat slavishly followed ever since.

In any case theory has everywhere preceded the systematic gathering of facts. Let us therefore look first at the theories propounded by various writers, notably by the physicists. Practically only two theories exist: (1) the simple intensity-theory as above outlined, and (2) a modified form of the intensity-theory in which the influence of partial tones is made to play a part.

1. Steinhauser (40, p. 185) may be made to represent the first theory. The perception of distance, he says, is wholly dependent upon intensity, and the distance may be estimated *if the source of sound be known.* "For then the distance at which the source of sound is situated can be empirically determined from the difference between the perceived intensity and the known absolute intensity, or that which the source of sound would have in the immediate neighborhood." Thus the distance of an enemy is estimated by the loudness of the cannonading, the remoteness of the sea by the intensity of its roar, etc. And the plausibility of this theory as applied to the experiences of ordinary life is very great. We do undoubtedly judge the remoteness of sounds on the basis of the auditory intensities, and for this reason we are sometimes subjected to illusions. In my own case, for example, the noise made by a chemical fire-engine distant perhaps a hundred yards behind intervening houses was taken to be the hissing of steam from the radiator close by. The intensity of the noise made by the escaping steam was about equal to that made by the distant fire-engine. Hence the illusion. When, however, the real nature and distance of the sound became known, deception was no longer possible. The sound seemed now to possess a distant character, and no amount of effort could make it seem like a similar faint sound near by.

2. Such an observation as this latter would seem to indicate that something beyond mere crude intensity is involved in the perception of the distance of sounds. In fact it remained only for physicists to discover more definitely the composite nature of sound to render inevitable the introduction of modifications in the simple intensity theory. Mach (24, A and B) may well represent to us the modified theory. The clang-color of any sound, dependent as it is upon the existence of overtones, will obviously be conditioned by the degree of intensity of the sound. Now the particular changes in clang-color are such that when the sound is increased there is for sensation a relative predominance of the *lower* partial tones, while if the sound be diminished there is similarly a preponderance of the *higher* partial tones. And since changes of distance are equivalent to changes of intensity, it will thus be possible to base all estimates of distance upon variations in the clang-color of the sound, the degree of prominence of the upper partial tones over the fundamental being correlated through experience with corresponding degrees of remoteness. There is thus, Mach says, a sort of *aërial perspective* for hearing.

These theoretical conjectures in reference to the perception of distance were confirmed by Mach to a certain extent. His experiments showed that the artificial increase of overtones relatively to the fundamental caused the sound to seem more remote. This was found to be true for tuning forks and for the human voice. For example, he increased the pressure of air against the drum-membrane to such an extent that the latter was incapacitated for the transmission of lower tones. Under these circumstances voices seemed to come from a distance, as if spoken by a ventriloquist.

Grinwis (13) some years later made a mathematical calculation of the relative intensities of partial and fundamental tones at extremes of distance. His results are that the clang-color alters with the distance of a sound in such a way that "the intensity of the *n*th overtone as compared with that of the fundamental is *n* times as great for very distant sounds as for those near by. (Page 447.) The superiority of this theory over its predecessor is readily apparent. It remains only to make a more systematic experimental verification of its claims.

A further suggestion towards a theory was made by Thompson (42, C, p. 415) but since he himself hardly took the matter seriously, it is not to be wondered at that the suggestion bore no fruit. He says, namely, that the perception of distance may be assisted by "acoustic parallax." That is, since the lines of direction drawn from the two ears to the sounding body tend to become more and more alike as a sound recedes into the distance, the perception of these changes of direction may be of aid in the estimation of the particular distance at which a sound may lie. A sufficient rejoinder to this suggestion is that the ears cannot thus function separately in the perception of the direction of a single sound.

II. RECORDED EXPERIMENTS.

The small amount of experimental work done in this line may be briefly summarized.

VON KRIES, 1890.

In connection with his research on the perception of direction, von Kries made a few experiments in reference to the auditory recognition of distance. (17, B, pp. 246, ff.) Using the sound of a telephone placed at either 25 cm. or 65 cm. from the ears, the subject was required to indicate at which position the sound was given. A resistance coil lay in the circuit and all degrees of intensity were irregularly given, in order that, if possible, any misinterpretations based upon the perceived intensities might become manifest. The results showed that under these circumstances illusions of distance are not easy to produce. The results were:

Series 1. Out of 27 trials, 24 were correct.
" 2. " " " " 23 " "

Then a sound of a different character was tried, that, namely, made by snapping together two wooden disks. Two sounds were given in succession, at 20 cm. and 40 cm., and then at 100 cm. and 140 cm. respectively from the subject. The latter was required to state whether the second sound was nearer or more remote than the first. *All judgments were correct*, and changes of intensity could not deceive the subject.

Thus von Kries finds the ability to perceive the distance of a sound greater than he had expected from the general theory of localizations based on changes of intensity. He is inclined to attribute this ability to the keenness with which slight acoustic differences are discriminated.

BLOCH, 1893.

Bloch also included experiments on distance in his series of investigations. (5, p. 56.) He found that with a well-known sound it was impossible to deceive the observer by increasing the intensity of the farther sound or by diminishing that of the nearer. But by employing two sounds of unlike character given successively at the same or different points, success in localizing was by no means as great. The two sounds were made respectively by the striking together of two metallic bodies (Politzer's Hörmesser) and by the impact of metal upon wood. Thus one sound was sharp and ringing, the other relatively faint and flat. By this means the ability to perceive differences of distance was investigated for directions in front, behind, above, below and at the sides. The universal result was that within a certain distance (25 cm. in front and at the sides, and approximately 40 cm. for the other directions) all perceptions were correct. But at points more remote confusions at once began to arise, and *the flat "wooden" sound was constantly perceived as the more remote.* This perceptual transposition of distances occurred even when the position of the metallic sound was farther by 50 cm. and over. We are not enlightened as to the number of observations made, but the results obtained seem to the author to justify the conclusion that in judging the relative distances of two sounds we are dependent upon the intensity not of the sound as a whole, but of the constituent parts that the sound contains. If we are in any way led astray in reference to these, we lose our sole acoustic means for perceiving distance.

MATSUMOTO, 1897.

Dr. Matsumoto (25, pp. 57–65) shows the general dependence of distance upon intensity by giving to his subjects sounds of unknown intensity and asking them to state the apparent distance. Two fusing sounds were used, one on each side, and various degrees of intensity were given in irregular order. The uniform result was that degrees of intensity from high to low were correlated with perceived distances from near to far.

The particular relation between the apparent distance of a sound and its intensity was found as follows: The sounding body being moved away from the observer in the direction of the line of sight, the observer was first required to state when it was distant one foot. Then by successive soundings at this and more distant points, positions were selected by the observer which were subjectively two, three, four and five times as distant as this point of reference. This being done the mental and physical scales were compared, the law of inverse squares was applied to the determination of the relative intensities, and the conclusion deduced that Weber's law seems to hold; that is to say, "that when the intensity of a sound diminishes in geometrical progression the perceived distance of the sound increases in arithmetical progression." (Loc. cit., p. 60.)

III. OUR OWN EXPERIMENTS.

1. *The perception of distance as dependent upon various factors.*—The general content of the above theories and experimental results was made the starting-point of our investigations. Those reported in this section were carried on at the Harvard Psychological Laboratory in the year 1892–93, but their publication has been deferred until now. Our purpose was to ascertain as far as pos-

sible the influence of the *pitch*, the *intensity* and to some extent the *quality* of sounds in the perception of distance. Various sounding-bodies were used, tuning forks, open and closed organ pipes, telephones and an electric bell. The method of procedure was to give two sounds successively at distances of 10 and 15 feet, or 15 and 20 feet, the subject being required to state the relative position of the second sound. Unless otherwise stated the sounds were always given directly in front of the observer, and the order in which any two succeeded each other was constantly varied. The two sounds were also given frequently at the *same* distance. Two well-trained observers, B. and W., acted throughout as subjects. The interval between the sounds was from 2 to 3 seconds and the duration of each sound was approximately 2 seconds. The following tables present the results of 1,200 experiments grouped with reference to their particular conditions. Manifestly *the character of the errors made* is the all-important thing to be noted.

Influence of Pitch.

A.

Tuning forks of various pitches. Distances, 15 and 20 feet. Intensity medium. Number of experiments = 80.

I.

Errors.	Character of Errors
25	Higher located nearer, 11. Lower " " 14.

N. B. Since in all our experiments the perceived position of the second sound was given by the subject, there were four sorts of judgment, "higher nearer" and "lower farther"; and "lower nearer" and "higher farther." It will be seen that the members of each pair are identical. We have reduced the judgments always to the nearer sound.

B.

Two organ pipes (König), *Sol*$_3$ and *Ut*$_4$, open. Distances, 15 and 20 feet. Medium intensity. Number of experiments = 80.

II (*a*).

Errors.	Character of Errors.
36	Higher nearer, 20. Lower " 16.

Same as above. *Fa*$_2$ and *Ut*$_3$, open. Number of experiments = 80.

II (*b*).

Errors.	Character of Errors.
49	Higher nearer, 47. Lower " 2.

C.

Two telephones, one slightly higher than the other. Distances, 10 and 15 feet. Intensities equal. Number of experiments = 80.

III.

Errors.	Character of Errors.
17	Higher nearer, 10. Lower " 7.

Combining tables I., II. (*a*), II. (*b*) and III. Total number of experiments = 240.

TOTALS.

Errors.	Character of Errors.
127	Higher nearer, 88. Lower " 39.

The interpretation of these tables is not altogether easy. Probably they should not be interpreted too rigidly. In fact the figures of Table II. (*b*) are the only ones with any appearance of decisiveness. Here the 47 errors in the direction "higher nearer" seem to have been due to some real cause and not merely to chance, since they are very nearly equally divided between the two observers. Thus B.'s errors were 25 and W.'s 22. Whether this predominance of error was due to the pitch in itself or to the possible presence in the higher sound of a greater number of overtones, cannot now be determined. Personally I incline to the thought that the particular structure or adjustment of the Ut_3 pipe caused it to give a tone relatively rich in overtones, a tone which seemed therefore relatively nearer.

INFLUENCE OF QUALITY.

A.

Organ pipes, both Ut_2, one open, one closed. Distances, 15 and 20 feet. Intensities equal. Number of experiments = 80.

IV.

Errors.	Character of Errors.
30	Open pipe nearer, 20. Closed " " 10.

B.

Organ pipe, Ut_3, open, and tuning fork, Ut_3. Distances, 15 and 20 feet. Intensities medium. Number of experiments = 80.

V.

Errors.	Character of Errors.
44	Open pipe nearer, 31. Tuning fork " 13.

C.

Tuning fork, Ut_4, and telephone. Distances, 15 and 20 feet. Intensity full for telephone, medium for fork. Number of experiments = 160.

VI.

Errors.	Character of Errors.
55	Tuning fork nearer, 36. Telephone " 19.

Telephone and electric bell. Distances, 15 and 20 feet. Full intensities. Number of experiments = 80.

VII.

Errors.	Character of Errors.
35	Telephone nearer, 27. Bell " 8.

Most of the results in these tables that express the influence of *quality* are much more unequivocal in meaning than in the case of pitch. The general rule seems here to be in evidence *that of two sounds of different quality that one is apt to be more often falsely perceived as nearer which contains more overtones, and which is ac-*

cordingly a fuller and richer sound. This is well seen in Tables IV. and V. In the former case the sound from the open organ-pipe was more often falsely perceived as nearer (or the closed pipe farther, as the case may have been). But open pipes are known to be richer in overtones than the closed. The whole sound is freer and fuller, while that of the closed pipe is comparatively muffled. Similarly for Table V. The tuning fork gives a comparatively pure tone, while the open pipe again gives a tone the components of which are of a good degree of intensity. The results of Table VI. are perhaps a little more difficult to interpret. The buzz of the telephone is certainly a more composite sound than the tone of the tuning fork. It might seem therefore that the errors should be in the reverse direction. But the fork was excited by striking with a rubber-shod hammer, and under these circumstances there is always a high, sharp overtone audible for some seconds after the striking. It is highly probable that the presence of this clearly audible overtone in conjunction with a prominent fundamental gave to the sound a strongly marked 'near' character. In comparison with another and richer *tone*, as in Table V., this characteristic was not to be noted, but in conjunction with a *noise* it became apparent. In regard to Table VII. it is perhaps impossible to say anything decisive. We know that the successive strikings of an electric bell tend to emphasize the partial tones at the expense of the fundamental. This may have been influential in making the bell appear more remote. Or, since only relative distances were required anyway, it may have been that the telephone sound, being well known to the observers, was more correctly located *absolutely*, while the ring of the bell being a relatively unknown sound tended continually to be placed at a somewhat remote point.

The above results in respect to quality agree substantially with those of Bloch (p. 162) who found that the false perceptions of relative distance were such that the flatter sound was heard as the more remote.

Influence of Intensity.

A.

Organ pipes, La_2 closed and Ut_3 open. Distances, 15 and 20 feet. Intensities variable, an equal number of faint and loud being given. Number of experiments = 160.

VIII.

Errors.	Character of Errors.
91	Louder nearer, 71. Fainter " 20.

B.

Two telephones. Distances, 10 and 13 feet. Intensities variable, faint and loud equally distributed. Number of experiments = 320, 80 each in front, at both sides and at points situated 30° from the median plane. In the last case one of each pair of sounds was given on either side of the median plane.

IX.

Errors.	Character of Errors.
114	Louder nearer, 110. Fainter " 4.

The influence of *intensity*, as apparent in Tables VIII. and IX., is unmistakable. The only point to be explained is why there were so many errors when, especially in the case of the telephones, the sounds were well known. Von Kries and Bloch, operating with distances never over 150 cm., found it impossible to deceive the observers by irregular variations of intensity. The reason for our large number of errors (over 35% of the whole number of experiments) is to be found evidently in the fact that we operated with much greater distances, and the possibility of deception by changing intensities varies certainly in some way directly with the distance.

The upshot of this whole section is then that in the localizing of distance we are dependent upon two factors, (*a*) the crude intensity of the sound, and (*b*) the relative fullness and richness of the sound, that is, the presence or absence of overtones.

Whether the relatively slow diminution of the overtones of a receding sound is one of the factors in the perception of distance, the above recorded experiments hardly allow one to judge.

2. *The delicacy of the auditory perception of distance and its relation to Weber's law.*

The general conformity of the perception of distance to the requirements of Weber's law have already been established by the experiments of Dr. Matsumoto (see p. 163). The *delicacy* of distance perception has however not yet been investigated. The end aimed at in the experiments about to be reported was twofold: (*a*) to ascertain the delicacy of the perception of differences of distance at various points; and, since an increase of the distance of a sound is, broadly speaking, a decrease of its intensity, to discover (*b*) whether the ascertained changes of delicacy tally with the demands of Weber's law for mere intensities at a constant distance. The investigation here described was carried on at Amherst College during the year 1897–98. There were two well-trained subjects. (*a*) The arrangements of apparatus and the manner of experimenting were as follows. The subject was seated at the end of a light wooden rod 1 ½ meters in length, graduated to centimeters. This rod lay in the median plane about 6 cm. below the line between the ears, and with its nearer end 20 cm. from the transverse plane. As before in most of our experiments, the sound was that of the telephone. The delicacy of distance perception was then determined as follows for the seven points from 20 cm. to 140 cm. from the transverse plane. The sounding body was held always in the horizontal plane, the guiding rod being sufficiently far below to obviate any possible conducting of the sound by it. The method employed was that of minimal changes, and the *nearer* threshold was determined for each point; that is, the sound was moved from a given point towards the observer until it was just perceptibly nearer, then moved back from a point of still clearer difference until it was just *not* perceptibly nearer. Experimentation was begun in a somewhat small room, but after the appearance now and then of unexpected fluctuations in the discriminative power exhibited by the subjects, and after much groping for an adequate cause for this, it was decided to experiment in a much larger room where the influence of sound-reflections from the walls would be excluded.

The following table contains the carefully found results obtained in the large room from one subject. The number of determinations will be seen to be *four* for each point. This is a small number, to be sure, but this need not, I think, be made an obstacle to the full acceptation of the results, because being taken in June, they represent the essence, so to speak, of all the practice that had extended at intervals from the preceding November. A degree of skill had been then reached which enabled the subject to judge quickly and with good subjective satisfaction.

TABLE XVIII.

JUST PERCEPTIBLE CHANGES IN DISTANCE OF SOUND.

Subject C.	Distances in Centimeters.						
Standard Distances.	20	40	60	80	100	120	140
Experiment 1.	+5 —4	+7 —5	+8 —6	+10 —11	+14 —12	+15 —13	+19 —18
" 2.	+5 —5	+5 —4	+6 —5	+10 — 9	+15 —12	+14 —14	+17 —19
" 3.	+3 —3	+6 —5	+7 —7	+ 9 —10	+13 —11	+12 —13	+19 —19
" 4.	+4 —4	+6 —6	+7 —4	+ 9 — 7	+12 —11	+14 —13	+17 —16
Average.	4.1	5.4	6.2	9.4	12.5	13.5	18.0

The plus and minus in the above table indicate respectively the movement towards and away from the subject. In unmistakable fashion then the table presents the naturally expected fact that the greater the distance of a sounding body, the greater must its change of position be to be recognized as such.

TABLE XIX.

AVERAGES OF RESULTS OBTAINED FROM THE TWO SUBJECTS IN THE SMALLER ROOM.

Distances.	20	40	60	80	100	120	140
Just perceptible change............	4.5	5.5	9.1	11.1	12.3	15.4	18.6

N. B. All numbers given in centimeters.

This table, though not wholly reliable, may serve as comparison table to the one given above.

(*b*) The next thing to be determined is the degree of conformity between the results of Table XVIII. and the figures demanded by Weber's law for the discrimination of intensities of sound. On the hypothesis that intensities vary inversely as the squares of the distance, and on the basis of the fact that in accordance with Weber's law $\frac{1}{3}$ of any intensity must be added to it to be perceived, the distances were calculated for each point experimented upon through which it would be necessary to move a sounding body in order to produce an increase of its intensity by $\frac{1}{3}$. The calculation and the results are as follows:

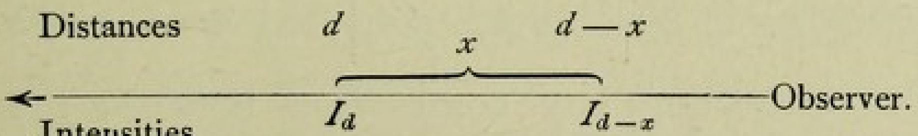


Fig. 18.

Let the unit intensity be that given by the telephone when at a unit's distance from a line joining the ears. Let d be any distance expressed in terms of the unit distance. Let x be the decrease in distance from any point d, corresponding to a just perceptible increase of intensity. Also, let I_d be the intensity of the sound at the distance d, and I_{d-x} the intensity at distance $d-x$.

Then

$$I_d = \frac{1}{d^2}$$

(standard intensity being 1) and

$$I_{d-x} = \frac{1}{(d-x)^2}$$

Then by Weber's law:

$$\frac{1}{(d-x)^2} - \frac{1}{d^2} = \frac{1}{3} \cdot \frac{1}{d^2}$$

$$\text{Whence } x = d\left(1 - \frac{\sqrt{3}}{2}\right)$$

$$= d(0.134 +)$$

Substituting the values of d corresponding to the seven distances experimented upon:

TABLE XX.[1]

Standard Distances.	20	40	60	80	100	120	140
A. Values of x..........	**2.7**	**5.4**	**8.0**	**10.7**	**13.4**	**16.1**	**18.8**
B. Figures from Table XVIII...	4.1	5.4	6.2	9.4	12.5	13.5	18.0

Before instituting any comparison between the two lines of figures in Table XX., it should be remarked that the law of inverse squares is not absolutely true for sound-intensities. Schäfer (34, D) found as the result of

[1] Strictly speaking the value of d to be substituted in the above formula should be, not the perpendicular distances from the center of the line connecting the ear drums, but the *oblique distances* from the ears themselves. But by using the perpendicular distances the problem is simplified and no error is introduced until the second place of decimals is reached.

a carefully executed series of experiments that up to a certain point the intensity of the sound diminishes more slowly, while beyond that point it diminishes more rapidly than the law of inverse squares demands. And since Schäfer employed the sound of a telephone, thus being under much the same conditions as ourselves, and since the point where the law of inverse squares held was at the distance of a meter from the subject, we cannot be far wrong in saying that the first four figures in row A of Table XX. are probably all too *small*. For if the intensities decrease less rapidly than we assumed, they must all be greater than assumed at the distances 20, 40, 60 and 80 cm. And, consequently the values of x, which represent these intensities multiplied by the fraction $\frac{1}{3}$, must actually be *greater* than those given in the table. In a similar way values of x for 120 cm. and 140 cm. should probably be smaller than those given. Comparing rows A and B then, with the above in mind, we see that with the exception of those for 20 cm. and 140 cm.,[1] *the figures for the least perceptible distance are somewhat smaller than those calculated on the basis of mere intensity*. Of course the drawing of any rigid inferences here would require a much larger mass of experimental data gained from many subjects. Still the assumption seems not to be altogether unwarranted that in the percep-

[1] The necessary objective movement of the sounding body at 20 cm. would be too large from the very nature of the case. For, as will be remembered, the sounding body was moved towards the face, and since the point 20 cm. was less than 10 cm. from the tip of the nose, the ability to make any sort of discriminative perception was largely precluded. As to 140 cm., judgments at that point were comparatively uncertain and doubtless the figure given is not wholly correct.

tion of distance-changes we have recourse to other criteria than mere variations of intensity. One such criterion we have already seen reason to accept, viz., the relative intensity of overtones with respect to their fundamental. Perhaps there is no other to be discovered. But that we are not wholly dependent upon intensity-changes would seem to be evident, for the introspective reports of both observers showed that other differences in the character of the sound were often noticed. For instance, when two sounds were given successively at the same point, the subject had no hesitation in saying 'the same.' But when they were given at slightly different distances, it happened repeatedly that a difference in the character of the sound could be noted of such a sort that the judgment 'the same' could no longer be given, while at the same time the judgment 'nearer' was equally impossible. A still further spatial difference was necessary in each case before the spatial perception 'nearer' arose. It seems to be true, therefore, that differences of acoustic *quality* coöperate with differences of *intensity* to constitute the basis for perceptions of auditory distance.

Of course it is not for a moment to be imagined that the perception of differences of distance is consciously based upon perceived differences in the character of the sound. Subsequent analysis shows, to be sure, that the spatial perception is first made possible by these differences, but *it is not* these differences themselves. At a certain point the spatial perception emerges as distinct and independent in character as when two compass points upon the skin pass the threshold limit of separation and are perceived as resting upon two spatially distinct spots.

CHAPTER 2.

THE LOCAL SIGNS OF DISTANCE.

THE localization of sound in respect to its distance is thus accomplished by the aid of two factors, changes of intensity, and changes of quality or clang-color due to the variations of relation between the constituent elements of the sound. This fact has been exhibited to us both in the section dealing with errors of localization and in that treating of the delicacy of the perception of distance. The two factors named may be well compared respectively to mathematical and aërial perspective in visual space perception. The intensity of sounds decreases with the increase of distance as does the apparent size of objects seen. And just as mathematical perspective can give but uncertain aid until assisted and supplemented by aërial perspective, so mere intensity of sound is often misleading unless modifications of quality are also present. Judging the distance of an unfamiliar sound on the basis of its intensity alone is much like determining the remoteness of an unknown seen object by means solely of its retinal image. A satisfactory perception of distance is simply impossible in both cases. Auditory local signs whose content is intensity merely are sorry helps to the perception of distance. Timbre must be added to intensity, and then localization is in somewhat better case. Still, at the best, the auditory preception of distance is based upon "secondary" factors only. "Auditory perspective," like mathematical and aërial perspective in vision, can be employed successfully only when the nature of the sounding body is more or less well known. Occasionally therefore we are liable to gross errors and illusions. And yet when our experience with sounds has become large, and when it becomes important for our well-being that we should perceive the distance of sounds with some degree of accuracy, we find that the ability thereto is not wholly wanting.

PART III.

CONCLUSION. THEORY OF AUDITORY SPACE PERCEPTION.

THE part of our task that deals with facts has now been discharged. The common division of the facts of space perception into those connected first with the *delicacy* and second with the *accuracy* of such perception has been kept mostly in the background, this scheme of treatment having been rejected in the interests of a clearer exposition along other lines. At the same time the endeavor has constantly been made to allow both of the above named aspects to receive full recognition at the appropriate time and place.

In addition to mere facts, we have also called attention somewhat to the psychophysical problems involved, and we have tried to meet these problems in part by a doctrine of local signs that strives to give expression to the underlying phenomena of sound-localization.

It remains now to come to close quarters with the question of an independent auditory space, and, having taken a stand in regard to this, to show as well as possible how our finished auditory perceptions have attained their spatial characteristics of position and arrangement. Two corresponding divisions of this section will therefore be necessary.

I. POSITIVE GROUNDS FOR ASSUMING AN INDEPENDENT AUDITORY SPACE.

The point of contention in the matter of theory has already been set forth clearly enough in the introduction. The issue is sharp between the theory of *direct* perception in accordance with which auditory impressions are themselves endowed with spatial attributes, and that of *indirect* perception, in accordance with which the seemingly spatial attributes of auditory impressions are simply borrowed in some systematic way from the visual and the tactual fields. The contrast is that between independence and dependence, and the direction of a search for positive grounds for an *independent* auditory space is thus somewhat clearly mapped out for us. For if it can in some way be shown that certain auditory localizations take place under circumstances which absolutely preclude the coöperation of either visual, tactual or motor factors, the point of independence will have been proved. But such localizations have been already met and described—the *intracranial* localizations—and to these we at once appeal in the interests of theory, endeavoring thereby to vindicate that independence which hitherto we have not only tacitly assumed but occasionally expressly implied.

The claim that we wish to make may be very simply stated. The intracranial space belongs neither to vision nor to touch, and hence these cannot be called upon to loan to hearing those characteristics by means of which particular auditory impressions become located within the head. *The intracranial localizations of sound must be independently accomplished.* Their positional determinations cannot possibly be borrowed. To be sure, we have in pictorial imagination images of the interior of the head, and we can readily fancy movements of the exploring fingers to various points within the skull, but neither vision nor touch has any actual experience with the endocephalic masses. It is they rather which must make use of hearing for the full development of the imagination of this, to them, forbidden region. Endocephalic local signs for vision and touch are impossibilities, and consequently the supposition of " ransferred " localizations, conceivable enough in the case of ordinary auditory localizations, becomes here altogether inconceivable. Unless, therefore, one be willing to rest content in the presence of palpable absurdities, or unless one be satisfied to simply disregard intracranial localizations and leave them totally unexplained, the assumption of the independence of these localizations seems once and for all unavoidable. Absurdities may be entertained and facts may be disregarded, but none the less the phenomena in question clamor for satisfactory interpretation—phenomena which are as well-attested and as deserving of recognition as any within the whole field of space perception. And it is, in our opinion, the failure to realize the full purport of these phenomena that has kept auditory space so long from its own rights and condemned hearing to the ignominious position of wholesale borrower from its more fortunate associates.

Two considerations may conduce to a clearer comprehension of the matter. (*a*) In the first place these experiences are as genuinely *auditory* as any of the extracranial localizations. To one who had never perceived them, these experiences might seem perhaps to be of the nature of touch or pressure. But any objection based upon such a conjecture may be immediately silenced by showing that the movement of the phantom-sound is continuous when passing from an extracranial to an intracranial position, and that changes of position within the head follow the same laws as external changes. When the two telephones are made to approach the head, the phantom-sound also usually moves gradually nearer and nearer until first it is "just outside" and then a moment later "inside." The sound does not ordinarily pass at a leap from some relatively distant point to an interior position. On the contrary, the latter position seems rather to be the objective point towards which the sound is moving as it approaches the external boundaries of the head. Then too when once within the head the position of the sound is determined as when without,—at least as far as its position with respect to the median plane is concerned. A greater intensity upon either side determines the sound to that side. And quality too seems not without influence as we saw in Chapter 6. For pitch and manner of combination of the objective sounds have some part to play, however impossible it is at present to define it. These intracranial localizations are auditory, not tactual.

(*b*) In the second place, these localizations are in no sense inferences. Münsterberg (26, pp. 210–211), referring to these phenomena in terms of his theory of motor impulses and muscular strains, says that since a single sound is heard "it must be sought at the point where a single acoustic excitation can evoke antagonistic strains that are equally strong on both sides; it is clear that these conditions are fulfilled only within the median plane and that too only within the head." As if an intracranial localization were accomplished only after reflections of this sort! But a more serious consideration demands attention. Are not these internal localizations made by a process of *exclusion?* Are not certain sounds placed within the head merely because special conditions always present in external localizations are wanting, thus leaving the interior of the skull the only possible locality at one's disposal? The question is well meant, but it is of only apparent moment. These sounds are not located within the head in the sense that they are somewhere there because nowhere else; as a man known to be within a house might be located within a particular room because not discovered elsewhese. They are not only within the head, but *at well-defined positions there.* They are towards the front or rear, towards the right or left, and subjective certainty of the position is so great that one has full assurance of being able to "pinch" the spot, were that possible, between the thumb and forefinger. No, the sound is not inferred to be vaguely within the head. It is immediately and definitely perceived there at a clearly describable spot.

If then these are genuinely auditory localizations, as immediately and confidently made as any that are external, and if it is clear that these cannot come about by the aid of extraneous factors belonging to sight and touch; the only outcome seems to be the assumption of the independent reality of an auditory space. And this assumption appears not to be of the sort that may be justified by acute argumentation, but on the contrary to be one that is inherent in the facts themselves and necessary for their full comprehension. We cannot understand auditory localizations unless we give them position in their own right, and we cannot refrain from assuming an independent auditory space since, in relation to sight and touch, auditory impressions may be assigned to positions in which these have no share, positions, namely, within the limits of the skull. Without further ado, then, we may rest our case here, allowing facts to speak for themselves.

But the admission of independence does not carry with it the conviction of a fully developed auditory space. Indeed we saw in the Introduction to what a subordinate place auditory space must be reduced at the very best. For perceptions of *position* are the only practical determinations that we make for sounds. It is, indeed, not wholly impossible of imagination that under artificial conditions something approaching auditory lines and figures could be perceived. For we know that the positions of two non-fusing sounds can be located with a fair degree of accuracy. Even in this latter case we get an empty auditory extent, and if a series of non-fusing sounds could be given in a line, it is not inconceivable that a linear series of discontinuous but *co-existent* positions could be perceived. The chief difficulty would lie in the matter of *distance*, and intensities would need to be very nicely regulated to produce the desired effect in this respect. But if auditory lines, then possibly triangles, squares, etc., could be produced. As I said, this is not wholly inconceivable. It remains to test the matter by experiment. But in respect to *position* auditory space may indeed attain a high degree of development, as in fact it does in the case of the blind who are forced to depend upon it for all distant experiences.

The reasons for the actual low development of auditory space among seeing people are probably all of a practical nature. Hearing serves vision, but then immediately vision takes the lead and hearing gets continually pushed into the background. But there is another consideration of much moment. Sounds are always liable to be *reflected*. Hardly any sound-waves reach the ear without being somewhat modified in this way. Consequently auditory judgments of direction and distance, not made under conditions that are experimentally controlled, are always exposed to the risk of being faulty. Sometimes we cannot make any judgment at all. We pass down a cross-street to take the electric for the city. We hear a car coming but have no notion in which direction it is going, nor have we any possibility of knowing by the sound, if the street on which we are is bordered by a continuous line of houses. We are all the time being inconvenienced or duped by these experiences. We may be getting direct sound-waves or reflected sound-waves. We cannot tell which. The conditions under which we hear sounds in daily life are such that attempts at localization are much like imaginable analogous attempts to localize visual objects in a field interspersed with mirrors, which would do for visible objects what reflecting walls do for sounds. The helplessness of seeing persons in the face of such conditions is simply inconceivable. Efforts to discriminate between the real and the reflected objects would cause unspeakable annoyance. One would never know whither to turn. And one may readily fancy that if such conditions actually prevailed, and if at the same time the ear were not troubled with reflected sounds, the development of auditory space would far exceed that of the visual. Its superior practical advantage would give it the supremacy. But as it is, the auditory is simply swamped in the visual space. We appeal to the eye rather than to the ear, denouncing the latter as unreliable. But this relative unreliability does not prove that definite auditory localizations are impossible. It only shows how needful it is to establish artificial conditions before the localizing capacity of the ear is fully revealed.

