ELEMENTS

THE

PSYCHOPHYSICS

FROM

GUSTAV THEODOR FEEDER.

SECOND UNCHANGED EDITION.

FIRST PART.

LEIPZIG
PRINTING AND PUBLISHING OF BREITKOPF & HÄRTEL
1889.

Content.

Foreword by the publisher
Foreword by the author

Introductory.

I. More General View on the Relationship of Body and Soul

II. Concept and Purpose of Psychophysics.

III. A preliminary question

IV. Conceptual about sensation and stimulus

External psychophysics.

The psychophysical gauge.

V. Measure of physical activity. Living power.

VI. Measurement principle of sensitivity.

VII. Measurement principle of sensation.

VIII. Measurement methods of sensitivity.

1) Measurement methods of difference sensitivity.

a) General presentation.
b) General considerations and precautions.
c) Considerations regarding the time and space of the experiments. Constant error.
d) Special about the method of right and wrong cases.
e) Special to the method of mean error.
f) Mathematical relationship of the methods.

2) Measurement methods of absolute sensitivity.

Fundamental laws and facts.

IX. The Weber's Law.

The own data Weber

1) light.
2) Sound
3) Weights.
4) temperature.
5) Extensive sizes. (Judgment and tact)
6) *Fortune morale et physique* .

X. The fact of the threshold

1) Intensive threshold

a) Threshold b) Difference threshold

2) Extensive Threshold
3) More general considerations concerning the threshold
4) Consequences of the existence of the threshold

XI. Details on the size and dependence of the thresholds in the various sensory areas

1) Intensive threshold

a) Light and color
b) Sound and pitch.
c) Weights
d) Temperature

2) Extensive threshold

a) sense of sight

Special regulations for the smallest identifiable quantities
Special provisions for the smallest recognizable distances.

α) Two Distant Points
β) Two Distant Threads
γ) Streaky and dicey figures

Behavior of the side parts of the retina in recognizing the smallest sizes and distances Distance differences (eye size)

b) sense of touch
c) perception of time and movement

XII. Parallel Law to Weber's Law.

1) weight tests
2) experiences in the field of sensation of light
3) experiments in the field of extensive sensation

XIII. Laws of Mixture Phenomena

Foreword by the author of the first edition.

By psychophysics I mean, according to the explanation given in more detail in the second chapter, a doctrine which, although age-old, yet presents itself as a new one with regard to the formulation and treatment of this problem, that the new name is not inappropriate and not unnecessary, in short an exact teaching of the relationship between body and soul.

As exact teaching, psychophysics, like physics, must be based on the experience and mathematical connection of experiential facts which require a measure of what is required of experience, and, if such is not yet available, seek it. Now that the measure of the physical quantities has already been given, the first and chief task of this writing will be the determination of the measure of the psychical magnitudes, where it had hitherto been absent; the second to address the applications and designs that tie in with it.

It will be shown that the determination of the psychic measure is not a mere matter of the study table or philosophical apercus, but requires a broad empirical basis. These I believe to have given sufficiency here, to the extent that the principle of measure is certain, but so far from the applications, that the utility of this measure will also be recognized. But the empirical basis for the development of the psychophysical doctrine requires great expansion, and what is given by the applications only reveals that without comparison there will be more.

In short, psychophysics, in the form in which it appears here, is still a doctrine in the first state of becoming; Thus, the title of these writing elements is not unjustly understood, as if it were an illustration of the essentials of an already founded and formed doctrine, an elementary textbook; but rather to depict the beginnings of a doctrine that is still found in the elementary state. So do not make any claims to this writing, which are to be made to an elementary textbook. In many cases it gives examinations, explanations, and compilations which would be quite inappropriate in such a case, but which can contribute to making it possible for such a textbook to become possible. What was to be demanded

Just as little as an elementary textbook, one has here to look for a collection of the entire material of psychophysics, but preferably only that which belongs to the foundation of the psychophysical theory of measurement and enters into its applications. Innumerable things that are an object of psychophysics could not find a place here, because it has not yet reached the stage of being able to find a picture in it.

Even though many things in this text are already too much, some are too few, one has cause at any rate to be indulgent in this regard, since formally there was almost nothing, materially only completely scattered, on which I could base myself and call myself; but a house can not be built without bringing stones to it; and where the plan can still be built in front of the house, in the first attempt, everything can not be all right and the right measure. Each successive attempt of this kind will be more complete on one side and shorter and more precise on the other.

It is no less true than with regard to the formal defects that I have to claim the leniency of the errors of fact which may have been left in this work, especially in the treatment of many fine, difficult, and new questions, and more so in what follows as being in this parts. In the long course of these investigations, I have gone through so many erroneous and uncertainties with fixed and steadily established general principles. The whole area was previously buried in obscurity. That I dare not dare to do so with the present editors to have everyone behind me. But I would not be able to give these examinations if I wanted to wait for complete assurance in this regard; and yet have the confidence that,

Lastly, it will only be wondering if what is offered and what is offered here is a durable and fruitful beginning. If you find it, do not take the missing and the mistakes too high; it will at least be their merit to have produced the better.

I am far from saying that what is in this document is something completely new, and it would be a bad recommendation if it were. On the contrary, in order to do justice to just claims for priority from the outset, and at the same time to show that the writing is subject to something more than a subjective idea, I touch upon a few historical points in the preface, which I refer to in its place and finally in a special historical one Chapter closer.

The experiential law, which forms the main base of mental Maßlehre has been already long ago been set by various researchers in various fields and expressed in relative general name of EH Weber, I would even call the father of psychophysics and proven experimental. The mathematical function on the other hand, which is the most common and most important case of application of our Maßprinzips is also already long ago by various mathematicians, physicists and philosophers, such as Bernoulli (Laplace, Poisson), Euler (Herbart, Drobisch), crystal healing (Pogson) for special , to be attributed to psychophysics, cases based on this law and reproduced or accepted by other researchers.

According to this, our psychic measure is in fact only generalization on one side, and on the other the clear statement of what already existed, in its meaning as psychic measure. The reference to this is likely to do something to lessen distrust, which may arouse the announcement of such a measure from the outset. The problem of this is in fact not the problem of squaring the circle or *perpetual motion,* but is actually solved by researchers whose name is a guarantee of the validity of the solution.

Having thought of the merits of earlier explorers concerning the main subject of this work, I would miss a principal duty if I did not wish to commemorate the support and encouragement which I found in my investigation by Volkmann. The willingness of this ingenious and fine researcher to respond to the interests of this investigation, which, by the way, has led him far beyond the demands first made by it, and the growth which has grown into the experiential documents of this document, obliges me in the In fact, thank you.

At the same time, however, I venture to assert it as a favorable sign of the principle and character of the doctrine of this document, that it not only provides support in exact investigations of the most excellent investigators, but also provides points of contact for such. In fact, apart from the theoretical and experimental investigations on which it is based, and which have already attached themselves to it, there has often been sufficient occasion in the course of this document to point to investigations to be made in the future or to be continued, some of which is in the further development of psychophysical measurements are necessary, partly entering into the applications of the same, and, notwithstanding some of great interest, without the view of this doctrine they would not have presented themselves. The psychophysical experiment, finding hitherto only a passing place in the physical, now physiological experimental room, now occupies its own room, its own apparatus, its own methods. Nor is it unquestionable that the more it is cultivated, the wider the field of these investigations will be. And so I look less for the main fruit of our investigation so far in the one she has worn so far than the one she promises to wear once. What is here is a poor beginning of a beginning. that the more it is cultivated, the more the area of these investigations will expand. And so I look less for the main fruit of our investigation so far in the one she has worn so far than the one she promises to wear once. What is here is a poor beginning of a beginning. that the more it is cultivated, the more the area of these investigations will expand. And so I look less for the main fruit of our investigation so far in the one she has worn so far than the one she promises to wear once. What is here is a poor beginning of a beginning.

As to the way in which mathematics is introduced in this text, and will occupy it in particular in the following parts, I wish that mathematicians wish to write these elements for non-mathematicians and non-mathematicians for mathematicians, in the effort to make one understand and to do enough for the others, which did not go off completely without conflict. In particular, mathematicians may excuse so many broad and popular arguments in the interest of non-mathematicians, and I have noticed that this work is likely to interest mainly physiologists, while at the same time they wish to interest philosophers. To see in both but of course also mathematicians, is not allowed today as it was actually required. On the other hand, may the non-mathematical derivations, to which they can not follow, accept mathematical facts, albeit only those of very small demands of mathematical understanding, and here and there overturn a chapter, interjection, or execution that goes too far. If I am not mistaken, everyone will find the course and content of this book as a whole comprehensible, who only knows what a mathematical equation is, and knows the properties of the logarithms, or to the brief recapitulation given in the following part want to hold the same. From others I did not wish them to care about this writing, least of all that they would judge what in no case could be an obvious one. which they can not follow - though only those of very low demands of mathematical understanding occur - as mathematical facts accept, and here and there a chapter, an interposition or execution overturn, which engage a little too deeply. If I am not mistaken, everyone will find the course and content of this book as a whole comprehensible, who only knows what a mathematical equation is, and knows the properties of the logarithms, or to the brief recapitulation given in the following part want to hold the same. From others I did not wish them to care about this writing, least of all that they would judge what in no case could be an obvious one. which they can not follow - though only those of very low demands of mathematical understanding occur - as mathematical facts accept, and here and there a chapter, an interposition or execution overturn, which engage a little too deeply. If I am not mistaken, everyone will find the course and content of this book as a whole comprehensible, who only knows what a mathematical equation is, and knows the properties of the logarithms, or to the brief recapitulation given in the following part want to hold the same. From others I did not wish them to care about this writing, least of all that they would judge what in no case could be an obvious one. - although only those of very low demands of mathematical understanding occur - as mathematical facts accept, and here and there a chapter, an interposition or execution overturn, which engage a little too deeply. If I am not mistaken, everyone will find the course and content of this book as a whole comprehensible, who only knows what a mathematical equation is, and knows the properties of the logarithms, or to the brief recapitulation given in the following part want to hold the same. From others I did not wish them to care about this writing, least of all that they would judge what in no case could be an obvious one. - although only those of very low demands of mathematical understanding occur - as mathematical facts accept, and here and there a chapter, an interposition or execution overturn, which engage a little too deeply. If I am not mistaken, everyone will find the course and content of this book as a whole comprehensible, who only knows what a mathematical equation is, and knows the properties of the logarithms, or to the brief recapitulation given in the following part want to hold the same. From others I did not wish them to care about this writing, least of all that they would judge what in no case could be an obvious one. who get involved too deeply. If I am not mistaken, everyone will find the course and content of this book as a whole comprehensible, who only knows what a mathematical equation is, and knows the properties of the logarithms, or to the brief recapitulation given in the following part want to hold the same. From others I did not wish them to care about this writing, least of all that they would judge what in no case could be an obvious one. who get involved too deeply. If I am not mistaken, everyone will find the course and content of this book as a whole comprehensible, who only knows what a mathematical equation is, and knows the properties of the logarithms, or to the brief recapitulation given in the following part want to hold the same. From others I did not wish them to care about this writing, least of all that they would judge what in no case could be an obvious one.

With diligence, I neglect, in this work, to somehow enter into the opposition which the mathematical conception of the psychological conditions in it will offer against Herbart's. Herbart will always have the merit of having not only first expressed the possibility of a mathematical conception of these relations, but also of making the first astute attempt to carry out such a conception; and everyone after him will only stay second in this regard. In fact, however, the following experiment is so fundamentally subordinated to its divergent basic points of view, that there is no need for a special emphasis on the difference between the two, as it would be idle and inappropriate to attempt here a dispute between the two. especially since such could not take place without a dispute over basic philosophical questions, which should be avoided here at all costs. The decision between the two, which will also be a decision on these basic questions, I have to leave to the future.

Perhaps one expects, in advance, an explanation of the position which this text will take on materialism and idealism and the fundamental religious questions with which every examination of the relationship between body and soul must necessarily come into contact. As far as the first is concerned, this work does not address the dispute over the fundamental relation of body and soul, which divides the materialists and idealists; their explanations and consequences will neither be one-sided in one sense nor the other in that they represent the experiential relations between the two sides of existence through a functional relationship which excludes this one-sidedness on its own.

As for the second, all the conclusions that we are hereby obliged to accept the implications of materialism concerning fundamental religious questions would be premature. It is obvious that especially the introductory brief, if rather the background, as the starting point of the developments of this work, can undergo a one-sided materialistic interpretation and utilization, and with regard to the question of immortality at first seems to have to lead to the same conclusion. But I do not wish to object here, except that this whole work has been based on and in the context of a very opposite interpretation and interpretation of the view which I have expressed in earlier writings, and I must refer to this, if one does Concerns want to follow, since this is not the place to continue to respond.

The present volume of this work contains the documents of the psychic measure, ie the establishment of its principle and the exposition of the methods, laws and facts which are the basis for the experience of the same: the following becomes the psychic measure function itself with its external, internal dimension To develop consequences. According to this, the present one occupies more of a mathematical and philosophical interest, a mathematical one, insofar as the field of new applications, which in the present part opens up for mathematics, is pursued to a certain extent, a philosophical, to the extent that these applications give rise to pertinent points of view concerning the relationship between body and soul.

Leipzig, December 7, 1889.

Foreword by the publisher.

Since the book in question has been sold out in the book trade for several years, the need for a reprinting of it has become ever more urgent. Fechner himself could not decide on a reworking of his main work any more than on an unchanged new edition of the same. He preferred to publish in special writings the investigations and critical discussions that would have to be made in a new edition. He has objectively chosen the right thing, I believe. A work which, like the elements of psychophysics, takes completely new paths of research, will always remain in the original form in which it has its effect, preferably also significant. All the more so was I willing to After the passing of Fechner's the request of the publishing company to comply and take over the publication of a new edition. It was self-evident to me that this could only be an unchanged impression of the first. Only I thought that I should facilitate the use of Fechner's later psychophysical works by giving hints attached to them in appropriate passages. These editors' notes are numbered to distinguish them from the author's asterisks. The main writings to be considered here are: to facilitate the use of Fechner's later psychophysical works by giving hints attached to them in appropriate passages. These editors' notes are numbered to distinguish them from the author's asterisks. The main writings to be considered here are: to facilitate the use of Fechner's later psychophysical works by giving hints attached to them in appropriate passages. These editors' notes are numbered to distinguish them from the author's asterisks. The main writings to be considered here are:

In matters of psychophysics. Leipzig 1877. Abbreviated quotes: In matters.

Revision of the main points of psychophysics. Leipzig 1882. Abbreviated: Revision. Also the last psychophysical work of Fechner:

On the principles of psychic principles and Weber's Law, in: Philosophical Studies, ed. W. Wundt, vol. iv. pp. 161-230; Abbreviated: psychic principles of measure.

The other rarely mentioned works are always listed under their full title.

Of course, the typographical errors and corrections noted at the end of the second volume of the first edition and elsewhere (notably the appendix to "In things") have been taken into account. However, where Fechner later confined himself to general rectifying or supplementary remarks whose introduction to the text would have necessitated an intricate reworking of the text, I believed I had to content myself with a reference to the subsequent correction attached in the note.

The correction of the printed sheets of this edition has been made by Dr. Ing. Oswald Külpe taken over. At the same time, the quotes given in the notes have been re-examined by him and completed several times.

Fechner's admirers, I hope, will be a welcome addition to the list of his numerous writings attached to the first volume. By the goodness of the family I was in the fortunate position of making this list a "Annuarium of the Works and Treatises of Professor G. Th. Fechner" by Dr. Dr. med. med. Rudolph Müller in Dresden, which the same Fechner had presented to celebrate his eightieth birthday. Incidentally, absolute completeness was not achievable with this directory, since numerous smaller works, some anonymous, especially in literary journals, could no longer be ascertained with certainty.

Leipzig, July 31, 1888.

W. Wundt.

Introductory.

I. More General View on the Relationship of Body and Soul.

Whereas the doctrine of the corporeal world has advanced in the various branches of natural science to a great development, and enjoys keen principles and methods which ensure its successful progress, the doctrine of the spirit in psychology and logic is at least to a certain extent firm foundations The doctrine of the relations between body and mind or body and soul has so far remained almost merely a field of philosophical strife without a firm foundation and without certain principles and methods for the progress of inquiry.

The most obvious reason for this unfavorable relation is, in my opinion, to be sought in the following factual circumstance, which, of course, makes one again ask for its further background. The relations of the corporeal world for us can be pursued directly and in connection with each other through experience; the relations of the internal or spiritual world no less; those only as far as our senses and their reinforcing aids reach, these, as far as one's own soul reaches; but in such a way that we are able to obtain basic facts, basic laws, basic conditions in each of the two fields, which can serve as sure documents and starting points for the conclusion and further progress. Not so with the connection of the physical and mental world, in that only two factors, one immediately at a time, enter into direct experience, while the other remains under the cover. For, while we are immediately conscious of our feelings and thoughts, we can not perceive anything of the movements in the brain; which are bound to it and to which they are bound, the bodily remains here under the spiritual cover; and while we may subject the bodies of other men, animals, and all nature directly to anatomical and physiological, physical, and chemical investigation, we can not know anything directly from the souls who belong to the first, and to the god, who belongs to the second; the spiritual remains here under the bodily blanket. And thus the hypotheses and the denial have a lot of room for maneuver. Is there anything under the one and the other ceiling, and what can be found underneath?

The uncertainty, the wavering, the quarreling over these questions of fact has hitherto admitted no firm point of departure and point of attack for a doctrine of the circumstances of which, for the most part, it is still in dispute.

And what can be the reason of this peculiar relation, that we each body and mind for each other, and yet never both, as it directly belongs together; also be able to observe directly together; but otherwise we most easily observe what is directly connected? After the immutability in which this relationship exists between the spiritual and the physical domain, we may assume that it is a fundamental one, grounded in its basic relation. But is there no similar thing that at least explains the fact of it, if not to the bottom of it?