Fully acknowledging, then, the humble position which our auditory space normally occupies, we may yet be entirely confident that it is to be regarded as an independent reality with its own peculiarities, its own laws and apparent caprices, and its own exclusive phenomena; an auditory space which coöperates normally with the visual, the tactual and the motor spaces, helping to develop them and being developed by them in turn, in order finally to bring about that harmony and possible interplay of spatial experiences which we know as the external world. If we are convinced of this, we may now proceed to consider how the auditory spatial experiences become systematized.

II. SUGGESTIONS TOWARDS A NATIVISTIC INTERPRETATION OF AUDITORY SPACE PERCEPTION.

After what has been said in the previous section, one can hardly expect any other than a nativistic interpretation to be suggested. For since the intracranial positions are inexorably withdrawn from eye and hand, and since by that very fact the auditory qualities corresponding to them can neither become associated with movements not united with movement-sensations through the process of fusion, the possibilities of both the empiristic and genetic interpretations of these phenomena are excluded. But if the nativistic interpretation may be given to one class of auditory phenomena, nay, *must* be given to these, we are logically compelled to extend the same interpretation to the whole field, provided that no insuperable obstacles exist for so doing. Accordingly we shall assume that auditory impressions originally possess positional characteristics, or that they are natively endowed with the attribute of externality ; in other words we maintain that auditory impressions are not in themselves and apart from associations mere *pure* tones or sounds, wholly "in the mind" as the older writers would have said. On this basis our problem is to show how from a condition of general, indefinite externality sounds come to possess more or less definite and well-ordered localizations in a spatial system. But first the examination of a further nativistic claim must detain us for a moment.

Do Sensations of Sound Primitively Possess Volume?

It has been claimed by some nativists, notably by Professor James,[1] that characteristics of *extensity* (as well as

[1] Hering also is said to hold this view. See Stumpf: Tonpsychologie, 3, 210.

of position), must be posited for auditory impressions: that varying amounts of *voluminousness* belong originally to all sounds. Introspection is appealed to for confirmation of the assertion that certain sounds feel more massive than others. James says,[1] "We call the reverberations of a thunderstorm more voluminous than the squeaking of a slate-pencil." In the same way a "dull thud" seems bigger than the shrill, piercing note of a whistle, and the boom of a cannon seems to have more volume than the buzz of a mosquito. The very epithet "piercing" denotes at least one side of the contrast. In general, loud sounds seem larger than faint ones. To cite James again: "Loud sounds have a certain enormousness of feeling." They are natively and primarily as different in point of "vastness" from the weaker sounds of a ticking watch or a humming gnat as the cutaneous sensation resulting from a total immersion is different from that aroused by a wet finger. In a word, sounds —and all other sensations too—possess a "feeling of crude extensity." All nativists have not agreed upon this point, and Stumpf and Külpe, though differing considerably in their attitude towards auditory space, have joined with others in ascribing these apparent differences in the spatial volume of sounds to the influences of association. Stumpf (66, A, p. 299, and B (*a*)) holds that all sensations are extended, *but all auditory extents are alike*. The ear's structure demands this, since it allows only total stimulation. That some tones and noises seem more massive and extended than others must then be due to secondary and extraneous factors. Deep tones

[1] Psychology, II., 134.

seem large because we are accustomed to associate their production with instruments of large dimensions. Then too the accompanying tremblings and vibrations of the whole room or building in which deep organ tones are heard lead to the thought of a vastness as connected with these tones. These accompanying sensations are wanting in the case of high or shrill tones. Hence the latter seem less voluminous. A tuning fork struck loudly close to the head may make the whole head seem to vibrate, but a buzzing in the ear is sharp and concentrated. The latter possesses none of the apparent largeness of the former. Külpe (53) comes to the same conclusion and characterizes as "allegorical" all use of terms that imply the extensity of sounds. We are trying, he says, either "to express the magnitude of the effect which they [the sounds] have upon us, or . . . to indicate the spatial character of the objective conditions of the sensations, or that of other sensations or ideas, visual or cutaneous, which we associate with them."[1] Three positions are thus seen to be possible in reference to this question. One may say (*a*) that sensations of sound originally possess differences in point of vastness and that these are sensed as such by the ear; or (*b*) that these sensations do indeed originally possess extensity, but that any apprehending of *differences* in point of extensity is a matter of association with other experiences; or (*c*) that there is not even an original massiveness of any sort, and hence, by necessity, apparent differences in volume are results of association.

Which one of these positions shall we adopt?

[1] Titchener's translation.

No considerations seem wholly decisive for any one of the three views. One or two points, however, may be noted and at the end we shall declare in favor of the first position, with certain modifications. It might appear at first thought as if any perception of auditory extent were conditioned upon the perception of auditory position. That is, one might refer to the phenomena of *delicacy of localization* and assert that unless one could first distinguish two points of an "extended" sound as differing in direction one would have no warrant for supposing the sound to be voluminous. Manifestly then sounds lying at the side would require much more objective extent to be recognized as extended than when lying in front or at the rear. Of course the above assertion would receive a slight qualification from the reflection that here, as in the case of touch, it might be possible to sense an increase of area before the two separate positions clearly emerged. But even then the amount of acoustic volume necessary for a sensing of voluminousness would vary with the direction of the sound from the head. But however this may be, the thought here is that any quasi-massive sound, like the boom of breakers or the shock of a blast, might be sensed as massive whenever its extremities were distant enough to yield the perception of two different positions. Difference in massiveness would then be noted between such sounds as allowed the discrimination of *two* positions and those allowing on the one hand *three* (or more), or on the other hand *none at all.* But any such supposition as this would be faulty through and through. For, in the first place, such a perception of different positions could be possible only for successive sounds. And in the case of a reverberating sound we do get some idea of vastness because we can note the positions from which the successive portions of the total sound seem to come. But for a single sound this does not hold. The situation is always like that represented by a mass of qualitatively similar telephones. The individual sounds simply fuse into a single resultant occupying *one* position. In the same way any sound, no matter how great its objective cause may be, will, if it is strictly *single*, be perceived as coming from *one* point. And, in the second place, we are speaking of a massiveness belonging *originally* to sounds. Now though we may assume that *position* belongs originally to sounds, this can by no means be construed to mean any *particular* position, any position, that is, which has its place in a system of positions. Hence *originally* no two just discriminable positions or better still no two positions simultaneously perceived, would have any meaning in terms of extension. Along this line of thought, then, we seem to gain no warrant for ascribing massiveness to sounds.

But there are two considerations that do, it would seem, give us this warrant, at least to the extent of assuming that sounds *may* natively possess differences in point of extent. (*a*) There are sounds that prior to all accessory experience are sharply and definitely located. These are the intracranial sounds, and the entotic sounds or buzzings in the ear. There is no question of *accuracy* of localization here. The point to be noted is simply that these sounds are located with subjective definiteness. (*b*) But over against these sharply located sounds are others that can be assigned no position whatever. They are everywhere. They envelop the whole head. They have absolutely no definite character.—Now the difference between these two classes of sounds is not alone that one is located while the other is not. The very fact that one is not located, while yet external, carries with it the further fact that this one is the more extended. The located sound is restricted, limited. The other is not, and hence is by that very fact vaster, more massive, more voluminous.

We may conclude then that sounds may originally possess crude extensity. Whether or not they possess this always seems impossible of settlement. In any case the extensity or voluminousness that we ordinarily ascribe to auditory impressions may be looked upon mostly as products of association, the magnitude either of the cause or the effect, or of both, being transferred to the sensation of sound itself.

How Do Auditory Localizations Come to be Arranged Within a System of Positions?

We may proceed now to the discussion of the problem stated on page 187, that, namely, of showing how auditory impressions, natively possessing the attributes of position in general, come to be capable of a definite arrangement. Stated in other terms the problem reads: how do the auditory local signs become available for the ends of localization: or how do these local signs come to have any particular spatial meaning—how do they become significant? Let us recall that the local signs have been formulated in terms of purely auditory qualities, the attempt having been made to give them such a content as to provide for the requirements of gradation and non-interchangeability. What we have to determine now is how these graded and non-interchangeable auditory complexes come to mean *particular* positions in space.

When I say that a sound is "here" or "there," what I *mean* is not that the sound is affecting the two ears with a particular ratio of intensity, or that it possesses a peculiar clang-color, or that its absolute intensity is such and such ; what I mean is not these uninteresting particulars but rather the sort of *reaction* that must be made in order that the sounding body may be seen or touched or brought to the position of most distinct hearing—in short, made use of in some way. The local sign is significant only in so far as it forms the means to some practical end, only in so far as it leads to some reaction to the sound. In comparison with this end the local sign drops out of sight and is only to be fully revealed under experimental conditions, just as "insipid joint-feelings" are hardly to be discovered by introspection, being buried beneath the more important and therefore more interesting phenomena that are concerned with the position of the limbs as expressed either in cutaneous or visual terms. The question how the auditory local signs get their meaning translates itself therefore into this: how do they become associated with actual or imagined reactions to the sounding body, that is, with actual or imagined movements of eyes, head, or whole body.

One may object that, in stating the matter as we have, we have mingled two problems, (*a*) how do the auditory local signs become arranged? and (*b*) how do they come to have meaning? We have mingled these problems and that too intentionally, for they are really the two phases of a single matter. The same process that subserves the one subserves the other also. The arrangement and the meaning both come about in consequence of "exploring" movements of the head or whole body, by which result, first, the successive experiencing of adjacent local signs and thereby the possibility of thinking of any one of them as belonging together with its neighbors within a certain scale of differences; and, second, the association of each locally colored sensation of sound with particular movements necessary for making the appropriate reaction upon the sounding body. Thus each local sign becomes able to recall its associates in definite order and takes its place little by little within the total system of local signs which gets built up. And, further, those systematic associations are gradually made between sensations of sound and sensations of movement, by the existence of which the former come to possess spatial meaning. The process is in general the same for hearing as for touch, where the cutaneous local signs obtain arrangement and meaning through the exploring movements of finger-tip and eye. In fact the process is the same as for all local signs, and consequently we need not pause longer here to elaborate the matter in greater detail. We need only add, perhaps, that just as positions upon the skin, the primary ideas of which were probably gained through motor experiences, may be finally represented in purely visual terms, so the motor reactions which are originally associated with the sounds may commonly be replaced in consciousness by the representation of the sounding body as occupying its place within the surrounding world of seen objects.

When now it comes to pass that the associations have been formed between the auditory local signs and movements of reaction—whether of eyes, head or whole body, the first being able to replace the last two which are primarily concerned—an acoustic stimulus from any quarter arouses *impulses* to make the appropriate reaction—in the normal individual impulses to bring the sounding body within the line of sight. That we have these impulses is certain beyond the shadow of a doubt. And in perhaps the majority of cases we allow these impulses to stand for the actual movements that would be necessary, basing upon them our localizations of the sound. But obviously the question of the actuality of these impulses is vastly different from that of their *origin*. While discussing the constitution of the auditory local signs (p. 151), we met with the theory which implies, at least, that these impulses originate reflexly, or physiologically,—that they are "set free in the brain through the action of the two-sided auditory stimulus." In the above connection we saw reasons for not accepting this theory, but now having made our own theory more precise, that these impulses arise in consequence of associations made between movements—or their representatives—and the local signs, we must endeavor to substantiate still further the position we have assumed.

Our contention is that these impulses are of exactly the same sort as those which we have to move the eyes to the bottom of the page we are reading, when we see an asterisk or other sign that indicates a footnote. This impulse is undeniable and it is sometimes almost irresistible. And yet from the very nature of the case it is a product of experience, of association. For why is the impulse to move the eyes *down* rather than in any other possible direction? Or if one think of the earlier stages of the process, these impulses, we contend, come into being as do the impulses in a chick to run to the mother hen at the sound of her call. Lloyd Morgan has shown that young chicks hatched away from the hen and first brought into her presence after several days have elapsed, will not respond in any way to her summoning "cluck" until an association has been formed between this sound and some pleasantly-toned experience that results from the proper reaction to the same. But let this association be formed and there is forthwith an impulse to run to the hen whenever the note of summons is heard. Similarly with our auditory experiences. The impulses to particular reactions are products of associative couplings which do not exist ready made at birth, but for which aptitudes may indeed be present in the nervous mechanism. Several considerations seem to make for this view.

(*a*) The process of median plane education described in Chapter 2 was certainly in part the establishing of associations between certain auditory qualities and the visual or motor representation of the known positions of the sounding body. The account of the matter given in the place referred to should have made it clear that the difficulties encountered were along the line of getting the local sign of any position associated with its requisite representative. If this could be done for even a single moment, the localization was for once accurate.

(*b*) The account given by Heller (52, pp. 538 ff.) of experiences with the blind would seem to indicate that if circumstances prevent the formation of these associations, correct localizations are impossible. Choosing a helpless subject, one who must be led about whenever he moved from his seat, Heller found that he could not tell from which side he was called, and when tuning forks were used such simple relations as *right and left*, front and back were confused. As usual, noises were better localized than tones, which shows the influence of experience to that extent. Such observations as these merit careful repetition and control, but accepting them as they stand, the proper interpretation appears to be that the motor experiences of this individual had never been rich enough to accomplish the needful arrangement of the local signs and the associations with them of the appropriate reactions.

(*c*) It sometimes happens, most frequently perhaps in the case of sounds given near the head, that a localization cannot possibly be made. The sound is external but without position. Under these circumstances some chance factor, such as for example the touching of the hair by the sounding body, or the catching of some slight odor from it, or the passing of its shadow before the eyes, serves to render possible a perfectly definite, though perhaps not wholly accurate, localization. An intermediate factor has served as the connecting link and an association is formed.

(*d*) But most of all is the theory of reflexly released impulses to movement antagonized by the observations made by Dr. Stratton while experimenting with an inverted visual field (65). Dr. Stratton heroically wore lenses for two periods of three and eight days respectively, by means of which the whole visual field was rotated through an angle of 180°. Outside of the general fact that he became gradually able to accommodate himself to his inverted world and could finally make the proper reaction to any object seen, none but the auditory experiences concern us here. Speaking of the experiences of the sixth day, he says (p. 360): "Localization of sounds, when the source of the sound was in sight, followed in most cases the visual position of the source, provided I did not voluntarily recall the older position of the object. And since the compass of the visual field was about 45°, the actual divergence from the older localization of the sound could thus be about as great as the diameter of the field of view. . . . When the source of the sound was out of sight, a much greater divergence of localization was possible. For in walking I actually felt my feet striking against the floor which I saw extending into the (old) upper side of the field of view before me; and the sound of my steps seemed to come from the place where I felt my feet strike—in this case a divergence of 180° from the old direction of the sound. But when I felt my feet in the old place, the sound too seemed to come from that direction." Manifestly the cases of divergence in a lateral direction are most interesting, because those in a vertical direction may be looked upon as misplacements in the median plane—misplacements which suggestions always bring about with comparative ease. But, plainly, hearing the sound from a position differing from the actual by 45° in a lateral direction indicates the breaking down of an old association and the formation of a new one. This latter process was not equally complete for the whole forward field. Of the eighth day he writes (p. 467): "The fire sputtered where I saw it. The tapping of my pencil on the arm of my chair seemed without question to issue from the visible pencil. Even when I tapped on the wall to one side, out of sight, if in making the stroke I invariably passed my hand and pencil before my eyes and in the direction of the unseen part of the wall, and attempted to picture the contact in harmony with this movement, *I actually heard the sound come from the new visual direction*, although not with full and unequivocal localization. There was a strong temptation to localize the sound on the other side also." But whence this possibility of hearing a sound issue from a point so widely distant from where it actually was? Surely any reflex tendency to move the head could not have been changed into its opposite by the rotation of the visual field. For the auditory factors of quality and relative intensity remained exactly the same. Impulses reflexly set free by the auditory stimulus cannot suffice to explain the facts here. We must suppose rather that the visual sensation, necessitating as it did under these conditions a new motor reaction, simply drew the auditory sensation along with it and helped give it also a new motor reaction. In other words, these phenomena noted by Dr. Stratton can be understood only on the basis of fresh associations demanded in the interests of novel visual conditions. Had these conditions continued a systematized set of new impulses would doubtless have arisen opposite in motor character to those formerly existing.

We must conclude then that we have sufficient warrant for the general position we have taken. We have, to be sure, considered local signs of direction only, but the line of thought applies equally well, *mutatis mutandis*, to the local signs of distance. One or two further matters remain however to be discussed.

What has been said above in reference to local signs and associations with motor reactions does not apply, it is plain, to intracranial localizations. But it must be remembered that we have been speaking of localizations of which accuracy or falsity may be predicated—with localizations, that is, which correspond to some actual object. But in respect to intracranial localizations there is not, and never can be, any question of accuracy. Indeed they exist only under experimental conditions. To be sure, they are localized with sufficient subjective certainty, but no reaction can be made to them except in a wholly ideal way, and they can never take their places in the scheme of objective auditory positions that has become systematized by experience. One must be content to say as little about intracranial localizations of sound as about the localizations of intracranial headaches or of pains and other sensations from certain deep-lying parts of the body. Frequently, of course, intraorganic sensations may be located approximately by the fact that they become modified by certain superficial pressures or tractions. But certain sensations—and among these are assuredly the intracranial pains—cannot be so modified from without. That pains are within the head might indeed be known by accessory means, but that they are in the frontal or occipital regions seems to be something given primitively with the pain itself. Professor James suggests[1] that such localizations may be due to certain associations of local signs with one another, and perhaps this is all that we may safely conjecture for intracranial phantom-sounds.

In Chapter 1 of Part I. (pp. 42 and 49) there was mentioned a difficulty, signalized by v. Kries and Stumpf, in reference to associations between auditory impressions and other disparate sensations which should assist in their localization. The difficulty came to light especially in the case of two simultaneous, non-fusing sounds, for it seemed impossible to give a satisfactory account of how the proper elements came to be rightly conjoined. This is indeed a serious difficulty for any theory that seeks to explain localizations by associations between auditory qualities and either reflexly evoked movement-sensations or tactual sensations. But the difficulty is avoided by the point of view we have assumed above. Suppose that two sounds are simultaneously located, there is nothing astounding in the fact. For each sound carries with it, as integral part of itself *qua* auditory impression, its own local sign. The local sign is not something extraneous which must be associated with the sound. It is given directly with the sound. Each local sign then reproduces its usual motor reaction, or the remembered sensations which stand for it, and that two local signs should simultaneously call up two tendencies to reaction is no more wonderful than that two stings upon the arm should prompt two reactions of the other arm to remove the offending insects. The conjecture made by Stumpf in this connection, namely that

[1] Psychology, II., 156 and 159, foot-note.

the sensations of the two ears are originally different, would seem to be very strongly opposed by the various localizations of two or more fusing sounds. Indeed Stumpf was forced to qualify his hypothesis by the remark that under certain circumstances the distinguishing characteristics of the two ears might coincide. To appeal, as he does, to the proper localization of entotic sounds would hardly seem justifiable, for these may be projected into space according to the usual laws of sound-localization, and it is not rash to suppose that they come to be recognized as "in the ear" either because they persist in a way that objective sounds do not, or because they move when the head moves and are thus seen to lack the stability of objective realities. Still it would seem that either ear may at times recognize its own stimulation. In a high wind a voice near by is heard double, and it seems possible to tell acurately which ear receives the later stimulation. It may be, however, that other factors enter to inform one of the order of the stimulation.

It may have occurred to the reader very often to criticise the discussion contained in this second half of the present section to the effect that what we have really been attempting is to show how the auditory and motor spaces become harmonized, how the local signs of the one come to be compatible with the local signs of the other. For what else can the association of auditory local signs with motor reactions mean? The criticism is just and we accept it at once, for it expresses in other words than we have thus far used the real nature of our endeavor. For we have been trying to show how the auditory local signs get their meaning in practical terms, and this meaning we believe them to get only as translated into the equivalents of motor space. Auditory sensations occupy no unusual place in this regard. What possible positional significance would cutaneous sensations have, for example, could they never be brought into systematic harmonization with motor experiences? Attention need hardly be called to the fact, however, that motor experiences have been called upon to give *meaning* to the auditory sensations and *not* first to give them by a process of psychic synthesis all the spatial characteristics that they in any way possess. Our attempt has been throughout to develop a nativistic as opposed to a genetic theory of auditory space perception.

As complete as it might be ideally, our auditory space is at a low grade of development, and its harmonization with the other spaces is far from complete by reason of the small practical utility of such an achievement. Very often we are compelled to make *inferences* in reference to the positions of sounds, since direct perceptions are not yet possible. But these inferences are of just such a nature as a person restored to sight is for a long time compelled to make, for so long in fact as perfect congruence between sight and the other senses is lacking; and therefore the existence now of such inferences in the case of hearing should not be made—as seems to have been the case—a ground for supposing that auditory space is altogether a dependent, nay more, a fictitious affair. So far as it goes it is no less real than its sister spaces, and to an extent much greater than is commonly acknowledged it contributes to their development.

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II.

STUDIES IN VISUAL SPACE PERCEPTION.

The Illusion of the Kindergarten Patterns.

The Poggendorff Illusion.

A New Explanation for the Illusory Movements Seen by Helmholtz on the Zöllner Diagram.

Further Illusory Movements in Connection with the Zöllner Diagram.

The Illusion of the Deflected Threads.

The Illusory Dust Drift.

Two Optical Illusions of Double Motion.

THE ILLUSION OF THE KINDERGARTEN PATTERNS.[1]

EVERY one that has had occasion to examine attentively a collection of patterns designed for the kindergarten occupation of mat-weaving, and of course every one

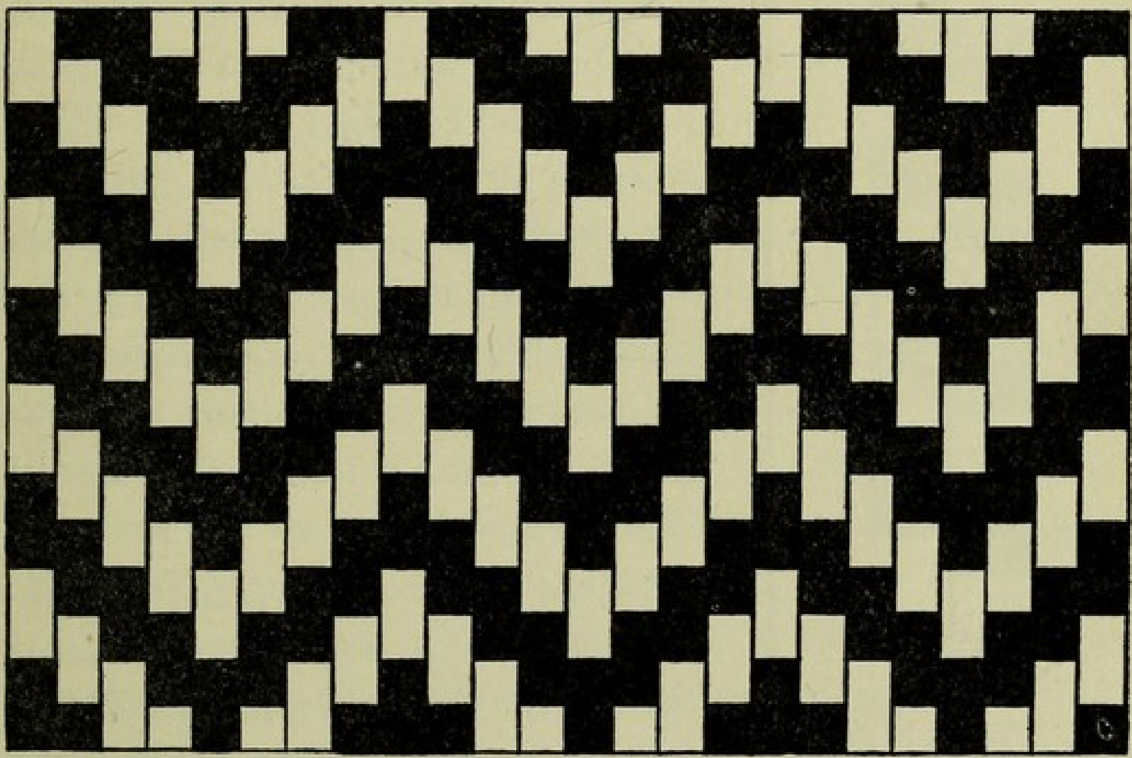


Fig. 19.

practically engaged in this work itself, must frequently have noticed the peculiar irregular appearance presented by the numerous patterns of which the above is a typical representative. Nor does this irregularity escape the notice of the children. A slight examination, however,

[1] Reprinted, with additions, from the *Psychological Review*, Vol. V., No. 3, May, 1898.

suffices to convince one that the irregularity of the vertical lines is only apparent, the seeming departure from rectangular perfection being due wholly to an optical illusion. While casting about for an explanation of this illusion the writer's attention was called to the articles of Heymans[1] and Münsterberg,[2] in which the elementary form of the illusion is figured and discussed. Fig. 20 represents this elementary form as first published by Professor Münsterberg in the Milton Bradley collection of optical illusions.[3] In this collection no name is attached to the illusion, but in the German article it is called "Die verschobene Schachbrettfigur," for which "The Shifted Checkerboard-Figure" may be a sufficiently exact, though perhaps less expressive, equivalent in English. The explanations given by the above-mentioned authors are widely at variance, Münsterberg appealing to *irradiation* as the chief factor in the matter, while Heymans rejects this and forthwith places this illusion in the same

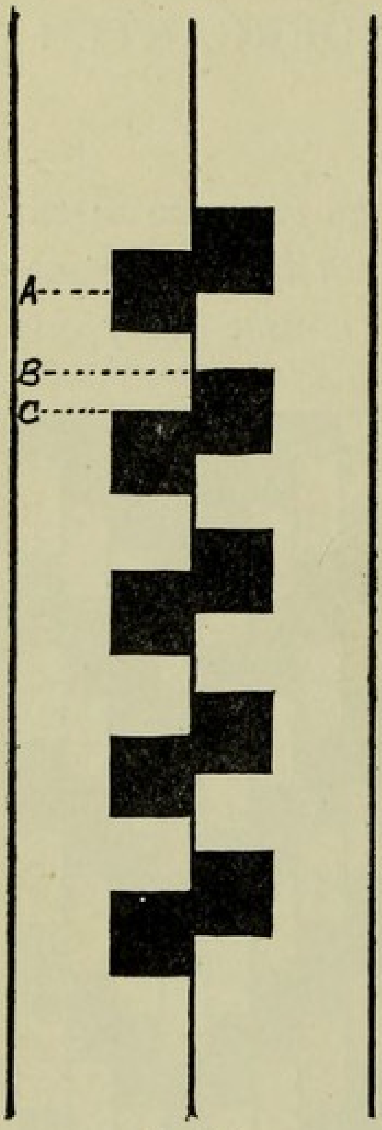


Fig. 20.

[1] *Zeitsch. f Psych.*, XIV., 118.

[2] Ibid., XV., 184.

[3] Bradley: Pseudoptics, B. 5.—According to the description given by its discoverer in the article above referred to, the complete figure from which this element was taken must have been much like the kindergarten pattern above figured, squares, however, taking the place of the rectangles.

category with those of Zöllner and Loeb, to be explained as a phenomenon of *contrast*. Neither writer gives a sufficiently detailed discussion of the matter to enable the reader to decide definitely between the rival claims. Then, too, while presenting strong arguments in favor of irradiation as the explanatory principle, Münsterberg acknowledges that other factors *may* possibly coöperate to produce the illusion—factors, namely, of contrast, or whatever they may be, which are commonly appealed to in explanation of those illusions of which the Zöllner pattern is a type. In view, then, of the somewhat unsettled state of the question, and in view too of the interesting character of the illusion itself, it seemed not amiss to enter upon a qualitative and quantitative examination, the illusion being subjected to as many of its possible variations as seemed likely to throw light upon the problem. It may be stated at once that the general conclusion arrived at as a result of the experiments to be described is that *irradiation* alone seems to be an adequate explanatory principle.

The exact nature of this irradiation must be made clear at the outset. This Professor Münsterberg does admirably in the article referred to, pointing out, at the same time, that it was the failure to rightly apprehend this that led Heymans to his sweeping denunciation and rejection of this explanation. The irradiation-effect here, namely, is not of that simple sort where dark areas surrounded by light are contracted uniformly in such wise that the apparent contours remain everywhere parallel to the actual outlines. For here, in consequence of the form of the figure, there is a point of maximum effect, the result of which is to give the whole line an oblique appearance. In Fig. 20, *e. g.*, consider the portion *AB* of the middle line. It is not true here that the upper half of this section has been seemingly shifted to the left in a direction parallel to itself, while the lower half remains unchanged. The fact is rather that at the upper end of the line *AB* the white square on the right has, as it were, *bored into* the black area, giving the angle at that point the appearance of being no longer a right but rather an *acute* angle,[1] so that the upper half of *AB* seems to slope obliquely downwards from left to right. Similarly *BC* slopes in the same direction. And the lower half of *AB*, influenced by the sloping sections above and below, seems to join with them and assume likewise an oblique position. Similarly for each section of the whole middle line. Accordingly, the irradiation that is effective in producing the illusion is that which occurs in the *corners* of the several pairs of squares. This is Professor Münsterberg's explanation of the matter, and its great plausibility is at once evident from an examination of the figure.

Heymans, on the other hand, has failed to see that a special variety of irradiation is here at work, and he very rightly opposes the idea that simple irradiation could in any way produce the illusion of deflection. That he may apply his own theory of contrast it is only necessary to regard the overlapping squares as equivalent in effect to the oblique transversals of the Zöllner pattern. The

[1] How great this boring effect is can be readily appreciated if the attempt be made to fill out the appropriate corners of Fig. 20 sufficiently to destroy the illusion.

same explanation is then admissible for the two illusions. As the eyes sweep over the figure, there are solicitations to move them both in vertical and in oblique directions. The *intended* movement is however along the verticals, for the interesting question to be decided is whether the latter are parallel or not. The difference in direction then between the attempted movements and those to which the eyes are powerfully solicited, gives rise to a feeling of *contrast*. The effective result of this is to give to the verticals a slope the direction of which shall be opposite to that of the obliques, and thus the illusion results. This principle of "movement-contrast" has met with limited acceptance, for like the similar principle of Helmholtz, it seems to be less a "principle" than a convenient logical term to cover the facts of the illusion. In any case we shall soon see that it is superfluous to attempt the application of this principle to the illusion here under discussion.

With this brief introduction let us enter upon the experimental examination of the illusion.[1]

A. Qualitative.

Strong presumptive evidence in favor of the supposition that one is here in the presence of irradiation effects is given at once by the fact that the inclination of the middle line (see Fig. 20) appears greatest when the figure is viewed with imperfectly accommodated eyes—a condition well recognized as most favorable for the appearance of such effects.[2] But above all, the presence of irradiation is shown by the entire disappearance of the

[1] See pp. 238ff. for brief descriptions of further attempted explanations.

[2] See Helmholtz, Physiologische Optik, 2d Ed., p. 395.

illusion when the figure is held in the full glare of a bright light and viewed at a distance of six or eight inches from the eyes. If the paper upon which the figure is drawn be semi-transparent, the same disappearance of the illusion may be accomplished by holding the figure between the bright light and the eyes.

But may not the alternating and contrasted position of the squares be also influential, wholly aside from the effect of irradiation? Before entering upon a quantitative investigation of the problem, the effort was made to gain

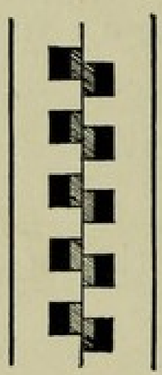


Fig. 21.

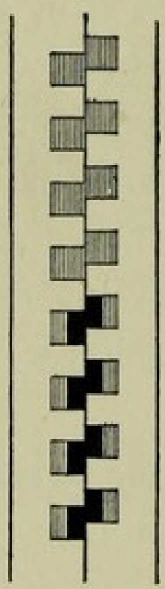


Fig. 22.

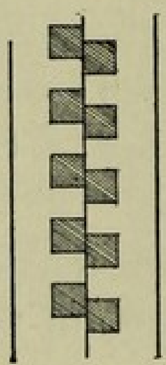


Fig. 23.

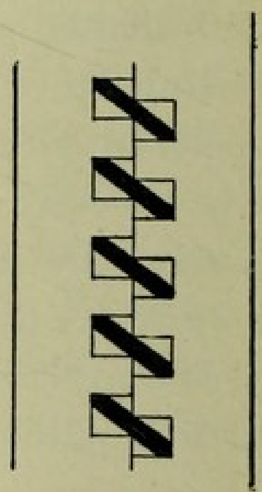


Fig. 24.

an answer to this question. To this end several figures were prepared, the object of each of which was to exclude the possibility of irradiation while retaining the other operative factors, if such exist. In these figures the form of Fig. 20 was adopted, but the squares, instead of being filled with a uniform black, were variously treated with vertical or oblique lines, etc., as shown in the adjoining cuts, 21, 22 and 23. Here the full effect of contrasted squares is present, but no illusion is produced, except in the lower half of Fig. 22, where irradiation becomes possible. Alternately covering either half of Fig. 22 with a piece of blank paper will serve to render clear the difference between the two halves.

Figs. 24 and 25, also, would seem to offer as much 'contrast' as even Heymans would demand, but it is evident that no illusion is produced.

Still more interesting, however, are figures 26 and 27. In Fig. 26 the black squares are brought to within $\frac{1}{48}$ inch of the middle line, the characteristics of the typical form

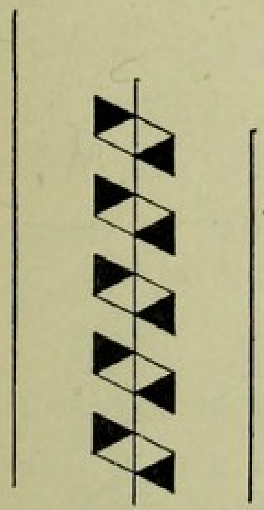


Fig. 25.

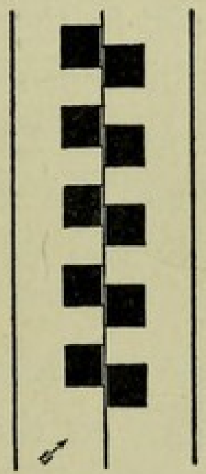


Fig. 26.

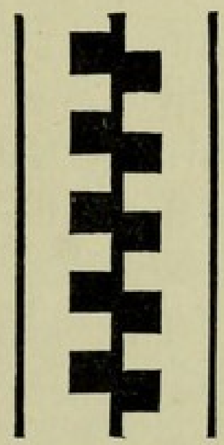


Fig. 27.

being thus retained to the greatest possible extent. If the figure be looked at directly, irradiation is excluded and the middle line is not deflected. If, however, it be viewed somewhat obliquely as indicated by the arrow, at a distance of 18 inches or more from the eyes, and with the plane of the paper at an angle of 30°–45° to the line of vision, irradiation can become effective to a limited extent and the illusion reappears in proportionate degree.

Fig. 27, on the other hand, offers every opportunity for the working of irradiation, but the heavy middle line precludes that particular direction of its effectiveness which is requisite for the production of the illusion. That is to say, the 'boring' effect in the corners formed by the several pairs of squares is no longer able to produce the tilted appearance of the middle line. For here not only the inner corners formed by each pair of squares, but each and every corner along the middle line offers a boring point for the action of irradiation. Consequently, in so far as the middle line is concerned, these effects neutralize each other, leaving this line unaffected, while instead the white areas at either side seem to have been dovetailed into the black, so tightly do the white squares appear to fit into the inner corners. If one recall in this connection that the original Zöllner pattern was constructed with heavy lines, the non-appearance of linear deflection in the case before us becomes all the more instructive.

That irradiation does produce a maximum effect in the corners of dark areas, so that a right angle is thus made to appear acute, may be readily seen by drawing a square (with an edge, say, of two inches), three-fourths of which shall be black and the remaining fourth white, as in Fig. 28. Here the inner corner of the white square is seemingly the vertex of an angle slightly less than 90°. In fact, by an application of this principle to a variation of the typical figure an illusion of an entirely new form may be brought about, as shown in Fig. 29. Here the central line is no longer tilted, for the place of maximum irradiation has been transferred from the outer corners to the centers of the squares. What now occurs

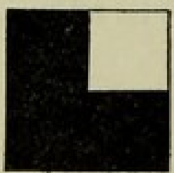


Fig. 28.

is this: The horizontal cross lines, which bound above and below the successive pairs of tiny white squares, receive each a slight deflection in the direction of the full black portion of the larger squares. The upper cross line is deflected upwards from right to left, and the lower downwards from left to right. At the same time the vertical boundary lines on the right and left receive deflections downwards to the right and upwards to the left, respectively. The result is, as Fig. 29 shows, that each pair of adjacent white squares (which may be apperceived as a single white bar crossing the central line) seems to slope slightly downwards from left to right. By diminished illumination this becomes still more apparent. In fact all these illusions due to irradiation come out very clearly when viewed under reduced illumination, as *e. g.*, when drawn upon semi-transparent paper and viewed from the back, the blank side, that is, being towards the face.

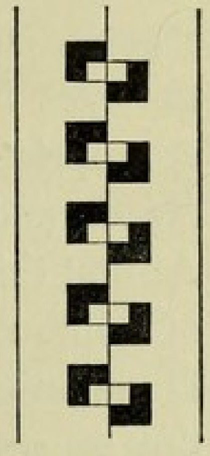


Fig. 29.

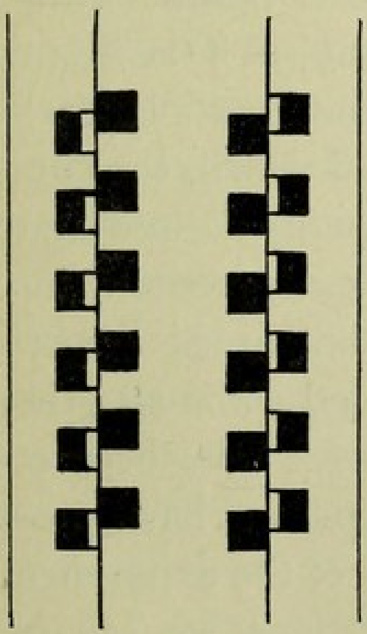


Fig. 30.

Of still greater interest in certain respects is Fig. 30, which grew out of the attempt to exclude irradiation by presenting one line of squares only to each eye, the two being united into a single figure by ordinary stereoscopic methods. Of course, the union into the desired figure is impossible unless certain well-marked portions of the original figure be retained and presented to each eye.

Otherwise the two vertical lines of squares would simply coalesce. Fig. 30 represents the adopted form which fulfills the required conditions very satisfactorily. Hold a paper cutter, or similar object, between the figure and the eyes in such a way that each eye receives only its appropriate image. Converge the eyes for some distant object and the single compounded figure will appear. At first this will present the appearance of Fig. 26, a stripe of white running down between the squares, but if the illumination be reduced by shading the diagram from the source of light, or by allowing the illumination to pass through several thicknesses of transparent paper, after a little the wished-for figure will appear wholly in black and exactly like the typical form, but *with no trace of the illusion.* To be sure, on account of retinal rivalry, the white stripes will frequently reappear, but if the illumination be properly adjusted and the white parts be darkened by rubbing lightly with a hard lead pencil, the compounded figure can be made to retain its desired state long enough for a thorough and satisfactory examination.

Of course, I am far from claiming that in this experiment irradiation alone has been excluded while all other factors, such, *e. g.*, as that of "contrast," have been retained in full force. Although, perhaps, we have most closely approximated the desired form of the experiment that shall possess these latter qualifications, we can by no means draw any absolute conclusion. For, while in the compounded figure every possible condition of *geometrical* form and contrast is present, it would be evidently unwarrantable to conclude from the absence of the illusion that therefore the factor of 'contrast,' or what not, is without influence in this particular illusion. For, if what is meant by 'contrast' here be a physiological matter, as it presumably is to a greater or less extent, its effectiveness in this case must be diminished, in some measure at least, since neither eye receives the full contrast-effect as from the typical figure seen by both eyes alike. And yet one must acknowledge that, owing to the manner of constructing the parts of Fig. 30, the greater portion at least of all contrast possibilities has been preserved in each of the two parts. Still, all that the experiment can be made to prove with absolute decisiveness is that, when irradiation in corners is rendered impossible, the illusion under discussion fails to appear. And, as a means of demonstrating this, the above seems to be a convenient and effective method.

Notwithstanding the reservations just made, the interpretation of the above experiment in the sense indicated seems to gain justification from certain observations reported by Witasek.[1] In the course of an experimental examination of the Zöllner illusion, the method of stereoscopic combination was tried. That is, the verticals alone were presented to one eye and the obliques alone to the other. The careful examination of these combined figures revealed the fact that *the usual illusion was still present*, though in diminished amount.[2] Now on the basis of this observation we should confidently expect that *if* the Zöllner and Münsterberg illusions are

[1] Ueber die Natur der geometrisch-optischen Täuschungen. *Zeitsch. f. Psych.*, etc., 1898, XIX., 149.

[2] Kundt had made this same observation long before. See *Poggendorff's Annalen*, 1863, CXX., pp. 118 ff.

due to the same underlying cause, the latter illusion ought not to vanish under precisely the same conditions that allow the former to still persist. But since it does disappear under these conditions, the supposed identity of cause for it and for the Zöllner illusion may indeed be viewed with very justifiable suspicion.

There has now been established, it seems to me, a rather strong presumption in favor of irradiation as the only necessary explanatory principle for the Münsterberg illusion. That this presumption becomes overwhelmingly strong will be seen when the supplementary evidence derived from the quantitative investigation has been considered. For, as the subsequent text will endeavor to show, all conditions that in any way alter the character and amount of irradiation alter in the same degree the amount of apparent angular displacement undergone by the middle line. Let us turn therefore to the quantitative portion of our task.

B. Quantitative.

The purpose of the quantitative investigation was to ascertain as far as possible the influence of various factors in determining the amount of the illusion. Such factors are, *e. g.*, the *vertical distance* between the squares on either side; the length of the *free edge* along the middle line; the *color* of the background and of the squares; the character of the *illumination* as changed by interposing colored media between the eyes and the figure, or as conditioned by causing the figure to be viewed through a pinhole, or under the momentary flash of the electric spark, etc., etc.

The apparatus employed throughout was of the most simple character. Numerous cards were prepared, all being furnished with half-inch squares at varying vertical distances, arranged along the inner edge of each card, much as in the case of the Bradley model, so that the desired amount of overlapping edge could be readily adjusted. These cards were brought together in appropriate fashion along the middle line *on* of the board *FH* (Fig. 31). Parallel to the actual middle line (which was normally nothing more than the unavoidable line between the closely juxtaposed cards) were stretched threads *am* and *bs*, fastened below to the board itself but above to a sliding rod *KL*. This rod, being attached to a car travelling upon a firm support could be moved to any amount and in either direction by means of a thumb-screw not indicated in the figure. By this means the vertical threads could be brought to a position of parallelism with the apparently deflected middle line *on*, assuming, *e. g.*, the positions indicated in the figure by the dotted lines *cm* and *ds*. A scale was attached to one end of the rod *KL*, and the amount *ac* ($= bd$) of horizontal movement could be read micrometrically to the hundredth of a millimeter. Then the angular displacement *e*, which stands for the amount of the illusion, could be calculated from the simple formula $\tan e = \frac{ac}{am}$.

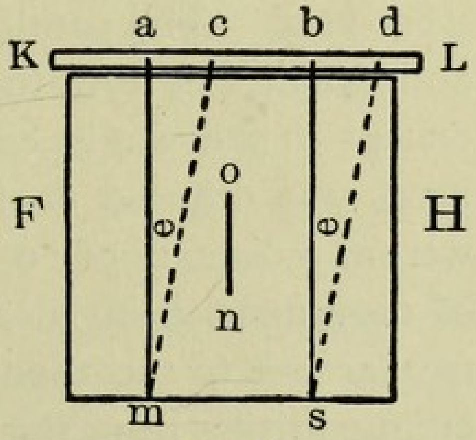


Fig. 31.

The entire apparatus was hidden behind a screen of black cardboard, a window in which disclosed the central part of the prepared cards and the parallel threads. This window was 17 cm. in width and varied in height from 16 to 20 cm., as the particular experiment demanded. In general, six pairs of squares were used, exceptions to this being in the experiments described in Tables I. and VI. (pp. 228 and 229), where eight and five pairs respectively were used. The parallel threads were two inches apart and equidistant from the middle line. The illumination, except in the single case where the electric spark was used, was diffused daylight. The eyes of the observer were at a distance of 70–80 cm. from the cards, this range of movement being deemed advisable in order to allow free access to the thumb-screw and in order to prevent fatigue arising from the attempt to maintain a fixed position. The plane of the cards formed an angle of 90° with the line of vision. The observer himself set the threads in the desired position, and as much time and as much shifting were allowed as were necessary to bring the threads into satisfactory parallelism with the middle line. In any series of experiments parallelism was established alternately from the left and from the right. The usual regulations as to practice, fatigue, etc., were carefully regarded.

In proceeding with the experiments it soon became evident that only a trained observer could give reliable and utilizable results. For the illusion is of such a character that a certain method of procedure must be acquired by each observer before a satisfactory parallelism between the three lines can be established. As

Münsterberg has pointed out,[1] the middle line does not always appear to be deflected *as a whole*. The fact is rather that each portion of the middle line that unites the pairs of squares seems to be deflected by itself, and these partial deflections must be apperceived together before the illusion can extend to the whole line. From this consideration it will be seen how difficult it is in many cases to decide upon a position of parallelism, and how necessary a sufficient training and the adoption of a particular method are in the production of reliable and comparable results. In general, the method employed was to allow the eyes to move freely, the gaze being directed principally to the *middle* portions of the figure and *both* threads being considered in the judgment of parallelism. In the attempt to carry out this method there arose, of course, slight individual differences of observation. These, however, were unessential, the only point to be insisted upon being that such individual peculiarities should remain constant throughout the experiments. In view of the difficulties just noted, the tables given below contain the records of only three observers, C., E. and P., each of whom possessed the requisite amount of preliminary experience.

In the following tables the letters of the first column designate the observers. Series 1, Series 2, etc., at the heads of columns signify that the amount of the *free* inner edge of the overlapping squares was respectively one-fourth, two-fourths, three-fourths and four-fourths of the length of the edge itself. This edge was in every case a half inch in length. The numbers in these

[1] Loc. cit., p. 187.

columns indicate the apparent angular displacement, or in other words the amount of the illusion under the given conditions. Column N. shows the number of observations, and the adjacent column gives the mean variation of the single judgments to the nearest minute of arc.

TABLE I.

V. D. (VERTICAL DISTANCE BETWEEN SQUARES) = ¼ IN.

Observer.	Series 1.	N.	M. V.
C.	8°23′.1	3	62′
E.	10°48′.6	5	30′
P.	10°49′.3	5	78′
Av. and Tot.	10° 0′.3	13	

TABLE II.

V. D. = ⅜ IN.

Observer.	Series 1.	N.	M. V.	Series 2.	N.	M. V.	Series 3.	N.	M. V.
C.	11°21′.2	3	66′	9°42′.3	3	71′	0°40′.5	4	6′
E.	11°19′.2	3	40′	9°31′.	3	28′	2°40′.4	3	40′
P.	13° 4′.4	4	6′	11° 5′.6	4	42′	1°14′.1	3	26′
Av. and Tot.	11°54′.9	10		10° 6′.3	10		1°31′.6	10	

TABLE III.

(NORMAL.) V. D. = ½ IN.

Observer.	Series 1.	N.	M. V.	Series 2.	N.	M. V.	Series 3.	N.	M. V.
C.	12°13′.3	4	49′	**12°51′**	6	16′	8°47′	5	71′
E.	11° 0′.5	4	44′	**11°56′.4**	4	71′	9°23′	5	38′
P.	16°10′.3	4	66′	**17°19′**	4	43′	8°16′.8	7	42′
Av. and Tot.	13° 8′	12		**14° 2′.1**	14		8°48′.9	17	

TABLE IV.

V. D. = ⅝ In.

Observer.	Series 1.	N.	M. V.	Series 2.	N.	M. V.	Series 3.	N.	M. V.	Series 4.	N.	M. V.
C.	8°40′	4	46′	10°31′.5	4	52′	8°48′.7	4	55′	5°52′.5	4	34′
E.	9°26′.5	3	28′	11°25′.8	3	49′	10°36′.	3	45′	6°52′.4	3	11′
P.	10°14′.5	3	59′	11°21′.7	4	54′	10°45′.8	4	84′	6°49′.5	4	55′
Av. and Tot.	9°27′	10		11° 6′.3	11		10° 3′.5	11		6°31′.5	11	

TABLE V.

V. D. = ¾ In.

Observer.	Series 1.	N.	M. V.	Series 2.	N.	M. V.	Series 3.	N.	M. V.	Series 4	N.	M. V.
C.	6°47′.4	2	39′	7°23′.9	2	23′	6°21′.3	2	13′	5°22′.3	3	33′
E.	7°41′.6	3	13′	8° 7′.2	3	44′	7°43′.	3	35′	3°39′.6	3	30′
P.	7°20′.4	4	49′	7°48′.2	4	45′	6° 2′.1	4	32′	3°59′.6	4	25′
Av. and Tot.	7°16′.5	9		7°46′.4	9		6°42′.1	9		4°20′.5	10	

TABLE VI.

V. D. = 1 In.

Observer.	Series 1.	N.	M. V.	Series 2.	N.	M. V.	Series 3.	N.	M. V.	Series 4	N.	M. V.
C.	5°23′.7	3	66′	7°24′.3	3	31′	5°31′.8	3	16′	3°56′	3	47′
E.	4°55′.7	3	19′	5°29′.3	3	35′	5°22′.3	3	48′	3°26′.5	3	48′
P.	6°14′.7	3	45′	7° 3′.4	3	69′	5°13′.2	3	16′	5°11′.8	3	49′
Av. and Tot.	5°31′.4	9		6°39′	9		5°22′.4	9		4°11′.4	9	

The following observations may be made on these tables. Taking a downward glance through the tables and comparing the corresponding series reveals the fact that increase of vertical distance between the squares is attended, first, by increase and then by decrease in the amount of the illusion. The maximum in each case is reached in Table III., where V. D. and edge of squares are equal. Table III. may be said to represent the *normal* form. Tables IV., V. and VI. show by series 4 that the illusion still persists where there is no overlapping but merely a touching of the corners.

A horizontal glance discloses the fact that, with the exception of Table II., the maximum illusion is reached in series 2, where free edge and overlapping edge are equal. In Table II., series 1 shows the largest results, but, as great subjective difficulty was felt here, as also in the experiments of Table I., because of the zigzag character of the line, no importance is attached to this fact. The *maximum illusion* is then in series 2 of Table III. That is, what we have previously called the *typical* form (Fig. 20) presents the illusion under its most favorable conditions.

The reason for this lies undoubtedly in the particular relation which the straight and deflected components of the lines bear to each other. Since the illusory inclination of the entire line is due to the manner in which it is apperceived, the maximum grade of the illusion must occur under those conditions that permit the greatest length of deflected edge consistent with the apperception of the line *as a whole*. If too short an edge is subjected to the influence of irradiation, there are manifestly too many undeflected portions of the line of superior length to allow much illusion. And if the affected edge is too long, the effort to bring the highly deflected portions into the unity of a single line also works against the illusion. But the conditions proper for maximal effects are reached when deflected and undeflected parts of the line are equal,—when their antagonism, that is, is at a minimum.

TABLE VII.

NORMAL FORM. MIDDLE LINE HEAVIER.

Observer.	Series 2.	N.	M. V.	Normal.[1]	N.	M. V.
C.	8°44′.3	4	36′	10°12′.8	4	49′
E.	9°38′.8	3	43′	11°41′	3	43′
P.	11°48′.7	4	17′	17°10′.2	4	2′
Av. and Tot.	10° 3′.9	11		13° 1′.3	11	

The facts noted in connection with Fig. 27[2] suggested measuring the illusion in its normal form but with the middle line emphasized by introducing between the edges of the cards a piece of No. 50 black cotton thread. The left-hand columns give the results. A 'normal' was measured at the same time, the results being given in the right-hand columns. It will be seen that a heavier middle line considerably reduces the illusion.

Fick[3] has prettily shown that the amount of irradiation varies with the width of the pupil of the eye. A 'normal' form was accordingly viewed through a pinhole whose diameter was approximately 1 mm., and records without the use of the pinhole were taken at the

[1] In this and the following tables, 'Normal' refers to the form of Table III., Series 2.

[2] Page 219.

[3] Fick, *Archiv für Ophthalmologie*, II., 2 (1856) : 70–76.

same time for comparison. The results are shown in the respective columns of Table VIII. The illusion is seen to be diminished. So far as I can judge, the illusion of the Zöllner patterns is not thus diminished when viewed in this way.

Table VIII.

Normal: Viewed Through Pinhole.

Observer.	Series 2.	N.	M. V.	Normal.	N.	M. V.
C.	4°33′.8	4	78′	9°32′.5	4	29′
E.	9°40′.9	4	68′	11°56′.4	4	71′
P.	14° 3′.7	4	85′	15°52′.5	4	39′
Av. and Tot.	9°26′.1	12		12°27′.1	12	

Table IX.

Normal: Electric Spark.

Observer.	Series 2.	N.	M. V.	Normal.	N.	M. V.
C.	9°33′.4	2	92′	10°36′.6	2	48′
E.	7°11′.8	2	169′	12°20′	2	49′
P.	8°57′.3	2	66′	16° 8′.6	40[1]	
Av. and Tot.	8°34′.2	6		13° 1′.7		

Here, again, the diminution of the illusion under changed conditions of illumination is very apparent.

Table X.

White Squares on Black. V. D. = ½ In.

Observer.	Series 1.	N.	M.V.	Series 2.	N.	M.V.	Series 3.	N.	M.V.
C.	8°23′.6	4	51′	8° 3′.8	4	6′	6°17′.8	4	42′
E.	8°59′.7	4	25′	10°24′.7	4	34′	8°52′.1	4	39′
P.	11° 0′.5	4	67′	14°41′.8	4	49′	9°20′.3	4	92′
Av. and Tot.	9°27′.9	12		11° 3′.4	12		8°10′	12	

N. B. *White* threads were used.

[1] The angle here given is the average of all normal observations. See Table XX.

TABLE XI.

BLUE SQUARES ON YELLOW. V. D. = ½ IN.

Observer.	Series 1.	N.	M.V.	Series 2.	N.	M.V.	Series 3.	N.	M.V.
C.	8°41′.8	3	49′	8°33′.5	3	30′	4°36′.4	4	99′
E.	6°57′.	4	11′	5°57′.2	3	78′	4° 4′.2	4	13′
P.	7° 8′.7	4	43′	6°59′.7	4	44′	2°30′.7	4	44′
Av. and Tot.	7°35′.8	11		7°10′.1	10		3°43′.8	12	

TABLE XII.

RED SQUARES ON GREEN. V. D. = ½ IN.

Observer.	Series 1.	N.	M.V.	Series 2.	N.	M.V.	Series 3.	N.	M.V.
C.	3°56′.	3	71′	5°38′.8	3	47′	3°31′.5	3	23′
E.	1°56′.4	3	18′	2°49′.3	3	35′	2°37′.	3	10′
P.	4°16′.2	4	36′	4°39′.9	4	43′	4°10′.1	4	67′
Av. and Tot	3°22′.8	10		4°22′.6	10		3°26′.2	10	

Tables X., XI. and XII. present interesting variations of color. In Tables X. and XII. the various series show in general the same direction of increase and decrease as did Tables II.–VI. above. In Table XI., series 2 is slightly less than series 1. But the point to be noticed is the universal decrease in the amount of the illusion, especially in Tables XI. and XII., where colors are used. Many interesting color combinations suggest themselves for similar experiments, but these were deemed sufficient to make clear the great changes produced by the introduction of colors without alteration of geometrical form.

Table XIII. gives the records obtained by viewing the 'normal' form through variously colored gelatine papers. A comparison series taken at the same time is given at the left. The table shows nothing decisively. There is a general tendency towards a diminution of the illusion,

TABLE XIII.

COLORED MEDIA.

Observer.	Normal Series 2.	N.	Red.	N.	Yellow.	N.	Blue.	N.	Green.	N.	Smoked Glass.	N.
C.	12°51′	6	11°49′.6	3	11°17′.8	3						
E.	12°16′.7	4	13°45′.4	4	12°10′.6	4	15°22′	4	12°18′.3	4		
P.	16°50′	4	13°44′.4	4	15°11′.4	4	15°42′.8	4	15°59′.6	4	9°23′	4
Av. and Tot.	13°59′.2	14	13° 6′.5	11	12°53′.3	11	15°32′.4	8	14° 8′.9	8		

TABLE XIV.

NORMAL. VARYING INCLINATION TO LINE OF VISION.

Observer.	30°	N.	M. V.	45°	N.	M. V.	60°	N.	M. V.	90° Normal.	N.	M. V.
E.	5° 8′	2	16′	4°41′.7	4	99′	8°52′.1	2	18′	13°17′.1	4	27′
P.	11°37′.8	2	20′	12°25′.8	4	68′	13° 7′	2	4′	17°10′.2	4	2′
Av. and Tot.	8°22′.9	4		8°33′.7	8		10°59′.5	4		15°13′.6	8	

especially in the one case where smoked glass was used. Observer E., however, shows a constant tendency to see the illusion increased. Careful questioning at the time failed to elicit any reason for this, but I suspect that E. was influenced by the greater ease of measurement here. For under these conditions the deflection of the line seems to be more constant and to extend more to the line as a whole.

Table XIV. shows that the illusion is greatest for two observers when its plane is perpendicular to the line of vision.

TABLE XV.

Observer.	Fig. 32 *a*.	N.	M. V.	Normal.	N.	M. V.
E.	10°24′.7	4	41′	13°17′.2	4	27′
P.	11°31′	4	69′	17°10′.2	4	2′
Av. and Tot.	10°57′.8	8		15°13′.7	8	

TABLE XVI.

Observer.	Fig. 32 *b*.	N.	M. V.	Normal.	N.	M. V.
E.	11°41′.2	4	31′	13°17′.2	4	27′
P.	15°47′.5	4	23′	17°10′.2	4	2′
Av. and Tot.	13°44′.3	8		15°13′.7	8	

TABLE XVII.

Observer.	Fig. 32 *c*.	N.	M.V.	Normal.	N.	M.V.
E.	8°43′.5	4	38′	13°17′.2	4	27′
P.	13° 0′.3	4	44′	17°10′.2	4	2′
Av. and Tot.	10°51′.9	8		15°13′.7	8	

TABLE XVIII.

Observer.	Fig. 32 *d.*	N.	M.V.	Normal.	N.	M.V.
E.	4° 7′.7	3	28′	13°17′.2	4	27′
P.	3°57′.2	3	27′	17°10′.2	4	2′
Av. and Tot.	4° 2′.5	6		15°13′.7	8	

N. B. Threads 3 in. apart.

From Fig. 32 it will be seen that various geometrical shapes may be used to produce the illusion, the 'wriggling' character of which in this case is due to the reversal of the figures at *b*, the lower being here upon the left. Tables XV.–XVIII. refer to experiments made upon figures composed each of six pairs, in the form respectively of *a*, *b*, *c* and *d*. The comparison column of 'normals' was taken at the same time and with all conditions identical except that of form.

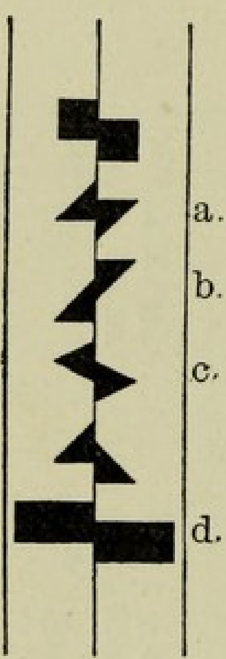


Fig. 32.

In Figs. 32 *b* and 32 *c* the 'irradiation angles' are respectively 135° and 116°+. It is interesting to note that these odd forms give a weakened illusion; also that the particular distribution of black in Fig. 32 *b* gives an illusion greater than that in Fig. 32 *c*, although its 'irradiation angle' is greater. A lateral increase of dark area strikingly reduces the illusion, the records in Table XVIII. being the smallest that we have anywhere encountered.

In Table XIX. are collected for convenient comparison the results of experiments of the 'normal' form, series 2, under the twelve conditions indicated at the left. The often striking deviations from the typical form require no special emphasis here.

Table XIX.

Comparison Table of 'Normal' Forms.

Character of Figure.	Observer.			Averages.
	C.	E.	P.	
Typical form. Fig. 20 . . .	**12°51′**	**11°56′.4**	**17°19′**	**14° 2′.1**
White squares on black. . .	8° 3′.8	10°24′.7	14°41′.8	11° 3′.4
Blue squares on yellow. . .	8°33′.5	5°57′.2	6°59′.7	7°10′.1
Red squares on green. . . .	5°38′.8	2°49′.3	4°39′.9	4°22′.6
Middle line heavier.	8°44′.3	9°38′.8	11°48′.7	10° 3′.9
Normal through pinhole . .	4°33′.8	9°40′.9	14° 3′.7	9°26′.1
Through colored media. . .	[1]11°33′.7	13°24′	[2]15° 9′.5	
Electrical spark	9°33′.4	7°11′.8	8°57′.3	8°34′.2
Fig 32*a*		10°24′.7	11°31′.	10°57′.8
Fig. 32*b*		11°41′.2	15°47′.5	13°44′.3
Fig. 32*c*		8°43′.5	13° 0′.3	10°51′.9
Fig. 32*d*		4° 7′.7	3°57′.2	4° 2′.5

Table XX.

Grand Average of 'Normals,' Form of Table III., Series 2.

Observer.	Normal.	N.
C.	11° 5′.2	25
E.	12°14′.6	26
P.	16° 8′.6	40
Av. and Tot.	13° 9′.5	91

Finally, Table XX. gives the grand average of results from the typical figure, gathered from the various control experiments of the different tables. The final average, 13°+, may be taken roughly as the probable measure of the illusion under most favorable conditions. But there is no wish here to insist upon absolute numbers, for that would involve both a consideration of our

[1] Red and yellow only.

[2] Smoked glass not included.

ability to establish parallelism between lines and a discussion of discriminative sensibility for angular magnitudes—matters which must remain unconsidered here.

The illusion under discussion has now been subjected to both a qualitative and a quantitative examination. The former has shown us that whenever irradiation is excluded the illusion vanishes. The latter has shown that whatever alters the amount and character of irradiation produces an alteration in the amount of the illusion, as conveniently seen in Table XIX. If now we combine these two lines of evidence, the conclusion seems irresistibly forced upon us that irradiation, and that alone, is adequate to explain the phenomenon of the Münsterberg illusion.

ADDENDA.

Since the first appearance of the present contribution to the study of the illusion under discussion, several writers have propounded theories which may be briefly stated here. No one of these writers seems to have been acquainted with the experimental evidence above offered for the validity of the irradiation hypothesis. Filehne[1] makes the absurd attempt to apply his *perspective* theory. The effort is too labored to carry any conviction with it. The line of thought is that, when the line of squares represented in Fig. 20 is placed in the horizontal position, a *bench*, or row of seats is suggested to the observer. The middle line represents the "seat," and the rows of squares above and below form respectively the back and the lower portions of the bench. And what one thus really sees is a bench the left end of which is nearer the

[1] *Zeitsch. f. Psych.*, etc., 1898, XVII., 42.

observer than the right, provided, that is, that the back of the bench be thought of as most remote from the observer. But now by the rules of perspective the middle line *should* have been drawn slightly inclined at the right to the ground line of the figure. For only thus can receding figures be represented in linear drawings. And since the middle line is actually parallel to the base-line, it must therefore *appear* to be tilted upwards at the right. There is of course a certain amount of ingenuity in this explanation, but the ingenuity is entirely misdirected. For it is an entirely arbitrary procedure to place the figure upon its side and then discover some reason for the illusion. In the *vertical* position there is no possible trace of any perspective elements, and even in the horizontal position the alleged perspective elements must be unconsciously operative since the presence of the illusion by no means demands the simultaneous conscious perception of any such thing as a bench. Of course the perspective theory of optical illusions finds no difficulty in this latter objection. But it is nevertheless a genuine difficulty which has never yet been satisfactorily met.

There is, to be sure, a secondary illusion in this figure, to which several observers have called attention, which might lead one to suspect the presence of real perspective elements. Hold Fig. 19 so that the plane of the paper makes a small angle with the line of vision. Then turn the diagram until the angle between the line of vision and the verticals equals about 30°. In this position the lines of rectangles that run away from the observer seem to form each a series of low steps. Running the eye along any vertical reveals this very clearly. The reason is evident. The background, as it were, for any rectangle viewed in this way is partly a white area, partly a similar black area. These areas are so distributed that when dark area is followed by dark area the middle portion of these joined areas must seem somewhat darker than the outlying parts, since the latter have received a grayish tinge from the white areas beyond. This darker portion can be interpreted only as a part lying in shadow, and hence the illusory perception of a low step, the "riser" being the shadowed portion. But as surely as this secondary illusion rests upon one of the accessory criteria of perspective vision, just as little can it furnish any basis for the perspective explanation of the primary illusion. That this is due to irradiation cannot now, I think, be doubted.