It is true that this and that can be pointed out. z. For example, if a person stands within a circle, his convex side lies completely hidden under the concave ceiling; when standing outside, the concave side reverses under the convex ceiling. Both sides belong together inseparably, as the spiritual and physical side of man and these can be comparatively also as inner and outer side; but it is equally impossible to see both sides of the circle at the same time from one standpoint in the plane of the circle, and from one standpoint in the realm of human existence these two sides of man. It is only as we change our point of view that the side of the circle that we see changes and hides behind the sight. But the circle is just a picture,

Now, it is not the task and purpose of this paper to enter into deeper or somehow penetrating discussions on the fundamental question of the relationship between body and soul. Search Everyone sees the riddle, insofar as it seems to him as such, to solve in his own way. It will therefore be without any binding consequence for the following, if I only here, um a question of the general view, which formed the starting point of this work, and which still forms the background for it, not entirely without answer, and at the same time to offer a clue in this field of fluctuating ideas to those who are looking for it believe to have already found, with a few words on this view, which is nothing essential for the pursuit will be included. In the case of very great enticement, at the beginning of a work like this, to lose himself in extensive and expansive discussions in this respect, and no little difficulty in avoiding them altogether, one becomes at least the brief exposition of the view to which I limit myself below , to apologize.

Previously, a second explanatory example of the first. The solar system is quite different from the sun than from the earth. There it is the Copernican, here the Ptolemaic world. It will always be impossible for the same observer to observe both world systems together, though both are inseparably bound together, and just as the concave and convex sides of the circle are essentially only two distinct modes of the same thing from different points of view. But again it is enough to change the point of view, so the other world appears for one world.

The whole world consists of such examples, which prove to us that what is one in the thing appears from two points of view as two things, and one can not have the same point of view from one point of view as from another. Who does not admit that it is always so and can not be otherwise. Only in terms of the biggest and most striking example is it not allowed or not expired. But that offers us the relation of the mental and physical world.

What appears to you on the inner standpoint as your mind, which you yourself are this spirit, on the external view, on the other hand, appears as this physical substance. It makes a difference whether one thinks with the brains, or looks into the brain of the thinker. [1)]There appears quite different things; but the point of view is quite different, there an inner, here an outer; unspeakably various even, as in previous examples, and therefore just the difference of the modes of appearance is unspeakably greater. For the double mode of appearance of the circle, of the planetary system, is in fact obtained only from two different external points of view; in the midst of the circle, on the sun remains the observer except the course of the circle, except the planets. But the self-appearance of the mind is gained from a true inner standpoint of the being subject to it, to itself, to coincidence with itself, to the appearance of the corporeality belonging to it from a true standpoint external to that of non-coincidence.

[1)] Equivalent to looking at is an adequate idea of inferences based on outward appearances, as the internal state would appear upon clearing away the obstacles of seeing.

Now it goes without saying that we first sought the reason why no one can directly perceive spirit and body as they belong together directly. There can be no one at the same time both outwardly and inwardly opposed to the same thing.

Therefore no spirit of the other mind immediately perceives as spirit, though one should think that he should most easily be aware of the same being; if he does not coincide with him as an other he has only the physical manifestation of it. For this reason, no spirit at all can perceive any other than with the help of its corporeality; for what seems to be external to the spirit is its physical manifestation.

For this reason the mode of appearance of the mind is always at one time only because there is only one inner point of view, whereas every body, according to the multiplicity of the external points of view, and the diversity of those on it, appears in many different ways.

Thus, the previous way of seeing covers the most fundamental relations between body and soul, which every basic view should seek to cover.

One more thing: body and soul go with each other; The change in the one corresponds to a change in the other. Why? Leibniz says: you can have different views about it. Two clocks fixed on the same board set their course for one another through the mediation of this common attachment (if they do not deviate too much from one another); that is the usual dualistic view of the relationship between body and soul. Someone can push the hands of both clocks so that they always go harmoniously, that is the occasionalistic, according to which God creates spiritual and vice versa in constant harmony with the physical changes. They can also be so completely furnished from the outset that they always go with each other exactly, without the need for tutoring; that is the view of the preestablished harmony of them. Leibniz has forgotten a view, the simplest one possible. You can also go in harmony with each other, never even go apart, because they are not two different watches. Thus, the common board, the constant tuition, the artificiality of the first device spared. What appears to the outward observer as the organic clock with an engine and range of organic wheels and levers, or as its most important and essential part, appears inwardly very different from its own mind with the course of sensations, impulses, and thoughts. It must not offend that man is called here a clock. If he is so called in one sense, he should not be so called in everyone. Leibniz has forgotten a view, the simplest one possible. You can also go in harmony with each other, never even go apart, because they are not two different watches. Thus, the common board, the constant tuition, the artificiality of the first device spared. What appears to the outward observer as the organic clock with an engine and range of organic wheels and levers, or as its most important and essential part, appears inwardly very different from its own mind with the course of sensations, impulses, and thoughts. It must not offend that man is called here a clock. If he is so called in one sense, he should not be so called in everyone. Leibniz has forgotten a view, the simplest one possible. You can also go in harmony with each other, never even go apart, because they are not two different watches. Thus, the common board, the constant tuition, the artificiality of the first device spared. What appears to the outward observer as the organic clock with an engine and range of organic wheels and levers, or as its most important and essential part, appears inwardly very different from its own mind with the course of sensations, impulses, and thoughts. It must not offend that man is called here a clock. If he is so called in one sense, he should not be so called in everyone. You can also go in harmony with each other, never even go apart, because they are not two different watches. Thus, the common board, the constant tuition, the artificiality of the first device spared. What appears to the outward observer as the organic clock with an engine and range of organic wheels and levers, or as its most important and essential part, appears inwardly very different from its own mind with the course of sensations, impulses, and thoughts. It must not offend that man is called here a clock. If he is so called in one sense, he should not be so called in everyone. You can also go in harmony with each other, never even go apart, because they are not two different watches. Thus, the common board, the constant tuition, the artificiality of the first device spared. What appears to the outward observer as the organic clock with an engine and range of organic wheels and levers, or as its most important and essential part, appears inwardly very different from its own mind with the course of sensations, impulses, and thoughts. It must not offend that man is called here a clock. If he is so called in one sense, he should not be so called in everyone. spared the artificiality of the first device. What appears to the outward observer as the organic clock with an engine and range of organic wheels and levers, or as its most important and essential part, appears inwardly very different from its own mind with the course of sensations, impulses, and thoughts. It must not offend that man is called here a clock. If he is so called in one sense, he should not be so called in everyone. spared the artificiality of the first device. What appears to the outward observer as the organic clock with an engine and range of organic wheels and levers, or as its most important and essential part, appears inwardly very different from its own mind with the course of sensations, impulses, and thoughts. It must not offend that man is called here a clock. If he is so called in one sense, he should not be so called in everyone. that man is called here a clock. If he is so called in one sense, he should not be so called in everyone. that man is called here a clock. If he is so called in one sense, he should not be so called in everyone.

But the difference of a phenomenon does not depend only on the difference of the standpoint, but also on the diversity of those who stand on it. A blind man does not see anything from the outside at the same favorable external point of view as a sighted person; and so does a dead watch, in spite of its equally favorable point of view of coincidence with itself like a brain, see nothing from within; she is only there for the external appearance.

Natural science consistently places itself on the external viewpoint of the consideration of things, the science of the spirit on the inner; the views of life are based on the change of standpoints, the philosophy of nature on the identity of what appears twice on two points of view; a doctrine of the relationship between mind and body will have to trace the relationships of both modes of the one.

These are the basic points of view, by which I do not seek to elucidate both the ultimate fundamental nature of the body and mind, as the most general factual relations of the same under a single point of view.

But, as I have said, it remains clear to anyone through which other view he tries to do the same, or even if he wishes to do so at all. What everyone finds most appropriate in this respect will depend on the context of his other views; and, of course, even backwards, justify the possibility or impossibility of finding a suitable general connection between them. But here it will matter in the first place whether he conceives body and soul only as two different modes of appearance of the same essence, or as two externally brought together beings, or the soul as a point in a nexus of other points of essentially equal or unequal nature, or wants to do without a uniform basic view at all Insofar as only everyone recognizes the experiential relations between body and soul and permits an experiential pursuit of them, he may try the most forced representation of them. For only in the experiential relations between body and soul will we proceed in the following, and moreover use ourselves to designate the actuality of the most ordinary expressions, which are held in the sense of a dualistic rather than a monistic view, even if a slight translation permits.

This is not to say that the doctrine that will develop here will be of no consequence to the conception of the fundamental relation between body and mind and without influence on it, on the contrary. But one confuses the consequences, which may someday flow from it, and partly begin to shape, not with a document of this doctrine. This document is in fact purely empirical and must be rejected from the outset.

Is it not possible to ask, is the possibility of such a document in direct contradiction with the fact that we assume that the relations between body and soul are beyond the comprehension? But they are not the experience in general, but only the immediate relations of immediate experience are removed. Our very conception of the general relation between body and soul was based on experiences of the most general kind, which can be made about their relation, even if it does not appear to everyone who comes to this writing with firm presuppositions as the necessary expression of it. The result will show that we have no less special experiences to offer, which can partly serve to orient ourselves in the field of indirect relations, and are partly suitable.

In fact, it could not be done with that general view, even if accepted. The assurance, fertility and depth of a general view does not depend on the general but on the elemental. The Law of Gravity and Molecular Laws (which indisputably include the former) are elementary laws; If they were thoroughly known, and exhausted in their implications, the doctrine of the corporeal world would be perfected in the greatest generality. Accordingly, it will be necessary to gain elementary laws for the relationship between the physical world and the spiritual world in order to gain a durable and developed doctrine instead of a general view; and here and there they can only be based on elementary facts.

Psychophysics is a teaching that has to be based on these points of view. The details of this in the following chapter.

II. Concept and Purpose of Psychophysics [1)].

Psychophysics is understood to mean an exact doctrine of the functional or dependent relationships between body and soul, more generally between the physical and mental, physical and psychological, world.

[1)] Revision p. 1-17.

To the realm of the spiritual, the psychic, the soul, we generally count that which is comprehensible or abstractable from inner perception, to that of the physical, physical, physical, material, that which is comprehensible by external perception or can be abstracted from it. This is merely to designate the areas of the world of phenomena, with whose relation psychophysics will have to occupy itself, assuming that one knows how to relate inner and outer perception to activities in the sense of ordinary usage, whereby existence becomes manifest at all ,

All discussions and examinations of psychophysics refer in general only to the appearance of the physical and mental world, to that which either appears directly through inner or outer perception, or can be deduced from the phenomenal, or as relation, category, context, sequence, law of It is apparently comprehensible; briefly on the physical in the sense of physics and chemistry; on the psychic in the sense of the empirical soul doctrine, without somehow returning to the essence of the body, the soul behind the phenomenal world in the sense of metaphysics.

In general, we call this the psychic function of the physical, dependent on it, and vice versa, insofar as there is such a constant or legal relationship between the two, that one can infer from the existence and the changes of the one to the other.

The fact of functional relationships between body and soul is generally undisputed, while there is still an undecided dispute over the reasons, interpretation, and extent of this fact.

Regardless of the metaphysical aspects of this dispute, which rather refer to the so-called essence as the phenomenon, psychophysics tries to determine the actual functional relationships between the phenomena of body and soul as accurately as possible.

What belongs quantitatively and qualitatively, far and near, in the physical world and the spiritual world together, according to which laws do their changes follow one another or do they go together? These questions are generally speaking psychophysics and seeks to answer them exactly.

To put it another way, but only saying the same thing: what belongs together in the inner and outer appearance of things, and what laws exist for their relative changes?

Insofar as there is a functional relationship between body and soul, nothing in itself would prevent anything from looking at it in the one direction and in the other direction, and following what can be appropriately explained by the mathematical functional relationship that exists between the variables x and yAn equation exists where every variable can be arbitrarily regarded as a function of the other, and the same in its changes depends on itself. One reason for psychophysics, however, to prefer the pursuit of the side of the dependence of the soul on the body from the opposite, lies in the fact that only the physical is directly accessible to the measure, whereas the measure of the psychic can only be obtained as a function of it will be shown later. This reason is crucial and determines the direction of the aisle in the following.

The materialistic reasons for such a preference are neither in language nor validity in psychophysics, and the dispute between materialism and idealism, as going on the relations of dependence of the one on the other in the essence, remains to her as mere relations of appearance, alien and indifferent.

One can distinguish immediate and indirect dependency relationships or direct and mediated functional relationships between body and soul. Sensory sensations are directly dependent on certain activities in our brains, as long as the others are or are in direct consequence of one another; but only indirectly from the external stimuli which these activities produce only through the intermediary of a nervous conduction to our brains. Our whole mental activity is immediately dependent upon, or directly implies, an activity in our brains, or immediately causes it, but from which effects pass on to the external world through the mediation of our nervous and locomotor organs.

The mediated functional relations between body and soul fulfill the concept of functional relationship only insofar as one considers the mediation in the relation with in detail, since when the mediation ceases, the constancy or legality in the relation of the body and the soul falls away Access to the mediation exists. Thus a stimulus triggers sensation only insofar as it is not lacking in living brains, nor in living nerves, which transplant the action of the stimulus to the brain.

Insofar as the psychical is regarded as a direct function of the physical, the physical can be called the carrier, the underlay of the psychic. Physical activities which are carriers or underpinnings of psychic, and thus in direct, functional relation to them, are called psychophysical.

The question as to the nature of the psychophysical activities, ie, about the substratum and form of them, is left undecided from the beginning, and no prerequisite is given. In the first place, it can be abstracted from this for a twofold reason, for one thing, because in determining the general foundations of psychophysics, it will be just as much a matter of quantitative relations as in physics, where qualitative relations are first made dependent on the quantitative; secondly, because, according to the following division of our doctrine in the first part of it, we have no particular regard for the psychophysical activities at all.

By nature, psychophysics divides itself into an external and an internal one, depending on whether the relation of the spiritual to the physical external world or the bodily inner world with which the spiritual is closely related, or otherwise, into one Doctrine of the indirect and immediate functional relationships between soul and body.

The basic experiences for the whole of psychophysics can only be sought in the field of external psychophysics, as long as this is accessible to immediate experience, and the result must therefore be taken from external psychophysics; but this can not develop without constant consideration of the inner, considering that the physical outer world is functionally linked only by the intermediate effect of the bodily inner world with the soul.

As long as we still stand in the consideration of the legal relations between external stimulus and sensation, we must not forget that the stimulus does not immediately arouse sensation in us, but only through the awakening of any bodily activities in us that are more sensitive to sensation Relationship. Their nature may still be completely unknown, but the question of this nature is at first left entirely open, as it is supposedly to be done by us; but their fact must be established, and more often resorted to this fact, when it is necessary to properly consider and persecute those legal relations themselves, which we are concerned with in outer psychophysics. So we become, even if the bodily activities which are directly subject to and follow our will activity, are still completely unknown, must not forget that what is worked by the will in the external world, but only by such activities by him is affected. And thus everywhere in thought the unknown middle link will have to intervene, which is necessary to complete the chain of effects.

The psychology and physics already related by name, on the one hand psychophysics on the one hand based on psychology and promises on the other hand, to provide the same mathematical documents. From physics, external psychophysics borrowed aid and method; the inner one is based on physiology and anatomy, especially of the nervous system, and presupposes a certain acquaintance with it. Unfortunately, of all the laborious, exact, and valuable investigations in this field, which the modern age has brought, the advantage for internal psychophysics is yet to be drawn, which, indisputably, will one day have to be deduced from those investigations and studies from another point of attack conducted investigations on which this document relies, to the point of encounter where they are able to fertilize each other. That this is still little the case is only the imperfect state in which our doctrine is still found.

The point of view from which we will attack the teaching here is this.

Before we are given the means to ascertain the nature of bodily activities which are directly related to our mental activities, the quantitative dependency relationships between the two can be determined up to certain limits. Sensation depends on the stimulus; a stronger sensation depends on a stronger stimulus; the stimulus, however, only affects sensation through the intermediary of an internal bodily activity. Insofar as legal relations between sensation and stimuli can be found, they must include legal relations between the stimulus and this internal bodily activity, which into the general laws of how physical activities evoke one another, to enter into general conclusions about the circumstances of this inner activity. In fact, the consequence will show that, despite all our ignorance of the details of psychophysical activities, and of the circumstances of these, which are relevant to the more important conditions of the universal psychic life, certain and certain ideas are already limited to fundamental ones To justify facts and laws that spill over from external psychophysics into the inner one.

But apart from this significance for internal psychophysics, the legal conditions that can be established in the field of the external have their importance for themselves. On the basis of this, as we shall see, the psychical measure results for the physical, and to this measure can be found applications which, for their part, are of importance and interest.

III. A preliminary question.

If all the dark and contentious questions of internal psychophysics-and almost all internal psychophysics consists of such questions at the present time-are to be postponed with it until the experiential gait provides the means for its decision, then one of them becomes the one Prospects of psychophysics in general, at least to be touched shortly in order to answer them as far as they can be answered from a general point of view, and to refer to the rest of the episode.

If we characterize thinking, wanting, the finer aesthetic feelings as higher spiritual, sensual sensations and impulses as lower ones, then at any rate here - the question of the hereafter we leave quite openly - the higher mental activities just as little can take place as the lower, without to carry out physical activities or to be bound to psychophysical activities. No one can think with a frozen brains. Nor is there any doubt that a certain sensation of the face, hearing-sensation, can only take place according to certain activities of our nervous system; this, too, is not doubted; indeed, the concept of the sensual side of the soul is based on the fact that it stands and goes in exact conjunction with corporeality. But the more is doubted whether every definite thought is bound to a movement so determined in the brain, and not rather an active brain in general sufficient for thinking and the higher mental activities in general, without requiring a special kind and direction of bodily activities in the brains, to go in certain kind and direction of Statten. Yes, the essential distinction of the higher of the lower spiritual realm (distinguished by some as spirit and soul in the narrower sense) is sought in this very fact. to go in certain kind and direction of Statten. Yes, the essential distinction of the higher of the lower spiritual realm (distinguished by some as spirit and soul in the narrower sense) is sought in this very fact. to go in certain kind and direction of Statten. Yes, the essential distinction of the higher of the lower spiritual realm (distinguished by some as spirit and soul in the narrower sense) is sought in this very fact.