Lipps[1] and von Zehender[2] both agree with Heymans to this extent, that this illusion is to be united with that of Zöllner under the same explanatory principles. What these explanatory principles are need not here especially concern us. The chief contention is in both cases that, though acute angles are here wanting and cannot, therefore, be invoked as elements the illusory perception of which is the cause of the linear deflections, the successive pairs of squares do form *oblique directions* in as effective a fashion as in the Zöllner pattern. For Lipps, therefore, it simply remains to apply his "æsthetic" principle in accordance with which there is aroused by the figure the idea of a

[1] Raumæsthetik u. geometrisch-optischen Täuschungen, 1897, p. 319.

[2] Ueber geometrisch-optischen Täuschungen. *Zeitsch. f. Psych.*, etc., 1899, XX., 85.

disturbance of balance, which idea becomes forthwith responsible for the illusion of perception, while von Zehender proceeds to apply an utterly defective principle based upon an entire misinterpretation of an observation of Volkmann. A reference to a criticism of this defective principle will suffice here.[1] But in either case simply examining any set of overlapping squares through a pinhole in a card will convince one that the deflections are not like those of the Zöllner illusion. In the latter the lines are deflected as *wholes*. In the Münsterberg illusion, on the other hand, the deflections are only along the inner sides of the squares, and the zigzag character of the whole line is plainly seen when the figure is thus viewed through a pinhole.

[1] Judd, *Psych. Rev.*, 1899, VI., 547.

THE POGGENDORFF ILLUSION.

When Zöllner remarked to Poggendorff that the illusion which now bears the latter's name was probably due to astigmatism, he little knew what long and complex discussions were to be aroused later by this apparently simple peculiarity of perception. In one form or another this illusion has been before the scientific world for nearly thirty years, yet few would venture to make the claim that all its features have been exhaustively examined or that even its main features have been explained with such convincing thoroughness as to render any further attempts superfluous.

Plenty of explanations have been offered, and many of these have been based upon ingeniously devised and carefully executed experiments. To be sure this illusion has not incited to the controversy and remarkable multiplicity of theory that has grown up about the Müller-Lyer illusion. Still the literature that concerns it could hardly be called scanty. At least ten explanations have been propounded, and of these four in particular have been earnestly championed by able advocates. Out of all the array of comments and discussions that have gathered themselves together about this matter, one thing seems to force itself upon the conviction, namely that the illusion is peculiarly complex. Explanations, therefore, that are apparently at variance may each contain parts of the explanation that shall finally prove to be satisfactory. It is, consequently, the task of any fresh investigation to probe as thoroughly as may be possible such explanations as are still able to stand aright, that their merits and demerits may be brought to light. As a preparation for the partial execution of such a programme, it may be well to glance briefly at the chief explanatory attempts that the literature of the subject contains.

A.

(*a*) As already remarked, Zöllner suggested that the existence of this illusion was probably due to *astigmatism*.[1] The form of the illusion with which he was acquainted and to which, in fact, Poggendorff had called his attention, was the discontinuity of the obliques which is to be seen in the heavy-lined "pattern" which he had just discovered. Zöllner spoke of this as the "Nonius-artige Verschiebung" of the obliques, a name that is still occasionally given to the phenomenon. His explanation possesses only historical interest to-day.

(*b*) Though appealing also to other factors, Helmholtz confidently puts forward *irradiation* as a partial explanation of the illusion.[2] Though it may be true that irradiation is effective in cases where the central strip and the intercepted oblique are, one or both, made of solid black, this factor may be entirely disregarded in the usual figures that are subjected to investigation. Indeed, if present, its supposed influence can hardly be made clear, since its effectiveness at any one point in producing an apparent increase in the inclination of an

[1] *Poggendorff's Annalen*, 1860, CX., 500 ff. and "Ueber die Nature der Cometen," 1872, pp. 380 ff.

[2] Physiol. Optik (2), p. 707.

oblique must be counterbalanced by the opposite effect at a point on the opposite side of the oblique. This explanation, then, also possesses only historical interest. Of more importance are the two explanations that follow.

(*c*) The cause of the illusion has sometimes been sought in *the underestimation of the width of the intercepting strip.*[1] Every one knows how our perceptions of relative distances are often based upon the interruption of contour lines, which thus indicates to us that a farther object has been obscured by one less remote. Especially perhaps is this true for our judgments respecting the distances of adjacent hills or other distant objects of known contours. But linear drawings may just as well suggest relative remoteness in this way. And this, it is said, is exactly what happens in the case

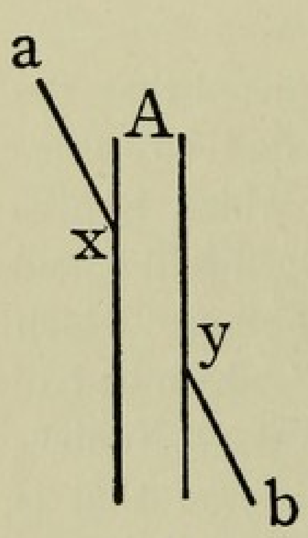


Fig. 33.

of Fig. 33. The middle part of the line *ab* is obscured by the strip *A*, and *A* must consequently be perceived as nearer than the line. But if perceived as nearer than the line, that is, as nearer than it actually is, it must also be perceived as *narrower* than it actually is. This is a necessary consequence of the most elementary laws of perception. Now the practical result of this false perception is that the two parts of Fig. 33 have been pushed somewhat together, and, the inner ends of the obscured oblique, *x* and *y*, remaining attached to the same points of the vertical parallels, the free parts of the

[1] See, *e. g.*, Thomas Foster in *Knowledge*, 1881, I., 10, where one or two interesting variants are given.

oblique must seem to have been shifted out of the line of continuity. If this explanation be true, it is clear that the greater the underestimation of the width of the strip, the greater the illusion of discontinuity. The experimental test of this explanation will be discussed later.

(*d*) As would be expected, certain writers have appealed to peculiarities of eye-movement as the various lines of the Poggendorff figure are followed. To be mentioned here are two explanations, which, though making use of eye-movements, do not connect them-

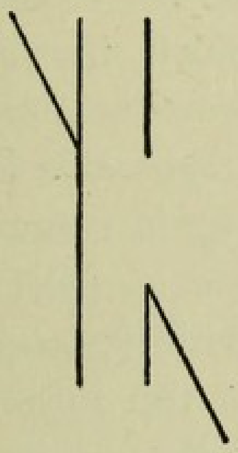


Fig. 34.

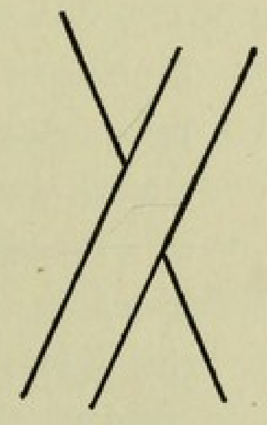


Fig. 35.

selves with the usual and well-attested false estimations of extents. The first runs thus:[1] From the point of view of the eye muscles, movements along obliques are more complex than those along vertical or horizontal lines. And such being the case, it is said, when the eye moves, or tries to move, along the oblique its true course "is not so certainly continued when the guide of the line is lost by the interposition of the space." Thus in Fig. 34 the illusion is diminished because, it is said, no simple movement is offered in place of the complex move-

[1] See W. D. Richmond: *Knowledge*, 1881, I., 57.

ment along the oblique. Again, in Fig. 35, where only oblique lines are offered to the eyes, this illusion, it is claimed, has vanished. This last alleged fact is manifestly open to dispute. And in any case the general lameness of this explanation is at once seen when one reflects that, while it might possibly account for the presence of *an* illusion, it cannot in any way account for the particular direction of the illusion, nor for its continued existence (as we shall later show) when the interrupted line lies horizontally or vertically. The second explanation referred to is that of Dresslar.[1] It rests itself upon a supposed analogy between the Poggendorff illusion and an illusion of tactual perception that superficially resembles it. The tactual illusion is experienced when the finger tip is run along one of two cutting lines made by pricking pinholes in a card. The illusion consists in the apparent shifting of the lower half of one of the obliques and may be readily explained thus. The "feeling point" of the finger tip passing down *BC* (Fig. 36) has not yet reached the point of intersection, *O*, when an adjacent point on the finger tip, not to be spatially discriminated from the "feeling point," comes in contact with a point on the line *AD*. It seems, therefore, as if the point of intersection had been reached. Accordingly, when this point is actually arrived at by the "feeling point," and when a moment later a second adjacent area on the finger tip touches the lower half of the line *AD*, it seems as if this lower half, *OD*, had been

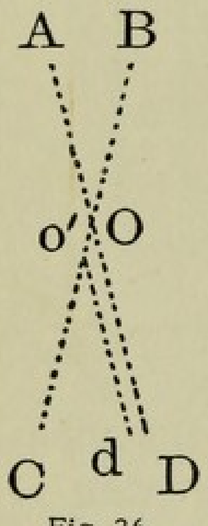


Fig. 36.

[1] *Am. Jour. of Psych.*, 1893-4, VI., 275.

bodily shifted into the new position, *o'd*. Now, then, the visual analogy. The fovea being looked upon as the "feeling point" of the retina, as the eye runs down, say, the left hand vertical of Fig. 33, "the point *x* will enter the field of clearest vision sooner than point *y*, and as the eye must move down some little distance before it gets the clearest view of *y*, the tendency is to make the displacement appear in the line, as if the point *x* had been met with in the same line as *y*, but as much higher as the difference in time of receiving the impression would indicate." But this is to make the eye the pitiful dupe of the figure. As if the eyes were not altogether sure that *y* is not in the same line with *x*! This explanation is too mythical to demand really serious attention. The figures, in the first place, are not analogous enough for the application of similar explanations. And, in the second place, the real cause of the tactual illusion, the failure, namely, to discriminate the adjacent touching-points from the true "feeling point," has here no parallel.

(*e*) The explanation in terms of *the overestimation of acute angles* has been more widely held than any other. And, next to that of Zöllner, it is the oldest of all. Hering[1] and Kundt[2] were the first to apply this principle, each independently of the other, and Helmholtz[3] followed them soon after. Having once admitted that in our perception of acute angles we constantly overestimate them, the application of the principle is simple enough. The free ends of the oblique are simply rotated about the

[1] *Beiträge zur Physiologie*, 1861, pp. 65–80.

[2] Untersuchungen über Augenmass u. optische Täuschungen, *Poggendorff's Annalen*, 1863, CXX., 118.

[3] Loc. cit., p. 709.

points x and y (Fig. 33) to that amount which shall express the perceptual enlargement of the two acute angles, and the apparent discontinuity of the two ends necessarily follows. To be sure there have been many conflicting theories in reference to the admitted overestimation of the angles, but, so far as I am aware, there has been perfect agreement in the application of the simple illusion to the more complex case. The clearness and directness of this explanation have gained for it many adherents. It was only a more detailed knowledge of the dependence of the illusion upon the position and structure of the figure that led, as we shall see, to the adoption of supplementary explanations.

One variation of this theory should be mentioned here. The theory of the overestimation of acute angles has usually appealed to the obverse fact that the adjacent obtuse angles are underestimated. Jastrow, however,[1] enunciates the principle that *both* acute and obtuse angles are underestimated, an important corollary of this being that when an acute and an obtuse angle are so placed that their false estimations conflict, that of the obtuse angle will so preponderate as practically to be alone effective. Thus for Jastrow the Poggendorff illusion is to be attributed to *the underestimation of obtuse angles*. The result is the same, the statement of the cause differs.

Greater precision has been given the explanation that we are here considering by applying to it the results of quantitative investigations. Plenty of these have been carried out, the object being always to ascertain the maxima and the minima of the illusion under all possible

[1] *Am. Jour. of Psych.*, 1892, IV., 381 ff.

conditions of variation in the figure. Burmester's results are the most definite.[1] The law deduced from them is that the amount of the illusion (v) is proportional to the width of the vertical strip (u) and to the cotangent of the angle of inclination (w) of the oblique. Or, in the form of an equation, k being a constant, $v = ku \cot w$. This of course presupposes that the length of the oblique remain constant. Hering had asserted that an angle of 60° is correctly perceived in its own magnitude, and that the Poggendorff illusion must therefore vanish when this is the angle of inclination of the oblique. Burmester readily shows this statement to be false. The degree of the illusion is indeed diminished under this condition, for the law shows that the larger the angle the smaller the illusion. But it does not by any means disappear. A mathematical discussion of the point of maximal overestimation of the acute angle has for its outcome that this is in the vicinity of 42°. At this point the amount of overestimation is calculated to be nearly 5°.

(*b*) The *perspective* explanation has been very earnestly advocated by a few writers. Two phases may be distinguished, of the first of which Thiéry may be taken as the representative.[2] In this first phase there is simply an application to this illusion of the ordinary perspective arguments as they are employed everywhere in the field of geometrical-optical illusions. The fact, early mentioned by Hering, is emphatically appealed to, that when a Poggendorff diagram (Fig. 33, *e. g.*) is monocularly viewed the free ends of the transversal seem not to lie in

[1] Beitrag zur experimentellen Bestimmung geometrisch-optischer Täuschungen, *Zeitsch. f. Psych.*, etc., 1896, XII., 355.

[2] *Phil. Stud.*, 1895, XI., 357 ff.

the plane of the paper but to pass into the third dimension, one running forward, the other back. The acute angle of the plane is therefore the *representative* of the larger tridimensional angle which a special scrutiny of the figure reveals. But whether this perspective quality of the figure be noticed or not, it has its effect none the less upon the perception of its angular magnitudes. One is thus inevitably forced to see the acute angles as larger than they actually are and thus the illusion is explained. Practically this explanation is the same as the preceding one since it also rests itself upon the overestimation of acute angles. Only here the grounds for this overestimation are not taken from general considerations but are sought in the particular figure itself.

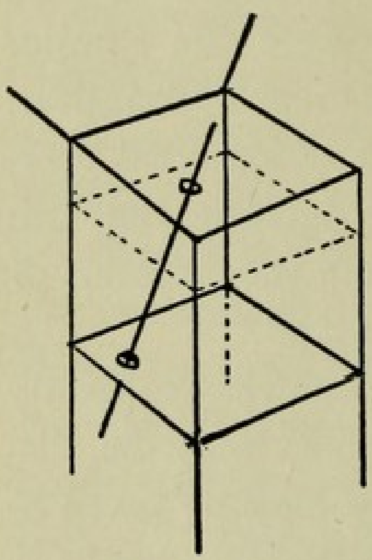


Fig. 37.

Somewhat different is the second phase of this explanation proposed by Filehne.[1] In the opinion of this writer, the vertical strip of the Poggendorff figure serves principally to sunder the two ends of the transversal to such a degree that there is no longer any sufficient reason for regarding them as belonging together. Now, remembering that, in accordance with the perspective theory in general, the lines of a plane geometrical figure act chiefly is the means of suggesting real objects of actual experience, we can easily see the line of thought. For there is not the remotest necessity that

[1] Die geometrisch-optischen Täuschungen, etc., *Zeitsch. f. Psych.*, etc., 1898, XVII., 30 ff.

two detached portions of a straight line represent objects whose bounding edges should appear continuous, merely because they would meet and form a continuous line in the linear drawing that represents them. It can be most readily and graphically shown by straight-line drawings of objects that two detached portions of one and the same line may represent objects in totally different planes of space, so that if the objects represented were to be prolonged in their own direction they might never meet at all, or at best only at an oblique angle. Such a case is presented in Fig. 37. In terms of the linear drawing the stick and the handle are strictly continuous. As represented *objects*, on the other hand, their directions are widely at variance. In the Poggendorff figure, consequently, it is highly probable that the sundered portions of the oblique recall some real experience, or set of experiences, in which the objects represented are absolutely unconnected. Such an experience may be suggested by a finger-post, an arm upon one side pointing obliquely towards the observer, an arm upon the other side—lower or higher, as the case may be—pointing obliquely away. Herr Filehne finds great support for this view in the alleged observation that every trace of the illusion vanishes in the above figure if only the two verticals be somehow united, or if some indications be present to show that the ends of the transversal are portions of a continuous whole, the missing part of which is hidden behind the vertical strip. The first condition can be secured by drawing within the

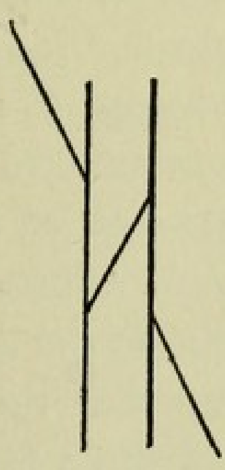


Fig. 38.

latter a short line which shall be oblique to the transversal and meet the edges of the strip at points opposite those in which the ends of the transversal terminate. (Fig. 38.)

The second condition can be readily secured by making the transversal represent a pointed stick, or by placing at the outer ends of the transversal the drawing of some such device as weights and pulleys, which shall make it clear that the two ends are really acting in unity. But though these conditions be fulfilled to perfection, the illusion simply does *not* vanish, despite the assertion of Filehne to the contrary, nor would one expect it to do so. For what these particular devices are expected to accomplish is the closer approach to the actual conditions of tri-dimensional vision, where only one interpretation of the lines is possible. The best conditions for testing the theory would be found, therefore, in normal objective experience. Stretch a rope obliquely behind a tree trunk and at a distance of some feet from it. The illusion persists, and yet there is no possible attempt to give an independent perspective interpretation to either end of the rope.

The perspective theory is indeed a captivating one, not only as applied to this particular illusion but also as employed in the attempted explanation of many other geometrical-optical illusions. For it is perfectly true, as is emphasized by the theory, that our ordinary visual experiences are preponderatingly tri-dimensional in character. And it is only natural to suppose that this fact might compel us to interpret in the light of these experiences every perspective motive that any linear drawing might contain. But in our own opinion, the searching and destructive criticism to which Wundt[1] has subjected this theory has once and for all thrown it out of court.

As to the particular assertion made by Filehne, that the illusion has disappeared in Fig. 38, the following experimental evidence is strongly to the contrary. Cards were prepared, in a manner to be described later, which allowed the vertical movement of one end of the oblique. This was then to be placed in a position of apparent continuity with the other end. The testimony of six observers is unanimous to the effect that while the degree of the illusion is diminished by the presence of the short inner oblique, it is in every case present. The amount of displacement necessary to bring the ends into apparent continuity was, in the case of the two trained observers P. and T., respectively 6.27 and 5.66 half-millimeters. Without the inner line these figures were increased to 10 and 9 units respectively.

(*g*) It has been long known that the Poggendorff illusion is reduced in degree by placing the figure horizontally. To make room for this fact, those who have desired a complete theory have been obliged to make use of various secondary lines of explanation. Thus Wundt, who sees the chief cause of this illusion in the overestimation of acute angles, appeals also to other well-known elementary illusions.[2] Thus vertical, as compared with horizontal, extents are constantly overestimated. Accordingly the vertical dimensions of the figure in question must be in-

[1] Die geometrisch-optischen Täuschungen. Leipzig, 1898.

[2] Loc. cit., p. 149. And Grundzüge, II., 148.

creased for perception and the discontinuity of the oblique lines be thereby emphasized, when the figure is in the vertical position. And, further, empty extents are always underestimated as compared with filled extents. Consequently the open oblique distance between *x* and *y* (Fig. 33) must be perceived shorter than it actually is. This also must coöperate to increase the degree of the illusion, supposedly because the vertical strip is thus falsely perceived as too narrow.

Thus the three varieties of simple illusion that the structure of the figure makes possible are collectively called upon in explanation of the larger and more complex phenomenon.

(*h*) Finally, a most interesting attempt has been made by Dr. Judd to discard entirely all "angle explanations," and to find the sole provoking cause in *misjudgments of linear extents.*[1] The influence of other elementary illusions than those thus far mentioned is invoked, and it is maintained that the fundamental causes operative here are the same as those that produce the well-known illusion of the Müller-Lyer figures. Negative evidence, to the effect that the misjudgments of acute angles is not to be put forward as the cause of the Poggendorff illusion, Dr. Judd claims to find in the frequently alleged observation that the illusion completely vanishes when the figure is so placed that the intercepted line is either vertical or horizontal. Now acute angles should be misjudged in

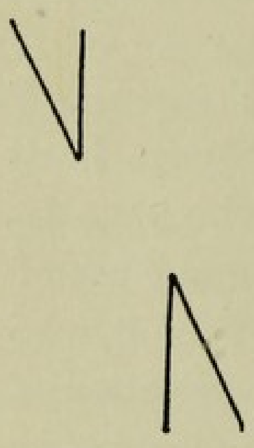


Fig. 39.

[1] Judd, A Study of Geometrical Illusions, *Psych. Rev.*, 1899, VI., 241.

this as in any other position. Hence the flimsiness of the angle explanation. Further negative evidence for the same thing is found in certain mutilated Poggendorff figures. Retain only the acute angles (as in Fig. 39) and the illusion disappears, while the retaining of the obtuse angles in a similar way leaves the illusion in full force. Now one would expect on the angle theory that Fig. 39 would present a strong illusion, and its entire absence shows, it is claimed, how little real influence the

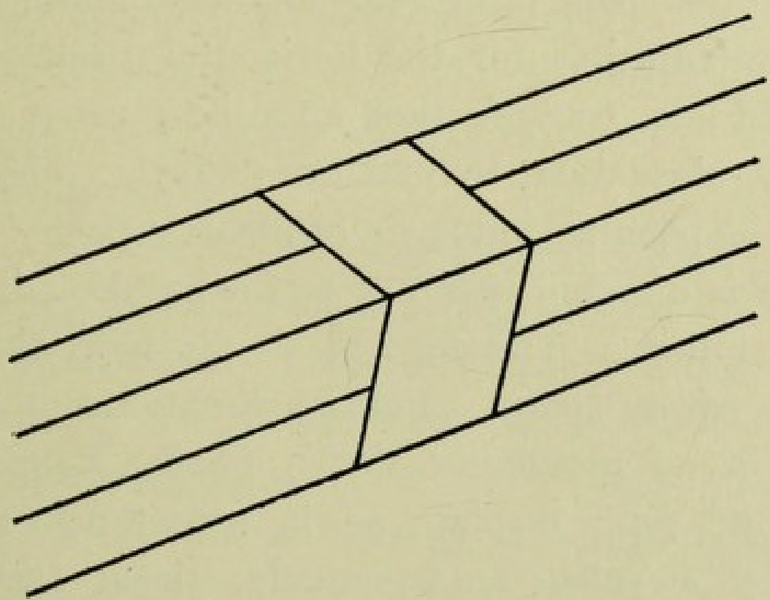


Fig. 40.

acute angles have. Again Fig. 40 shows the regular illusion, yet all the obliques of the figure are seen to be perfectly parallel, which could not be the case were the illusion due to angular enlargements.

Evidently then other causes must be looked for. Dr. Judd finds these first in *the underestimation of the empty interval between the inner ends of the interrupted oblique*, and, second, in the fact signallized by Müller-Lyer *that, if an oblique line cut a vertical* (*or other line*), *distances on the latter are overestimated on the obtuse angle side and* *underestimated on the other*. The operation of this second factor produces false estimations along the parallels that bound the vertical strip and thus contributes to the illusory discontinuity of the parts of the oblique. The first factor is quantitatively examined, the figure being for that purpose put into the position where the intercepted line is horizontal and thus where, it is claimed, the illusion is wanting. The underestimation was found to be greatest when the various lines retained were identical with the typical Müller-Lyer figure for underestimation. But if the illusion vanishes for the horizontal position of the intercepted line, some further annulling cause must be working against these two factors named. This is to be found in the peculiar influence of *the direction of attention*. The latter is directed here not to the parallels but to the intercepted line. The former simply help the eye to cross the empty space. False estimations along them are here of no moment. When, finally, figures of the Poggendorff type display, as is sometimes the case, perceptible angular enlargements, these are directly due, it is argued, to false linear estimates along their sides.

This explanation arouses a number of interesting inquiries, notably in connection with the alleged vanishing of the illusion, and in connection with the particular bearing upon the illusion of the false estimation of the empty interval. We shall return to these points later. For the present, however, we may call attention to the fact that Fig. 39 (p. 254) can hardly be employed to furnish evidence against the overestimation of acute angles. For in reducing the figure to this form an entirely new illusion has been introduced which militates against any linear deflection that might otherwise come from the misjudgment of the angles themselves. In fact the Poggendorff deflection is not only annulled but, for most observers, *reversed* in direction. Now this new illusion may be due, as Brentano[1] suggested in reference to his own identical figures, to the influence upon the eye of the general direction to which the vertices respectively point, or to the more subtle factors proposed by Thiéry.[2] In any case we have wholly deserted the Poggendorff figure when we come to any such dissevered remnants of it, and consequently the alleged evidence from Fig. 39 is hardly to the point.[3]

(*i* and *j*) Two relatively unimportant attempts to propound new explanations for the Poggendorff illusion may be merely mentioned. They are those of Einthoven[4] and von Zehender.[5] The former sees the cause of such illusions as the one in question in the influence of indirect vision and the particular manner in which overlappings of dispersion circles occur. The latter attempts, in an incomprehensibly erroneous manner, to apply to the Poggendorff figure the well-known observation of Volkmann that two lines must diverge slightly above in order to appear strictly parallel.[6]

[1] *Zeitsch. f. Psych.*, 1893–4, VI., 1.

[2] *Phil. Stud.*, 1895, XII., 117.

[3] Similar remarks may be made in connection with Fig. 34, p. 245.

[4] *Pflüger's Archiv*, 1898, LXXI., 1.

[5] *Zeitsch. f. Psych.*, 1899, XX., 65.

[6] For a concise statement of the matter see Judd: *Psych. Rev.*, 1899, VI., 547.

B.

From the foregoing it will doubtless be fairly evident that something remains yet to be said upon the subject of the Poggendorff illusion. In view of the conflicting claims made, several questions press forward for renewed examination. Among those that are accessible to experimentation are the following :

1. Does the illusion disappear when the intercepted line is horizontal or vertical ?
2. Is the width of the intercepting strip falsely estimated ?
3. Is the space between the inner ends of the oblique misjudged ?
4. Are the acute angles of the figure overestimated ?

It was in the hope of contributing towards the solution of these questions that the following experiments were undertaken. The diagrams used in the investigation were drawn upon heavy paper, and were invariably of the following dimensions :

Length of parallels	= 60 mm.	
Width of strip	= 10	"
Free ends of oblique	= 30	"
Angle of intersection	= 35°.	

There seem to be many advantages in moderately small diagrams, not only because they are more convenient to handle, but, more important than that, because the eye more readily takes in the whole figure and can thus attain greater subjective certainty in the required adjustments. The method employed was that now current in such investigations. Fig. 41 will make everything clear. A slit, *ab*, accurately made with a sharp knife along the outer edge of the left vertical, allowed the insertion of the part *B* which could then be moved vertically as desired. If, as in some cases to be described, the part *B* was to move horizontally, the lower edge of *A* was accurately folded over to form a shallow trough in which *B* could slide. In such cases the parts of *B* above and below the middle tongue were not cut away, but being separated from this by horizontal slits were kept upon the face of the diagram. All settings were made alternately from either direction. Upon the back of each card was a scale graduated to half millimeters. At the moment of investigation each diagram was fastened to a flat board held perpendicular to the line of sight, and, except in a few unimportant instances, the field of view was restricted to the figures themselves by means of tubes. The constant distance of the diagrams from the eyes was 475 mm. Openings in the board allowed the reading of the scales.

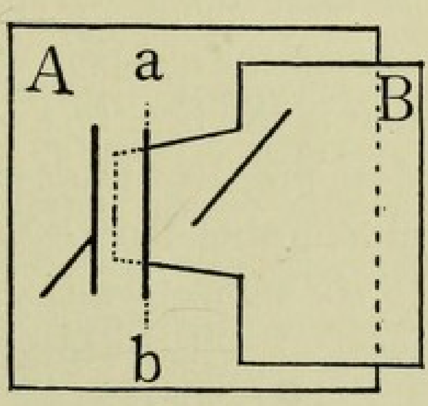


Fig. 41.

I.

Does the illusion disappear when the intercepted line is horizontal or vertical?—It is often asserted dogmatically that when the Poggendorff figure is rotated so as to make the intercepted line vertical or horizontal, the illusion is destroyed. Thus Thiéry[1] makes this affirmation and

[1] *Phil. Stud.*, 1895, XI., 363.

finds comfort therein for his perspective theory, since in this position of the figure no tri-dimensional motives are aroused by association. The same is maintained more recently by von Zehender;[1] and Judd,[2] as the reader will recall, made this alleged observation the basal condition of his experiments.

Now of course it is rash to make an unqualified contradiction of this claim, but since other observers have been unable to concur in any such statement, the case is at least open to suspicion. And, as always when disagreement exists in such matters, a quantitative examination is the best test of the truth. To this end diagrams were prepared as described above, only with the particular modifications necessary here, and these were submitted to six subjects with the challenge to set a true apparent vertical or horizontal. The uniform result was that the apparent and the real lines never coincided. The illusion is slight, to be sure, but present notwithstanding. The accompanying table gives the records of two trained observers.

TABLE I (*a*).

Intercepted Line Vertical.			Intercepted Line Horizontal.		
Subject.	No. of Deter.	Avg. Error.	Subject.	No. of Deter.	Avg. Error.
P.	10	3.99	P.	10	3.35
T.	4	3.25	T.	4	3.16

The errors are given in half-millimeters and are to be compared with the errors given in Table II., p. 262, where the figure was held with its parallels vertical.

[1] *Zeitsch. f. Psych.*, 1899, XX., 86.

[2] P. 254.

Our first result then is that *the illusion persists, though in diminished degree, when the intercepted line is either vertical or horizontal.*

To make this matter doubly sure a large diagram was submitted to two groups of fifty students each. The results were as follows: With the intercepted line in the *vertical* position, 60 of the 100 subjects saw the illusion, 7 were undecided, one saw the illusion reversed, and *32 saw no illusion.* With the intercepted line horizontal, 76 saw the regular illusion, 6 were undecided, 2 saw the illusion reversed, and *16 saw no illusion.* But mass experiments such as these are never entirely satisfactory. The question arose, accordingly, whether those who reported 'no illusion' in connection with the large diagram would fail to see the illusion if a smaller diagram were more carefully scrutinized, or if a setting of the adjustable diagram were requested. It so happened that 10 subjects had seen no illusion in either position of the intercepted line. These were examined individually, and together with them 10 chosen at random from those who saw no illusion in one of the two positions of the transversal. If a simple inspection of the small diagram sufficed to reveal the illusion, no further examination was made. In about half of the cases, however, there was still no illusion visible. The adjustable diagram was then resorted to with the result that all but two gave distinct evidence of the presence of the usual illusion under the experimental conditions of a restricted field of view. The two referred to made errors of setting that were about equally positive and negative. None were correct. With the exception of these two then—and they seemed not wholly free from the effects of an illusion of some sort—there were none of those carefully examined who did not perceive the illusion.

As a corollary to the general proposition given above, it may be stated that *the magnitude of the illusion in the positions of the figure here considered varies directly with the angle of intersection of the transversal.* The following table is self-explaining.

TABLE I (*b*).

Subjects.	Angle of Inclination, and Errors.					
	Intercepted Line Vertical.			Intercepted Line Horizontal.		
	20°	35°	60°	20°	35°	60°
B.	10.	2.75	1.69	8.25	2.41	1.81
C.	7.77	2.37	1.18	3.37	1.28	1.15
P.	8.76	3.99	2.	10.	3.35	2.2

2.

Is the width of the intercepting strip falsely estimated?—In preparation for the reply to this question, a series of experiments was carried out with three different fillings of the space between the vertical parallels. Fig. 42 will show what these were. Table II. contains the experimental results.

TABLE II.

SETTING OF THE OBLIQUES OF A, B, AND C.