Suppose, however, that the higher mental activities were really relieved of a special relation to bodily activities, yet the general relation of them, which is to be recognized as real, would be subject to contemplation and investigation by internal psychophysics. For this general relation will in any case be bound up with general laws and include general conditions which will be ascertained; yes, these may always remain the most important task of internal psychophysics. And one of the next chapters (V) will lead us to such circumstances.

I want a picture: may the thought itself be involved in the flow of physical activity, and be it only by means of this action, or may it only require the river, like the oarsman in the aftermath, to steer over it, and with the oar indifferent waves beat; in both cases the relations and laws of the river are taken into account when it comes to the flow or progress of thought; in both cases, of course, from a very different point of view. Even the freest shipping is subject to laws that relate to the nature of the element and the means that serve it. So psychophysics, in any case, will be related to the relation of the higher spiritual to the physical foundation; but from which point of view and up to what limits,

May everyone limit the idea and the scope of internal psychophysics so far and for so long as the compulsion and the band of facts does not compel it to abandon the restriction. According to my belief, which is claimed for now only as a belief, there is no limit in this regard.

In fact, I consider that the sensation of harmony and melody, which indisputably bears a higher character than that of the individual tones, requires the ratios of the same numbers of vibrations as a basis, which individually depend on the individual sensations, and that they are only in exact connection with the way they can come together and follow each other; It seems to me, then, that there is only a hint of a higher, but not lacking, special relation of dependency between a higher spiritual and a physical basis, and that everything agrees with this suggestion, which is easy to elaborate and broaden. But neither the execution, nor even the assertion of the same is in our business here in the beginning.

IV. Conceptual about sensation and stimulus.

In spite of the incompleteness of psychophysical examinations, the enumeration, definition, and classification of all psychic states, which may once be the subject of it, would have little use. First and foremost, we will deal with sensual sensations in the ordinary sense of sensation, using the following distinctive nomenclature.

I will distinguish intense and extensive sensations according to the sensible conception of something whose magnitude can be understood as intense or extensive; For example, to the intense sensations the sensation of brightness, to the extensive sensations, the conception of a spatial extension with face or touch, and accordingly will also distinguish the intense and extensive magnitude of a sensation. If one object appears brighter to us than the other, then the sensation it gives means to us, when it seems greater to us than the other, to be intensely greater. This is only a matter of definition, and, as generally understood, does not presuppose any particular measure of sensation.

In all sensations, both intensive and extensive, we can distinguish size and form; except that in the intense, the magnitude of frequent strength and the form of quality is called. In the case of the notes, the height, though conceivable as the quality of the note, has a quantitative side, as far as we can distinguish a greater and less height.

Eber Weber undoubtedly distinguishes very well the faculty or the sense in which we receive extensive sensations according to the linguistic usage assumed here, or the sense of space as a general sense of the senses, which give us intensive sensations, as specials, if the former sensations do not exist like the latter by impressing on individual, independent nerve fibers or their respective branching circles (sensory circles), but only by a co-ordination of impressions on several, whereby not both the strength and quality of the impressions, than the number and arrangement of the same or the circles of nerve branches on which they occur is essential to the size and form of the extensive sensation. His arguments about this [1]are very well suited to contribute to the clarity of the general conditions of the senses; but in the first place it may be sufficient to have pointed to the difference just noted in the circumstances on which intense and extensive sensations depend; How, indeed, these brief preliminary discussions are merely intended to initiate the discussion of the measurements to be given to sensitivity and sensation, and therefore do not go further into the doctrine of sensations than is required for this purpose.

[1] Reports of the Saxon Soc. 1853, p. 83; in excerpt in Fechner's Zentralbl. 1853. no. 31st

The various natures and the various relations of dependence of the extensive and intense sensations require a special investigation of their laws. It might be thought that the magnitude of the extensive sensation or the extensive magnitude of the sensation in a corresponding manner, according to the same law, depends on the number of irritable sensory circles, as that of the intense sensation on the intensity of its irritation; but neither can this be presupposed from the beginning, nor has it been proved so far. Our future investigations and information will preferably, if not exclusively, refer to the intense sensations, and to understand them in terms of sensations par excellence, unless the opposite of the accompanying prefix illuminates extensively or out of the context of itself.

Next to the distinction of extensive and intense sensations is to commemorate the distinction between the objective sensations and the common feelings, the so-called positive and negative sensations. Objective sensations, such as the sensations of light and sound, are those which are related to the existence of an external source of excitement, whereas the modifications of the common sense, such as pain, pleasure, hunger, thirst, are felt only as the conditions of our own body become. Weber's classical investigations into tact and sense of the senses are also to be looked into in this relation.

As positive and negative sensations one tends to oppose the sensations of heat and cold, pleasure and pain, which have the common, that the mode of their excitement, or the relation to what excites them, includes an antithesis, by the feeling of coldness Deprivation of heat, like that of heat through the increased absorption of heat, arises and grows, the sensation of pleasure with a striving for the cause that excites it, as unpleasure is related to a counter-striving.

However, one can not ignore the fact that the so-called negative sensations, taken psychologically in themselves, have nothing negative, not a defect, a less of sensation, a removal of sensation because they are just as violent or even more violent than the so-called positive ones, and can express or carry as strong positive bodily effects, as the sensation of frost shakes the whole body, the screams of pain, and otherwise vivid bodily Can cause utterances.

The term stimulus is to be referred in the narrower sense only to the physical means of revival, the stimulus of intense sensations. Insofar as they belong to our bodily external world, they are external stimuli; insofar as they belong to our bodily inner world, they are inner stimuli. The concept of the former is to be explained by the presentation of external stimuli, such as light, sound, and the concept of the latter is yet to be clarified more precisely, and perhaps finally, to a certain extent, eliminated. Noise in the ear can be caused by the external action of the air vibrations that a waterfall sends into our ears. A similar noise can occur without any external effects caused by causes in our body. They are generally unknown; but insofar as they produce the equivalent of the action of an external stimulus,

Should the soul be stirred by external and internal stimuli only to the extent that its effects have reached a certain point of the body, then all sensations, insofar as they are dependent on the body, would only be consequential effects of bodily movements, and thereafter themselves the most inward bodily conditions of sensations come under the concept of stimuli. Whereas in the case of the boundness of the sensations of essentially accompanying, bodily movements in functional relation to it, it would not be permissible to include among the stimuli such simultaneous conditions of the sensation, with which the sensation is directly set, but only such Those who themselves serve to produce them, one does not want to mix different things. In the meantime, we need not yet decide between the two views, and the differing conception of inner stimuli has no influence on our factual considerations, as long as we limit the existence and magnitude of the internal stimuli only to their equivalent effect with external ones Accept stimuli and take into account. At first they are unknown to us in terms of their location and their quality$x$, which nevertheless enters into the appearance circle with a definite quantitative effect, comparable to external stimuli, and receives its name and value according to it.

Much of what one would be afraid of in ordinary life, to understand under the name of attraction, we shall not hesitate to take under it, as z. Weights, for example, inasmuch as they depressively cause the sensation of pressure or elevate the sensation of heaviness. On the other hand, a transference of the word stimulus to the causes, which causes extensive sensations in us, would be unfortunate, especially as there is still little clarity about these causes. Even without access to external causes, in the closed eye we have a black-filled field of vision of a certain extent, and even without contact with compass points or the like, we can become aware of a certain extent of our body surface when we pay attention to it. What's outside, partly marks boundaries in this field of sensation already given by nature, partly it determines forms, partly there is support for proportionate estimates of size and distance, without first producing the sensation of extension itself. This seems innately grounded in the combination and organic connection of nerves, and their central endings, although nothing definite has yet been decided. If, according to this assumption, one really wanted to speak here of stimulus at all, then only the coordination of the internal excitement of these nerves could be brought to bear. But since they are probably simultaneous conditions of sensation, the expression would become unrealistic again. Also, what some people put weight, the experience with the help of movements contribute to the expansion estimation. But it would be out of place to go further into this still rather obscure subject, where there are only linguistic determinations.

Regardless of this obscurity and the question of how the word "stimulus" somehow finds any place here, it can be said that the magnitude of the stimulus in intense sensations is represented by the number of active sensory groups contained in given points the perceived extension decreases and increases in proportion to it, so that, in terms of quantitative relations of dependence, this number, together with the size of the stimulus, can be taken under a common, though only very general, viewpoint for both sensations; but without being able to assert that either the law of dependency is the same or both, or that the extensive magnitude of sensation can not be dependent on circumstances other than that number,

Upon the action of most external powers, on the basis of which sensation depends, the sensation, after it has become noticeable at all, increases continuously in the same sense with the strengthening of the acting potency, and, with its weakening, continuously drops into the imperceptible. But with regard to some, such as heat and pressure on the skin, the organism is so arranged that, on the contrary, sensation arises only in proportion to the difference of a given mean or usual action, such as ordinary temperature, ordinary atmospheric pressure, and so, but with different character, as a sensation of warmth or coldness, pressure or tension, increases as one increases the action above this degree, or lower it below that degree.

Insofar as the effects of stimulus and sensation are considered in the following, the stimuli are always presupposed as really acting, and indeed as acting under comparable circumstances, unless the contrary is expressly stated or is self-evident from the connection. But comparability can be abolished by a different mode of applying the stimuli, as a different state of the subject or organ in which the stimulus meets the same, with which the concept of a different sensibility is related, of its concept and measure in the sixth chapter the speech will be.

For the sake of brevity one speaks of a stimulus that stimulates a sensation, as well as a stimulus difference that carries a sensory difference; it is felt, stronger or weaker, according as the sensation, the difference in sensation is stronger or weaker, a mode of expression of which we also agree to be able to serve without misunderstandings.

External psychophysics.

The psychophysical gauge.

V. Measure of physical activity. Living power.

No stimulus acts as a carrier; on the contrary, many stimuli, such as light and sound, are immediately comprehensible as movements; and if this is not true of others, such as weights, odorants, and taste-stimuli, we may presuppose that they produce or alter sensation only by evoking or altering any activity in our body, and thus, in their greatness, represent the greatness of physical, with sensation-related activities, which are in any relation of dependence to it.

However, without occupying ourselves here with the special dimensions of the various stimuli and thus stimulatory bodily activities, but, as far as such exists, presupposing such knowledge from physics and chemistry as known, we want to divide the general measure of bodily activity into a few relevant ones To enter into discussions.

Even in ordinary life one sets a certain measure of the size or strength of a bodily activity, and seeks this partly in the rapidity of the completed movements, partly in the size of the moving mass, but without having more definite ideas about it. To begin with, it seems most natural, as a measure of the magnitude of an activity, of the product of the size of the mass moved to the speed with which it is moved. ie the quantity of movement to accept. In fact, at the moment of shock, and in general at the communication of the movement, the velocity which the impacted body assumes, or the magnitude of the mass which can be given a given velocity, is proportional to the quantity of movement of the abutting body, and if one were to regard this effect as determining for the magnitude of the activity, one would be able to find a measure of it in the quantity of the movement. It is undeniable that this depends on the definition of physical activity. In the meantime, if one wants to grasp such in the sense as is grasped in exact physics, mechanics, physiology, and even in ordinary life, it is not the quantity of motion that can serve but the living force as a measure of bodily activity.

The living force of which we are speaking here is in no way to be confused with the life force of the philosophers, but a sharp concept of meaning of the following meaning.

The kinetic energy of a material particle, whether or not combined atomistic atomistic, is obtained by its mass *m* with the square of its velocity *v* multiplied, so that the expression of the living force with respect to particles mv^2 is [1]. The living force of an entire system is then the sum of the living forces of its particles, that is, a system of three or more particles with the masses m, m', m''.... and velocities v, v', v''. ..

$$= mv^2 + m'v'^2 + m''v''^2 \ldots ,$$

which is short for any number of particles through

$\sum mv^2$

wont to express it is only to be careful that the sum sign $\sum$ does not mean a summation of several equal products mv^2, but as many different products as there are particles of different mass and speed.

[1)] Strictly speaking, in mechanics only half of the product *mv2 is understood* to be living force of the particle; but some also apply the name to the whole product, which I likewise do here for the sake of convenience, since this different use understandably has no influence on the circumstances which depend on the living force, but only change the unity of the same.

Without wishing to go into the more profound reasons for the introduction of this concept of measurement, a few more obvious ones can be cited.

According to the whole spirit of mathematical theory of motion one must designate oppositely directed velocities of opposite sign; and it follows that, if one wonders what sum of action has been developed within a given time in a system whose particles are in vivid vibrations, this sum of activity would find noticeably null, given the quantity of energy To make movement the measure of activity, since the speeds of the reciprocating movements, by their opposite sign, give with the ever-positive mass products which compensate for the summation; which would not be appropriate, provided that as much power is needed for the onward movements as it is for those who go there;

Secondly, by measuring the physical activity by the living force, one does nothing other than measure it by the bodily performance or work that can be done, thereby relating and relating to the concepts of daily life and practical mechanics occurs. A man, a machine, has worked twice or three times as much, according to the usual concepts of labor, when he has raised a given weight to twice or three times its height; and if he does a different kind of work than lifting weights, then one can always reduce them in this way work, in order to have a comparable measure for it.

Now, according to known laws, the height reached by a stone thrown vertically upward, apart from the drag, does not grow in proportion to the simple speed given to it at the moment of the throw, but to the square of that speed, and consequently to the living force. which is given to him at the moment of the litter. The same speed given to it when thrown at once (or rather in very rapid increments) is given to it by gradual increases in the slow uplift, and so the height of the lift as well as the height of the throw depend on the magnitude of the living force which gives it Stones, more generally a load, a weight in which direction is planted against gravity, or inhabits by itself.

A person, in order to climb a mountain, apart from incidental circumstances, must produce so much living force in upward motion himself, as would be necessary to raise his weight to that height.

And so, quite generally, the living force which a body of given mass possesses in a given moment, as, incidentally, its velocity is directed, represents a certain height, which will attain this or an equal mass by virtue of the same velocity over a given point, if the same speed at this point would be thought to be planted against the direction of gravity. And, indeed, what to consider, on the assumption that the previous force, which implanted the mass of the velocity, ceased, and no new force effect except the directly counteracting constant gravity.

In the case of an upward throw or the elevation of a load in empty space, it is merely the counteraction of gravity, which gradually deprives the body of the speed once it has been generated, until finally, when a certain height has been attained, all speed is withdrawn, beyond which point accordingly Performance can not go. But instead of or in conjunction with the counteraction of gravity, the resistance of elasticity, of friction, of the so-called resistance of means, or of any other resistance-and in any capacity it is necessary to overcome resistance-can give the same result Counteraction of severity; but in this way every overcoming of a given resistance, and thus every power of the lifting height or throwing height of a given load, can be compared by means of a given living force in empty space. Every achievement is equal in size, for the achievement of which an equally great living force is used and consumed.

If we thought of a body moving in empty space without the resistance of a remedy and the counteraction of a force, then it would, by virtue of the speed once acquired, and thus living force, fly away to infinity without any reduction in speed, and no living force would be consumed. This is called a movement, but not a power, which always presupposes the overcoming of a counteraction and a proper consumption of living force. But the living power of this body remains the measure of achievement which it would be capable of producing, and such a counteraction would take place. For many services, eg. B. the uniform course of a car by the horse, the same size of the living force persists; but only because

Living force can develop in a system through the interaction of its parts, so in the planetary system, in every organism; - transmitted and propagated through communication and propagation of the movement; so at the throw of a stone; in the propagation of the movement through fixed and liquid means; - finally, the internally generated effects are modified by external influences; thus the living force which the system of two two world-bodies produces by their interaction, by the action of a third; so the inner living force of a living organ through every external stimulus.

Finally, as far as we are able to trace it, not only all creation, but also transference, reproduction, and modification of living force have their cause in the interaction of the parts. When a hand throws the stone, the living force that is implanted in it arises through organic interactions, and transplants itself to the stone through an interaction between its parts and those of the hand; and every propagation of the movement is based no less on the interaction of the parts.

All of nature is a single, interconnected system of interacting parts, but in which different partial systems generate, use, and transfer the living force under different forms, while respecting general laws, whereby the connection is governed and maintained. Insofar as all physical processes, activities, processes, whatever their name may be, are reduced in the exact natural theory, the chemical, the imponderable, the organic not excluded, reduced to motions, be it larger masses or smallest particles, all can also scale to find their vitality or strength in the living force, which, if not everywhere, is directly measurable, but dependent on effects dependent thereon, at least everywhere in principle.

The vagueness in which we find ourselves from the outset of the nature of bodily processes, on the formation of which our sensation depends, and which go along with our thoughts, in short the psychophysical activities, does not, in any case, bring with it an indeterminacy about the extent to which we have to create it. If they still find themselves under the physical place, the measure by the living force also finds its place; if they do not fit into it, do not approach us here.

This is important from a dual point of view, once it gives us a basis of clarity, secondly, as long as it provides us with a foundation of legality on which to build.

Without knowing the peculiar nature of psychophysical activities, we know what we have to understand by their greatness in order to keep psychophysics in a clear relationship with physics, physiology, mechanics, ordinary life general universals and laws of living force establish valid conclusions. But insofar as there can be a doubt as to whether the psychophysical activities do not escape this universal validity, the investigation itself must be directed towards this.

Consider, therefore, here some of the most important general conditions and laws of living force, which provide a clue to this investigation, or permit otherwise obvious applications to our field.

A system may seem to be calm, yet develop a very great living force in imperceptibly small movements, which, by virtue of the transferability and viability of the living force into various forms, are often only the revenue of great powerful movements.

If a heavy bell is struck, one does not see their little trembling. Yet the living force of these tremblings (including those with generated heat vibrations) represents the whole living force of the blow that fell upon them; and if one wanted to sum up the reciprocal movements of the same in one direction, it would thereby be thrown off a good deal.

Apparently a very insignificant or none at all, but in reality an undoubtedly very great, living force is developed in the act of chemical compounds. We notice no noticeable movements; but the phenomena of light and heat that take place, based on the vibrations of the ether, allow us to suppose that even the weighable particles in the act of this connection enter into lively vibrations, which are communicated or communicated to the ether. Now, as the living force of the blow may seem to disappear in the invisible ringing of the bell, so, conversely, the living force of imperceptibly small trembling may, by appropriate mediation, turn into powerful visible motions.

Thus the whole living force of the rolling steam-van is only a turnover of the living force of the imperceptibly small tremblings caused by the combustion process in the fuel (including the ether which penetrates it), from there to the parts of the engine, and thence to the Cart have been transferred. And what here comes to light in visible movements disappears in the realm of the invisible movements of the heating material, with which the continuing maintenance and development of the heating process by new material and constant train is necessary, he himself should keep going. Even without the addition of the machine and the car, it would be necessary to do so by weakening the vibrations themselves by communicating to the environment, radiation into the surrounding space;

So also is the living force of the visible movements which man externally performs with his arms and legs, nothing else than a turnover or a resultant of the living force of the little inner movements produced by the chemistry of the nutritional process. To every outward achievement man consumes something of this internally developed living force; for the living force which the bodies set in motion escapes him, and even without visible movement he loses it continuously through communication to the outside world, excretions, charisma, what makes a continuous replacement by the nutritional process necessary, is the organic machine keep going.

Just as the living force of the imperceptibly small trembling may not be neglected against the invisible movements, but rather forms a major part of the living force of the world, the living force of movements in the sphere of the unpredictable can not be neglected against those in the area of the weighable, but forms itself a major part of the living power of the world, and has itself a major share in the processes and achievements that we perceive in the area of the weighable, by virtue of the feasibility and transferability of the living force from one area to the other.

For though we have to assume the mass of the etheric particles to be almost vanishingly small, it is not nothing, and is compensated to an extent by an unspeakably great speed, which we have to settle from another side in its vibrations, yet a great living one Force developed in these vibrations and in the transfer to the weighable a significant performance can be achieved.

The living force experiences in the act of transference from one body to the other, from one part of one system to another, whether weighed or not, by impact, by friction, resistance of the means, as much as the form in which it occurs , thereby being changed, neither propagation nor diminution.

Apparently, with every bump, every friction, through every resistance, living force disappears: the living force of all the stones that fall to earth seems to have disappeared; the living force of a vibrating string is diminished by the resistance of the air; Under the influence of friction on the ground, a cart in motion would not be able to maintain its living power unimpaired, unless the draft animal continued to inflict new increments, which itself must grow with the progress of the feeding process.

But all the living force that is lost here for the visible movement is found in invisible trembling of weighable and imponderable parts. The latter corresponds to a certain generation of heat, so that the entire loss suffered in the act of impact, friction, etc., by living force on the part of the weighable parts is covered by a definite and determinable equivalent of heat, through the proper use of which Quantum of living force in the area of the weighable, through whose disappearance the warmth arose and could be produced again. Yes, this is one of the most compelling reasons to deduce the heat phenomena from vibrations of a substrate, which is not incomparable with weighable substrates,

One undisputed, popular account of the principles of the important doctrine of the mechanical equivalent of heat contains the following essay by Baumgartner: "The mechanical equivalent of heat and its importance in the natural sciences." A lecture delivered at the solemn meeting of the kaiserl Akad. on May 30, 1856 "in Grunert's Arch. f. Math. 1858 p. 261; from which I borrow here some places. In this case, the working unit is assumed to be 1 pound of pound, that is, the work rate through which 1 pound is lifted 1 foot, and the heat quantity is the quantity of heat which 1 pp of water can bring to 0 ° to 1 ° C.

"Consumption of a given quantity of heat also produces a certain amount of work and vice versa, and according to the results of numerous attempts made with all precautions, some work has been done in heat, some in heat and where heat is applied The most diverse origins had to do with the consumption of a heat unit of 1367 working units and vice versa, which is based on Austrian measures and weights. "

"Translated into the language of common life, this means: The tub, which heats 1 p. Water from 0 ° by 1 °, exerts the same mechanical force as a weight of 1367 pounds falling 1 foot high."

"The conversion of heat into work, and vice versa, is not a matter of mood or coincidence, but of certain rules that express the conditions under which the change takes place, for heat can only be transformed into work when it is put into a body This happens, however, with conducted heat only in the direction from the warmer body to the colder and only insofar as temperature differences exist, but the supplied heat decomposes into two parts, one of which serves to increase the temperature at a constant volume, the other does work For example, by putting a load in front of him, for example, where there is no such thing, there is no change of forces, which explains why an air mass cools when it expands, overcoming a pressure,while its temperature remains unchanged, if the expansion takes place without overcoming a resistance, as is the case when it flows into an empty space. "

"Every grain of coal that burns completely under the boiler of the steam engine or air machine supplies 0.908 heat units or 1241 pounds pounds of work as a result of the chemical process of combustion, when all heat is used to produce steam or to increase the tension of the air and completely in progress is implemented. "

By now, it would be useless to say that the living force in the world is a constant quantity at all. Only by the act, in the moment of communication and propagation of the motion, does it not change, if we take into account the equivalent of heat produced; but through the continuous and continuously changing effect of the forces in the course of the movement. When one body encounters the other in its course; Thus, in consideration of the vibration of the weighable particles and the attribution of the equivalent heat produced by the collision, the sum of living force in both will still be as great as before after the collision; on the other hand, we see the living power of each planet growing as it approaches as it approaches the sun, decreasing as it moves away from it, and those of a swinging pendulum in descending increasing, decreasing in ascension. But if the living force in these cases does not remain the same, it always restores itself in the same greatness as the bodies of the system, which at first is formed by the sun and the planet, and secondly by the sun and the earth, under the influence of the internal Forces of the system again assume the same situation to each other. Now in many other systems, too, under the influence of the forces inherent in them, a circulating or oscillating movement of the kind takes place, that after a while the parts return again and again to a given situation, and in this case also generally applies under the name the Law of Conservation of the Living Force known law, according to which the living force in one,

If we strike a piece of steel, the living force implanted in the steel particles in the act of impact together with the generated heat will fully represent the living force lost to the beating body, and if the body is completely elastic, the particles will become , swinging from the moment of impact, under the influence of their own forces, while passing through their original position of equilibrium, always gaining the same living force, but not keeping it for the duration of the vibration, leaving the original position; and if, instead of steel, we have a little bit of elastic lead, then it will remain constantly compressed, and the living force produced in the file of the impact, with which the particles would escape from equilibrium, can not recover. Rather, under these circumstances, truly living force disappears, which, as it is expressed, is used to produce a continual change in the position of the particles.

The law of the preservation of living force thus does not prevent the living force of a system or part of the infinite world-system from temporarily changing, increasing, diminishing, or constantly changing; it merely states that it recovers when the parts of the system return to their original position under any influence of internal forces, after any previous impulse; but it can not generally guarantee this return, and in many cases does not take place. It does not even take place in the simple systems of three bodies that attract each other under the law of gravitation, except under special conditions. And, as we know, because of the incommensurability of their orbital periods, the planets of our solar system never exactly, but only approximately in larger periods,

It is undisputed that in the infinity of the world the decline of the living force, which a part of this infinite system undergoes in such a way, is experienced temporarily or permanently; can more or less compensate with the increase that another part experiences at the same time; but there is no principle which places the decrease in the one and the other parts in such a relationship that a precise and lasting compensation would have to be expected, and there is the less reason to assume such a reduction than one There exists another principle which establishes another constant relation for the living force, but not that of persistence in the same state.

Not the size of the existing living force, but the size of the existing living force together with the size of the living force, which is still possible to generate by virtue of the existing causes of movement, which we briefly call potential power (the more common term is tension), is a constant size for any system extraneous to foreign influences, and hereby indisputable to the world.

For explanation, let us imagine a string in empty space swinging without resistance and giving up nothing of movement to the documents about which it is stretched, as it would be the case if it were stretched between two simple fixed points in order to be a foreign one To denote the effects of the system of material particles. The living power of this string is variable. It is zero at the limits of the excursion; but the potential power here at the same time greatest. For in every point that the string passes from there to equilibrium, it produces a new quantity of living force, which joins the former until, when passing through the position of equilibrium, it attains the maximum of living force. When she was on the border of the excursion, this, now real, living force its potential power, that is, the living force that was not yet created, but could still be produced by virtue of the existing causes of movement. In the movement from the limit of the excursion to the middle position, all this potential power has been translated into living force; but so much of living force arose, lost in potential power; for what was already produced by living force could no longer be produced until, at the time of arrival in the middle position, all the potential force was exhausted, and thus no further increase of living force at its expense was possible. From then on, conversely, after a corresponding course, the potential force grows at the expense of the living force and so on, until it reaches the indefinite,

What applies here about the string is true of the world. The living power can only grow at the expense of the potential and vice versa. Only that not all parts of the world in parallel go through their alternation between rising and falling, living and potential power, like the parts of the string; on the contrary, the most diverse parts of the world can find themselves in quite different circumstances in this respect; and they contribute only in solidarity to the fulfillment of the law, so that what one body loses in living power through communication to the other, does not grow to itself in potential power, and vice versa, what it receives through communication; is not won by him at the expense of his potential power; only for the whole system does the constant sum of both forces apply. By communicating its movement to the air, a string can at once lose all living power with all its potential power by resting in the equilibrium position; But if you take them in connection with the air, the sum of living and potential power for the system of string and air has remained the same.

This is the great principle of the so-called conservation of force, coherent with the above of the preservation of living force, but of even more general significance than this, a principle founded, indeed, by long-known general principles of mechanics, but first by Helmholtz with clarity has been developed, emphasized in its full meaning and explained in its main applications. Since then it has found the most extensive consideration and application in the field of inorganic and organic physics. It generally applies only to central forces that are not a function of time or speed; but until now no reason has been found to doubt its universality in the domain of the organic and the inorganic.

This may seem obvious at first. In the field of electricity and magnetism, insofar as it is traceable to electricity, there are forces which, according to Weber's investigations, depend on speed and acceleration. But it seems that these elemental forces combine in such a way that the law remains valid in all natural phenomena. This is self-evident for the magnetic and substitutable electrical current effects, insofar as they can really be represented as the effects of central forces which are independent of speed and acceleration. Moreover, in response to my questioning, Prof. W. Weber informed me orally that in all cases, to which his investigation led, he found the law in force even beyond the limits of those effects.

According to this law, in a system left to its internal effects, the living force produced by previous extraneous impulses or the previous internal action of force can only grow further at the expense of its potential force, and the capacity of that growth is accordingly exhausted as the potential power is increased the progressive growth of living force is exhausted, and on the contrary increases with the diminution of the living force, so that, indeed, a change of living force between increase and decrease and a transfer from one part of the system to the other, but neither a continuous growth nor to unlimited Height, even a decrease until the permanent extinction in a system left to its internal effects, and herewith indisputably in the world-systems, can take place,whereby the preservation of the activity of the world within certain oscillation limits is secured from the most general point of view.

On the other hand, the living force can grow in one part of a system without decreasing the potential force and decrease without increasing it, as it simultaneously decreases or increases in another part of the system, by transferring the living force from one part to the other. Insofar as every finite body is part of the general world-system, the law is applicable to every one only under this consideration, ie the constant balance between potential and living force applies to him, in particular only with regard to his inner effects; in connection with the larger system to which he belongs, in the last instance of the whole world.

One should notice that the principle or law of conservation of force does not tell us anything about the gait, the mode of reciprocal turnover between living and potential power, and nothing about the state of a system in this regard at any time; On the contrary, this is connected with the special conditions and conditions of each system, which can not be determined by any general principle but can only be deduced from experience. the principle of conservation of power merely tells us that; just as the conversion between living and potential force takes place in a system left to its internal effects, it can only take place in such a way that the constant sum of the same is preserved as a whole, but with which there still exists the freedom to succeed in infinitely different ways. It thus only binds from a certain very general point of view; the complete determination of the course of the phenomena is not to be found in it.

However free man may be, for his will and spirit there are in fact not only in the mastery of the outer, but also of the inner powers of nature actual limits drawn by the general laws of nature.

Man can go on the earth wherever he wants to, shifting his center of gravity in any direction he chooses, no known natural law binds and prevents him from doing so. But he can do it only so far as to preserve the law of conservation of the center of gravity, which itself is a consequence of the principle of equality of action and reaction. Falling or falling from a height, he is unable, with all the freedom of the will, to dislocate his center of gravity a hair and a half out of the fall of gravity, unless, for example, air resistance constitutes a weak possibility. For, according to that general principle, no physical system can shift its center of gravity by its own inner activity. It involves an external help or an external resistance.

It will not be the same with the living power. The will, the thought, the whole mind is as free as he wants; but he will not be able to express his freedom again, but only on the basis of the universal laws of living force. If his gait is bound to the course of psychophysical activity and bound to the law of the conservation of strength, he himself will be bound by it.

That is no misfortune; for the law of the conservation of power is a law of the preservation of the world; and it is no misfortune that the mind is bound to feel, to think, to want in the sense of this preservation.

A general and clear proof of the extension of the law's validity to psychophysical activity has not yet been made; but it can be asserted that all experiences, insofar as we can do such things, are in this sense, and without compulsion can be interpreted only by means of the law; we will therefore have to adhere to it, as long as there is no proof to the contrary.

Let us consider some chief conditions in this respect, to direct our attention chiefly to what is most easily inclined to deprive the validity of the law, that is, the realm of higher freer mental activities.

From the outset it might be meant that if not the mental activities at all, but in any case the higher ones could proceed without being bound to living force, whose laws, decrease and increase in general. Everything speaks against this requirement. Let us now also say whether such a special dependence takes place between bodily and higher mental activities, that a certain mental movement can arise and exist only on the basis of an equally determined physical one; it must have been admitted, and it must always be admitted that the higher mental activities here as well generally require the bodily activity as a basis more than the lower ones; but then they also need the living power of this activity to go from Statten, and experience teaches; that they require a sufficient strength of them; to go from powerful to self.

But one can further suppose that the mind from its own source of physical activity allows the vital force necessary for its course, or at the same time the vigorous preservation of its course, to grow, that is, to absolutely increase the living force in the world without the living force elsewhere or Therefore, the potential power of the body itself needs to diminish, that is, against the law of conservation of force, which requires a general consideration of all existing living and potential force in this regard. in short, that he is a generator of entirely new living force in the body.

Let us consider some facts which, with the explanation, at the same time give a basis for the decision of this question.

Play and consumption of the living force in the brain to psychophysical and in other parts to non-psychophysical activities actually exist in the ordinary course of life at the same time and with each other. We can think and do other things with our bodily organs, and usually do it. But now the power of thinking should be increased. Immediately we see how, instead of being able to create vital force from its own source to intensify the psychophysical activity it needs for its own amplification, it robs such other bodily activities, and in any case can not amplify it. Someone has just been engaged in a hard physical work, and then a thought comes to him that occupies him more than usual; immediately his arms sink and hang, as long as the thought, and thus its psychophysical activity, works inwardly to begin its external work anew, when the inner self subsides. Where was the living force of the arm movements at once? It served to start the movements in the head.

Just as an intense thought necessarily interrupts any external bodily performance, conversely, a leap interrupts every train of thought. The living force which the leap of the legs needs escapes the course of the psychophysical movements that thinking requires; and the mind has neither the power to continue the course as before, despite the loss, nor to replace the loss of its own power.

We can be the living force; which is available for arbitrariness, though sharing, but it has its maximum at all times, and this can only take place for one kind of employment as long as the others rest. Just as we need to rest others in order to use the greatest possible strength in one arm, we must let all parts of the body rest in order to use the greatest possible force in the head, and conversely let the activity in the head rest as much as possible perform powerful movements with the limbs. And so we see the deep-thinking sitting as still as possible, and someone who walks, lifts loads, never at the same time in deep thought. It contradicts itself, does not work.

Even involuntary functions, such as digestion, are, to a certain extent, in a condition of balancing and exchanging the living force with that which the thinking needs. Although after a wholesome device, the fact of which we can only acknowledge here, we can not explain, man is neither able to rob so much living power by involuntary functions by thought, that the proper course of the organic machine thereby faltered, conversely, by other functions, to rob so much power of thought as to bring it to a complete standstill.

Thinking is an example; but what applies in this relation to thought holds for every spiritual activity. Intensive feelings, passions, sensuous intuitions behave in the same way as intense thinking; only that the psychophysical activity of many of these mental processes by the organic device with certain external activities is in natural nexus, which then tend to rise and fall together with it, while at the same time antagonizing the rest. This association principle of physical activities will be discussed further below.

The same relation as between the psychophysical and non-psychophysical activities also takes place between the different areas of the psychophysical activities. It is not possible to be completely absorbed in an external view and at the same time reflect deeply. At the same time attentively see and hear, does not work. In order to reflect more sharply on something, we must abstract more from others; and as the attention divides, it weakens for the individual. Here one could, however, see a game of purely psychological laws if these facts stood alone. But they are too much in common with the previous ones, in order not to see in it an extension of the law of conservation of force to purely psychophysical play. In order to strengthen it, thought does not need to deprive the non-psychophysical activities of living power, if it can withdraw such from other psychophysical activities in progress. This does not deny the existence of psychological laws or reduce them to physical ones; it is only asserted that the laws of the course of mental and physical activities are no less closely connected than they are themselves; and this has nothing disconcerting, but the opposite would be strange. that the laws of the course of mental and physical activities are no less closely connected than they are themselves; and this has nothing disconcerting, but the opposite would be strange. that the laws of the course of mental and physical activities are no less closely connected than they are themselves; and this has nothing disconcerting, but the opposite would be strange.