Figures.	Subject P.		Subject T.	
	Avg. Error.	No. of Deter.	Avg. Error.	No. of Deter.
A	10.11	50	8.97	10
B	8.25	50	7.84	10
C	8.87	50	8.31	10

It will seen from Fig. 42 that the vertical strip of *B* appears wider than that of either *A* or *C*. In accordance with this we should expect the error of setting in *B* to be less than in the other cases. For, as we saw in the converse form on p. 244, the illusory increase of the strip most involve a decrease of the illusory displacement of the oblique. Table II. shows that this is actually the case. But in a similar way the vertical of *C* seems the narrowest of all, and on the same reasoning the error in connection

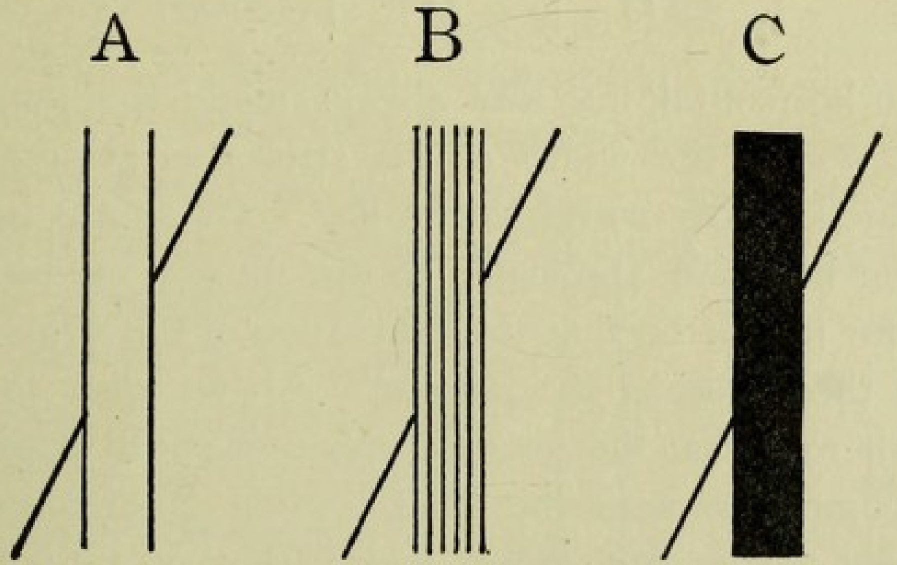


Fig. 42.

with its oblique should be the largest shown in the table. This is not the case, nevertheless, and we see at once that, however much the width of the strip may be falsely estimated, there are other factors at work with it to determine the illusion under discussion.

The apparent relative widths of the strips were verified by preparing diagrams which presented these alone without the obliques. A straight horizontal line, emerging as usual from the side of the strip, was then set at a point of apparent equality with the width of the latter. To make the conditions uniform for each figure, the horizontal distance inclusive of the lines was regarded as the width of the strip. Table III. gives the results.

TABLE III.

APPARENT WIDTHS OF STRIPS OF A, B AND C.

Figures.	Subject P.		Subject T.	
	Avg. Error.	No. of Deter.	Avg. Error.	No. of Deter.
A	+0.33	24	+1.18	10
B	+3.80	10	+3.11	4
C	+0.13	24	+0.99	10

The horizontal line was always made too long. In other words, the width of the strip was always overestimated as compared with a line. Of course this fact can not be made applicable to the illusion of discontinuity, for it may be that the estimation of the strips differs when the obliques are present. All we wish to insist upon here is that the greatest apparent width (that of *B*) is accompanied with the least illusion of discontinuity, whereas the least apparent width (that of *C*) is *not* accompanied with the greatest illusion of discontinuity.

The certainty that this latter fact gives to the thought of other operative factors is still further reinforced by a simple mathematical calculation. The illusory widening or narrowing of the middle strip has the effect, of course, of a lateral movement of the point of intersection of, say, the right free end of the oblique with the vertical on that side, and a vertical movement of the point of intersection with the other oblique—the latter being considered to remain fixed. Now the amounts of these two movements may be regarded as the base and altitude respectively of a right triangle, the angle at whose upper vertex is equal to the angles made by the intercepted line with the parallels. The amount of illusory lateral displacement being known the supposed amount of accompanying vertical displacement may then be calculated from the formula

$$b = a \cot \alpha,$$

the three factors of which represent respectively the vertical and the lateral displacements, and the angles of the oblique. Now the excess of apparent width of B over A is 3.47 (Table III., Subj. P.). Substituting this value for a in the above formula, and remembering that $\alpha = 35°$, the value of b is found to be 4.95. That is, the vertical expression of the illusion of discontinuity should, were the misjudgment of the width of the strip the only cause of the illusion, be greater for A than for B by 4.95 units. Table II. shows however a difference of only $(10.11 - 8.25 =)$ 1.86 units. Other powerful factors then must be at work either in the direction of increasing the illusion of discontinuity in B or of decreasing it in A. Presumably the former is effected, the numerous vertical lines tending to increase unduly the vertical estimates.

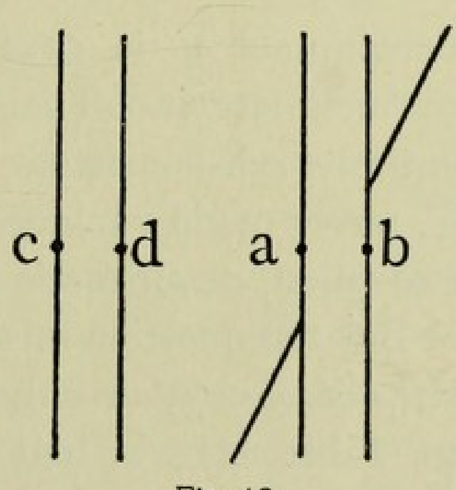


Fig. 43.

The next step was to examine the influence of the obliques in the estimate of the vertical strip. A diagram like that of Fig. 43 was used, the horizontal distance ab being adjustable. To aid in the precision of the judgment tiny dots were placed as shown in the figure, and the centers of these were taken as the points of reference. At the same time special pains were taken to distribute the attention over the whole figure. For any unintentional disregard of the presence of the obliques would be destructive of the ends desired. Table IV. contains the results. It will be seen that the two observers do not agree. P. makes the length *ab* too large, T., too small. P., that is, must slightly underestimate *ab* in comparison with *cd*, T. must slightly overestimate it. The settings of both P. and T. were entirely self-consistent in respect to direction of error. T. never made *ab* larger than *cd*, while P., with a single exception, never made it smaller.

TABLE IV.

APPARENT WIDTH OF *ab* AS COMPARED WITH *cd*.

Subject.	No. of Deter.	Avg. Error.	M. V.
P.	20	+0.36	0.189
T.	10	—0.42	0.33

But the question arises: do either P. or T. misjudge the strip *absolutely*, or only in terms of *cd*. Now according to Table III., T. had somewhat largely overestimated the strip in terms of a *line*. Might it not be possible therefore that though *ab* was made too small relatively to *cd*, it would not be thus estimated relatively to a line? To test this for both observers, a horizontal line equal to *cd* was made to take the place of the parallels. Table V. gives the results. Relatively to a line then the direction of the error is still the same for each observer. But the amounts of these misjudgments are entirely insignificant as contributing to the illusion of discontinuity. For, as the formula on page 265 shows, a misjudgment of the width of the strip by *one* unit in either direction means a change in the degree of the illusion the vertical expression of which would be only 1.4 units. When one compares this with the readings given in Table II., the complete triviality of this supposed factor is apparent. For all the misjudgments recorded in Tables IV. and V. are less than a single unit and whether

TABLE V.

APPARENT WIDTH OF *ab* (FIG. 43) AS COMPARED WITH A HORIZONTAL LINE OBJECTIVELY EQUAL TO IT.

Subject.	No. of Deter.	Avg. Error.
P.	20	+0.66
T.	10	—0.07

they express underestimations or overestimations makes little difference. At the same time the influence of this factor must be admitted and perhaps emphasized. For misjudgments of the width of the strip are undoubtedly made and that too, apparently, in different directions for different observers. Tendencies must therefore be present which, as the case may be, shall increase or decrease the general illusion. But it may also be admitted that the influence of this factor is minimal and relatively insignificant.

3.

Is the space between the inner ends of the oblique misjudged?—Both Wundt and Judd have maintained that this misjudgment exists, the former because of the general tendency to underestimate empty as opposed to filled extents, the latter on the basis of experimental verification of this conjecture. It is with the claims of the latter that we have to do. As the reader will recall, the procedure of Judd was to place the Poggendorff figure so that the intercepted line was horizontal. Measurements made under these conditions showed that the interval between the points of interception was underestimated relatively to the similar distance on a figure where the parallels were perpendicular to the horizontal. We have repeated this experiment, using a horizontal line as the standard distance, and have obtained similar evidences of underestimation. But the question arises: What right has one to examine the figure in the position mentioned and then draw conclusions that shall be valid for other positions as well? Especially when, as Judd asserted, the primary illusion had disappeared under the conditions chosen. When the primary illusion has disappeared under particular conditions, how can the existence of a secondary illusion under the same conditions be made the cause of the primary illusion under *other* conditions? Suppose the inner interval *is* underestimated when the intercepted line is horizontal, what warrant have we for assuming that the same will be true for a vertical position of the parallels? And, further, if this underestimation fails to produce any illusion in the horizontal position of the intercepted line, how should it avail to cause the illusion in other portions of the figure? To appeal to the direction of attention as influential in the first case is certainly to leave the second case unexplained. For it would be perfectly plausible, on the basis of experiments made in the first position, to suppose that the underestimation of the interval served only to accomplish the general contraction of the figure in such a way as to leave as little illusion in *all other* positions of the figure as in the position examined. Judd admits, indeed, that the underestimation is less in other positions of the figure,[1] but he entirely fails to explain how any underestimation is to be understood as contributing to the primary illusion. If it is by entailing a narrowing of the intercepting strip, then this influence is, as was shown in the foregoing section, relatively minute and sometimes not present at all. If it is by drawing together the inner ends of the intercepted line, the illusion should be less than it would be without this influence. For, supposing the apparent width of the strip to remain the same, this illusory approach of the inner ends of the line could only mean a diminution of the *vertical* distance between them and thus a decrease in the illusion of discontinuity. In other words, this linear misjudgment must be looked upon as prejudicial to the illusion if its effect is anything else than the apparent narrowing of the strip. And if this be its effect, it must be confessed that it plays a very secondary part in determining the degree of the primary illusion.

But the real question is : *is* the interval misjudged in, say, the normal position of the figure, and if so what is the direction of this misjudgment ? To investigate this conditions were arranged as shown in Fig. 44. The attempt was made at first to bring a variable horizontal line *ab* into apparent equality with the interval *cd*. The results showed that the interval was *overestimated* in this

[1] Loc. cit., p. 257.

position. But it is difficult to compare a horizontal with an oblique line, and, without regard to that, the more proper comparison should be between the interval and an oblique line, $a'b'$, parallel to the intercepted line. The result under these conditions was even more marked. *The oblique interval was overestimated.* The accompanying table contains the records.

TABLE VI. (*a*).

JUDGMENT OF THE OBLIQUE INTERVAL.

Subject.	No. of Deter.	Excess of Line $a'b'$ Over Interval cd, when in Apparent Equality with It.	M. V.
P.	10	6.63	0.42
T.	10	2.30	0.66

The line $a'b'$ was made too long. That is, the interval cd, when compared with a straight line, is overestimated. The normal position of the figure then shows a judgment of the interval opposite to that revealed when the intercepted line is horizontal. And this overestimation is exactly what we should expect as the expression in oblique form of the universal tendency to enlarge vertical dimensions. How the different judgments for the two positions are to be harmonized, we shall try to see later.

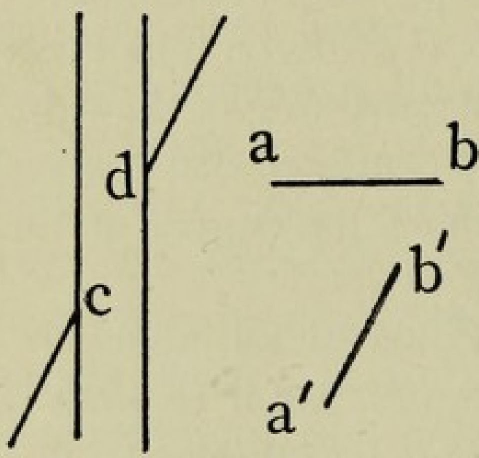


Fig. 44.

Further instructive results were obtained by employing diagrams similar to that of Fig. 44 but with the vertical strips of the forms named *B* and *C* in Fig. 42.

TABLE VI. (*b*).

Subject.	Form.	Excess of Line $a'b'$ Over Interval cd, when in Apparent Equality with It.	No. of Deter.
P.	B.	2.54	10
	C.	3.58	10

The overestimation of the oblique interval is less for *B* and *C* than for *A*. And this fact falls entirely into line with the various preceding results obtained from observer *P*. The width of strip *C* is, by Table III., slightly less than that of strip *A*. This by itself should make the illusion of discontinuity greater in the case of *C*. Table II. shows that this is not the case, and we see now that the reason for this lies, partly at least, in the less overestimation of *C*'s oblique interval. Again Table II. shows that the illusion of discontinuity is least of all in *B*. To this decreased magnitude of illusion not only the apparent width of its vertical strip (Table III.) but also the lessened apparent length of its oblique interval contribute.

4.

Are the acute angles of the figure overestimated?—However strongly investigators have clung to the hypothesis of angular overestimation as causing the Poggendorff illusion, their quantitative studies have invariably proceeded upon the assumption, tacit or otherwise, that this overestimation was not to be the subject of special investigation. That is to say, the assumption of angular enlargement has involved the further assumption that the acute angles on either side of the vertical strip are equally enlarged, and that consequently the two free ends of the oblique are rotated in identical amounts about the points of contact, thus being put into a state of apparent *parallelism*. The restoration of strict apparent continuity is accordingly to be effected by a simple *vertical* movement of one free end of the oblique, this being kept always parallel with itself. It would seem, however, that an unprejudiced approach to the problem should lead one to make provision for any possible *angular* displacement that might be required in the process of establishing apparent continuity. For while the two angles may be enlarged, they may not be enlarged by like amounts. The attempt to rectify this defect of method and thus make room for the discovery of any possible angular, in addition to the vertical, displacement was made by means of a diagram pictured in Fig. 45. The part *B*, bearing the right free end of the oblique, could be given the usual vertical movement, as on the more simple diagram, and at the same time could be rotated in sufficiently large angles about the movable point *a*. The folding over of the left edge of *A* formed a vertical trough in which *B* moved. Though the field of view was, as usual, restricted by tubes, certain edges of *B* could inevitably be seen, and to prevent any influence from them upon the judgment, they were cut in irregular lines and curves. Scales, graduated as before to half millimeters, were placed at the inner and outer edges of *B*. The excess of the reading upon the outer

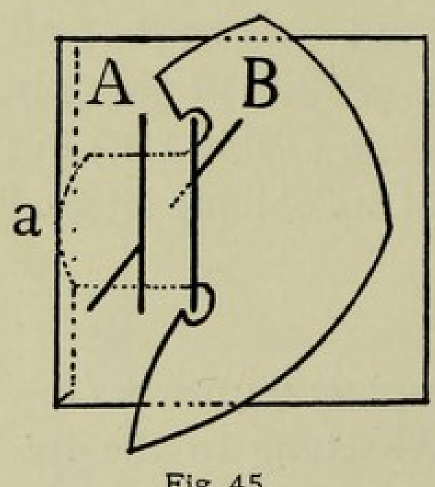


Fig. 45.

over that upon the inner scale gave a number from which the amount of angular displacement could be ascertained. Settings were made alternately from positions of too large and too small. Two series of results were obtained, one in the position indicated in Fig. 45, the other with the diagram turned through 180°. Thus in one case the adjustable line was upon the right, in the other upon the left.

The results of the experimentation could hardly have been anticipated. For it turns out that the angles are indeed misjudged but *in different directions*. One is overestimated while the other is underestimated. If α and β represent respectively the upper and lower acute angles, and the oblique line run down from right to left, α is overestimated and β is underestimated in the case of the two observers P. and T. If, on the other hand, the oblique run down from left to right, this misjudgment is reversed for T. Table VII. gives the direction and amounts of these misjudgments, together with the accompanying amounts of vertical displacement.

How great a uniformity in the direction of misjudgment is expressed by these results may be seen from the fact that, of the fifty settings made by P. in each of the two positions of α, there were respectively 1 and 6 in the opposite direction; in the two positions of β respectively 0 and 2 in the opposite direction. And in no one of these cases was the misjudgment greater than 0.5°.

The making of α too small (part *A* of Table VII.) is of course the expression of its overestimation. At the same time it is probably the expression of a concomitant underestimation of β. At least β is *relatively* underestimated. Whether it is so absolutely is hard to say. In the same way the making of β too large expresses its own relative underestimation and the overestimation of α. There can hardly be a doubt that these two opposite misjudgments occur. They may even be noticed when the simple vertical adjustment is in question in connec-

TABLE VII.

VERTICAL AND ANGULAR ADJUSTMENTS COMBINED.

A. Oblique Running from Right to Left.

α (upper angle) Adjustable.				β (lower angle) Adjustable.		
Subj.	No. of Deter.	α set.	Ver. Error.	No. of Deter.	β set.	Ver. Error.
P.	50	too small by 1°	8.84	50	too large by 2.25°	8.86
T.	10	“ “ “ 0.5°	8.27	10	“ “ “ 0.7°	5.09

B. Oblique Running from Left to Right.

α Adjustable.				β Adjustable.		
Subj.	No. of Deter.	α set.	Ver. Error.	No. of Deter.	β set.	Ver. Error.
P.	50	too small by 0.5°	7.43	50	too large by 0.8°	8.57
T.	10	“ large “ 1°	5.83	10	“ small “ 0.5°	6.18

tion with the diagram of Fig. 41. As the lines are being brought into apparent continuity a careful observer may see that the upper acute angle seems too large, the lower too small.

As already remarked, and as part B of Table VII. shows, observer *T* reversed the direction of this misjudgment when the oblique ran from left to right. It was then the lower angle which was relatively overestimated. But the point to be insisted upon is not so much that any particular angle is misjudged in a particular way as that one of the two acute angles involved is overestimated relatively to the other.

The effect of this misjudgment of the angles in adding its increment to the illusion is seen in the fact that when this may be annulled by setting, the vertical expression of the illusion is diminished. A comparison of the vertical errors in Table VII. with similar errors for Figure *A* of Table II. shows a difference of from 1 to 3 units. And yet the general conditions in the two cases were identical. These angular misjudgments are small, and the part that they play in the production of the illusion is certainly a subordinate one. Nevertheless they must contribute their share to the total result. But they can in no case be looked upon as solely responsible for the illusion, after the manner of the well-known theory that would make them so.

C.

The questions with which we started the experimental inquiry have now been mere or less thoroughly answered. The material gained from this inquiry proves more conclusively than ever that the Poggendorff illusion is the product of the complex interaction of multifarious influences. The degree of the illusion in any position of the figure must therefore be the balance of the various coöperating and conflicting tendencies. Indeed it is the position of the figure which largely determines what influence shall be uppermost at a given moment, provided of course that the constituent parts of the figure have not been changed meanwhile. If one attempt to assign to these various influences, favorable or antagonistic to the illusion, their positions of relative importance, the first place must unquestionably be given to the overestimation of the vertical dimensions of the figure. This tendency is clearly revealed in the overestimation of the empty interval between the inner ends of the intercepted line when the figure is in the upright position. The diminution of the general illusion when the parallels are horizontal, and the *under*-estimation of the empty interval when the intercepted line is horizontal, combine to show how powerful this factor in question is. In the upright position of the figure it is doubtless interacting directly with at least two other factors. One of these may well be the tendency, signalized by Professor Judd, to overestimate linear extents that form the sides of obtuse angles and underestimate those that form the sides of acute angles. In the present case this would amount to a shifting of the inner ends of the oblique in a direction that would be favorable to the illusion. So far as I am aware no evidence is at hand to show that this factor is really operative over and above the usual tendency to enlarge vertical extents. If it is operative, it simply acts in harmony with the latter. Perhaps, when the parallels are horizontal, it is influential alone. The other factor referred to above is antagonistic to the illusion. The underestimation of the empty interval in the horizontal position of the intercepted line is very properly explained by Judd as due to the presence in the surrounding lines of appropriate Müller-Lyer motives. Now these same motives are still present in the upright position of the figure. Only their influence is then overshadowed by the more powerful tendency connected with the vertical dimensions. But it seems most plausible to suppose that this overestimation of the empty interval in this position is the balance between these conflicting factors.

As secondary, but none the less real, factors in the complex of interacting influences, we must name the misjudgment of the width of the strip formed by the parallels, and the overestimation of one of the acute angles relatively to the other. The former is favorable when the judgment is one of underestimation. But, as Tables IV. and V. show, there may be a judgment in the opposite direction. This factor must then be unfavorable to the illusion. As to the angle factor, it must, it seems to me, be admitted to a place in coöperation with the rest. It may indeed be visibly absent, as in Fig. 40 (p. 255), when antagonistic influences are too great for it; but it may also be visibly present, as in the figure given by Helmholtz[1] and modified by Wundt,[2] where the intercepted line is very long and the intercepting strip exceeding narrow. In this latter case the angular distortions at the points of contact are clearly manifest. Still, as already noted, this angle factor is relatively subordinate and certainly has far less influence in causing the primary illusion than any of the advocates of the angle theory would be generally disposed to admit. To the influence of angular misjudgments is presumably due the residual illusion present when the intercepted line is vertical or horizontal.

A final coöperating factor, about which I think all are agreed, is the length of the free ends of the oblique. An increase in the length of these diminishes the degree

[1] Lot. cit., p. 707.

[2] Die geometrisch-optischen Täuschungen, p. 123.

of the illusion. Hence in experimental investigations it is important that the lengths of these free ends be kept constant.

In some such way as this, it seems to me, by some such appeal to the interaction of various concomitant influences, is the Poggendorff illusion to be explained. To go behind these several influences and investigate their fundamental causes is to search for the explanations of the elementary geometrical optical illusions. This is another matter than that here in hand.

A NEW EXPLANATION FOR THE ILLUSORY MOVEMENTS SEEN BY HELMHOLTZ ON THE ZÖLLNER DIAGRAM.[1]

EVERY one who has given any considerable attention to the Zöllner illusion is familiar with the strange gliding movements of the vertical columns which, under special

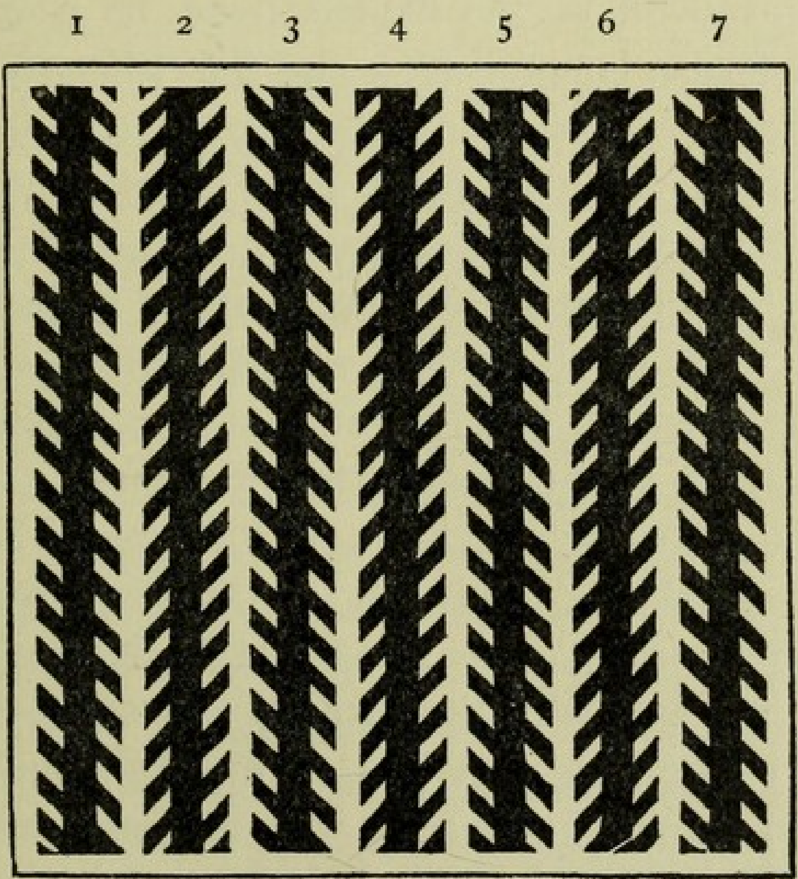


Fig. 46.

circumstances, are to be seen on the heavy-line pattern here figured. (Fig. 46.) Helmholtz was the first to de-

[1] Reprinted from *The Psychological Review*, Vol. VII., No. 4, July, 1900.

scribe these movements.[1] According to him, they are to be seen where one fixates the point of a needle moved horizontally across the diagram. If one be sufficiently practiced in following steadily a moving point a most striking and unusual phenomenon is presented. The vertical columns are seen to shift their position with a graceful gliding movement in the direction of their length. Those verticals that bear upward-running transversals glide in one direction, while those with downward-running transversals pass as rapidly in the opposite direction. If the movement of the fixated point be from left to right, the former columns (2, 4 and 6 of Fig. 46) dart upwards, the latter downwards, just the reverse being the case when the moving point goes from right to left. Moving the fixated point alternately back and forth, and taking care to maintain an appropriately moderate rate of speed, there results a state of "strange unrest" over the whole diagram, totally unlike anything else in the whole realm of geometrical optical illusions. One is reminded most forcibly, perhaps, of what might happen if the illusory creeping motion of the threads of rapidly rotating endless screws were presented to the eye in seven parallel lines and with alternate differences of direction. Not every one gets this illusion directly, but a brief practice in moving fixation, as well as some attention to the most favorable rate of movement and distance of diagram from the eye, will reveal it in all its vividness.

Now as to the existence and general nature of this illusion there can be no question. The apparent slippings and slidings of the columns are incontestable. It

[1] *Physiologische Optik* (2), 712.

is only as to their *explanation* that there may be disagreement and discussion. It is accordingly the purpose of this paper to offer an entirely new explanation for these phenomena, an explanation which is based upon a consideration of the peculiar manner of stimulation experienced by the retina as the fixated eye passes over the diagram. That is, the explanation here given will be couched wholly in retinal terms, and not in terms of any elementary geometrical illusions which arise under the given conditions and which themselves cause the phenomena in question.

But let us see first what explanations are in the field. I have been able to find three: (*a*) Helmholtz's own is as follows:[1] The path of the moving eye is along a line which makes an acute angle with the oblique transversals which it crosses; or, more generally stated, every sensitive point of the retina is moving in a direction oblique to the actual obliques. There results a multitude of ideal acute angles, or "direction-differences," and since acute angles are always enlarged for perception, or what amounts to the same, since direction-differences are always magnified by contrast, the numerous transversals must appear to swing in such a way as to express this illusory angular enlargement. The upward-running transversals will consequently swing upwards, the downward-running downwards, and since any given system of transversals will move in concert, this illusory movement must be communicated to the vertical strips themselves. This is the substance of Helmholtz's explanation. It rests, plainly, upon a supposed angular illusion, the production

[1] Loc. cit., 712.

of which draws the further illusion in its wake. Some of the minuter considerations, which in Helmholtz's view go to support this explanation, must be carefully examined later.

(*b*) Thiéry[1] claims to find the cause of these movements in the equivocal character of the prism surfaces upon which, to many observers, the vertical strips of the Zöllner pattern seem to be projected. To those gifted with the power to see perspective effects in linear drawings, these prism surfaces appear to tilt alternately backwards and forwards, and thus on the perspective theory of optical illusion, the ordinary Zöllner illusion is produced. In the usual forms of the figure the intersections of these prism surfaces must of course be imagined lines lying in the *white* verticals between the black columns. Now these edges of intersection being imagined, as the eye moves horizontally over the figure, each successively fixated edge, Thiéry says, steps into the foreground —as fixated points or lines of equivocal figures always do —and thus each imagined intersection becomes in turn the front edge of a prism. By an immediate consequence the adjacent intersection on either side must retreat into the background, only to step forward once more when the moving fixation-point has reached the next intersection. It is this ceaseless movement backwards and forwards, consequent upon the movement of the eyes and due to the equivocal nature of the figure, that to Thiéry's mind is responsible for the 'strange unrest.'

Filehne[2] seems to entertain an identical opinion of the

[1] *Phil. Studien*, 1895, XI., 320–321.

[2] *Zeitsch. f. Psychol.*, etc., 1898, XVII., 47–48.

matter, though the expression of his opinion is nowhere explicit.

(*c*) Judd[1] adopts still another point of view by attempting to apply the observation of Müller-Lyer that if an acute and an obtuse angle have equal legs, those of the obtuse angle seem longer. Now, he says, the transversals of the Zöllner pattern make both acute and obtuse angles with the intersected verticals, and the horizontal movement of the eye, allowing, as it does, the successive fixation of the various points of the figure, permits the successive false estimations of the sides of these angles to come into prominence. Of course the mal-estimations that are effective for this illusion are those connected with the legs formed by the verticals themselves. And the particular slope of the transversals of a given column will determine the direction, up or down, in which this column will appear to move.

It will be seen that Helmholtz alone attributes any influence to the imaginary line drawn over the figure by the moving eye. The other two writers look upon the eye's movement merely as necessary to bring out characteristics latent in the figure, the conditions being thereby supplied for the arising of the further illusion of the gliding columns.

It is really remarkable that this curious illusion of motion has received so little serious attention. Either because this is less easy to see than the usual illusion that has engaged the almost exclusive attention of observers, or because the weight of Helmholtz's authority has tended to confine explanatory attempts within a particular

[1] *Psychological Review*, 1899, VI., 260.

realm of spatial phenomena, the true cause of this illusion, simple and near at hand as it is, has been persistently overlooked.

An Explanation in Terms of Peculiarities of Retinal Stimulation.

Suspicion against the current explanations may be readily aroused by noticing that the illusory movements are only faintly perceived upon the *light-line* models of the Zöllner figure. Attention is always directed to the original heavy-line diagram, when this matter is under discussion. This would seem at once to indicate that purely retinal influences are powerfully operative in determining at least the vividness of the illusion.

My own suspicions were first awakened by accidentally noticing what happened when, with the diagram in full view, the convergence of the eyes was unthinkingly relaxed. As the eyes diverged the illusory movements began. They occurred again while the eyes were returning to converge upon the diagram. And if, when the optical axes were nearly parallel, there were slight movements to a state of greater or less divergence, the columns exhibited on their part their appropriate shiftings. In themselves, of course, these observations contributed nothing decisive to the problem in hand, but they served to arouse a line of investigation which has not been altogether without positive results.

It may be said here that for ease of observation a copy of the Zöllner pattern, like that of Fig. 46, should be pasted upon a small piece of cardboard of convenient size for handling. With this device one may demonstrate the Helmholtz phenomenon much more readily than by the usual method. Holding the columns in any desired position—vertical, horizontal or at any angle—it is only necessary to shake the diagram slightly back and forth in its own plane and in a direction perpendicular to that of the columns to produce very vivid effects. The "shaking" of the diagram is essentially the same as moving the eyes over it, for the eyes cannot readily follow its motions and consequently the image of the diagram moves over the retina, the result being the same for perception as if the retina moved over the diagram. The relations between objective and illusory movements in respect to their directions are the same with this as with the usual method of producing the illusion. The slippings and slidings of the columns to be seen under these circumstances strongly suggest that we are in the presence of phenomena analogous to those of the "fluttering hearts." There is a similar jelly-like movement here, but instead of a dim illumination the full daylight is requisite for the best effects. That the observation of the illusion is possible by the method just described would seem at least to show that Thiéry's explanation in terms of the equivocal perception of prism-faces cannot find any reasonable application under the conditions here in force. No trace of such equivocal swingings of the prism surfaces is to be discovered.

Further, it becomes hard to reconcile the theory of Helmholtz with the observation that *the columns move in such a way that the obliques are everywhere parallel to their original direction.* That is, if the apparent movement of the columns is due to a principle of direction-contrast working upon the oblique transversals, or, in other words, if this movement is due to the illusory enlargement of acute angles successively formed by ideal lines passed over by the moving eye, there should be some vestige of a *rotary* motion observable on the obliques. For angular increase must take place about a vertex as center of rotation. And if such angular increase is the underlying cause of the phenomenon under consideration, it surely must be possible to perceive some *twist* in the transversals, some slight departure from their original direction. But as a matter of fact no such thing is to be seen. The columns move smoothly and evenly upwards and downwards, the obliques never changing in the slightest the course of their original slope. They rotate neither about the point of intersection with the verticals, nor about either of their ends. They move rather as if impelled by some push given to the verticals, to which they seem rigidly attached. If correct, this observation most certainly discredits Helmholtz's explanation.