Depending on the nexus in which the parts stand, some may operate only in a certain context or sequence, and some more readily in this than in that activity, and some activities in general only, or more easily, through a given context Sharing, as being performed by individuals, a principle that conflicts with the previous in that the distribution of the living force between the parts working together in activity then weakens the performance of the individual from one side, which the connection from the other side makes possible makes or promotes. Due to the consideration of this principle, a lot of apparent contradictions with the previous principle explain themselves, where activities, instead of being mutually restricted by their respective increase, rather, they rise and fall with each other, and together they keep up, pull each other along, and follow suit. In the game of the machines we find the corresponding thing again; and it is therefore nothing here to see the laws of the preservation of the power of contradiction.

In our organism such connections may be partly fixed, partly reconstituted, or solved by habituation, practice, and with the growing practice of putting parts into action in isolation, the possibility of putting them into more vigorous activity increases. This principle too, as easily further explained, intervenes in the context of psychophysical and non-psychophysical activities.

And so the generation as well as the use of the living force of the psychophysical activity in us, as far as we can observe it and establish an inference on observation, is everywhere under a common law with the living force of the non-psychophysical activities in and outside us, and so on free as the mind may be, it can not do anything against this law, but only on the basis of this law.

But how are facts of the following kind to be interpreted?

Suddenly we see a person in consequence of pure mental excitement accomplish a tremendous physical or mental achievement, after he just sat indifferently and calmly, so that neither in psychophysical nor psychophysical activities a supply of great living force was present. Where does the living power come from? And this strong activity may well continue under the influence of a strong will. Where is the sustainable source of this power to seek, if it is not the will itself?

But as far as the first is concerned, we can only make a sudden effort in a certain direction, by suddenly concentrating the previously scattered and therefore now strong force in one direction, and even taking part in it with the involuntary functions. And if, under the influence of a strong will, we ourselves are able to carry out lasting, unusual achievements which we can not carry out without this will, then the generation and consumption of the necessary vital force is neither contrary to the law of the preservation of power; nor by the purely spiritual power of the will.

In fact, we find that any voluntary effort exhausts us all the more, that is, the more the power of the further expression of power diminishes the more and longer it continues, which proves that the arbitrary development of living force in our body is so well, only at the expense of potential power, that is the force which it is still possible to produce, that is, according to the law of conservation of force, as the development of living force in areas where no will takes place. It is not disputed, therefore, that under the influence of free will, truly living force can arise which would not have arisen without it, but only at the expense of potential power, ie from the source from which it otherwise arises, if no will participates. It was undisputed that or psychophysically, the activities which themselves are subject to the will give rise to an event in which the turnover of the potential force took place in living and lasting; Only the will from itself can not create the living force without the otherwise generally valid conditions.

The living power of our organism, in general, according to the changing state of nourishment, of health, of waking and sleep, is in an ups and downs, whereby on the whole it can rise and sink deeply; Under normal conditions, however, it does not seem capable of sudden, sharp changes in the whole, but only of a sudden, other distribution, which is effected partly by stimuli, partly by arbitrary direction of attention or displacement of the sphere of activity. The idealist can also attribute the effect of the stimuli to a spiritual reason, the materialist to that of arbitrariness and attention to a material one; but here we take the facts, as they directly represent the observation, which soon the material,

It is, in a sense, like a steam engine on which a compound engine depends. Depending on the condition of the heating, its living force can rise high or sink low; but in the normal course neither the one nor the other can suddenly enter; but by opening or closing one valve at random, this, sometimes that part of the machine, can now get going again, and another can pass over in peace. The only difference is that in our organic machine the machinist is not outside but inside it. Now, undoubtedly, in vigorous bodily exertions, more vital force can be developed at the expense of potential power than at rest of the body; for where else does the faster exhaustion and the need for greater substitution; but then it is not the will which develops this force at any moment for spiritual reasons, but the increase of the chemical process of nourishment induced thereby. If we run fast, we also breathe faster, the blood runs faster, and it has the same success as if we were to increase the tension in the steam engine heater, and thereby more rapidly develop a given amount of effective living force at the expense of the potential power of the fuel. If the organic machine is not quite able or poorly supplied, so that those chemical processes do not proceed effectively, then the strongest will can do nothing. as the resulting increase in the chemical nutrition process. If we run fast, we also breathe faster, the blood runs faster, and it has the same success as if we were to increase the tension in the steam engine heater, and thereby more rapidly develop a given amount of effective living force at the expense of the potential power of the fuel. If the organic machine is not quite able or poorly supplied, so that those chemical processes do not proceed effectively, then the strongest will can do nothing. as the resulting increase in the chemical nutrition process. If we run fast, we also breathe faster, the blood runs faster, and it has the same success as if we were to increase the tension in the steam engine heater, and thereby more rapidly develop a given amount of effective living force at the expense of the potential power of the fuel. If the organic machine is not quite able or poorly supplied, so that those chemical processes do not proceed effectively, then the strongest will can do nothing. and thereby more quickly develop a given amount of effective living force at the expense of the potential power of the heating material. If the organic machine is not quite able or poorly supplied, so that those chemical processes do not proceed effectively, then the strongest will can do nothing. and thereby more quickly develop a given amount of effective living force at the expense of the potential power of the heating material. If the organic machine is not quite able or poorly supplied, so that those chemical processes do not proceed effectively, then the strongest will can do nothing.

I do not say with the foregoing that the living force in the body is really distributed like the steam in a steam engine; but only that the law of conservation of force leads to corresponding successes.

The last source of the living development of force in our body is, according to all that we may presume, in the process of nutrition, and in that each part has its nutritional process within it, it also has a source of living force in it. But experience proves from the other side by facts of the kind asserted here that this process takes place in the whole organism in solidarity, so that not only no part can nourish itself, but also quantitative relations of the balance between to enter into the nutritional processes of the various parts which are in the sense of the law of the conservation of the power. The circumstance also explains that the nutritional process of all parts is under the influence of the circulation of the blood and the activity of the nerves, which establish a connection by the organism. easily this general nexus of the nutritional process of all parts. Notwithstanding, therefore, neither the living force, nor any special bearer of it, as the steam in the steam engine, really overflows directly between the various parts, is distributed, attracted by stimuli, attention, wills, and so forth, we are always short We must be able to use the expression of the distribution of the living force and the corresponding figurative expressions after we know how to convey the correct idea.

The specifics of all these relationships are still poorly understood; but the general is quite clear and open in the sense expressed here; and the given general allusions may suffice for now; but a further execution of them would partly lead to uncertainty, partly not being there at the entrance.

The living force used for chopping wood, and the living force used for thinking, which is related to the underlying psychophysical processes, are quantitatively not only comparable, but also mutually implementable, and hereby, both of them are physical Page measurable by a common scale. As good as a certain quantity of living force is to split a log of wood, to lift a given load to the given height, to think as well a certain quantity, a thought of a given intensity; and that power can change into it. This is not an obstruction of thought; his dignity depends on the manner, direction, and purpose of his course, not on the measure or immeasurability of the bodily movement which he needs to go about; as the journey of discovery of Columbus does not lose in value and importance, that the living force of the ship which carried it was as well measurable as that of a randomly thrown stone or the wind, and even the one into the other. The physical in general receives value or worthlessness from the spiritual, which is related to it, and for that very reason can neither give nor take such from the spiritual. It is certain that a silent process of feeling and reasoning has great value, and yet it can make such weak movements that it would carry out a wholly worthless or no significant external physical exercise if it were to be implemented in such a way; but it is just as certain that if the life of feeling and thought should grow to greater intensity,

The relation of dependency, in which the intensity of mental activity is of the magnitude of the underlying physical, must be asserted no less in the opposite direction. As little as a thought of a given intensity can be conceived without a given living force of the underlying motion being developed, so little can it develop unless the thought is conceived with that intensity. Not that for every living force of a given greatness a thought of a given intensity belonged, but to the living force of such a physical walk, which is capable of conveying a train of thought. Now everyone is free to look with us for the reason of every single thoughtful movement in the world in a backward or more general, and finally, the ground of all the movements of the world in a system of motions, bearing only a highest and ultimate unit of thought and a supreme and final will can exist; except that here we have as little access to matters of faith as a measure of value.

And diligence avoids any argument about a dispute about freedom of choice, and it would be just as improper to drag him here as to miss here. Rather, by expressly pointing out that the general laws of living force restrict the free disposition of the same over the same from a very general point of view, freedom is granted every right, which is granted to it by reality. Neither can the law dictate whether and how we can translate potential power into living, nor whether and in what direction. In this respect, the will remains entirely free as far as the limits of this law are concerned. But to what extent there are other barriers, again our task here is not to investigate

VI. Measurement principle of sensitivity. [1)]

Even with the same method of attachment, one and the same stimulus may be felt more or less by one subject or organ than by another, or by the same subject or organ, at one time stronger or weaker than another; Conversely, stimuli of different sizes can be perceived equally strongly according to circumstances. We then measure the subject or organ at one and the other time greater or lesser sensitivity.

[1)] Revision p. 18-23. Psych. Maßprinzipien p. 179 ff.

Where the sense organs are paralyzed; even the strongest stimuli are no longer felt; the sensitivity is zero; In many excited states of the eye or ear, on the other hand, even the weakest stimulus of light or sound causes a lively, and indeed annoying sensation; the sensitivity for it is tremendously increased. In between there are all intermediate levels of sensitivity. Accordingly, there is sufficient reason to distinguish and compare degrees of them; but it wonders how it can accurately, how it can really be measured.

Here are the following options. In general, the measure of a quantity is that it determines how many times a quantity of the same kind, taken as a unity, is contained therein. In this sense, sensitivity as an abstract faculty has as little measure as the abstract force. But instead of measuring it itself, one can measure something relating to it, of which dependent upon it, which according to its conception increases and increases with it, and with which it conversely decreases and increases according to its concept, and thus gains an indirect measure of it, in the same Senses, as it is also the case with the force. Instead of measuring them ourselves, we measure the related velocities, which are the same masses, or the masses, which are implanted at the same speeds. And so we can either try to measure the size of the sensation produced by stimuli of the same magnitude, or the size of the stimuli, which produce an equal sensation, and say, at first, that the sensitivity is twice as great, if the same stimulus causes twice the sensation; in the latter case, it is twice as large when a stimulus half as large causes an equally great sensation.

However, the first way is impracticable, because we do not yet have a measure of sensation, and, as we shall show later on, such a one must first be based on the measure of sensitivity, which is grounded in another way. There is nothing to stop you from sticking to the second one. The magnitude of the stimuli is accessible to the utmost, and the equality of sensation may well be established by the necessary measures, which will be discussed in more detail in the future. Accordingly, we set the sensitivity to stimuli of the size of the stimuli, which give an equally strong, or more generally, to grasp extensive sensations, an equal sensation, inversely proportional, reciprocal with a brief expression.

It may be admitted that in the end it is only a matter of the definition that we call the sensitivity twice as great when half the stimulus gives the same sensation. If sensitivity were something measurable in itself, this freedom would not be open, but the relationship would have to be established through experience or inferences. This is not the case; the explanation about it is arbitrary, and the simplest possible, and which allows the simplest use, to be preferred.

So comprehensively, this measure will be of help to us, and has no other meaning than to orient ourselves in the realm of actual relations between stimulus and sensation, and to make possible their connection through calculation, without asserting the least of the greatness of the abstract sensibility can and should. Certainly it always remains that in a subject, at one time, a twofold attraction belongs to it, in order to fall as equally into the sensation as in another subject, at another time. Instead of saying this in many words, we briefly express it with the few, that in one case there is half as much sensitivity to the stimulus as in the other case. Any other measure implies a different factual relation in this regard, and shall not signify anything other than that.

The strength or liveliness of the bodily activities, which the stimulus awakens in us, and on which the sensation directly depends, in short the psychophysical activities, does not come into play in this measure, which belongs to external psychophysics. The question whether these activities are proportional or not to the strength of the stimuli is indifferent to its concept and its application; for, as a measure of the sensitivity of stimuli, it is only a matter of a relation of sensation to these, not to the activities thus induced; and that question must be raised, but it can only be decided on the basis of facts which presuppose this measure.

It is still important to avoid the following mistakes. If, with twice the sensitivity of a stimulus, half the stimulus suffices to induce a sensation of the same magnitude, it does not follow that the same stimulus then causes a sensation twice as great. First and foremost, we can not judge this as long as we have no measure of sensation, and later, when we have it, it will become clear that this relationship does not exist.

Sensitivity to stimuli is used to distinguish the sensitivity to stimulus changes, stimulus differences. The measure of the same, however, is subject to corresponding points of view, except that the change of stimulus, the difference in stimulus, takes the place of the stimulus.

In fact, just as a stimulus of equal size, twice, or three times may be required to produce an equal sensation, may also be an equal, double, or three-fold change in a stimulus, or the same, twice or three times great difference of two stimuli are required to produce an equal change of sensation, or an equal difference of two sensations. Here, the change of stimulus as a stimulus difference in the time sequence with the difference of simultaneously occurring stimuli can be taken under common viewpoint and name; as will be generally said below, without wishing to say that it does not matter whether one understands the components of a difference simultaneously or successively.

Superficially, one might be inclined to keep the degree of sensitivity to stimuli and that of stimulus differences to one another reducible. Given two tones of different physical strength, one may think of a third whose magnitude is equal to the difference in strength of those two, and For example, consider the weakest possible tone that can be heard by itself, and the least possible difference that can still be recognized between two tones are generally the same size. But this is virtually non-existent. Rather, casual experiences already teach, and later it will be proved more precisely that the difference between two physical sounds, lights, and so on, must be all the greater, in order to be recognizable, the greater their absolute strength, while the absolute strength,

However, this makes it necessary to distinguish the sensitivity and the sensitivity measure for stimuli and stimulus differences.

Inasmuch as the same stimulus difference is more or less easily recognized, according as it exists between small or large stimuli, and, in general, according to later investigations, as to the size of the sensory difference which a stimulus difference gives, to its relation to the stimuli, or the relation thus established the stimulus to each other is essential, the difference sensitivity is not merely variable according to the state of the individuals, but also according to the size of the stimuli, generally smaller in the case of large, than small. The determination of the law according to which the difference-sensitivity-wedge depends on the size of the stimuli, ie, according to which the magnitude of the difference of the stimuli must change with the magnitude of the stimuli, in order to fall equally clearly into the sensation.

Further, the following investigations in various sensory realms will show that, at least within certain limits, a difference between given stimuli always remains equally noticeable to the sensation, if it increases or decreases in the same proportion as its components, hence the relative difference in stimulus, and what is connected with it, if the ratio of the stimuli remains the same, just as the absolute size of the stimulus difference and the stimuli changes.

Relative differences in stimuli mean in general the difference of the stimuli in relation to the sum, or to the means, or to the stimulus, which here is indifferent, provided that the constancy of the other is inherent in the constancy of one relation. No less is the constant difference between the relative difference of stimuli and the relation of stimuli always connected in solidarity, so that it does not matter whether one refers to the constancy of one or the other.

If z. If, for example, components 5 and 3 both double, the ratio of both and the relative difference of both remain unchanged, whether as a result of being doubled or doubled , which fractions agree with the *previous ones ,*

On the other hand, when the stimulus ratio changes, the relative stimulus difference always changes in the same direction and vice versa, but not in proportion to it. Because if z. For example, if the ratio between the components 5 and 3 passes into that the component 5 changes without the component 3, then the relative stimulus difference goes in or out in over which is a change rather than in the ratio of 5 : 6 rather of 3 : 4.

Insofar as the law is that the difference remains equally noticeable, if it increases or decreases in the same proportion as its components, and consequently the relative difference of stimulus and the stimulus-relation remain the same, it will have to be said that the sensitivity of difference varies with size the stimulus is in inverse proportion, provided that twice the size of stimulus requires twice the difference to produce the same difference in sensation.

It may, however, be convenient to classify the sensitivity of differences as equals, that is, to equate them, not so much as the same absolute difference, but insofar as the same relative difference of excitement, or if the same stimulus produces the same difference in sensation and reciprocates one or the other , Whether one or the other, again is only a matter of definition and has no influence on the results of the applications of the sensitivity measure, if one uses only up the measure of the definition. Later, however, for formal reasons, it will be found more expedient in the whole context that the sensitivity for differences, insofar as they are to be construed as proportionate, is modified by the reciprocal value of the stimulus ratio; as that of the relative stimulus difference, at which an equal sensation difference arises, to be regarded as measured; whereas the equality of the relative sensitivity can always be related both to the constancy of the relative stimulus difference and to the stimulus ratio.

Summing up the above, we have to make a double distinction in terms of sensitivity. We must distinguish between: 1) the sensitivity to absolute stimuli and to stimulus differences, in short absolute sensitivity and difference sensitivity, the first of which is measured by the reciprocal of the absolute stimuli producing one sensation of the same magnitude, the second, as the case may be she understands, is measured in one of the following two ways. We have 2) to distinguish difference sensitivity into absolute and relative or relative sensitivity, according to the reciprocal value of the absolute difference or of the ratio of the stimulus quantities. The first we will usually be the simple difference sensitivity,

These distinctions may now appear minute and idle. But it will later be shown that they are by no means so; on the contrary, the clarity in the conception of the most important factual relations depends on this distinction, and the lack of clarity which has hitherto prevailed in the doctrine of irritability depends on the previous lack of a clear distinction between them.

In general, the name sensitivity means nothing other than what is otherwise called irritability, excitability, and sensibility; but that these names are used more generally, not merely in the elicitation of sensations, but also in movements through external or internal stimuli. But inasmuch as all sensations in the end depend on inner movements, one could also relate the concept of sensitivity instead of sensation to the underlying psychophysical movement; For example, say of absolute sensitivity that it is equal in size, twice or three times as large, according as an equal, half or double external or internal stimulus is required to produce the same psychophysical movement; except that this terminology is not practical,

Irritability and excitability are otherwise partly used as equivalents, sometimes arbitrarily distinguished, without such distinctions ever having been based on clarified factual relations. But after clarification of the concept of the different sensitivities, it will be convenient to introduce a discriminating use, and accordingly I will in the future use irritability exclusively for the absolute, excitability for the difference sensitivity, the former for sensations, the latter for perceived differences.

In the previous determinations we have had in mind the intense sensations in which, strictly speaking, the concept of stimulus applies alone; however, the measure of sensitivity is transferable from the domain of intense sensations to that of the extensive following facts.

It is well known that according to Eber Weber's experiments a certain span of a circle set with its points on the skin is necessary, so that the distance may appear noticeable; and it does not hinder, after a modification of its method, of which I speak in the future, to determine equally appearing distances on different skin sites, whereby it shows that the actual size of the distances, which appear just noticeably, or generally the same size, very much is different on different skin areas. No less can it be proved by methods to be given later that the differences in the distances which are still recognized on different parts of the skin are different. Analogous differences in the conception of spatial sizes and differences in size as between different skin sites can be found between different parts of the retina, namely more central and peripheral. Thus one can speak of a different sensitivity in the conception of extensive magnitudes as well as in the conception of intensive magnitudes, and in short contrast both as extensive and intense sensitivity.

The absolute measure and degree of difference of the extensive sensitivity of the various parts of the skin or retina will then be found in the reciprocal values of the expansions, differences in expansion, and relations of expansions appearing to be the same , as the measure of intense sensitivity in the equally large, intense quantities or size differences, or size ratios of the stimuli, thus z. For example, one skin site, taken absolutely, has twice as much extensive sensitivity as the other, when half the circle distance on the same appears as large.

In spite of the fact that the extensive sensitivity of given parts is indisputably dependent on the number of so-called sensory circles contained in a given stretch of it, it would be just as pointless to refer the measure of extensive sensitivity to this unknown number of sensory circles want, as the measure of the intense on the unknown size of the psychophysical movement. It is not disputed that in the given range there are much fewer sensory circles on the back than on the fingertip, and this justifies the lower extensive sensitivity of the back than of the finger; but the concept of extensive sensitivity now also refers to the fact that, by virtue of the organic device and mood, an organ is different in this respect, as the other one. If one were to make a reduction in the measure of sensitivity because of the different number of sensory circles, apart from the fact that one would not have the data, and thus the whole measure would remain floating, the concept of a different sensitivity would probably be omitted, as is undoubtedly a universally valid one , only to us, as yet unknown, dependency relationship exists in this regard, which everywhere wants to lead to the same value. Of course, measuring data on the extensive sensitivity as well as on the intensive principle established here for this measure have only the value of observational data, which in itself does not give any insight into the constitutional relations of the sensation to the physical support.

From the outset one may entertain the reservation that in the great variability of the sensitivity to the diversity of individuals, of time, and of innumerable internal and external circumstances, it is quite fruitless to strive for a measure of it, even because no one ever changes Secondly, because the results have no constancy and are of no value, provided that the results observed in certain individuals, at some time, under certain circumstances, would not be found elsewhere and elsewhere.

In fact, it can not be denied that in this respect there are difficulties for the measure in our psychophysical field which do not exist for the measure in purely physical or astronomical fields. But instead of the measure or the possibility of achieving fruitful results being thereby eliminated, the circle of inquiry is only broadened, and considerations are introduced which do not exist for those other regions.

Inasmuch as the sensibility is a variable, we have no measure of the same as a fixed one; but we can consult 1) limits, 2) averages of them; 3) examine the dependence of their changes on the circumstances; 4) seek laws that are preserved by their changeability. The latter are the most important thing. But for the investigation and investigation of all this, the measurement methods of sensitivity to be discussed offer not only adequate means but also sufficient sharpness.

An exhaustive investigation in this respect, however, necessarily goes much further than that of a fixed unchangeable object, which can not be mastered by the forces of a single one, and has not yet been accomplished for any single sensory area. Rather, in this respect, a rich field of future inquiry, especially for younger forces, can be found by the methods to be discussed below, a study which is not difficult in itself, but requires patience, attention, perseverance, and faithfulness.

VII. Measurement principle of sensation.

The measure of sensitivity discussed in the previous chapter, as a measure of mere faculty of sensation, is neither confused with a measure of sensation itself, nor presupposes such in the sense given, but only the observation of equals of sensation, partly among them , partly under modified, irritative conditions. In fact we do not measure the sensation, but only the stimuli or the difference of the stimuli, which produce an equally great sensation or an equal difference of the sensation; and it remains to be asked whether and to what extent a measure of the sensation itself and of the spiritual is at all possible.

In fact, no such thing exists, or, more cautiously, until now recognized as such, but has been doubted or denied to date until such time as it has been found. Even Herbart's attempt at a mathematical psychology was unable to base it on such; the most important objection ever given to him; notwithstanding Herbart had the measure in her hands, so to speak. Meanwhile, the principle of this measure is set up in the following, and its feasibility is shown theoretically and experimentally. At first this will only happen for sensations; for although the applications of the psychic measure principle go much farther than sensations, as will be seen in the future, these are to be taken for granted,

From the outset and in general it can not be disputed that the spiritual is subject to quantitative relations at all. For not only is one able to speak of a greater and lesser strength of sensations, there is also a different strength of impulses, there are greater and lesser degrees of attention, the liveliness of memory and fantasy images, the brightness of consciousness as a whole, such as Intensity of individual thoughts. In sleeping man, consciousness is extinguished altogether, intensified in deep reflection to the highest intensity; and in the general brightness individual ideas and thoughts rise and fall again. Thus, the higher spiritual is no less subject than the sensuous, the activity of the mind as a whole no less subject than in the single quantitative determination.

But first, and immediately, we have only one verdict on a greater or less or equal in all these relationships, not on how many times what is required to a true degree, and what it will be to gain. Without having any real measure of sensation-and henceforth it suffices to trace the object in relation to sensation-we are able to say that this pain is stronger than that sensation; this sensation of light is stronger than that; but to the measure of sensation belonged that we could say that this sensation is twice, three times, ever so, and so many times as strong as that, and who can say so far. We may well judge equality in the realm of sensation; our whole measurement methods of sensitivity, of which we will deal in detail later, our photometric measurement methods are based on it; but with all this we have no measure of sensation.

We do not have a measure of that yet; but with it we have the basis of measure, which demands the how many times the same, and herewith above all the judgment of the like in the field of sensation. In fact, it will be shown how our psychic measure comes out in principle on nothing other than the physical, on the summation of a Soundsovielmal of the same.

In vain, of course, we would try to make such a summation directly. The sensation does not automatically divide into equal inches or degrees, which we could count and sum up. But let us remember that this is no different for physical quantities. If we measure the time segments directly from the time when we measure the time, are we counting the sections of space directly against the space when we measure the space? Rather, we apply an external scale, and to time a scale that does not consist of mere time, to space a scale that does not consist of mere space, to matter a scale that does not consist of mere matter. The measure of each of the three requires both others. Why should it be in the spiritual, mental areas are not appropriate? The fact that one has always sought the measure of the psychical in the pure realm of the psychic may be a major reason why one could not find it so far.

It seems that one has often confused something in this regard. Each size can only be related to a unit of its kind; and in this respect, one can say that space can only be measured by space, time only by time, weight only by weight; but it is another thing with the means of measure and the measure procedure. Insofar as the quantities to be measured are not abstract in the nature of things and can not be abstracted from each other and abstractly handled by each other, the abstract unit of measure and a method of measurement can not be found in the nature of things; and it is only important to set up the practical method of measurement with the concrete dimensions of reality in such a way that the size relation of the measurer to the unit of measure nevertheless turns out to be pure.

Thus, if we want to think of a measure of the psychic, as the strength of sensations and impulses, and, in the further pursuit, the intensity of our attention, the brightness of our consciousness, etc., we must, however, also require a unit of the same kind, but It is not necessary to seek the means of measurement and the method of measurement in the pure realm of the psychical, that is to say, the inner perception, but to arrange such only in such a way that a pure relation to a psychic unit of measure results therefrom. It will never be possible to place one sensation immediately above the other in such a way that one measure of the other grows out of the other; but it may be possible by the addition of something else to which the sensations are so well connected, as the extension of the cubit to the matter of the cubit,

But what should we think about in this regard?

Without going into indefinite possibilities, I develop the principle of measurement itself.

Just as we, in order to measure space, require the matter of the cubicle, which is couched in space, so as to measure the psychical we need the physical, which is subject to it; But insofar as we are unable to observe directly the psychophysical activity which directly underlies it, the stimulus by which it is excited, with which it grows and diminishes by law, will be able to represent the place of this ulna in external psychophysics from which we may hope to arrive also at the attainment of the inner Elle in the inner Psychophysik.

This would be very simple if the magnitude of the sensation could be proportioned to the size of the stimulus. Then we should have a sensation twice as big, where a double stimulus works. But this is not allowed. For there is no justification for assuming a proportionality of stimulus and sensation as long as we do not yet have a measure of sensation which guarantees the validity of this proportionality; nor will the measure actually attained confirm it. But as simple as a physical inch of physical stretch, the stimulus can not be applied to the sensation. In the meantime, it shines that any other functional relationship between stimulus and sensation and that of direct proportionality can as well convey a measure of sensation to the measure of the stimulus, if only such a stimulus can be gained without presupposing a measure of sensation. Because if we are in an equation*y* as a function of *x* have expressed, we can *y* after the values of *x*and vice versa, if the manner in which they change with each other is also quite different from that of the progress proportional to one another. So it would only be important to express stimulus size and sensation as a function of each other, no matter what this function would be, in order to be able to find one another's size after another; only that we have to have a function grounded in reality in order to be able to make applications to the reality of it again. This brings us back to the main difficulty, how can it be attained, as proved in reality, without having already measured the sensation, in order to be able to show that the sensation proceeds in this and in no other relation to the stimulus than that Indicates function. In short, the measure of sensation, what to look for,

One has to make this difficulty perfectly clear in order to gain a clear insight into the meaning of its upliftment. In short, this improvement is based on the combination of two circumstances. 1) That we derive the function between stimulus and sensation from a function between the elementary, from which both can be regarded as adults; 2) that we base this function on the evaluation of equality in sensation-areas, which is possible in experience, and which can be verified by exact methods.

This is explained in more detail as follows:

The difference between one stimulus and the other can always be considered as a positive or negative growth for one or the other stimulus and it can be considered a whole stimulus in mathematical form as positive increases from zero to adult by always adding up to the sum of Before joining, think until the full appeal is there. Similarly, a sensory difference in mathematical form can be considered positive or negative growth to one or the other sensation, and a whole sensation can be considered as arising from positive increases from zero to their full strength. If we now know the functional relationship between the sum of the stimulus increases from zero, and the sum of the associated increases in sensation, then we have them *eo ipso*for all the charm and all the sensation it causes.

The three methods of measure of sensitivity for differences, which are set forth in the following chapter, now evenly teach, as was already indicated in Chapter 6, that the stimulus growth necessary to produce a given growth of sensation, or sensation, is always reversed to increase the same amount, does not remain the same, depending on whether it leads to a weaker or stronger stimulus, but grows with growing stimulus itself. Ie. An increase of stimulus must be more to a stronger stimulus than to a weaker one, in order to be noticeable, or even noticeable, as a growth. If 1 lot as an addition to a pound gives a just noticeable increase in sensation to the sensation of the weight of the pound, then at two pounds there will be no more such

Thus the necessity of having a measure of the whole sensation, in order to establish its functional relation to the whole stimulus, is circumvented by going back to the relation between the elemental increments, from which stimulus and sensation can be regarded as adult not a measure of sensation, but merely the judgment of the equality of differences of sensation, sensory growths, which belong to our command and which can be brought to a great advantage by means of the methods of sensitivity to differences, demands, and that we have the functional relation of the sums derive from it increases, whereby we receive the measure of the sensation after the measured stimulus.

In principle, then, our measure of sensation will be to decompose every sensation into equal divisions, that is, the same increments from which it arises from the zero state, and the number of these same divisions as by the toll of a scale by the number of the corresponding ones variable stimulus growth determines to think which are able to produce the same increases in sensation; how we measure a piece of stuff by determining the number of equal parts of it by the number of cubits which they are able to cover; only that instead of the ceiling here is the production. In short, we determine the magnitude of the sensation, which we can not directly determine, as a how many times of the same thing contained therein, which we are able to determine directly; but do not read the number at the sensation, but from the stimulus that carries the sensation, and makes it easier to read. Lastly, we substitute the counting of an infinite set of infinitesimal increments, which in fact is not feasible, by an infinitesimal summation of them, which gives us the result of counting, without having to make them in detail.

This difficult measure for the first sight can be reduced to simple, clear points of view, methods and formulas. But before we go into the execution in the following chapters, some general discussions may serve to explain the principle a little more.

The measure of the physical is more closely based, in its most general and ultimate ground, on the fact that equal and equal psychical impressions are produced by equal and equal physical causes, the number of times being determined by the multiplication of those psychic impressions, the size of the cause which produces the unique psychic impression, or any sum of them, as a unity. In the same way as we can obtain the physical measure on the basis of the relation of the physical to the psychic, in accordance with our principle, conversely, we obtain the psychic measure on the basis of the same relationship, pursued only in the opposite direction.

According to the general principle of continuity, no sensation stands abruptly and suddenly at the full height above which it does not thrive, but passes through all intermediate degrees of degree of imperceptibility, often, of course, in so short a time that the whole height of the sensation suddenly appears to us , An increase of the sensation from zero to new increments up to its full height is therefore not a fiction, but grounded in the nature of the thing; the reference to it, however, is at the same time the trick that only makes it possible for us to measure it. No measure can be applied to the adult sensation, inasmuch as no quantitative majority can be distinguished. But in the growing sensation the increments from which it grows,

From a certain point of view, this artifice offers advantages for the treatment of psychic values, rather than the corresponding trick for the treatment of room sizes. A curve, a surface exists; but the infinitesimal calculus, instead of taking it as a whole, lets it grow out of its increments, and, for As the genauesten insight into the whole situation of the aisle of the curve by giving a general expression of how to continue gehends constant increments the x-axis, the variable increment of the ordinate, to continue gehends constant *dx* the variable *dy*behaves. In the same way, we shall give the most accurate insight into the relation of stimulus and sensation, giving a general expression of how the variable increment of the stimulus behaves in relation to the constantly constant increments of sensation, and thereupon establish a function between stimulus and sensation which are no less expressible by an equation between x and y , and, if one wishes, can be represented by a curve. In the future we will need the letters *ß* and γ instead of x and y . In the meantime, this is only a prospect for now, not yet an insight that we are opening up.

The psychic measure in construction as well as in application will always remain less easy and simple than the physical one; in particular, because in the physical measure, in general, equal parts of the scale correspond to equal parts of the object to be measured, whereas the circumstance, which turns out to be quite general in experience, that with increasing magnitudes of stimulus and sensation ever greater stimulus increases are necessary In order to cover the same growth of sensation, it is as it were comparable to the case that unequal parts of the scale correspond to equal parts of the object to be measured. Although this does not prevent, as we have said, in the case of a well-known relationship between the two, from the sum of one to the other, what is the essence of what matters. But the magnitude of the stimulus and the sensation are no longer proportionally proportional, and the simplest possible relation, which was conceived between scale and object, and which actually takes place in the physical measures of space, time, and weight, thus exists between the psychic objects and his physical yardstick not. This is a second reason that has delayed the discovery of the psychic measure.

Meanwhile, the experimental investigation shows that the next simple relationship exists, which was conceivable here. It is found that while the *absolute* size of the stimulus increases more and more for the same increases in sensation as the sensation increases, the *relative* size of this increase remains constant for equal increases in sensation, provided that the sensitivity remains constant and normal or moderate ; so that always the same *relative* increases in stimulus correspond to the same increases in sensation, if we understand, as before, under relative increase the size of the absolute growth, in proportion to the size of the stimulus or divided by the size of the stimulus to which it takes place.

From this, the fact that with increasing sensation the absolute size of the stimulus increases more and more for equal growth of sensations is itself only an inference, insofar as in the stimulus growing sensation the same proportion of the stimulus must be absolutely greater than the stimulus becomes greater whose fraction he makes.

Insofar as we wish to call for an analogy with the scales of the physical to the concept of a measure of the psychical, that equal divisions of scale correspond to equal divisions of the object to be measured, we will be able to satisfy this demand, as the actual customs or departments of the psychic Scale rather than the absolute, consider the relative stimulus growth. The determination and summation of progressively equal stimulus increases in the ascendancy of the stimulus and the sensation then represents a summation of just as many corresponding equal increases of sensation, the sum of which we must relate only to one unit of its kind in order to have a measure of the whole sensation.

Strictly speaking, this summation is to be made with infinitesimal small increments, because only for infinitesimal small increases in sensation the corresponding relative increases in stimulus have an exactly determinable value. For, if we wish to consider the relative stimulus growth for a finite accumulation of sensation at once, it must be considered that the stimulus here goes through different quantities in ascension itself, each of which claims to act as a divisor for the growth, to the relative growth to give. The difficulty which seems to arise from this arises, however, in a more varied way, in the fact that a simple mathematical function can be set up which requires, without the in principle necessary determination and counting of an infinite set of infinitely small stimulus increases in detail.

And so the last middle link of the mental measure finally rests in a function which itself can be regarded as spiritual, while the physical has its last middle link on physical scales, only that also that middle link could not be found by movement in the pure realm of the spiritual It is still permitted in its application to be limited to this, since it is based on the relationship between the physical and the spiritual just as much as the bodily measure.

The law that in the higher parts of the stimulus scale greater stimulus increases are required than in the lower ones, in order to produce an equal amplification of sensation, has long been known, being a matter of daily experience.

The word of his neighbor is heard very clearly in the silence or in the weak daytime sounds; on the other hand, as one says, one does not hear one's own word anymore, and thus finds the increase in this imperceptible imperceptibly when there is a great deal of noise.

The same weight difference that is felt very much with small weights becomes imperceptible on big weights.

Strong light intensities, which differ very significantly photometrically, appear almost equally bright to the eye. For example, even a light in the mirror appears almost as bright as the light outside, despite the fact that there is a strong loss of light when reflected.

Analogous examples can easily be set up in the area of all sensory sensations.