And, finally, Judd's explanation in terms of mal-estimations of sides of angles is just as little able to maintain its claims. *For the verticals are not at all necessary for the illusory movements.* By cutting suitable strips from striped cloth or from properly ruled paper, the Zöllner pattern may be reproduced *without its verticals.* Under these circumstances the Helmholtz illusion persists unchanged in all particulars. But Judd's explanation is no longer applicable, for the sufficient reason that there are now no acute or obtuse angles whose sides are to be successively mal-estimated as the eye moves over the diagram. The main lines which supplied the indispensable condition for this explanation are no longer present, and it seems hardly reasonable to suppose that imagined verticals may take the place of the actual lines that have been removed.[1]

It being now evident that no one of the current explanations can square with the simplest facts of the case, let us attempt the statement of the purely retinal theory promised above. Consider, for convenience, a single column of upward-running obliques without the central vertical (*A*, Fig. 47), and let the eyes be supposed to follow a point moving horizontally from left to right. That the illusion persists with only a *single* column may be easily ascertained by carrying out the usual movements of the eyes after all but one of the columns of the regular pattern have been covered. As to the position of the fixated point, experience shows that the clearest perceptions emerge when it moves along a line situated at the side of the column, along the line *ab*, that is, in Fig. 47. For my own eyes the most satisfactory results are obtained when this line is at the *right*. Others may find that the left side is preferable. In any case indirect vision seems best adapted to the perception of the illusion.

[1] The observation of Witasek (*Zeitsch. f. Psychol.*, etc., 1898, XIX., 153) to the effect that he observed no movement of the columns when the obliques alone were presented to one eye, though such movements were continually being observed on the complete diagram upon any slight movement of the eyes, was probably due among other things to the lightness of the lines he was observing. He was making a study of the usual Zöllner illusion under the conditions of stereoscopic combination—the verticals being presented to one eye and the obliques to the other—and under these circumstances the attention was presumably too fully occupied with other matters to notice any faint illusion. If, however, the eye had been moved intentionally over the part bearing the obliques only, the illusory movements could not have escaped his notice.

What happens under these circumstances, as the eyes follow the fixation-point along *ab*, may best be seen by considering the experience of a single vertical line of retinal elements. Let this line be thought of as resting on the extreme left edge of the column before us. It will then receive along its length alternate excitations

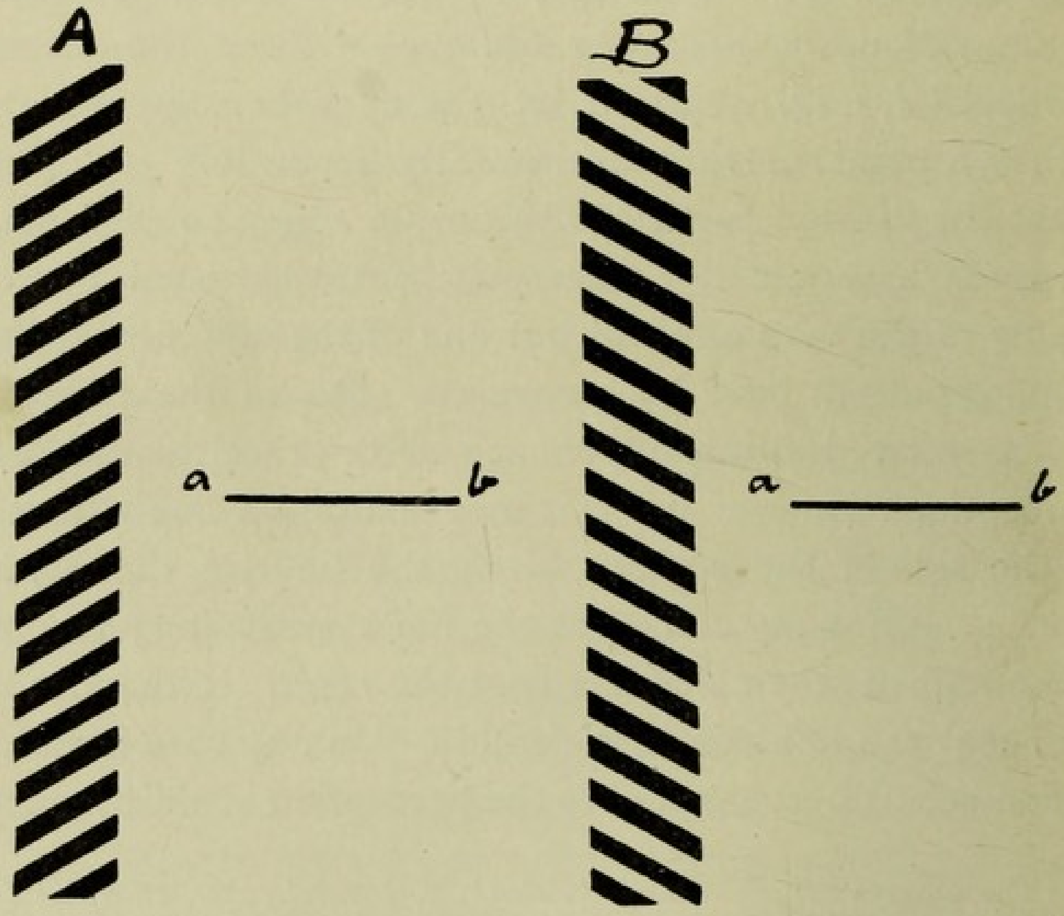


Fig. 47.

of black and white. Now let the eyes move. As the supposed line of retinal elements passes from the left to the right edge of the column, the alternate excitations of black and white will fall higher and higher upon the line. The practical result for perception is therefore exactly as if numerous stimuli had *moved* up over a resting retina. This line of elements has experienced the full conditions for the perception of motion. But what happens to the single line happens in like manner to all vertical lines of elements that pass over the column of obliques, the upward-creeping excitations on each succeeding line being in each case lower down than those on the line ahead, as is of course determined by the slope of the stimulus-giving obliques. Manifestly the visual effect of this is entirely similar to the moving of the column bodily upwards over the resting eye.

If now the eyes move from right to left, the vertically arranged spots of stimuli, as we have pictured them, will travel downwards, and the visual appearance will be that of a descending column of obliques.

The directions of these vertical movements, it will be noticed, are in exact accord with the observations of Helmholtz. Columns bearing upward-running transversals run upwards when the eyes move to the right, downwards when the eyes move to the left.

Exactly the same style of consideration is to be applied to the columns of form *B* (Fig. 47). With a movement of the eyes to the right successive stimulations will fall upon lower and lower points of the retina and the column as a whole will appear to move downwards, just the reverse being the case when the eyes move to the left. This again is wholly in accord with Helmholtz's observations.

The conclusion of the whole matter then is simply that the illusion under discussion is caused by the peculiar manner in which stimulations travel upon the retina. The horizontal movement of the eyes across lines lying oblique to their direction is equivalent in retinal terms to an ascending movement of the entire set of obliques over a resting retina. For perception the two processes have identical effects. Hence the illusion.

The general correctness of the view just expounded may be vividly brought out by the following procedure. Taking any piece of cloth or paper with closely lying stripes and placing upon this a piece of cardboard in such a way that the stripes run upwards across the vertical edge, move the cardboard in a direction perpendicular to its edge, the eyes meanwhile following some point on the latter. The effect will be that of an apparent ascending of the portions of the cloth successively uncovered by the moving cardboard. What occurs here is essentially the same as in the typical cases. Stimulations are mounting higher and higher along the vertical lines of the retina, and hence all the usual conditions for the perception of upward motion are fulfilled.

If the above explanation is correct, several important corollaries follow immediately from it:

1. The *rate* of the illusory movement must depend upon the rate of the horizontal movement of the eyes. Helmholtz does not fail to notice this fact,[1] though he simply mentions it without discussion. It may be readily verified on the regular 'pattern' or upon isolated columns of obliques. Now on the theory of moving retinal stimulations this coincidence of rates *must* occur. The more rapid the horizontal movement of the eyes, the more rapidly will any bit of stimulus traverse its path from end to end of any oblique, and consequently the more illusory speed will the vertical column seem to possess.

[1] Loc. cit., p. 713.

2. The *excursion* of the illusory movement must depend directly upon the *slope* of the oblique. This again is evidently a direct consequence of our explanation, for

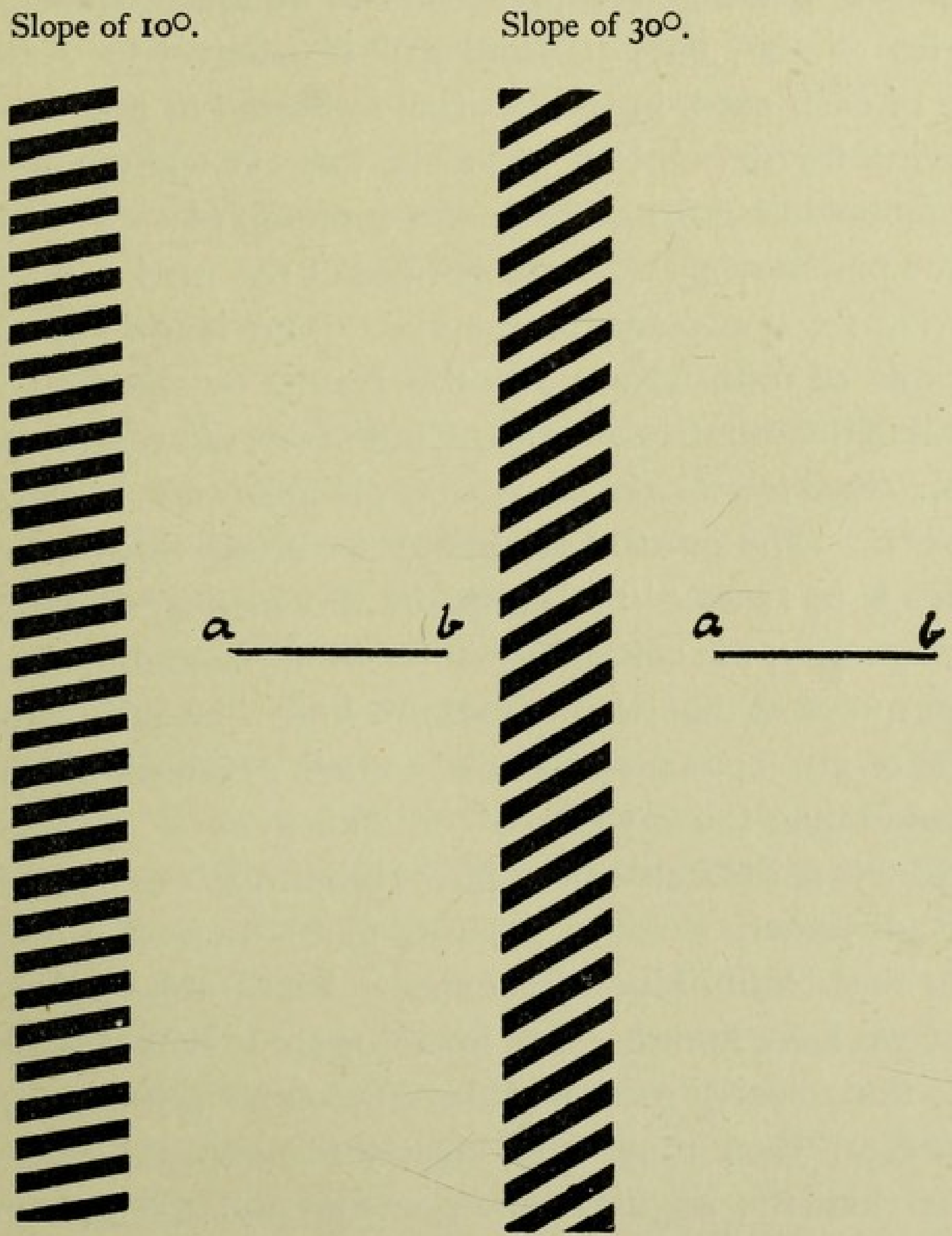


Fig. 48.

the vertical distance from tip to tip of each oblique increases with the slope of the latter, and accordingly the amount of vertical movement of the various stimulations along any retinal line, such as we have supposed above, must vary directly with the tilt of the oblique. To subject this deduction to the experimental test, cards were prepared bearing columns whose obliques sloped at angles of 10°, 20°, 30° and 40° respectively. At the side of each card, in the position indicated in the accompanying figure (*ab*, Fig. 48) a slit was cut which allowed the introduction from above of a common pin. The head of the pin, moving upon the surface of the card, served as moving fixation-point for the eye. The length of each slit was 18 mm. Naturally the results obtained do not admit of quantitative statement, but *the anticipated increase of the illusion with the increase of the slope was everywhere realized.* The most striking way to bring this into evidence is to 'shake' the cards, in the manner described on page 285, the columns being held horizontally. There is no question that the columns whose obliques have a slope of 40° appear to traverse a much greater horizontal distance than those with 10° obliques.

Now it is difficult to see how Helmholtz's explanation can adequately account for this fact. Indeed it would seem that Helmholtz must expect exactly the reverse of what we have found. For in accordance with his principle that clearly recognizable differences are estimated as greater than those less clearly recognized,[1] it would seem that the angular enlargement due to direction-contrast would be greater for a slope of 10° than for one of 40°. For in the former case the difference of direction between the obliques and the imaginary paths traced by the eye must be clearer than in the latter case. Or,

[1] Loc. cit., pp. 705 and 714.

if not clearer, at least of a similar grade of clearness, which should cause the illusion to be equally great in the two cases. But it is unnecessary to attempt to strain Helmholtz's explanation into any form which shall pretend to cope with these facts in regard to the amount of the illusory displacement of the columns. These facts fall so readily into line with the retinal explanation proposed that they form one of its strongest confirmations, just as their absence would completely overthrow the theory here defended.

(*c*) Again, on the view here presented, the illusory movements should progress smoothly, without the slightest change of inclination on the part of the obliques. We have already emphasized the fact that this is so. And, in addition to the above, any slight movement of the eyes along the line of fixation should give a correspondingly slight illusion. And movements of the eyes by *stages*, that is with momentary stops between short movements, should be accompanied by an exactly parallel behavior of the columns. Both of these deductions are unmistakably verified. Especially may one see what is meant here by substituting for the column of Fig. 48 a rather large patch of oblique lines. The movement of the eyes by tiny stages makes the patch appear to move stealthily up and down, creeping slightly along, then resting, then creeping farther, etc. The whole impression during such an observation as this is that of some set of visual stimulations making their way gradually up and down the retina. Of the rotation of the obliques, which if it existed should assuredly become evident with these relatively long lines, not a trace is to be observed.

There is the same smooth upward march that is to be seen in the case of the usual columns.

Another way of demonstrating this same thing is by diverging and converging the eyes upon the diagram as described above. This experiment may be varied by taking a single column from the 'pattern.' As the axes of the eyes diverge towards parallelism two columns are of course seen moving in opposite directions. But what interests us here is that every chance alteration in the degree of divergence produces a corresponding movement of the columns; slow, if the change in the eyes is slow, and of short excursion, if the change of divergence is slight. In short, when the eyes move there are immediate response and complete correspondence on the part of the moving column.

Now these facts can hardly be made harmonious with the thought of direction-contrasts that develop as the eyes move. Surely some length of movement must be requisite for the development of the consciousness of such direction-contrasts, and one should expect on this hypothesis, not the smooth movement that we have observed, but rather the progression of the columns by jerks, as the ideal horizontal line became long enough to evoke a feeling of difference in direction. Such jerky movements might not reveal themselves under the usually observed conditions of continuous eye-movement, but it would seem plausible at least to expect them under the special conditions here in force, were Helmholtz's explanation correct. The entire absence of such unsteady motion in the illusion, and the total impossibility of discovering the faintest trace of rotation on the part of the obliques, combine to form a strong bit of evidence against the explanation here combated.

(*d*) That columns of upward-running obliques should run upwards, and that columns of downward-running obliques should run downwards, is an inevitable deduction from our premises.

Now Helmholtz records this fact with perfect clearness, but for some strange reason he does not attempt to show why either sort of column should have *its peculiar direction* of movement. And indeed on his theory this would be an awkward task. For instance, consider a single upward-running oblique. If the imaginary path, which is to establish the direction-contrast, be supposed to cut the oblique at its point of intersection with the vertical, the oblique must execute opposite movements with its two ends. With the eye passing to the right, the left end of the oblique must move down, the right end up. Which of these two movements is to be decisive for the final movement of the column? The one last seen? But why should not the movement first seen be just as effective, and consequently why should not the opposite sides of each column move in opposite directions? But there is a deeper difficulty. We have no basis for supposing that the imaginary path intersects the oblique at any particular point. All the sensitive points on one side of the retina are sweeping over the oblique and are cutting it at all conceivable places. Therefore, if for convenience we choose to mention a special point of intersection, we have no right to select one that is favorable to the result desired in preference to one that is absolutely unfavorable. For example, in our supposed case, we are in no way warranted in placing the point of intersection at the left end of the oblique rather than at the unfavorable right end. Under the former circumstances the angle would enlarge upwards, in the latter case downwards. In the one case we should have found an apparent explanation ; in the other we should have met with a blank contradiction. The truth is that Helmholtz's theory is simply inapplicable. And may it not be that his silence in reference to the cause of particular directions of the illusory motions is not without important significance?

Though the evidence now adduced in favor of the purely retinal hypothesis may be regarded as sufficiently conclusive, one or two further points may give added weight.

The illusion does not always appear instantaneously when the eyes begin to move. A certain degree of retinal fatigue seems requisite for the full intensity of the phenomenon. This may be most effectively obtained by a moment's steady fixation of the point before beginning the movement of the eyes. This secures a slight after-image and provides a retinal point of reference for the ensuing movements of the various stimulations, and the illusion attains its most lively form.

At the same time a good illumination is a prime necessity. In a dim light the movement of the fixated point must be much slower than in a strong light, and the illusory movements are thereby greatly reduced in vivacity. Or if the movement of the eyes be the same for the two conditions of illumination, the illusion will almost completely vanish with a degree of illumination that is still sufficient for the clear perception and distinct discrimination of the lines when the eyes are at rest. Thus we find that the most advantageous conditions for the illusion itself are precisely those which are conducive to the most marked sensational effects.

A further point of interest is the heightening effect of *contrasting* illusory movements simultaneously present. In the Zöllner pattern these contrasts are already provided for, since alternate lines are moving in opposite directions. An isolated column, however, whether taken from the 'pattern' or constructed without the central vertical, gives an illusion of greatly diminished liveliness. Consequently in the experiments above described it is much more satisfactory to have a column of oppositely sloping obliques—most conveniently taken from the 'pattern'—arranged alongside of the particular column under investigation.

A peculiarity about these contrasting effects is that in the majority of cases only one column seems to move. This one column absorbs, as it were, and appropriates to itself whatever motion the other may have in the opposite direction. In my own case it is the nearer column which appears to stand still. This seems to discharge its functions by acting as a reference column. As such, however, it undoubtedly contributes strikingly both to the clearness and to the apparent excursion of the illusory motion of its companion. This effect of contrast is prettily demonstrated by taking any pair of adjacent columns from the 'pattern' and then comparing the vividness of the illusion on that column which exhibits movement with the relatively feeble illusion resulting when the 'quiet' member of the pair is replaced by a column of short horizontal lines. The latter column might indeed be supposed to furnish a basis of reference, but its lack of illusory movement makes it practically without effect upon its companion.

It is, presumably, this principle of contrast which must be appealed to in explanation of certain minor phenomena to be observed when the complete diagram is 'shaken' before the eyes. Holding this in the horizontal position and shaking with a quick, short motion of the hand, all columns fall into their appropriate movements. But if the shaking movement be somewhat longer and less rapid, only columns 2, 4 and 6[1] will present the illusion. Now and then, possibly, the other columns will momentarily take no motion, but in general they seem to play the reinforcing rôle. That columns 2, 4 and 6 should display the illusion seems to depend upon the fact that they are most advantageously situated within the set of columns for receiving the full influence of contrast. That it does not depend upon the kind of the column, that is, upon the sort of slope that the obliques of 2, 4 and 6 happen to have, is shown by the fact that if a diagram be constructed by replacing each column by its opposite, the illusion will still be confined—at least most vividly—to the columns occupying the same positions as 2, 4 and 6.

It remains now to examine those widely accepted elementary phenomena upon which Helmholtz principally based his explanation. I refer to those oft-quoted cases,

[1] See Fig. 46.

which the accompanying figure, Fig. 49, will call to mind immediately, where a direction-contrast, developing under the very eyes, graphically performs its work of producing the overestimation of the acute angle.

Helmholtz's claim was, as every one knows, that, if the end of one leg of a pair of compasses be followed by the eye as it moves over the path *CD*, across the line *AB*, the two halves of the line will appear respectively to assume the positions indicated by the lines *aa'* and *bb'*. That is, the path of the compass-point has made acute angles with the intersected line, these have been perceptibly enlarged, and the left and right halves of

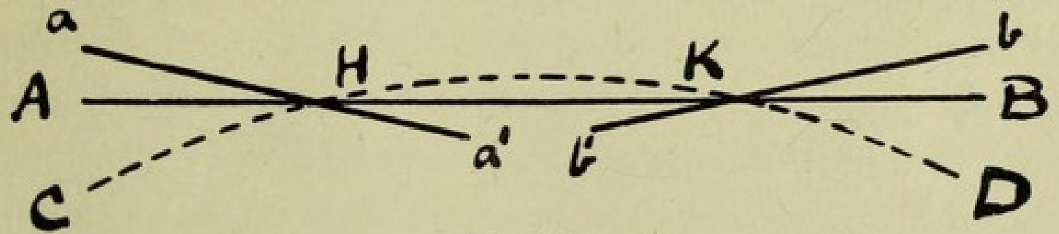


Fig. 49.

AB have been seemingly forced out of position in the process of accommodating themselves to this unavoidable and insurmountable illusion of space perception. At first sight the phenomenon appears to be the isolated element of the illusory movements that we have been discussing above. The path of the compass-point as it cuts the line is the representative of the many paths swept out by the retinal elements as they intersect the obliques of the Zöllner pattern. And as either half of *AB* in Fig. 49 swings about the point of intersection, so the intersected obliques of the 'pattern' swing about and give the appearance of vertical motion to the columns. We have already seen plenty of reasons for rejecting this interpretation as applied to the larger phenomenon. And if now we are able to find some other interpretation for this supposed elementary illusion, all possible grounds for clinging to the explanation of Helmholtz will have been taken away. That an illusion of direction is to be seen when the conditions of Fig. 49 are fulfilled is unmistakable. But a really complete description of what happens seems never to have been given. For the more convenient study of the matter a heavy line of 4 mm. thickness and 50 mm. length was drawn upon paper and the latter fastened to the wall directly in front of the observer. A slender rod 475 mm. long and tipped with a steel knitting needle swung from a center below in such

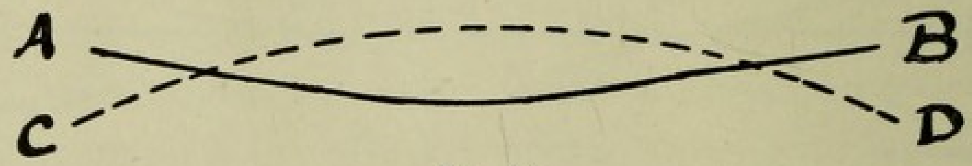


Fig. 50.

a way that the tip of the needle traced an arc 22 mm. above the line at the highest point. This arrangement reproduces all the essentials of the original, while allowing greater facility in the way of control and observation.

What happens to the line *AB* as the point is carefully followed in its course is certainly not adequately represented by the usual oblique straight lines of Fig. 49. There is rather an apparent *sagging* of the line at the center, somewhat as represented in Fig. 50. And furthermore this whole appearance of distortion may be produced by confining the moving point to the portion of the arc that lies between *H* and *K* (Fig. 49). That is, the actual intersection of the line by the path of the moving point is not at all essential to the illusion. This in itself is sufficient to cast an interesting suspicion on the hypothesis of direction-contrasts. For here we are obviously dealing with the path of the fixation-point, and not with ideal lines traced by any point whatever of the retina. A careful examination of the behavior of the line during the movement of the eye from H to K will, I think, reveal the following: If the moving point stop short of the crest of the curve, the line AB seems simply to move downwards, keeping always parallel to its original position. This is a necessary consequence of the eye's movement obliquely upwards, for the perceptual effect, or at least the retinal effect, is precisely what it would be were the line to move downwards and to the left. The sidewise component of the movement is not noticed unless the line has some conspicuous marks upon it. Again, if the moving point pass from a position just beyond the crest of the arc down across the line, the latter will seem simply to move upwards. In neither of these cases is any departure of the line from its original direction perceivable. But if now the moving point be followed throughout its entire course, or even if it be confined to the position HK, the sagging of the line appears. But this appearance is manifestly only a necessary perceptual interpretation of three contradictory perceptions that come about successively as the point moves. First, while the eye is approaching the summit of the arc the line moves down, then as the summit is being passed it ceases altogether to move, and finally as the eye sweeps downwards the line moves upwards. That is, during the passage of the eye over the length of the curve, the line has presented three successive phases—upward movement, no movement, downward movement. Now bearing in mind that the attention is most sharply given to that portion of the line that is at any moment just below the fixation-point, and considering further that the final perception of the whole line must be made up of these three partial perceptions which follow each other so rapidly as to fuse into a single resultant, the illusory sagging line seems the only possible spatial form that could be perceived. The illusory sagging appears just because three incongruous perceptions are forced into one by the fact that the line *AB* must be perceived as a continuum, and the three modes of behavior of its three different parts must be apperceived as occurring upon an unbroken, continuous line.

There is no need of appealing to the influence of direction-contrasts here. We are merely in the presence of one of the so-called "illusions of interpretation." And as thus viewed this phenomenon has not the slightest connection with those other illusory movements which form the subject of this paper. Different causes are operative in the two cases, though both are reducible to relatively simple retinal happenings and need no underlying spatial peculiarities for their full and satisfactory explanation.

Helmholtz, it will be remembered, claimed to find evidence for the working of a direction-contrast when the path of a moving point is along a *straight* line—whether actual or ideal is indifferent—cutting a horizontal line at a small angle. The use of the compass-point arc in place of the straight-line path was, according to Helmholtz, simply to secure more vivid results. But I cannot refrain from thinking that Helmholtz's zeal to obtain a preconceived result led him here to fall into error. And I cannot find that others have verified the alleged observation that the main line seems to incline away from the path of the moving fixation-point as the latter cuts across it. I have carefully examined the matter, using the same heavy line as above and letting the imaginary oblique be traced by the tip of a stiff wire fastened below to a tiny car, sliding upon an inclined plane. The moving tip was of course close to the plane of the paper bearing the line, and the angle of intersection was 5°. Now with an excursion of the moving point of about 10 cm. on each side of the line, nothing whatever beyond the usual and inevitable up-and-down movements of the line was to be seen. There was not the faintest vestige of tilting of the line. Nevertheless such tiltings were occasionally seen, yet so inconspicuous as to be easily overlooked, when the imaginary line was made as long as the horizontal itself. But this, once more, is only an illusion of perceptual interpretation. As said above in discussing the companion illusion, it is the portion of the line just above (or just below as the case may be) the moving fixation-point which seems most of all to be in movement. Accordingly when the eye, passing say from left to right, arrives at the end of its path, it is the extreme right end of the horizontal that is at that instant in most vivid apparent movement downwards. The rest of the line is still visible, of course, but it is relatively motionless. What other possible result then for perception than that the horizontal seems to be assuming a slight inclination downwards. The correctness of this view of the case is made overwhelmingly probable, it seems to me, by the fact that the illusory inclination of the line does not appear at about the time the line is being intersected, as we should expect on the theory of direction-contrast, but comes rather at the end of the whole path. Any one who will carefully repeat the experiment will be convinced, I feel sure, that the illusion here is due to the manner of interpreting motions on the retina and not to the overestimation of angular magnitudes.

It may seem that these elementary phenomena have been dwelt upon at needless length. But I cannot help thinking that a real error has been firmly attached to them ever since Helmholtz first made his communications in respect to them. And since they have been so often referred to and so often made the explanatory basis for other illusions which have no actual relationship with them, no amount of pains is too great which shall contribute to a more accurate understanding of their provoking cause.

Summary and Conclusion.

We have now the various facts and arguments before us. In conclusion these may be gathered together for convenient inspection. To begin with, we saw that no one of the current explanations for the movements to be seen on the Zöllner pattern could stand aright before certain easily verified facts connected with the illusion. That of Thiéry, in terms of equivocally perceivable prism-edges, could not meet the case where the illusion was produced by simply 'shaking' the diagram before the eyes. Grave discredit was seen to be cast upon Helmholtz's explanation by the observation that no change of inclination was to be seen on the part of the obliques during the presence of the illusion, though such change of inclination was supposed by Helmholtz to be the underlying cause of the apparent movements. And finally Judd's explanation was rendered inapplicable by the removal of the central verticals, there being thus no sides of angles to be falsely estimated.

The proposed explanation in terms of peculiarities of the movements of stimulations upon the retina was then supported by several considerations: First, that the rates of the actual and illusory movements correspond; second, that the excursion of the illusory movement depends directly upon the degree of the slope of the obliques; third, that the illusory movements, presenting, as they do, a behavior corresponding perfectly with that of the eye in respect to rest, movement and kind of movement, give the impression always of the passing of visual stimulations along the retina; and, lastly, that the particular directions in which the differently constructed columns move are now for the first time adequately accounted for.

And then finally those alleged basal phenomena in connection with single lines were seen to be entirely capable of a new interpretation, which removes them completely from any kinship with the more complex illusion which Helmholtz supposed them to explain.

Not only then have the curious movements of unrest that the Zöllner pattern may be made to show found an explanation that satisfactorily accounts for all the peculiarities connected with them, but in addition the fundamental basis for the original and most widely accepted explanation has been swept away. This new explanation, by being substituted for the old one, will probably have no far-reaching effect upon the interpretation of other geometrical optical illusions. Nevertheless it is decidedly worth while to get each individual illusion set in its true light.

FURTHER ILLUSORY MOVEMENTS IN CONNECTION WITH THE ZÖLLNER DIAGRAM.

A.

SEVERAL years ago Herr Professor Filehne, of Breslau, published a description of certain illusory movements observed by him in connection with the Zöllner diagram of the heavy-line type.[1] These movements are similar in general character to those described by Helmholtz which we have been considering in the previous essay. Nevertheless their completely distinct nature is insisted upon by Herr Filehne. Not only is their manner of production different entirely from that of the Helmholtz movements, but also some of their alleged characteristics are altogether unique.

The manner of obtaining these movements is described by Filehne as follows: Place the diagram[2] in a fixed position so that it may be viewed through a tube. The most favorable position to begin with is that in which the main lines are horizontal. Cover one eye, and do not accommodate for the plane of the diagram. If these preliminaries are properly attended to, it is asserted that a few moments' careful gazing—with unmoved eyes—will reveal the fact that the horizontals, two, three or more at a time, are darting about to the right and left, during a period of from one to three seconds. The motion of any particular horizontal is seen to be always

[1] Filehne, *Zeitsch. f. Psych.*, etc., 1898, XVII., 15.

[2] See p. 279.

in the direction of the overhanging ends of the obliques. Though most easily seen, perhaps, when the lines of the diagram are placed horizontally, these movements are none the less to be perceived in the vertical position. Those lines that before darted to the left now pass upwards.

But now a curious fact. If the figure be so turned that the main lines slope upwards to the left the movement is particularly lively. Those lines that before moved to the left and upwards respectively now move obliquely upwards. *If, however, the main lines slope upwards to the right, no movement is to be seen.* This is due, we are told, to the fact that in the first case the horizontal and vertical movements have reinforced each other; in the second case they are antagonistic and have cancelled each other. But at any rate, whenever motion occurs, it takes place in the direction of the overhanging ends of the obliques. But why any movement at all? The reason for these phenomena Filehne claims to find in the consideration of 'memory-images of motion.' His theory here is supplementary to the general perspective theory of which he is an earnest advocate. Many of our visual experiences are had while we are in motion. In such cases the angle made by any upright object that is perpendicular to the earth changes as we approach it or recede from it. This change is always of such a kind that, as we approach, the angle appears to diminish from an obtuse to a right angle, and as we reach and pass beyond the object the angle seems to increase, the perpendicular appearing, that is, to fall gradually away from the moving observer. But whether the observer be approaching or receding, the apparent movement—which may be regarded as a rotation about the point of contact with the ground—is always opposite to the direction of the observer's progress, and always towards that position where it shall seem to tip away from the observer as he has passed by. Most strikingly, possibly, is this seen in railway travel. The telegraph poles seem to rotate as they fly past, and always in the direction of that position where they shall appear to overhang. Well, countless experiences of this kind have stored up such a mass of memory-images that when, as in the Zöllner diagram, similarly overhanging obliques are viewed, these latent images are brought to the threshold of consciousness and the diagram itself becomes enlivened with an illusory motion, occurring in strict accord with actual objective experience. Such is the explanation offered by Filehne. Psychologically considered, the language used is not wholly free from objection, but the meaning of the theory is on the whole clear enough.

Now it is particularly hard to subscribe to such a theory as that just outlined. And in view of the fact that the 'perspective' explanation of optical illusions has fallen largely into disfavor, it seems advisable to cast about for some other explanation that shall meet the facts. But here at once lies a great difficulty. The observations recorded above are by no means easy to verify. Personally I am unable to obtain the proper movements at all under the experimental conditions demanded. I have tried again and again, and at widely separated intervals of time, but always without success. Nor have I yet succeeded in finding an observer more successful than myself.[1] Now it is of course hazardous to assert, on the basis of a failure to perceive a given illusion, that therefore the illusion is not genuine. And I should by no means presume to make such an assertion. Nevertheless one may be allowed, I think, a certain measure of incredulity in the matter, for it may be asked very pertinently, how any visual impression, of whatever characteristics, should be capable of causing illusory perceptions of movement at a moment when every actual movement of the eyes is excluded. Certainly, if the observations recorded be true, we have something novel in the realm of psychology—a perception of motion, but a motionless object and a motionless eye. And the very novelty of the matter throws it open to suspicion.

In view of these considerations, and particularly in view of the explanation that the previous essay proposed for the Helmholtz movements, I am strongly tempted to believe that the eyes of the successful observers were not entirely motionless. The observer views the diagram monocularly, and it is insisted upon that accommodation be not too exact. Here at once are conditions which render involuntary eye-movements almost unavoidable. Also the effort not to accommodate exactly may easily entail slight movements in the way of closing or opening the eyes. And it can readily be shown that such slight closings or openings of the eyes are accompanied by slight upward and downward movements respectively of the eyeball. Any such involuntary movements of the

[1] Herr Filehne informs me in the course of private correspondence that the per cent. of persons able to perceive the illusory movements is undoubtedly very small.

eyes would, as the reader of the previous essay is aware, induce illusory creepings and dartings of the columns gazed at.

Now I am able to see plenty of these creepings and dartings whenever, for example, I am setting the diagram up for inspection, or when I view it somewhat carelessly through a tube. But the movement of any column may take place *in either direction.* Whereas the Filehne movements are under no circumstances reversible. They must take place invariably in the direction of the overhanging ends of the obliques. For me, also, there are these movements in *both*, and not alone in one, oblique position of the diagram. But I am certain that there are always movements of my eyes when any part of the diagram seems to be in motion.

B.

If a diagram be prepared in the manner suggested on p. 284, a peculiar variety of these illusory movements, entirely similar in form to those of Helmholtz, may be obtained by moving the card back and forth before the eyes. The peculiarity lies first in this, that only two of the columns seem vividly to move. These are the columns numbered 2 and 6 in Fig. 46 on p. 279. Other columns may indeed move occasionally but in no such marked fashion as these two. But, more peculiar still, these columns, though identical in structure, *move in opposite directions.* And this happens whether the diagram be moved while the head is stationary or the head moved while the diagram is stationary, whether the columns be vertical, horizontal or oblique. And whether both eyes be used or only one is entirely indifferent. The most favorable rate of movement can easily be obtained by trial.[1]

There are two facts then to be accounted for; the restriction of the movement to columns 2 and 6, and their movements in opposite directions.

The first fact is undoubtedly due to contrast effects precisely similar to those described on page 298. Columns 2 and 6 are favorably situated for 'absorbing' the adjacent motions which are of a character opposite to their own.

But the movement of these two columns in opposite directions is something entirely new. Yet the necessity of this is evident when one considers what takes place when the diagram is moved rapidly towards or away from the eyes. The appropriate convergings and divergings do not follow the movement of the card with sufficient rapidity to keep the point of fixation upon it. This may be shown clearly by the crossed and uncrossed double images of a dot upon a moving card. Now under these circumstances the sudden increase or decrease of the retinal images, as the case may be, reveals itself in these opposite movements. For as the retinal image expands, columns 2 and 6 move asunder and consequently, on the basis of the explanation for the Helmholtz movements, must have longitudinal motions of opposite character. The same is true for the contraction

[1] In the correspondence referred to in a recent footnote Herr Filehne informs me that he has used these movements of the diagram for the purpose of showing his subjects the *sort* of illusory movement they were to expect under his more restricted conditions.

of the retinal image. And all this holds good in whatever position the columns of the moving diagram may be. That the various illusory movements as observed are actually such as would be demanded by the retinal theory set forth in the previous essay, may be seen from the following table :

a = direction of actual lateral movements of columns.
b = direction of illusory longitudinal movements of columns.
c = analogous Helmholtz movements for same lateral movements of columns.

Position of Columns.		Diagram Approaching.		Diagram Receding.	
		Column 2.	Column 6.	Column 2.	Column 6.
Horizontal	*a*	↑	↓	↓	↑
	b	→	←	←	→
	c	→	←	←	→
Vertical	*a*	→	←	←	→
	b	↓	↑	↑	↓
	c	↓	↑	↑	↓
Oblique ╱	*a*	↖	↘	↘	↖
	b	↗	↙	↙	↗
	c	↗	↙	↙	↗
Oblique ╲	*a*	↗	↙	↙	↗
	b	↘	↖	↖	↘
	c	↘	↖	↖	↘

N. B. For the various positions of the diagram, column 2 is respectively above 6, at the right, at the left, and at the right.

An examination of the table will show that every direction of movement demanded by the retinal theory is exactly paralleled by the observed movements. It seems rather certain therefore that these peculiar opposite movements of columns 2 and 6 are caused by the expandings and shrinkings of the retinal images consequent upon the movements of the diagram. And under these circumstances column 4 does not move as in the case described on page 298, because being placed in the center of the diagram it forms that portion of the retinal image about which the expandings and contractings take place, while it is itself relatively unmoved.

THE ILLUSION OF THE DEFLECTED THREADS.

In the *Psychological Review* for May, 1898, Professor Judd described an optical illusion which, though at first difficult to obtain clearly, turns out to possess peculiar novelty and interest. The peculiarities presented seemed to the above writer to have valuable implications for a correct doctrine of space perception. In the same *Review*, for September, 1900, the present writer attempted a brief criticism of the explanation offered for the illusion in the above-mentioned article ; and in the following number of the *Review* Professor Judd returned to the matter once more and again defended his original explanation.[1] With the single exception just noted, Professor Judd's proposed explanation has, so far as I know, stood unchallenged, for the reason, perhaps, that readers of space literature are more and more well disposed towards the general position that his conclusions represent. But the illusion itself, and not its implications, concerns us here. What this is and how it was explained may be quite briefly recalled. The essentials of the matter as originally presented, are as follows : Two threads, lying in different horizontal planes, are stretched across a box in such a way that their intersection forms an acute angle. The line of sight of the observer should form an acute angle with the planes of the threads. Under these cir-

[1] For these three articles see : *Psych. Rev.*, V., 286 ; VII., 490 and 606.

cumstances there are obviously two points of intersection of the threads, one for each eye. Alternately closing either eye will reveal the location of these two monocular points. It is in the region between them that the following illusory deflections occur.

1. If either monocular crossing point be properly fixated, one of the double-images of the other (not fixated) thread will seem to turn away from its true plane towards that of the fixated thread. Suppose, for example, that the upper thread runs from left to right, the lower from right to left. If now the lower thread be fixated at the nearer monocular point, the left eye's image of the upper thread will seem to pass almost vertically through this point.

2. Choose now a fixation-point midway between the two threads and in about the middle of the region between the monocular points. If this point be correctly chosen, two illusory threads will suddenly appear lying at the right and left respectively in *vertical* planes and thus joining the actual threads. This is the most complete form of the illusion. The slopes of the phantom threads are not identical. One is nearly perpendicular to the planes of the threads, the other inclines strongly down and away. If viewed from the side they would appear in the form of a somewhat distorted X. Taking the threads in their entirety into consideration, as one thus looks down upon them, they seem to be two oddly bent wires placed side by side, with their ends farther apart than their middle portions and with the nearer end of each lying in the same horizontal plane as the farther end of the other. In this second case the illusory sloping threads

are not single images from either eye. Each is a peculiar fusion of images that do not normally belong together. Thus, on the basis of the arrangement presupposed above, the illusory thread at the right is made up of the left eye's image of the upper and the right eye's image of the lower actual thread. As a consequence the illusion is much more marked in this complete than in its partial form.

The explanation offered for these curious deflections is this: Each eye alone sees a pair of intersecting threads without being able to perceive any difference of depth between them. That is, for a single eye the threads appear to lie in one plane and cross at a point peculiar to that eye. Binocularly, however, there are differences of depth perceivable. With both eyes open, therefore, there are two monocular points in the binocular field. Accordingly vision is under the necessity of somehow adjusting itself satisfactorily to both sorts of demands, the monocular as well as the binocular. The result is in one case the apparent deflection of one of the double images of the non-fixated thread, in the other case the two illusory threads running through the third dimension and connecting the monocular crossing-points. The theoretical deductions are wholly along the line of emphasizing the importance of *binocular* factors in all visual perceptions of depth. Impressions that are strictly *monocular* have no definite meaning in terms of distance, whether relative or absolute.

Now while I am in most hearty accord with the general position towards space perception that Professor Judd has expressed both here and in other papers, I cannot see my way clear to accept the connection that he makes between this particular illusion and his theoretical comments. For I am quite convinced that a more thorough experimental analysis of the factors and conditions of the illusion leads to an interpretation somewhat different from that outlined above and, unless I am greatly deceived, more closely in line with accepted principles of vision.

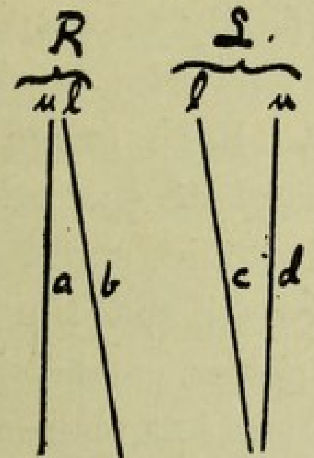
(*a*) Let us first examine the second, or more complete, form of the illusion. (1) In the first place, inclination of the line of sight to the planes of the threads is not at all necessary. In fact the definiteness of the illusion is perhaps greatest when the line of sight is perpendicular. (2) The angle at which the actual threads cross must not pass beyond a certain maximum, else the illusion is not obtainable. For a convenient examination of this and the immediately succeeding propositions, the following arrangement is very suitable. Fasten to the wall in front of the observer a black thread of considerable length, letting it incline downwards from left to right. Let the fastenings be readily adjustable. Then to two stand rods, one on each side of the observer, affix two horizontal strips so that they are perpendicular to the wall and lie on the right above and on the left below the line of vision. The second thread may be fastened to these strips, and any desired adjustment, whether of angle or of perpendicular distance, may be made. Now with the line of sight perpendicular to the wall, and keeping the distance between the planes of the threads constant at say two inches, let the angle at which the threads cross increase until the illusion no longer emerges when the point midway between the two threads is fixated. Of course this experiment presupposes that some facility in obtaining the illusion has already been reached. The difficulties naturally encountered at first may be somewhat lessened by affixing a slender wire to one of the stand rods and so bending it that its end shall furnish the fixation-point between the threads. If everything succeeds and the angle be increased as described, it will be seen that a limit may be reached beyond which the illusion is no more obtainable. In may own case this limit lies in the vicinity of 40°, though it is possible that this figure might be raised slightly by an increase of illumination. This result suggests that for ease of investigation the angle between the threads should be considerably less than this maximum. (3) The angle made by the illusory lines with the planes of the threads varies directly with the angle between the threads. For instance, with angles of 15°, 25° and 35°, the inclinations of the illusory lines are respectively 50°, 65° and 70°. (4) The angle of inclination of the illusory lines does not vary with the distance between the planes of the threads. Having obtained the illusion with some suitable angle between the threads, the nearer thread may be moved in and out from the wall without causing the inclination of the illusory lines to vary in the slightest degree. The only changes to be noticed are greater length and increased lateral separation of the illusory lines as the planes of the threads are drawn asunder.

(5) With these relatively general results in mind, we may now return to the 'box' form of apparatus and examine some more minute questions. In view of Professor Judd's explanation of the illusion before us as a

necessary outcome of the attempt to satisfy both the monocular and the binocular demands imposed upon vision by the peculiar conditions here in force, the following observation seems to possess considerable weight. It is to the effect that *there is absolutely no necessity that the monocular crossing points be in the field of view at all.* Two strips of black cardboard may be so placed upon the edges of the box in which the threads are stretched that the two eyes may clearly see the region between the two monocular crossing points while these points themselves are screened from view. The illusion is now only the more vivid. Yet under these circumstances there can be no thought of a necessary adjustment to two sets of relatively conflicting factors. The cause of the illusion must be sought elsewhere. I find this cause to lie solely in the particular fusions of images which take place when the point midway between the threads is fixated. What happens may perhaps be most clearly described by noting what changes take place in the binocular images as the point of fixation is raised from a position *below* both threads. The following figures represent these successive changes. Let the threads be stretched in the way that we have uniformly adopted, namely so that the upper one runs from left to right, the lower from right to left. Let a and b designate the images of the two threads received by the right eye, c and d those received by the left eye. Whether the image comes from the upper or the lower thread will be indicated by the letters u and l respectively. Finally let R and L be the inclusive or single designations for what is seen by right and left eye respectively.

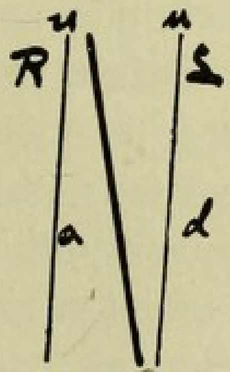
Fig. 51 presents the case where the point of fixation[1] is below the level of both threads. There is no fusion of double images. The pairs *ab* and *cd* are upon disparate points, and of course four separate impressions are received, the double images being of the "crossed" variety. In Fig. 52 the fixation-point is on the lower thread.

Fig. 51.



Fixation-point on bottom of box.

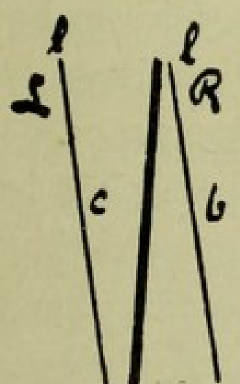
Fig. 52.



Fixation-point on lower thread.

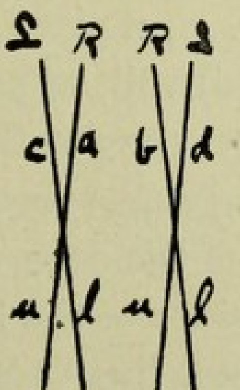
The images *b* and *c* have united. Crossed double images of the upper thread remain. In Fig. 53 the fixation-point is raised to the upper line, whereupon *a* and *d*

Fig. 53.



Fixation-point on upper thread.

Fig. 54.



Fixation-point between lower and upper thread.

[1] A movable fixation-point within this region may be conveniently secured by introducing a wire, or something similar, through an opening in the side of the box.

fuse. Uncrossed double images of the lower line remain. In Fig. 54 is depicted the state of things when the fixation-point is *between* the threads. *a* and *d* have not come near enough to fuse, as in Fig. 53, while at the same time *b* and *c* no longer fall upon corresponding points, as in Fig. 52. The entire process due to the successive fixations is, of course, to be pictured first as an approach, then as an overlapping, and finally as a crossing over of the two monocular fields. Now Fig. 54 gives the stage of most perfect *overlapping*. Each image of one eye falls obliquely upon the image of the other. Consequently the images must fuse at and about the points of intersection, for the simple reason that the latter represent corresponding retinal points. And just in this state of partial fusion of two linear images—such fusion that crossed double images remain on one side of the point of fusion while uncrossed double images remain on the opposite side—just in this state lie the indispensable conditions for the perception of lines running obliquely towards or away from the eyes. Any straight line (*e. g.*, a piece of wire viewed against a uniform background) will, when held so as to slope obliquely *towards* the eye, present the right hand cross of Fig. 54. A line sloping *away from* the eyes will appear as the left hand cross of the same figure. The double images existing on both sides of the fused point of fixation are often hard to see, but they are none the less there. Running the point of sight along the perceived direction of either illusory line brings about their progressive fusion, exactly as when the eyes pass along an objective line of a similar slope. And herein, of course, lies the ground for any and all perceived linear *directions*. For any such perceived direction means neither more nor less than that the point of sight must necessarily take this course in order to bring the various binocular impressions of the line upon corresponding points. If this view of the matter is the correct one, nothing but the universal laws of tri-dimensional vision are here in operation. Vision is not adjusting itself to novel conditions supplied by the experiment. It is simply behaving as it always does in constantly recurring experiences of everyday life.

Several further facts are to mind entirely confirmatory of the position above taken. It will be remembered that, when two rows of retinal elements are in *complete* fusion, the resulting perception is that of a line perpendicular to the line of sight. The necessity for such perception under these conditions was, as the reader will recall, the explanatory basis of the interesting illusion described by Mrs. C. Ladd Franklin in Vol. I. of the *American Journal of Psychology*. In fact the illusion here in question is at bottom only a complication of that. Both involve identical principles. But to return. By properly adjusting the line of sight to the planes of the threads, one of the illusory threads may be made to lie perpendicular to the line of sight. As things were arranged in my own experiments, this phantom thread could be made to appear at the *right*. Meanwhile the phantom thread at the *left* assumed a marked slope. And at the same time this latter thread possessed neither the solid color nor the spatial stability of the former. While the former remained steadily there, the latter tended to break up into its components and vanish. The double images of the latter were too obtrusive, though a proper movement of the point of fixation could successively unite them and keep the line alive. With the line of sight perpendicular to the planes of the threads—the position recommended above—the slopes of the illusory threads are the same, though in different directions. And in this case both threads are seen by careful gazing to possess double images on both sides of the region of fusion. In fact one is reminded of the appearance of the lines in the stereoscopic view of a truncated pyramid. The double images are essentially the same in the two cases.

The covering up of the monocular crossing-points as above described is certainly a procedure that is methodologically justifiable. For it is but the elimination of certain attending conditions for the purpose of gauging the effective part that they may be playing. And if now the illusion be obtained from the threads as seen in their entire length, and then all but the very middle parts be covered without the slightest attending change in the illusion, the conclusion is certainly warranted that the parts concealed are not operative in producing the illusion. Professor Judd seems, however, to object to this covering up of the monocular crossing-points,[1] apparently for the reason that an explanation is thus rendered too easy because of the too great simplification of the problem. Professor Judd agrees fully, I think, with the explanation tendered above in so far as this applies to the particular slopes of the illusory lines as due to peculiar fusions of specific images. But there seems to be a further, and to him more fundamental problem in his

[1] Loc. cit., VII., p. 608.

mind: Why, namely, should these specific fusions take place rather than others equally possible? "There is," he says, "a perfectly regular mode of fusion between the images belonging to the same thread which would result in a perception of each thread as a continuous whole lying at a different level from the other thread. This usual mode of fusion takes place outside of the monocular crossing-points. It takes place there so readily and completely that it is very difficult to perceive the double images."[1] On this view then the *usual* mode of fusion takes place outside the monocular crossing-points, while it is only *within* the region bounded by three points that the *unusual* mode of fusion occurs, the usual mode still being present as a possibility. If this is so, one must then explain why within the restricted central region the unusual mode of fusion is preferred. As thus stated the problem seems sharp and definite enough. And it was, I suppose, to clear up this problem that the explanation in terms of an adjustment to mixed binocular and monocular conditions was propounded. But I cannot admit the existence of this further problem for the simple reason that my observations do not agree with those of Professor Judd. For me, that is, *there exists no fusion outside the monocular points*, when in obtaining the most complete form of the illusion the fixation-point is midway between the threads. The threads outside the region of the illusion are to be seen in the form of double images which though not exactly obtrusive may be seen by careful observation. And that these double images, seen in indirect vision, represent lines at different distances from

[1] Loc. cit.

the eye is only what is always happening in vision. When I gaze at a point between the rod of my student-lamp and the upright of a chair, I do not lose the proper spatial relations of these two objects just because I have only double images of them. There is no fusion then anywhere except within the region of the illusion, and the particular fusion that takes place there is rendered necessary by the sole and sufficient reason that here and here only there are images falling upon corresponding retinal points. How these images come together has been sufficiently discussed in connection with the figures above.

(*b*) But there remains an objection which at first sight threatens to be a powerful one. It is this. Even if the above explanation be accepted for the complete form of the illusion, it cannot be applied to the *partial* form described on p. 316. For here a *single* image is deflected. and apparently some cause other than that of fusion is efficiently at work. Professor Judd has clearly called attention to the apparent defect in the explanation defended above in that it is inapplicable to the case where one image of one thread suffers illusory deflection.[1] This latter fact " shows clearly," he says, " that deflection is prior to any recombination, and, so far from being dependent on the recombination, is the source of the conditions which make a new and unusual mode of combination possible." Here, if anywhere, it would seem that the theory of visual adjustment to conflicting claims should find itself unassailable. But let us examine the matter carefully, eliminating one after the other the various factors involved, in the hope of hitting upon some factor

[1] Loc. cit., p. 607.

whose presence is indispensable. Suppose the lower thread to be fixated at the nearer monocular crossing-point, and this time let the line of sight be somewhat oblique to the planes of the threads. The left eye's image of the upper thread will now seem to pass down into the third dimension, cutting the lower thread at the fixated point. The question to be answered is, then: Why is there this deflection of the monocular image? And together with this is a further question: What determines the particular angle at which this illusory deflection occurs? The answer to the first may contain that to the second also.

With a card or piece of stiff paper, held preferably near the face so as not to diminish the illumination upon the threads, cut off first from the field of view the farther monocular point. The illusion is not destroyed. Its vividness is even increased. And if a second screen cut off all the nearer parts of the threads to within, say, a half inch of the fixated point, the illusory line will seem to stand out in entire independence, running down through the fixated point to the bottom of the box. It cannot well be the farther monocular crossing-point, therefore, whose presence in the field of view is essential to the illusion. Of course the other monocular point cannot be covered up since that is the point of fixation.

Next let us in similar fashion eliminate the various single images of the threads. The right eye's image of the upper thread and the left eye's image of the lower thread may both be removed without detriment to the illusion. But *if the right eye's image of the lower thread be screened from view, the life of the illusion is sapped,* and the image that has been deflected (the left eye's image of the upper thread), becomes a semi-indeterminate line in space. But, further, there is a quite definite portion of this right eye's image of the lower thread that is necessary if the illusion is to remain. This is *the portion immediately beyond the fixation-point. If this be left visible to however small an extent the illusion will persist, no matter how much else may be screened from view.* While if this restricted section just beyond the nearer monocular point be screened from the right eye, both crossing-points may be distinctly in the field of view without an accompanying illusion. But what do these facts mean? By referring to the right half of Fig. 54 (p. 321) it will be seen that it is just these two images (the deflected image and the image found essential to the illusion), that fuse to form the right illusory line in the complete form of the illusion. This is a noteworthy coincidence. And when one observes that the slope of the illusory line is identical in both the partial and complete forms of the illusion, the conjecture is in no way remote that it is the influence of the right eye's image of the lower thread that gives to the left eye's image of the upper thread its apparent deflection. The direction of this deflection is as it were a prophecy of the direction in which the point of fixation must be moved to bring about a progressive fusion of the two images concerned.

The entire difference between the two appearances of the left eye's image of the upper thread under the conditions of visibility and non-visibility respectively of the right eye's image of the lower thread, can hardly be too much emphasized. In the first case the direction is definite, determined. In the second, the direction is *equivocal*, as a single linear image ordinarily is. By drawing a line upon the bottom of the box, a little beyond the point of fixation, and, by employing the usual eye-movements for the correct perception of equivocal figures, the line in question may be given either an upward or a downward slope. Under the conditions of the first case such equivocal perception is in no way possible.

We are thus led to the important inference that one of two double images may be influential in two directions at the same time. It may remain with its original companion, while at the same moment tending to break loose and join with another image, particularly if a slight movement of the eyes is to make this new mode of fusion more easy.

But the new mode of fusion may not present attractive possibilities. In that case the illusion fails. Let the angle between the threads be about 65°. As shown above, there is no possibility of the complete form of the illusion when the angle between the threads passes a certain maximum. Traces of the partial form seem, however, to still linger for a short time after this maximum is passed, though the illusion suffers unmistakably in clearness. But with an angle of 65° I think every one will admit that the left eye's image of the upper thread no longer undergoes deflection. And yet in so far as the simultaneous presence of binocular and monocular factors in the field of view is concerned, all the conditions of the successful experiment are here in force. But the greater angle between the threads has caused a too great separation between the two images that before tended to fuse. The failure of the illusion under these circumstances of increased angle between the threads furnishes a striking bit of evidence against the view here combated and in favor of that here defended.

The two questions propounded on p. 327 have now been answered. The monocular image is deflected through the influence of a neighboring image, and the direction of its deflection is the path along which the eyes must move to bring about a union of these two images. That an image already in one mode of fusion may extend its influence to an adjacent image may be less difficult of comprehension when one remembers that the images which represent oblique lines are in complete fusion only about the point of fixation. The detaching of one image from the other is therefore no very great matter when conditions arrive which favor such a process.

It has been very evidently assumed throughout this discussion that visual images are always treated not primarily with reference to the spatial conditions that produce them, but rather with reference to the manner in which they fall upon the retinæ. For whether a monocular image shall appear to lie here or there, to slope in this direction or in that, is, I take it, to be determined by factors not to be discovered in the single image itself. When, therefore, we are dealing with single images we may very properly neglect the fact that the outer object lies in a particular plane. The real query is—in what plane will the image seem to lie when a given set of conditions surrounds it? And Professor Judd's illusion affords a peculiarly interesting combination of conditions for study in connection with such a query.

THE ILLUSORY DUST DRIFT. A CURIOUS OPTICAL PHENOMENON.[1]

It is of course improbable in the highest degree that the phenomenon here to be described has entirely escaped notice hitherto, but the writer at least is unaware of any existing description of the same. The conditions under which the illusion arises are so easy to fulfill, and the resulting appearance is so odd in many ways, that a brief description of the matter may be interesting. The only 'apparatus' required is a set of black and white lines like those of Fig. 55, and a dark background near by. The illusion may be obtained from this figure, but the best results, perhaps, are secured by using a square yard of common black cloth bearing narrow white lines not more than two millimeters apart. Such cloth may be obtained at any large dry goods store. If now this be hung upon the wall in a strong light, and a square of dull black cardboard be placed above it, or at the side, everything is ready for the observation. Picking out some point near the center of the cloth, let this be fixated steadily for not less than twenty seconds. Then transfer the gaze quickly to the black cardboard, and the illusory dust drift will appear. The appearance is that of *a thin cloud of fine white dust moving across the field of vision.* Or the tiny particles seen may be likened to the motes in a sunbeam, since they much resemble these in density.

[1] Reprinted from *Science*, N. S., Vol. XII., p. 208.

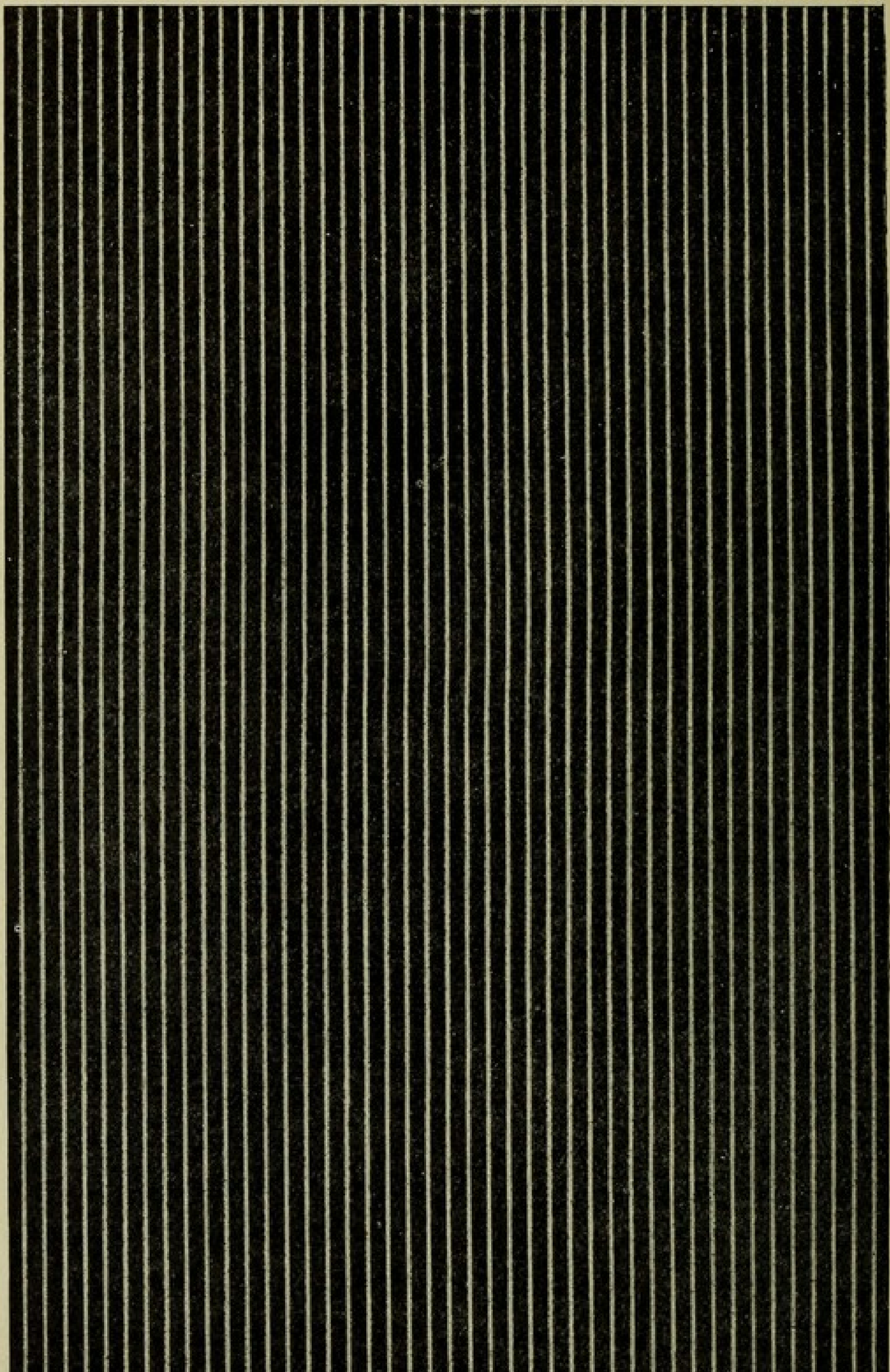


Fig. 55.

A steady fixation of the eyes is at no time absolutely essential. They may roam freely over the cloth and then later over the dark background, though the illusion under these circumstances is diminished in strength. The best results are unquestionably secured by as resolute a fixation of the cloth as possible. The necessary *duration of this fixation* seems to depend upon the retinal sensitiveness of the observer. Probably 5 sec. is the minimum for any noticeable after-effects, while no advantage seems to be gained in any direction by prolonging the fixation beyond a period of 30 sec. In practice, successive renewals of the illusion may be accomplished by very brief fixations, provided only that the time of the first fixation be moderately long.

The *duration of the illusion* seems also to be an individual matter. One observer can still see traces of the 'dust' after a lapse of 30 sec., while in another case everything had disappeared at the end of 4 sec. Perhaps 10 sec. would be a fair average duration. At about this time the regular after-image is apt to make its appearance, and this tends strongly to drive the illusion away.

But the really interesting point in the matter is the *direction* of the moving drift. This turns out to be directly dependent upon the direction of the lines in the field of fixation. The most general statement of the matter is that, however these lines may lie, the illusory dust currents run in a direction *perpendicular* to them. Quite often, however, it is impossible to speak of the direction as strictly perpendicular, since the course of the drift may be along curved lines, as if a tiny whirlwind had caught up a bit of light, fluffy snow. Or, further—and this is perhaps most often the case—there are secondary currents visible whose directions do not coincide with that of the main stream. Nevertheless some particular direction is almost invariably more prominent than any other, and the statements of various observers show that the direction of the most vivid stream is most decidedly perpendicular to that of the lines. If the lines are vertical, the drift is usually to the *left*, though some subjects see it always to the right. If the lines are horizontal, the tendency to see the drift running *downwards* seems to be slightly more marked. Nearly as many subjects, however, see an upward drift, and quite often currents are seen to run side by side in *both* directions.

If the experiment be so arranged that half the field of fixation is occupied by vertical and the other half by horizontal lines, two clearly separated currents will appear in the illusion with horizontal and vertical directions respectively. Or if the usual field of fixation be divided by a vertical strip of some uniform color, no "drift" will be seen in that portion of the field corresponding to the strip. If the center of a set of concentric black circles upon a white ground be fixated, the resulting illusion suggests a confused boiling movement, sometimes running in converging lines towards the center, sometimes passing in diverging lines towards the periphery of the field.