But this general fact was not sufficient as a basis for the psychic measure. The more precise assertion that the magnitude of the stimulus growth, especially in proportion to the magnitude of the stimulus which has already grown, must further grow in order to do the same for the growth of sensation, has in some generality first been done by EH Weber and proved by experiments It is called by me the *Weber's law* .

However, for individual cases where it comes into consideration, it has already been stated and proved earlier, as can be seen in more detail from chapter 9, where this law specifically deals.

The mathematical function, on the other hand, which links the magnitude of the stimulus with the size of the sensation, is, according to particular aspects, more than a hundred years ago by Euler, later repeated by Herbart and Drobisch, for the dependence of the sensation of the intervals of sound on the ratios of the numbers of vibrations; a little before Euler by Daniel Bernoulli, later by Laplace and Poisson, for the dependence of the *fortune morale* on the *fortune physique,* finally on Steinheil and Pogson for the dependence of the stellar differences, which are nothing other than differences of sensations, on the photometric intensity of the stars, to which I shall return partly in chapter 8, partly in a later historical chapter.

If one had previously recognized the universality and meaning of that law and function, the psychic measure would have been recognized earlier.

Weber's law, that equal relative increases in growth correspond to equal increases in sensation, is to be regarded as fundamental to psychic measurement because of the great generality and because of the breadth of the limits in which it is strictly or approximately valid. but its validity is limited and subject to complications, which are to be carefully discussed later. Even where this law ceases to be valid or pure, the principle of psychic measure discussed here retains its pure and complete validity; in that every other relation of constant sensory and variable stimulus increments, even if only empirically determinable and expressible by an empirical formula, can serve both as a basis for the psychic measure and actually serve in the parts of the stimulus scale, where that law loses its validity. In fact, such will give as well as Weber's law a differential formula which leads to an integral formula containing the expression of the measure.

This is a fundamental point of view, in that Weber's law, with the limits of its validity, does not appear as a counterpart to the psychic measure, but only as a limited means by which the general principle of measurement extends. This, in fact, does not derive its validity from Weber's law, but the application of Weber's law only enters into the principle.

Accordingly, the investigation in the interest of the most generalization of the psychic measure will not have to be essentially based on generalizing Weber's law as much as possible, which might easily lead to a dubious tendency to generalize or raise concerns beyond its natural limits to suggest that it had been generalized in that interest beyond; but one will be able to ask without question: how far is it enough, how far is it not enough; for even where it does not suffice, yet the three methods which serve measure, and thus the measure, are sufficient.

In short, Weber's Law only provides the basis for the most numerous and important applications of psychic measure; but not the general and necessary. Rather, the most general, more remote, basis of the psychic measure lies in those same methods by which the relation between stimulus and sensory growth can be determined in general, both within and outside the limits of Weber's law; and the development of these methods into ever greater sharpness and perfection is therefore what is important above all in psychic measurement.

With all this great advantages would be lost, if the so simple Weber's law could not really be applied in wide limits exactly or with a satisfactory approximation in psychophysics. Similar advantages as if in astronomy we could not base on Kepler's laws, in the doctrine of dioptric instruments, that of simple lens-refraction. But now it is quite analogous to that law, as to these laws. In Kepler's Laws the disturbances in which the simple lens refraction is abstracted from the optical aberrations. Yes, they can become completely invalid if the simple requirements that they apply to no longer apply. But they are always the main conditions in astronomy and dioptrics, remain authoritative. And so Weber's law may lose its validity completely if the mean or normal relations under which the sensation of sensation works are greatly exceeded or abandoned; but it will always be decisive for them.

Nor will we be no less, as happens in physics and astronomy, in psychophysics, in order to abstract from the disturbances and small deviations of the law the general, the chief conditions with which it is chiefly to be done Therefore, without forgetting their existence, a finer education and a further advance of doctrine with the acquired possibility of determining and calculating the disturbances will also have the task of this determination and calculation.

The determination of the psychic measure is a matter of external psychophysics, and its immediate applications fall within the sphere of the same; its further applications and implications, however, necessarily infiltrate the field of internal psychophysics, and its deeper meaning rests therein. Let us remember that the stimulus does not act directly on sensation, but only through the mediation of bodily activities, to which sensation is more directly related. The quantitative dependency relations of the sensation on the stimulus translate finally into such of the bodily activities, which are directly subject to the sensation, in short the psychophysical activities, and the measure of the sensation by the size of the stimulus into such by the strength of these movements. This translation is necessary to know the relationship of dependence of these inner movements with the stimuli; insofar as it is not the object of direct experience to develop such in a precise way. In fact, this whole investigation will be done in a precise way, and can not be missed, one day, if the target is not yet reached, to have the success of exact investigation.

Whereas Weber's law, in relation to the relation of stimulus and sensation, shows only a limited validity in the field of external psychophysics, it has transferred to the relation of sensation to living force or otherwise to a definite function of the underlying psychophysical movement. probably an unlimited validity in the domain of the inner; in that all deviations from this law, which we observe in the production of sensation by the external stimulus, may be due to the fact that the stimulus only under normal or medium conditions triggers a living force of internal movements which is proportional to its size, and which is directly subject to sensation. According to this, it is foreseeable that this law, after it has succeeded, to carry out the transfer to the psychophysical movements in an exact manner, for which the field of the relations of body and soul will acquire just as important, general fundamental significance as the law of gravitation for the field of celestial motions. It also bears quite the simple character we are accustomed to find in basic laws of reality.

So while the psychic measure in the field of external psychophysics can only be based on Weber's law to a certain extent, it is likely to find the unconditional basis in the domain of the internal. But for now these are only views and perspectives whose assurance can only be expected from the future.

This is the principle of psychic measure in general. His more specific justification and execution will now include the following.

First of all, the methods that allow to ascertain how large proportionate increases in stimulus in the ascending scale of stimulus and sensation are necessary to produce the same progressive increase of sensation will have to be discussed. These methods coincide with the methods of measuring the sensitivity of difference, insofar as this measure, according to its stated concept, consists only in determining the stimulus differences which correspond to the same differences in sensation. Insofar as such a measure in itself is of importance and interest, these methods, apart from the basis which they give for a measure of the sensation, have their importance and their interest, and at first become without regard to those later applied be dealt with the same.

Secondly , it will be *necessary* to show how, in what universality and in what limits, Weber's law is based on the experiments according to these methods, and this law itself must be discussed. This law, too, apart from the support of the psychic measure to it, as one of the most general psychophysical laws, has its great importance.

Third , there will be a fact (the fact of the threshold) and another law (the parallel law) to be discussed, which, without being substantially included in Weber's law, are factually connected with it and intervene in the general justification of measure.

Fourthly , it will be *necessary* to show how the general mathematical function, which expresses the relation between stimulus-size and sensation-size, without presupposing a comparison of the size of the sensation, and without taking a fall upon a count of the individual sensory gains, can be substantiated.

Fifth , this feature will be self-setting up, discussing and pursuing in their applications.

Sixth , it will be *necessary* to show how, even where Weber's law ceases to be valid, a psychic measure is still possible.

Seventh, finally, with this measure, from the field of external psychophysics to the inner one, the transition will have to be sought.

The first three of these tasks will be dealt with in this, the rest in the following volume.

VIII. Measurement methods of sensitivity.

According to the concepts established in Chapter 6, the measure of absolute sensitivity in intense sensations is the reciprocal value of the absolute stimulus quantities; in the case of extensive reciprocal values of absolute expansions, which produce an equal sensation, the measure of simple difference sensitivity is the reciprocal value of stimulus differences or expansion differences that produce a same sensory difference; as a measure of the relative sensitivity of difference, the reciprocal of the ratio of the stimuli or expansions, which produce an equal sensation difference.

The measurement methods of simple and relative difference sensitivity do not separate, since in both cases it is important to determine the two stimuli that give a given difference in sensation. Only one can pay attention either to the absolute size of the difference or to the ratio of the stimuli, and to measure the sensitivity according to the reciprocal values of one or the other. Each of the two dimensions will receive its meaning; but here it will suffice to discuss the methods concerning the first.

The execution of the measure on the basis of these determinations presupposes that we can really judge and establish the equality of sensations and differences of sensibility under different circumstances, which does not seem quite easy for the first sight. However, as we have already recalled, the well-known photometric measure is based on the assessment of the equality of sensations; in music one often enough has the agreement of two tones, as well as the equality of two intervals of sound, ie differences in tone; and very general methods of stating the equality of sensory differences will soon be at issue. Even the methods of measurement of sensitivity, which are related to differences, have so far been far more developed than those of the absolute, and they are therefore to be treated first and foremost by them.

This is to be done in so far as a general insight into the nature and mutual relationship of these methods and the common conditions of their accuracy becomes possible, that the essential, what is important in the experiments and their calculation, is sufficiently designated, as well as applications methods and to understand the results to be given in the following chapters. But if I wanted to demonstrate all the experimental and analytical specialties of the methods, which can be considered in more detailed investigations, and explain all the rules that will be given theoretically, and prove them by experiments, then, contrary to the interest of those who would more a general insight into the methods than to do their own use, The course of the contemplation is so much delayed by the fact that I prefer to refer to a more detailed description of the methods and subsequent series of experiments on an addition to this document, which I intend to join under the title "Measurements and Measures in the field of psychophysics" and subsequently briefly cite the term "measurement methods". Much of what can only be briefly indicated here and hinted at, one is executed there and partly more accurately proved theoretically, partly find by special experiments. which I intend to attach to it under the title "Measurements and Measures in the Field of Psychophysics" and which will shortly be cited under the term "measurement methods". Much of what can only be briefly indicated here and hinted at, one is executed there and partly more accurately proved theoretically, partly find by special experiments. which I intend to attach to it under the title "Measurements and Measures in the Field of Psychophysics" and which will shortly be cited under the term "measurement methods". Much of what can only be briefly indicated here and hinted at, one is executed there and partly more accurately proved theoretically, partly find by special experiments.

l) measurement methods of difference sensitivity .

a) General presentation.

So far, there are three methods of differential sensitivity, which I will use for the sake of brevity

1) method of just noticeable differences,
2) method of right and wrong cases,
3) method of mean mistakes

call.

In order to give a first superficial insight into the nature and mutual relations of these three methods, they may first be briefly explained in relation to one and the same task, namely, to examine the fineness with which differences in weight are recognized, if only the first two So far, these methods have really been used.

In order to apply the method of just noticeable differences to our task, one compares two vessels *A, B* brought to a somewhat different total weight by loading with a given weight . If the difference of the weights is large enough, one will feel it, otherwise it will not noticeably find it. The method of just noticeable difference consists in determining the magnitude of the difference in weight which is necessary to be recognized as just noticeable. The size of sensitivity for weight differences is the size of the difference thus found reciprocal.

In general it is expedient in this method to reduce the difference just as often from an over-sensible to the degree of the just-noticeable, as to bring it up from an imperceptible to it, and to take the mean result.

If one takes the weight difference very small, one will often be fooled by the direction of the difference by repeating the experiment more often, by taking what is really too light a vessel for the heavier one, and vice versa; but the greater the overweight or the sensitivity, the greater will be the number of correct ones to the number of wrong or to the total number of cases of judgment. The *method of right and wrong cases*It consists in determining the size of the overweight, which, under the various conditions under which the sensitivity is to be compared, is required to produce the same ratio of right and wrong cases, or right cases, to the total number of cases. The magnitude of sensitivity under these different ratios is set reciprocal to the size of this overweight.

Cases where one remains doubtful are not to be left aside, but half to the right, half to the wrong cases.

If one has merely given the weight of one vessel as *normal weight by* means of the balance, then one can try to make the other, the *false- weight ,* after the mere judgment of the sensation equal to that. In general, one will commit a certain error, error, which one finds when one weighs the second vessel, after having estimated it the first as equal. If one repeats the experiment often, one will receive many mistakes, from which one can gain a mean error through middle education. The sensitivity to weight differences will be reciprocal to the magnitude of the mean error thus obtained. This is the *method of mean error.*

In the same way, since the positive and negative errors depend on a lack of proper conception, they are to be used in the same way as a measure, that is, not to be subtracted from one another by absolute values, but added together.

In the same way as in the field of sensations of weight, one can apply the same methods in the field of sensations of light, of sensations of sound, and so on, as well as of extensive sensations; In the case of the latter case, for example, the difference in the spans of two circles held in front of the eyes or placed on the skin must be examined by the method of just noticeable differences, in order to appear as just noticeable. by the method of right and wrong cases, how often, in the case of two circle distances, which are a little bit different from one another, a correct and often the wrong judgment is made, if one appreciates the greater; according to the method of the mean error, what is the average error which one commits, if one tries to make one circle distance of the same size with the other.

These three methods lead to the same goal in different complementary ways. In the first, the boundary between super-noticeable and subtle differences is observed as just noticeable difference, in the second one, over-discernible differences are counted (which, by chance, soon turn out to be in the right, sometimes wrong, sense), and in the third, subtle differences are measured.

All three methods are used as a measure of sensitivity relatively small, sometimes vanishingly small, differences. It will be shown later that this is most advantageous when it is necessary to seek a measure of the sensation in terms of sensitivity.

As far as one can overlook, each of these methods is applicable to all areas of the senses, but much is still lacking in the execution of even one of these methods by all, and neither are all three completely carried out by any one of them.

The method of just noticeable differences [1] has probably been applied earlier in individual cases; for example, by Delezenne for testing the sensitivity for deviations from the purity of the sound intervals; but in a particularly large extent and with the happiest success, but by EH Weber, for the investigation of the relations of susceptibility in the field of the subjective measurement of weight, touch, and eye [*] . I myself have made only a few not very extensive experiments in the field of intense sensation of light, the measure of proportion, and the measure of temperature according to this method.

[1] Revision p. 119 ff.

*) Comp. in particular, his treatise on sense of touch and common sense, and his programmata collecta.

The method of right and wrong cases Touching, no earlier and other trials of which are known to me than that of Hegel Mayer **) , stud. med. in Tübingen in the field of sense of proportion, and Renz and Wolf ***) in the field of sound measurement, both by young people under Vierordt's auspices, so we must assume that Vierordt was the method at hand, although this is not expressly stated , I myself have used it for very extensive experiments in the field of weight measurement 2) .

**) Vierordt's Arch. XI. P. 844.

***) Vierordt's Arch. 1856. H. 2. p. 185 or Pogg. Ann. XCVIII. P. 600.

2) Revision p. 84 ff.

The method of mean error is, in a sense, as old as observing, and its precision determined by the magnitude of the errors committed; but, as far as I know, so far only from the point of view of the objective determination of the precision of physical and astronomical observations or the determination of the size of occurring error sources †) , but not as a psychophysical measurement method for investigating the sharpness of the senses has been envisaged and used. It seems to me now one of the principal to be for this purpose, and I have used in conjunction with Volkmann to study the sharpness of eye and Tastmaßes 3) .

†) So from Steinheil in his elements of the brightness measurements p. 75; from Laugier in Compt. rend. XLIV. p. 841 etc

3) Revision p. 104 ff.

In practical terms, the method of just noticeable differences among the three measurement methods is the simplest, most direct, relatively quickest to accomplish goals, and least demanding accounting assistance. While one must first observe a large number of right and wrong cases or errors in the other methods in order to make a judgment about the equality of the sensation of a difference, and to impart this judgment by means of an accounting operation, the difference just noticeable is taken directly as one for the sensation they are immediately alike; and if, in order to affirm the individual judgment, a repetition and the accuracy of the drawing of a remedy are needed here too, this can be based on much fewer cases, because every single case of observation gives itself a result. Accordingly, for the first more general statement of fundamentals and where one does not have to spend much time on observations, this method will usually appear to be the most expedient. However, for more detailed investigations, it seems less capable and capable of so much definite precision than the other two methods, which should therefore always be found driven by investigation. In particular, she is opposed by the fact that the degree of being able to be considered as a player leaves the subjective judgment more scope than with the other methods. He is nothing absolute; neither the first point, where a difference in sensation becomes perceptible, nor where it disappears, can be determined exactly; one goes through an interval of doubt whether it is noticeable or not.

However, the experience of communicating with oneself about the feeling of a small, but still certainly felt, difference teaches this, if not absolutely, but closely, reproducing it in various experiments and reproducing the experiments by duplication good result. Moreover, the foregoing remarks are by no means intended to belittle the value of this method, but merely to put the advantages and disadvantages of the same in the correct light against the other methods. It would be lost with it the psychophysics, so to speak the most handy tool. It has worked well in the hands of its Master through the help of the same basic data obtained, and others, myself, have had ample opportunity to convince themselves of their usefulness.

The method of right and wrong cases is probably the most tedious, and it is better if you do not have much time and patience to not embark on the same, because with a few right and wrong cases, as much as nothing is done, while one out of many To obtain very good results, that is, to agree with one another, and to ascertain and establish legal relations in the realm of sensation. This requires the help of the invoice, which, however, can be attributed to easily executable operations. Although in the method of just noticeable differences one points in principle to a single difference, which is just noticeable, as the measure of the sensitivity of difference, in the method of right and wrong cases one can draw somewhat larger and smaller differences at will into the experiment,

Also, the method of mean error requires large numbers of experiments and a slight invoice help. Both of these methods have the great advantage of being able to rely on the proven principles of probability calculus and of being able to contribute something to their own testing. In fact, the interest in what I found in the long pursuit of these methods, entertained by this point much with and been increased [4)].

[4)] About the method of middle gradations: In matters p. 22, 178 f. Psychic Measure Principles p. 182 ff.

b) General considerations and precautions [5)] .

As simple as the above briefly discussed methods seem, and in principle are, in the first place, they require many considerations and precautions in their execution and execution, partly observation, partly calculation, which in part specialize according to the method and field of experimentation , More or less general, however, the following apply.

[5)] Revision pp. 25-42.