Now the oddity of this illusion consists precisely in this: That without intentional movement either of eyes or of object, there is yet an after-effect in the form of a definite and unmistakable perception of motion. An ordinary after-image of motion requires a previous objective movement of some sort. Here, on the other hand, we can only say that the resulting perception is *as if* there had been a previous and actually perceived motion. And this latter is exactly the case with another peculiarity not directly connected with the illusion itself. After steadily viewing the cloth for say 30 seconds, the closely set lines begin to appear beaded. They are no longer straight, but wavy. And even the after-image, when it appears, presents the same aspect. Now this result is identical with that produced by actual movement of parallel lines across the retina.[1] Accordingly we have, in connection with this illusion, two phenomena that ordinarily follow actual movement. This fact would seem to indicate the direction in which an explanation is to be sought. For while there is no intended movement of the eyes during the fixation of the cloth, there are certainly *impulses* to movement aroused by the various lines about the fixation-point. Every one knows how hard it is to let the eyes come to rest in a field occupied by such lines. Each one of the latter solicits the center of fixation to rest upon it. The impulses to movement are then in directions perpendicular to the lines, in other words, in the same directions as the currents of the illusory dust drift. And, taking all into account, it cannot be very far out of the way to conjecture that the same fundamental factors are at work here as in the familiar cases of the artificial waterfall and the rotating spiral. Mere impulses to movement have taken the

[1] This has been well described by von Fleischl, *Sitzungsber. d. Weiner Acad.*, 1882, Bd. 86, III. Abth., S. 8.

place of actual movements in the production of after-effects.

An attempt to obtain the illusion with *monocular* vision is attended with quite surprising results. For even after a full minute's fixation of the cloth, *no 'drift' is to be seen on the black background.* And the result is the same whether both eyes, or the one eye only, be open at the moment of transferring the gaze to the black surface. There is instead an interesting set of phenomena which do not appear in the binocular experiments. Now the illusory dust-masses come to view *during* the fixation. They are not wholly like those above described, but present rather the appearance of fine meshes formed of light gray cobwebby lines. Sometimes these meshes appear to lie slightly in front of the cloth, and if the effort is made to fixate them they temporarily disappear. Movements are by no means wanting, but there is an intermittence about them which the binocular phenomena never show. A closer examination of this network character of the illusion reveals the fact that each eye, the closed as well as the open, is contributing to the total effect. This may be readily demonstrated as follows: Let either eye, the *left* for example, be entirely screened from the cloth by a tiny box, or something similar, blackened within, the eye remaining open and free to move. Let the *right* eye fixate the lines. Now while this right eye remains open, the most prominent illusory movements are decidedly those running *perpendicular* to the direction of the lines. This is true no matter how the lines may lie in the field of vision. But if this right eye be closed after a brief fixation, another set of movements is seen projected into the dark field of the covered *left* eye. These movements, though possessing neither vigor nor great vividness, are invariably in the *same* direction as the objective lines. That which moves here is less a dust-cloud than a set of fleecy or worsted-like bands, in the midst of which the binocular after-image of the lines of the cloth soon appears. In addition then to the regular transference of an after-image to the field of the unstimulated eye, we have here the transference also of an illusory after-effect. The illusion is to be sure not wholly the same for the two eyes, but neither are crossed after-images entirely identical in character with the direct after-images. The interesting features then of the monocular experiments are that the illusion appears for the stimulated eye during the period of fixation only, and that the unstimulated eye also presents illusory effects of the same general character as those experienced by the open eye.

It can hardly be said with full certainty that these monocular phenomena have contributed anything decisive towards the explanation of the binocular form of the illusion. Nevertheless there is a point of difference between the two forms which cannot be wholly without meaning. There is, namely, in the monocular experiments a relative absence of the feeling of unrest during the period of fixation. The single eye seems to fixate the chosen point with far less effort. Solicitations to its movement are noticeably absent, and the time of stimulation can be prolonged without discomfort to a point where the binocular stimulation would have become exceedingly disagreeable. Now whether this absence of vivid impulses to movement may be regarded as alone responsible for the difference in the illusion can of course not be affirmed with complete confidence. But it seems probable on the whole that the ultimate explanation of this, as of all after-images of motion, will be somehow formulated in terms of impulses to movement aroused by the particular stimulation that precedes. Perhaps the experiments here recorded may contribute their mite towards this final explanation, if that ever comes.

TWO OPTICAL ILLUSIONS OF DOUBLE MOTION.

COMPARATIVELY little seems to be known at present in regard to the retinal processes which accompany and succeed the stimulation of the eye by moving objects. To remain within the limits of safety one must be content to speak vaguely of waves of nervous disturbance passing over the retina, leaving it to the future to determine the relative parts played on the one hand by the excited visual elements, and on the other, by the aroused muscular impulses. In this state of our ignorance, however, one thing appears evident, namely, that a retina stimulated by a moving visual field is peculiarly susceptible to other impressions that may suddenly supervene. This much at least seems to be shown by that curious optical appearance known as the Münsterberg-Jastrow phenomenon.[1] The disk bearing the unequal sectors of color rotates rapidly enough to produce a perfect mixture upon the retina, yet under these circumstances the shadow of a light rod flashed upon the eye as the lighter sector passes a given point is distinctly and separately perceived. In fact this shadow-impression is not immediately obliterated, but persists for about 0.15 sec. in spite of the succeeding stimulation from the moving disk. The two illusions about to be described seem to give added evidence to the fact of peculiarities of retinal

[1] See *Am. Jour. of Psych.*, 1892, IV., p. 201.

reactions under the conditions of moving stimuli. Their description under the general head of illusions of space perception may be justified by the various geometrical factors involved.

ILLUSION A.

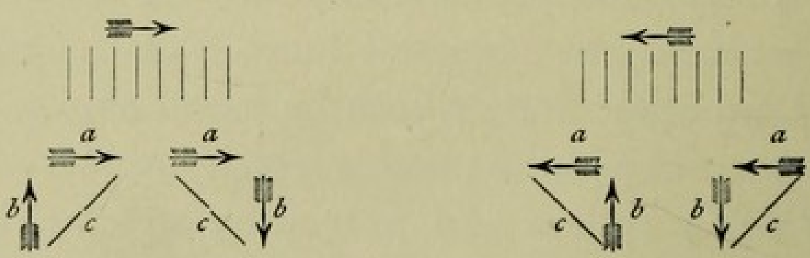
The first illusion is really only a complex variant of the Münsterberg-Jastrow phenomena. It is to be seen when parallel lines pass with a fair degree of rapidity across the field of vision in a direction perpendicular to their length, if at the same time a slender rod of a dark color be passed between the eyes and the moving lines. If the rod be held parallel to the direction of the lines and be given a rapid rate of movement, the entire area covered by this movement will be filled with illusory lines like the objective background in color. Meanwhile the color of the background upon which the illusory lines lie is a fusion of the two objective colors. When the movement of the rod is rapid, the illusory lines are not noticeably different in geometrical characteristics from their objective counterparts. The objective field is merely reproduced in changed colors. For slow movements of the rod the lines are somewhat nearer together, but for quick movements the spaces between the objective lines differ too little from those of the objective field for the differences to be noted. At the same time, as the next paragraph will show to be necessarily the case, the illusory lines must be slightly nearer together than the objective when the movement of the rod is opposite to that of the band, and slightly farther apart when the movements of rod and band are in the same direction.

This form of the illusion is obviously the same at bottom as that obtained with the rotating disc. Just as there the shadow-bands of the moving rod were seen during the passing behind it of the lighter disk, so here the shadow-bands are cast by the light lines or, if the lines be dark, by the light intervening spaces. Only here it is interesting that the geometrical character of the moving objective field is such as to be reproduced in the illusion.

New features are presented by holding the moving rod perpendicular to the objective lines. The conditions for practical experimentation, both here and above, are easily secured by using endless bands which are stretched horizontally upon rollers and moved by an electric motor. These bands may be made readily from the common lined cloths of the dry goods stores. The lines may be black upon a white ground or white upon a black ground, or light and dark colors may be used in place of the white and black. To get any desired combination colored papers may be used, the lines being drawn upon them with colored ink. In the following description one has ordinarily to think of white lines, about 0.5 mm. in width, upon a black cloth background. If now rates and direction of movement can be controlled, everything is ready for the experiment. To secure the best results it is well to have the band move rapidly enough to mix the colors upon it, and the rod should move at a rate of say 600 mm. per second. The best results are given usually when the color of the rod is dark.

If the observer stand with the line of sight parallel to the moving objective lines, and if the moving rod be held, as said, perpendicular to these lines, the illusory lines will tilt to either side as the rod travels back and forth. The direction of this tilt depends upon the directions of the objective movements in the following manner. If the band moves to the right, the inclination of the illusory lines is to the right when the rod moves away from the observer, to the left when it moves towards him. Letting the band travel to the left reverses these inclinations.

The following figures will present this graphically.



a = direction of band's movement.
b = direction of rod's movement.
c = direction of inclination of illusory lines.

Obviously, now, since the *direction* of the slope of the illusory lines depends upon the directions of the movements involved, the *degree* of the slope is presumably a function of the rates of these movements. Experiments show that this is the case. Increasing the rate of the band's movement causes the illusory lines to incline further away from the vertical. Increasing the rate of the rod's movement lessens the inclination of these lines. The degree of the illusory slope varies, therefore, directly with the band's rate and indirectly with that of the rod.

Now with the Münsterberg-Jastrow phenomenon in mind it is not difficult to see the ground for these characteristics of direction and degree of slope that we have just noted. What happens is simply this: Suppose for convenience that the band moves to the right and that the rod moves away from the observer. Now each successive position of the rod corresponds to a particular position of the lines of the band. And the shadow cast by the rod in any given position must be, on the analogy of the typical phenomenon, a line of black spots or patches, separated by spaces equal to those between the lines. For manifestly only the white lines can cast shadows, and the intersection of the lines by the rod's image is at a given moment a series of tiny lines or spots. But now if the rod move on to the next position beyond, the whole series of new shadow-spots will lie farther to the right, since the band has meanwhile travelled in this direction. And so on for all positions of the rod during its whole course. But, further, these shadow-spots must be continuous with each other since the rod's progress is continuous, and if the after-image of the first line of spots still persists when the last line is reached, the result for perception must be an illusory line sloping away to the right. And this slope must be determined in degree by the difference in the positions of the band at the beginning and at the end of the rod's movement. This is the reason, then, why rapid movements of the rod give a diminished slope while rapid movements of the band give an increased slope.

In a word then the illusory lines here are fusions of successive shadows cast upon a retina stimulated by a moving field. We have considered in detail one pair only of the two objective movements. Precisely the same kind of analysis applies to the other combinations of directions.

The above manner of accounting for the illusion is reinforced by two considerations. First, if a rod be suddenly introduced into the field of vision by letting it fall between the eyes and the band moving in a vertical plane, a single row of the black shadow-patches may be seen. Such a row of shadow-patches, so obtained, manifestly corresponds to what is seen at each instant for each successive position of the moving rod. It is one of the elements that fuse into the complex lines. Second, if the objective lines are made to tilt so that they are no longer perpendicular to the edge of the band, the slope of the illusory lines changes in a manner consonant with the above explanation. When the objective lines are perpendicular to the direction of their movement, the illusory inclinations are equal to right and to left, that is, for both directions of the rod's movement. But suppose that the objective lines are inclined to the direction of their movement. The illusory slope will now be greater upon one side than upon the other. Let the objective lines slope to the right and let the band travel in the same direction. The illusory slope to the right (rod moving $\uparrow$) will now be greater than that to the left (rod moving $\downarrow$). For, clearly, as the rod moves away from the observer, the succeeding rows of shadow-patches must pass to the right at an increased rate, since the slope of the objective lines augments the influence of the band's movement. On the other hand, as the rod moves back towards the observer, the objective slope is antagonistic to the band's movement, the succeeding rows of shadow-spots travel less rapidly to the right, and the illusory slope is diminished. Just this state of things must be expected on the shadow theory.

It remains to mention one or two minor details. If a wheel with small and rather closely set spokes (a bicycle wheel for instance) be rotated above the moving band, the illusion takes on, to all appearances, the characteristics of a real outer object. The phantom lines remain persistently in view. There are, naturally, slight movements forward or backward of the set of illusory lines, for the eyes tend to change their point of fixation, and the distances between the spokes may not be wholly uniform. But in any case a high grade of steadiness in the illusory field is obtained and the whole phenomenon may be examined more thoroughly than when the entire illusion lasts for but the fraction of a second. In our experiments splendid results were reached by making the rotary speed of the outer ends of the spokes about 4,000 mm. per second. The color factors noticed in this variant of the Münsterberg-Jastrow phenomenon are in general the same as in the typical case. The illusory lines, that is, take the color of the objective background, being darker than the background when this is dark, lighter when this is light. When, for instance, the lines are yellow on a red ground, the illusory lines are a somewhat darker red than this. If the lines are black on a white, or, more strictly speaking, a light gray ground, the illusory lines are a still lighter gray.

There seems to be an exception to this general fact when the lines and spaces of the moving band are equal, or, more properly speaking, when the band is made up of equally wide stripes of different colors. In one case the stripes were blue and white, each about 2 mm. wide. In another case they were black and white, each 12 mm. wide. In the first case the illusory stripes were two shades of lightish blue; in the second, one stripe was a medium gray, the other a somewhat darker gray. A preponderance of one color over the other seems necessary therefore, if the illusory lines are to be darker, or, as the case may be, lighter than the objective background.

As Professor Jastrow has suggested,[1] the usually prevailing color features are doubtless due to contrast. This contrast takes place probably between the shadow and the mixed color of the moving background. In this there is nothing particularly surprising. What strikes one as odd is that spots of the retina, which have been reacting, apparently, to a mixed color, should on a sudden when patches of shadow are thrown upon them react as if only the color of the background were concerned in the process. If we knew what transpires in the retina when colors are being perceived and also what processes are set up by a moving visual field, we might perhaps be able to say why this is so. Pending the arrival of this knowledge, however, we can do little else than record the facts as we observe them. Nevertheless one thing seems certain; that the reaction of the retina to the stimulation that comes from the movement, added to the reaction from the stimulations of light and color, makes possible such phenomena as we have recorded. To say this is hardly to give an explanation. Nor has this variant which we have described contributed materially to the

[1] Loc. cit.

understanding of the original phenomenon. But it is just this adding of new forms that may ultimately put us in a position to propound more definite and exact theories.

Illusion B.

This second illusion is more complex than the first. The essentials of its form and conditions are as follows: If any set of parallel *vertical* lines be moved in the direction of their length across the field of vision, and if, at the same time, the eyes fixedly follow a *horizontally* moving point, there will appear over the whole field a set of illusory lines lying obliquely and colored like the objective background. It is as if the original lines had, with changed color, been tilted into the new position. For the illusory lines are like their objective counterparts in width and distance apart. Of course, a certain rate of the two kinds of movement is requisite. The direction and amount of the illusory slope depend respectively upon the directions and rates of these movements. Perhaps the most convenient way of securing the conditions necessary for the illusion is to rotate horizontally a cylinder which has been wrapped with dark cloth bearing narrow parallel lines of a light color. The moving fixation-point may then travel immediately in front of the rotating cylinder. In fact, it was under circumstances similar to these that the illusion was originally noticed. I am indebted to the college mechanician at Amherst, Mr. E. F. Thompson, for first calling my attention to it. He was engaged in sand-papering a wooden cylinder from which a wrapping of wire had recently been removed. This wrapping had left its imprint upon the soft wood. The cylinder was revolving rapidly in the lathe, and as the edge of the moving sand-paper was accidentally followed by the eye, the lines upon the surface were seen apparently to sway now to one side and now to the other, as the fixated edge of the sand-paper moved to and fro to right and left.

It will be noted that the wrapping of wire upon the cylinder had left a helical imprint upon the wood. Of course, therefore, the coils of this helix appeared to travel horizontally when the cylinder rotated. The illusion being originally observed under these conditions, it was but natural to suppose at first that it was dependent somehow upon the helical character of the lines. The investigation proceeded for some time upon this supposition, when it was found that no slope whatever of the objective lines was necessary. The illusion was just as marked when each line upon the cylinder formed a closed circumference. And herein is to be found the peculiarity of the illusion. For, geometrically speaking, a moving surface of this kind presents nothing more to the eye than the same surface at rest, except, of course, the fact of motion itself. Yet this motion is absolutely essential to the illusion. But before discussing this point, let us look further at the details of the illusion.

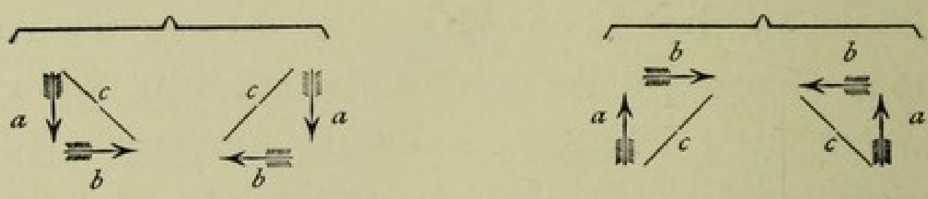
For the quantitative study of the illusion apparatus was arranged by which the apparent slope of the lines could be accurately measured. A window in front of the revolving cylinder concealed all but the illusion-producing parts. On a ledge just below this window was slid a small carrier to the upper end of which as center was fastened a black cardboard sector of about 30°. This sector extended up over the window and could be adjusted at any angle. A point of fixation was marked at about the middle of each edge of this, that on the left edge to be used when the movement of the carrier was to the left, that on the right when the movement was in that direction. The slope of the edge could then be adjusted so as to coincide with the slope of the illusory lines. The excursion of the carrier was made constant by means of buffers upon the window ledge, and its rate of movement was controlled by a metronome. The cylinder was rotated by an electric motor, the speed of which could be regulated within sufficiently wide limits. The objective geometrical conditions were obtained by using, as in the case of illusion *A*, lined cloths with variously colored backgrounds. The lines upon these were usually about 0.5 mm. wide, and their distances apart varied from 1.5 mm. to 11 mm. Ordinarily the lines used were white upon a black background, with a separation of 7 mm.

Of course the two movements, rotary on the part of the cylinder and lateral on the part of the eye, are equivalent to a compound motion of the cylinder while the eyes are at rest. That is, a lateral movement of the rotating cylinder to the right is equivalent to a lateral movement of the eyes to the left. In practice it is better to move the eyes, difficult as this is for most people, keeping the cylinder fixed.

If the conditions as above described are fulfilled, the illusion will not be hard to see. It is often difficult, however, for most people to follow the horizontally moving point with sufficient fixity. This accomplish-ment ordinarily comes only after long practice. Of course it is absolutely necessary for any investigation of this illusion, though the general character of the illusion may be passably well observed by the unskillful.

The Direction of the Illusory Slope.

Any illusory line always slopes in the direction of the path that any point on the cylinder would, under the conditions of double motion present, trace upon the retina. Thus if the lines move *down* over the field of vision, the slope is always in the direction of that side to which the eyes are moving. If the lines move *up*, the illusory lines slope in the direction opposite to that in which the eyes are moving. Or, if we think of the eyes' movement as replaced by the lateral movement of the cylinder as above described, the slope of the illusory line is, under the conditions above stated in respect to the objective lines, that of the resultant of the two motions of the cylinder. This may be graphically represented as follows:



a = direction of cylinder's rotary movement.
b = " " " lateral "
c = resultant = direction of illusory slope.

The coincidence of the illusory slope with this resultant may be tested by marking one or more small but conspicuous spots upon the surface of the cylinder. As these pass over the retina, the paths swept out by them may be seen to be identical in direction with the illusory lines.

The Degree of the Illusory Slope.

As would naturally be expected, a certain minimum speed of both the rotary and the horizontal movements must be reached before the illusory lines distinctly emerge. What, under these conditions of minimum speed, the particular slope of a given set of lines will be, depends upon the character of the objective lines in a way that we shall examine later. For the moment what we have to note is that any increase in the rates of these two movements is accompanied by a change in the degree of the illusory slope. This degree of slope varies, namely, directly with the rate of the horizontal eye-movement and indirectly with the rate of rotation. That is, the more rapidly the eyes travel across the moving lines the greater is the angle between the illusory and the objective lines, while on the contrary this angle becomes less as the rate of the cylinder's rotation is increased. Within the range of possible rates that are above the minimum, an increase of the illusory slope may be brought about, therefore, either by moving the eyes more rapidly or by slowing up the speed of rotation of the cylinder. In other terms, whatever will change the direction of the resultant of motion changes the illusory slope in like degree. The two coincide in every particular. Slopes of 20° and 5° are respectively the largest and smallest that we have accurately measured. But 20° is by no means to be taken as the upper limit. Rough observations seem to indicate that 35° is perhaps in the near vicinity of the highest observable inclination.

The objective lines may be made to form a helix about the cylinder by allowing their ends to be continuous not with themselves but with some of their neighbors at the line of overlapping of the edges of the striped cloth. Any desired inclination may thus be given the lines upon the cylinder. An interesting and predictable feature of the illusion under these conditions of objective inclination is that the illusory lines appear much more readily for one direction of the eyes' motion than for the others. If the objective slope is to the right, for example, and the lines are traveling downwards over the eyes, it is the movement of the latter to the left that more readily gives the illusion. The reason is obvious. Under the conditions of slope and rotation named the coils of the helix formed by the lines must appear to move to the *right* across the field of vision. As the eyes move to the left, accordingly, the two motions conflict and it is as if the eyes were traveling with an increased rapidity. To the right, however, the two motions coincide in direction and the practical result is that the eyes pass with such diminished rapidity over the lines that the lowest necessary lateral rate is in danger of not being reached. To secure the same clearness of the illusion it is necessary, therefore, under these circumstances to move the eyes much more rapidly to the right than to the left.

The Minimum Rates of the Two Movements.

The lowest rates at which a distinct and perfect illusion appears depend upon the character and separation of the objection lines. The general law is that *the farther apart the lines the higher are the rates required.*

Naturally no universally valid statement of particular rates can be given for the simple reason that no two persons would agree, perhaps, upon the point where a perfect illusion is reached. But a few illustrative figures may be given to exemplify the law just stated. The following table deals only with cases where narrow white lines were on black or dark blue backgrounds.

Space Between Lines.	Character of Illusion.	Rate of Rotary Movement.	Rate of Lateral Movement.
1.5 mm.	Perfect.	358.8 mm. per sec.	65 mm. per sec.
3.0 mm.	Indistinct.	552	170
	Perfect.	1270	195
7.0 mm.	Indistinct.	368	146
	Fair.	865	138
	Perfect.	1710	112
11.0 mm.	Indistinct.	1325	140
	Nearly perfect.	2760	276

With wider lines than those used above the illusion is more difficult to obtain. The following table gives the results of an experiment with black lines 2.5 mm. wide drawn on white paper with spaces of 7.5 mm. between them.

Character of Illusion.	Rate of Rotary Movement.	Rate of Lateral Movement.
No illusion.	534 mm. per sec.	608 mm. per sec.
Barely perceivable.	828 " " "	608 " " "
Fair.	1270 " " "	608 " " "
Good.	2760 " " "	608 " " "

What the conditioning factors are that determine these differing rates it is not wholly easy to say. Presumably the most influential is to be looked for in the general lessening of the intensity of the stimulation as the number of the lines over a given area is diminished. There is then too much background in proportion to the lines. That is, the *diversity* of the stimulation appears to be a prime necessity. And when this diversity is lessened by increasing the distance between the lines, it must be compensated for by greater speed either by one or by both of the movements. But still this consideration, it must be admitted, does not account for the high speed necessary when the lines are made heavier.

The minimum lateral rate is not by any means such that the two colors, of lines and of background, mix perfectly. The coming of the illusion seems not to be dependent upon this. There may be perfect mixture and there may be almost no mixing at all. Under these two conditions the illusion varies to be sure in quality, but its distinctness is equally marked in the two cases.

An effort was made to obtain the illusion by interchanging the movements involved. To this end the moving band described in connection with illusion *A* was used. The lateral movement of the eyes, then, over the horizontally lying cylinder was replaced by the movement of this band in the field of vision, while the movement of the lines in the direction of their length, as provided for normally by the rotating cylinder, was replaced by the movement of the eyes in a direction parallel to the lines. Rates were so adjusted that their equivalents were reproduced in the new form.

It would seem *a priori* that the illusion ought to be obtainable in this form as well as in the other. Such appears, however, not to be the case. At least no efforts in that direction have yet been successful. Of course, the movement of the eyes must be very rapid to equal the rotary speed of the cylinder. But it was by no means impossible to accomplish this requirement. Still a moving fixation-point was necessary, and the lines of illusion *A* were always to be seen in whatever part of the field the rod or wire moved whose point was fixated, and this was a seriously disturbing factor in the observation.

Thus far we have supposed the lateral movement of the eyes to be from side to side, the illusory lines tilting alternately to right and left as the eyes' movement changed in direction. In this form the illusion is a decidedly fleeting one. For purposes of more prolonged study, or for purposes of demonstration, an interesting one-sided form, so to speak, is perhaps more feasible. Here the eyes may remain fixated, and the illusory lines too are fixed and persistent. The outer conditions are simply those of the helix form, described on page 352. Using lines that are 7 mm. apart, let the edges of the cloth that meet on the surface of the cylinder be so placed that there are, say, four free ends on both sides. It will then be as if a set of four parallel lines were wound in a helix about the cylinder. Let the slope of these helix coils be to the right. Then as the lines move down over the field of view, the coils will appear to travel to the right. Now it is evident that if this sidewise movement is sufficiently rapid, the conditions for the production of the illusion are all present upon the cylinder. The rotary and lateral movements are provided for without the coöperation of the eyes. The latter may remain fixed and the whole attention may be given to the observation of what happens. Fixating some point held before the cylinder, the illusory lines are seen to lie vertically in the field. With a fairly high rate of rotary speed they are sharp and distinct. A speed of about 2,000 mm. per sec. gave excellent results. The objective lines appeared to move at a rate of 150 mm. per sec. With this particular helix form that I have described the coils slope at an angle of about 6° from the vertical. I have mentioned this form because it is, perhaps, better adapted than any other to the conditions that give a clear illusion. Other slopes, both greater and smaller, may be used. There is a point, however, at which the slope becomes too great and the illusion vanishes. A slope of 15° is already too great for any satisfactory results. On the other hand a slope of 1.5°, the smallest possible with the lines used, was sufficient to produce the illusion. But whatever the objective slope the illusory lines are always vertical.

That the lines of this illusion and of illusion *A* are not identical may be seen, when the slope of the coils is moderately great, by passing a rod (like that used in connection with illusion *A*) up and down before the rotating cylinder. Under these circumstances both sorts of illusory lines are seen, the steady vertical lines of illusion *B* and the momentary oblique lines of illusion *A*.

A strikingly clear and sharp state of the illusion, and one that together with the facts just described gives the clue, it seems to me, to the direction in which an explanation is to be sought, comes out when the slope of the coils is from 1°.5 to 3°. This corresponds to an overlapping of one or two lines. The illusory lines here, still to be seen with fixated eyes, lie between the objective lines, and the latter being white, are of a deep black color upon the black background. For the objective background is still to be seen in its own color. There is no mixing of the colors of lines and background possible here. The lateral motion of the coils is too slow for that. The objective lines too are separately perceivable, only, as is always the case when parallel lines move before the fixated eye, they present a wavy appearance. The deep black lines seem almost to leap out from the wavy white lines, springing to the left when these move to the right. Slight movements of the eyes to either side often contribute to the striking vividness of this phenomenon. In fact such slight eye movements will produce this same feature of the illusion when the lines have no objective inclination. Indeed, they were first noticed in this way. The black lines seem then to leap out on the farther sides of the white lines, sometimes seeming almost to be entangled in the wavy edges of the latter. They slope also slightly in the direction of that side towards which the eyes are moving. Even a single objective line—the remainder being screened from view—will give the illusion. But whether the objective lines tilt or not, the point to be emphasized is that while the objective background is still to be seen virtually in its true state so far as color and geometrical conditions are concerned, the illusory lines may also be seen and their width, slope and color be compared directly with their objective counterparts. As to the color, we have seen in the present case that the illusory lines are like the objective background, only darker. Another experiment was made where the lines were dull yellow upon a medium red ground. The illusory lines were dark red.

THEORETICAL.

In attempting an explanation of this second illusion of double motion there is very little that can serve as a guide. Since the illusion was originally seen with the objective lines in the form of a helix, it seemed at first as if the illusory lines were simply stretches of clear spaces left upon the retina in the midst of a field of mixed color. Just as the moving black sector in the illusion of the strobic circles is due to this cause. But in the present case the geometrical structure of the outer field is such as to entirely preclude this explanation.

Then again the wavy character of the lines seen in motion suggested a second possibility. Some parts oi certain waves can be seen to be much thinner than others, as if a given line were about to give way in spots. Might it not then be possible for the moving eyes to actually break through the lines, as it were, and thus have a set of clear paths over the cylinder in the direction of the resultant of the two motions? Notwithstanding the hypothetical possibility of this supposition, it is invalidated by the fact that the illusory and the objective lines are at the same distance apart.

A partial clue to what happens seems to be given by a fact observed in connection with the *steady* form of the illusion described on p. 355. It may be shown that all the vertically lying illusory lines pass through the point of intersection of the helical coils and the edge of overlapping of the cloth. Points at this edge of overlapping must then be somehow responsible for the illusion. Any slight projection, or irregularity of adjustment of the lines along the edge, in short anything that may give a slight preponderance to the black, acts like the moving rod of illusion *A*, and, as it were, throws a shadow upon the stimulated retina. This seems to be a justifiable generalization of the observed fact. Any preponderance of black, that is to say, traces a path upon the retina, the after-image of which remains unobliterated until a succeeding stimulation passes over the same path. If a tiny portion of one of the white lines be blackened and a somewhat extended clear stretch of black be thus left for the eye, a vivid illusory line appears. Now under the condition of overlapping of the edges of the cloth that leaves five free ends on either side, this line should, it would seem, be darker than the surrounding region, but still not so dark as the objective background, since four white lines (the fifth being broken through) sweep over this retinal path at every rotation of the cylinder. The fact is, however, that this line is, like the illusory lines in general, darker than the objective ground. The peculiar susceptibility of the retina to shadows, when stimulated by two simultaneous movements in different directions seems to be evinced by this fact.

But short free spaces and slight irregularities at overlapping edges are not entirely responsible for this illusion that we are considering. If a smooth cylinder be put in the lathe and the circumferences of circles be traced upon it by a sharp tool, the illusion may be seen just as well. Indeed the first observation of the illusion was, as will be remembered, made upon a cylinder bearing the continuous coils of a helix. What happens in these cases seems to be that any chance points that are unduly prominent upon the surface get independent paths established upon the retina.

The observation, noted on page 357, in accordance with which vivid illusory lines were seen lying in between the white lines and sloping in the direction of the eyes' movement seems also to find explanation in this general way. The lines are to be regarded as the tracings of some one point or stretch, situated in this case presumably at the overlapping edge.

We may now see, too, I think, why the minimum speeds necessary for producing a good illusion vary with the distances apart of the lines. With a given lateral speed, the nearer the lines the more often will any given set of tracings be reinforced by others coinciding with them. To produce an equally vivid illusion, therefore, with widely separated lines, a similarly frequent coincidence of tracings must be secured by a more frequent reappearance of the tracing points, in order that the first tracings may meet them and be reinforced. This means simply that with a given lateral speed there must be higher rotary speeds when the spaces between the lines are greater. At the same time, too, as remarked above (p. 352), when lines of some considerable separation are used, the same amount of general retinal stimulation can only be reached by higher rates of movement.

It would appear from the above remarks about multiple tracings over the same lines that certain special rates of movement were best adapted to a vivid illusion. Such seems indeed to be the fact. In any event the obverse fact seems true enough, that certain rates are unfavorable. Thus it sometimes happens that the illusory lines do not appear in the geometrical form of the outer field, but are closer together and at irregular distances apart. Tracings seem to have been made everywhere over the field. Here, manifestly, the illusion is not in its perfect form.

When stripes of blue (or red) and white, each about 2 mm. wide, were used instead of lines with relatively wide intervening spaces, the illusory field was made up of badly defined stripes like the objective stripes in width.

All this matter of theory is vague in the extreme, and it may well turn out that the truth lies in an entirely different direction from that in which we have sought it. Nevertheless these two illusions of double motion seem to rest upon variously combined processes of retinal reaction, contrast and what not, which as yet are all too imperfectly recorded and understood. And it is only to be hoped that more and more facts of this sort may be assembled until general principles, at least, become firmly established.