In all three methods, irregular coincidences, some of which adhere to the manipulations and are partly based on subjective conditions of the conception of the compared quantities, play a major role. If the latitude of contingencies is considerable, then the method of just noticeable differences will soon greatly increase the size of the difference to be recorded, and will soon appear greatly reduced, and, to be sure to explain it as noticeable, must have a considerably greater size than it otherwise; the value, which is recorded as a noticeable difference, will thus be magnified by great coincidences. With the method of right and wrong cases, let the accidental influences, one weight soon much heavier, now much easier, than the other, so that the influence of the extra weight against this influence of contingencies is not very much in question, the number of correct and false cases becomes remarkably equal in view of the fact that the irregular contingencies on average work as often multiplying as diminishing on either side large, at least those of the right cases are reduced against the case that no or less coincidences would have place. Finally, with the method of mean error, we immediately overlook the fact that the errors must on average be the greater, the more randomities make the quantities being compared seem sooner larger and now smaller. On the average, that the irregular contingencies act as multiplying and diminishing as they do on this and that side, that the number of right and wrong cases is remarkably equal, and at least that of the right cases is reduced to the case that there is no or less chance. Finally, with the method of mean error, we immediately overlook the fact that the errors must on average be the greater, the more randomities make the quantities being compared seem sooner larger and now smaller. On the average, that the irregular contingencies act as multiplying and diminishing as they do on this and that side, that the number of right and wrong cases is remarkably equal, and at least that of the right cases is reduced to the case that there is no or less chance. Finally, with the method of mean error, we immediately overlook the fact that the errors must on average be the greater, the more randomities make the quantities being compared seem sooner larger and now smaller.

In short, the more irregular irregularities affect, the smaller, by all three methods, is the value which gives the measure of sensitivity, and there is no way at all to obtain a measure free of these accidents; their average size is always included as a factor in the measure. This does not hinder the attainment of comparable measures of sensitivity as long as this factor remains constant, that is, as long as the irregular randomnesses remain the same in average; yes, without these coincidences, the measurement methods of right and wrong cases and mean mistakes would not exist. But it attaches to the previous consideration the important consideration, just to see such measures of sensitivity as comparable, whereby one can presuppose an equal play of the coincidences, which demands a precise comparability of the external and internal experimental circumstances. If the manipulation somehow changes during the experiments, another game of chance happens immediately and hears the measurements being comparable; In the same way one can not presuppose the same latitude of chance because of a possible change of inner conditions in different individuals and at different times in the same individual. Wherever there are deviations between the measures of sensitivity, one must therefore always ask whether they depend on actual deviations of the sensitivity, or on the lack of comparability of the circumstances under which they were tested. which demands a precise comparability of the external and internal experimental circumstances. If the manipulation somehow changes during the experiments, another game of chance happens immediately and hears the measurements being comparable; In the same way one can not presuppose the same latitude of chance because of a possible change of inner conditions in different individuals and at different times in the same individual. Wherever there are deviations between the measures of sensitivity, one must therefore always ask whether they depend on actual deviations of the sensitivity, or on the lack of comparability of the circumstances under which they were tested. which demands a precise comparability of the external and internal experimental circumstances. If the manipulation somehow changes during the experiments, another game of chance happens immediately and hears the measurements being comparable; In the same way one can not presuppose the same latitude of chance because of a possible change of inner conditions in different individuals and at different times in the same individual. Wherever there are deviations between the measures of sensitivity, one must therefore always ask whether they depend on actual deviations of the sensitivity, or on the lack of comparability of the circumstances under which they were tested. Immediately enter another game of contingencies and hear the measures of being comparable; In the same way one can not presuppose the same latitude of chance because of a possible change of inner conditions in different individuals and at different times in the same individual. Wherever there are deviations between the measures of sensitivity, one must therefore always ask whether they depend on actual deviations of the sensitivity, or on the lack of comparability of the circumstances under which they were tested. Immediately enter another game of contingencies and hear the measures of being comparable; In the same way one can not presuppose the same latitude of chance because of a possible change of inner conditions in different individuals and at different times in the same individual. Wherever there are deviations between the measures of sensitivity, one must therefore always ask whether they depend on actual deviations of the sensitivity, or on the lack of comparability of the circumstances under which they were tested.

The experiments generally need to be greatly duplicated and, as already noted, a very large number of them are required, especially in the case of correct and incorrect cases and mean errors, in order to obtain reliable results. The large number of observations has in fact a significantly different meaning here than in physical and astronomical measurements. A physical or astronomical quantity can be determined very precisely by means of less accurate measurements according to the usual methods of operation. On the other hand, with the method of mean errors and right and wrong cases, the great number of experiments themselves is an essential condition of accuracy. The single observation is of no importance here, and a small number of observations, no matter how precise, leads to no accuracy. The individual right and wrong cases, the individual mistakes indeed fall quite irregularly; small experimental fractions, in spite of the fact that they are externally employed in quite comparable circumstances, can give exceptionally divergent results, whereas it is often astonished to see the most consistent results arising from these irregularities in the larger experimental fractions. In this case, the law known in the probability calculus under the name of the law of large numbers applies, which governs chance, provided that it accumulates. while one is often astonished to see the most consistent results from these irregularities in the larger experimental fractions. In this case, the law known in the probability calculus under the name of the law of large numbers applies, which governs chance, provided that it accumulates. while one is often astonished to see the most consistent results from these irregularities in the larger experimental fractions. In this case, the law known in the probability calculus under the name of the law of large numbers applies, which governs chance, provided that it accumulates.

In this respect we can hardly compare our methods with anything more fitting than with a Proteus, who, instead of simply and willingly responding to the questions asked, seems to evade any answer by the most varied forms he draws; but it suffices, unswervingly, to hold him steady only on the same point, so he forces him to give a sure answer. In the past I have lost a lot of time, especially with the method of right and wrong cases, because I wanted to draw conclusions from a few hours of experiment or day without being able to come to something firm; until I decided to repeat the experiments always with respect to the same point for whole months, daily with about an hour's trial time, where I obtained results with which I have cause to be satisfied.

Apart from the influence that can not be eliminated, which, according to (i), is the size of the scope of the irregular contingencies on the size of the measures, the randomities must be compensated by more frequent repetition of the experiments in such a way that, as long as that leeway and Sensitivity remain the same, it finds the same standard of measure in experiments made at different times, so that the individual case loses its influence, and the definitive results become independent of chance. To be sure that this is the case, each series of experiments will have to be repeated or repeated until the larger fractions or their repetitions in the result in question coincide, of course, with deviations of such small order. as one must also allow as observation errors in physical observations; because the not absolutely compensable contingencies represent the observation errors in our methods. If a small faction matches, you can not calm yourself down by letting yourself fall to chance. For the rest, the probability calculus grants the means, on the one hand, to predict in advance the degree of accuracy which one may, with a given probability, expect to obtain from a given number of observations; on the other hand, to calculate the degree of accuracy attained by the number of observations and the degree of agreement which the individual observations or fractions of a series of observations show. because the not absolutely compensable contingencies represent the observation errors in our methods. If a small faction matches, you can not calm yourself down by letting yourself fall to chance. For the rest, the probability calculus grants the means, on the one hand, to predict in advance the degree of accuracy which one may, with a given probability, expect to obtain from a given number of observations; on the other hand, to calculate the degree of accuracy attained by the number of observations and the degree of agreement which the individual observations or fractions of a series of observations show. because the not absolutely compensable contingencies represent the observation errors in our methods. If a small faction matches, you can not calm yourself down by letting yourself fall to chance. For the rest, the probability calculus grants the means, on the one hand, to predict in advance the degree of accuracy which one may, with a given probability, expect to obtain from a given number of observations; on the other hand, to calculate the degree of accuracy attained by the number of observations and the degree of agreement which the individual observations or fractions of a series of observations show. For the rest, the probability calculus grants the means, on the one hand, to predict in advance the degree of accuracy which one may, with a given probability, expect to obtain from a given number of observations; on the other hand, to calculate the degree of accuracy attained by the number of observations and the degree of agreement which the individual observations or fractions of a series of observations show. For the rest, the probability calculus grants the means, on the one hand, to predict in advance the degree of accuracy which one may, with a given probability, expect to obtain from a given number of observations; on the other hand, to calculate the degree of accuracy attained by the number of observations and the degree of agreement which the individual observations or fractions of a series of observations show.

Although the attempts are to be set up from the outset in terms of a specific purpose as planned as possible; however, a preliminary tru- tulation can often be of great use in determining the most favorable conditions for measurement and the circumstances to be considered in order to establish the plan of the experiments thereafter, and also where there is no co-examination of the aisle that the exercise takes has the advantage of allowing the first stage of the exercise to pass through and thereby eliminating some of the changes in the main examination that are dependent thereon. Meanwhile, the influence of exercise always remains an element to be considered; and it is therefore useful to consider the knowledge and pursuit of it in the first preliminary experiments; because later attempts

In order to obtain results that are not one-sided and valid only for particular circumstances, the widest possible methodological change of circumstances should be applied. I have so often had the experience that what appeared quite legally under certain conditions, under other circumstances turned out quite differently [6)] , that I have become very careful to pronounce results that have not proved itself in very different circumstances as general. But now comes a conflict. The more circumstances connected with the experiment, the fewer attempts one can make to each individual, and the less precise the overall determination of the measure. One must, therefore, be so careful not to want to investigate all at once, whereby one accomplishes nothing quite right, as one-sidedly to limit the procedure to certain fixed circumstances.

[6] This is especially true of the ratios of the constant errors to be mentioned.

To illustrate it by the example of the weight tests, one can examine how the sensitivity for weight differences changes according to the size of the main weights. But if one has determined the conditions in this regard by lifting the weights with one hand, one will also find the same results, if one raises the weights with the other hand, or if, instead of both with one and the same hand, the one with the one, the other with the other hand lifts? Or if you change the handle or the mode of attack of the vessels or the position of the weights in the vessels? Will not the rate of elevation of each vessel, the interval between the elevation of both, the consequence of whether the heavier is the first or second-degree, to carry the amount of elevation differences? Will the same results be obtained if the experiments with the main weights are increased from the smaller ones to the larger ones and if they are done in reverse order? What influence does it have if you do it with a tired and not fatigued arm? How does the ratio of right and wrong cases change with the size of the additional weight? etc

An exhaustive study of susceptibility to weight differences really requires a determination of all of these influences, and in other experimental fields of sensitivity, there are only other influences that need to be investigated. But every such influence, in order to be sure of its size, direction, dependence on circumstances, requires a great number of attempts upon it.

Where it is to be compared with the influence of different circumstances, the experiments shall be alternately and alternately ascending and descending with the greater and lesser values, whether on the same days or in the change of days, the influence, the consequence of the experiments by changing the sensitivity or for other reasons to success, partly recognize, partly compensate, partly to be able to take into account. In the case of the example of the weight tests, this applies to the series of different weights, different weights, different time intervals of lifting, etc., which are subjected to the experiment.

Be z. If, for example, attempts are made with a number of different weights, it is possible to proceed on the same day through the series in ascending order, then in descending order, the next day descending, then ascending; or in such a way that one goes through it in one day only ascending, then only descending; Which changes methodically through the whole series of days, which takes the series of experiments to continue.

In some series of experiments, instead of always starting and closing with the smallest or greatest values, I have also begun and closed the series with each of the values to be tested, going through the series backwards and forwards as if it were in a circle where the starting point to go through the circle can be taken arbitrarily. Perhaps, however, the advantage to be expected from this for a complete compensation of the influence of the order of the experiments does not weigh the disadvantage of the reduced simplicity of the method or predominates only under special circumstances.

In general, with the influence of the time sequence of the experiments, different, partly counteracting, circumstances come into consideration, partly in conflict, and may soon outweigh each other in this, now in that sense. On the one hand, especially in the case of lack of exercise, the attention and activity of the sensory organs come by train for a certain length of time, and begin to work with a certain uniformity; on the other hand, they are exhausted, fatigued or diminished by a prolonged continuation Circumstances overwrought; finally, from the very beginning, and often through a long series of experiments, the influence of increasing exercise is asserted to a certain extent. All these influences can be made the subject of special investigation; insofar as they come into play by themselves in every investigation,

If they are not themselves the subject of the examination, one must avoid strong, dependent, modifications as far as possible, that is, not to continue the experiments to severe fatigue or irritation, and to prefer experiments with slow or completed exercise progress to those with rapid exercise progress. But since a certain continuous continuation of the experiments partly on each day, partly in the succession of the days, on the other hand, is as advantageous for the uniformity as for the practicability of the same in a given time, then one has a closer determination of individuality and circumstances, To look for a measure in this respect, which must be left to the own tact of each, but at least not to put the bill on the exclusion, but the exact determination and compensation of those influences. which can not be totally excluded at all; to which belongs a proper methodical arrangement of the experiments, and on which the closer information is to be sought in the discussion of the individual methods.

However useful and necessary a methodological change of circumstances is to investigate the influence of their differences, it is understandable that they should have the greatest possible constancy, or, in so far as they were not obtained, the greatest possible compensation for their variations in all experiments which are appropriate for given circumstances to unite to a common result. If the external circumstances in this respect are also in his power, then not the internal ones; in the same way that the sensitivity itself, like many internal relations of which the same circumstantially participates, is subject to a not inconsiderable variability by causes which can neither be calculated nor eliminated. This necessitates two considerations, one being the use of measures from different epochs, albeit under identical external circumstances, not easily comparable if one did not convince oneself of the fact of comparability; secondly, that one divides longer series of experiments not only into fractions according to the different experimental circumstances, but also according to the time, in order to study them specially, and generally tends to compile the result of calculations of the fractions of longer series of experiments than the result of the entire unfractionated series at once draws.

In general, fractionation has the advantage of assuring us the greater or lesser consistency of results, of having the eventual progress of the exercise followed, and, importantly, the influence of internal disturbances, often asserted in opposite directions in long series of experiments to be able to eliminate it more securely on billing records than if the observations were treated as a whole; as can be seen from the special discussion of the methods.

However, the billing result of each faction has less security than that of totality because of the smaller number of observations it makes. But the probability calculus shows that by combining the results of the fractions one regains certainty, which has been lost by the fractionation of the individual; according to which the stated advantages of fractionation still exist.

On the other hand, however, the fractionation increases the complexity of the treatment and presentation of the experiments, and the number of experiments which one puts together into one fraction has one method of correct and false cases and mean errors, which disappears only in the case of a large number of experiments. In the case of a smaller size to be taken into account by correction, or rendered harmless by the use of an always identical number of tests, influence on the size of the values, as can be shown theoretically and proved by experience.

Since each somewhat more extensive series of experiments requires a continuation of several days, or even weeks and months, the experiments shall be carried out in intervals as regularly as possible and regularly divided, equally experimental, equally or symmetrically arranged. Strict adherence to a fixed order in these relationships not only makes a significant contribution to making the attempts of the various days comparable and related to one another, to prevent confusion and oversight in the arrangement of experimental circumstances, but also to simplify the calculations and any To facilitate the use of the observations at all. Whereas, if you soon observe so much soon, soon this soon in that episode, soon afterwards, under these circumstances, without any fixed rule to those circumstances, the usefulness of the observations suffers in every respect. The general advantage which order has everywhere is asserted all the more perceptibly in our method, the more details it generally has to be arranged and kept in order.

In general, I always make the experiments of the same series of observations, which run through a series of days, at the same time of day; since the distance from the time of sleep and the intake of food may possibly influence the sensitivity ratios to be examined. Perhaps such an influence is often negligible, the more so if it always meets the circumstances compared in the same way; However, this will first be examined in detail, and before such investigation this caution always be advisable, which, incidentally, only enters into the general rule of observing a fixed order in the time-relations of the experiments.

Insofar as the judgment in our methods is to be based on the pure statement of sensation, great care must be taken that it is not due to an influence of the idea, the expectation of the results to be obtained, in short what one calls the influence of the imagination maintains, is determined. From another side, however, one should not, in order to avoid an imaginary influence of the imagination, make the procedure so blind. Both offer occasions in our methods.

The arrangement of the experimental circumstances, recording of the observed values, counting of errors or right and wrong cases, as well as all calculations to be based thereon, shall be arranged and controlled by repetition or otherwise, that otherwise in the quantity of the person to be recorded, summed up and calculated inevitable accident should be avoided as much as possible; and to observe in the recording and use even unbreakable fidelity.

Compliance with the latter rules is more important and more difficult than one might think for the first sight. After the experiences that I have made on myself and my fellow observers, I do not trust any narrative and account that is not controlled by repetition or otherwise. Even if repeated counting, calculating, especially when it happens in quick succession in the same form, one overlooks mistakes so easily as a mistake in correcting a script. Care and caution in this respect is not enough to recommend; and as troublesome as the repetition or other control of boring operations may become, it is necessary so as not to impair the merit of careful observation by accident in their use.

Even before the recording, however, in the generally necessary methodological change of circumstances, even slight mistakes can be made by confusing one circumstance with the other in the arrangement, or by holding it through several experimental departments without the required change; therefore one has to make a controlling neglect in this regard the rule.

As far as the fidelity in the record is concerned, one feels too often tempted to falsify without the results even single unusual observation values, z. B. in the method of average error unusually large, for example, by a discount of debt indebted to rule out mistakes. But this has neither principle nor limit and leads to a caprice, which has to rely only on an indeterminate Apercu. Such cases must be avoided, but, if they occur, they can only be compensated for by the great number of experiments. In the probabilistic laws of chance, on which the methods of right and wrong cases and mean errors are based, the rare occurrence of extraordinary cases is also justified; and you would not find any advantage in excluding them from bills who have to rely on these laws. The attention can not always keep exactly the same strength in long-continued series of experiments, if one must already seek to obtain as much as possible. Now the unintentional variations of them themselves belong to the accidents of these methods, and one should not disturb the law of these contingencies, which appears in large numbers, by arbitrary interventions.

It is important not only in the general interest of the order to observe the date of the observations, but also, in particular, because periodic or progressive changes in the sensitivity, which may take place in the course of the experiments, are only so recognized and in the composition and use of the experiments necessary to be considered. In addition, one will probably do all the additional circumstances, which may possibly have an influence on the success or the comparability of the experiments, as z. For example, the temperature, even where such influence has not been proven, can be recorded, and in this respect rather too much rather than too little.

In our field of observation, it is of particular great benefit if several observers unite for the purpose of joint investigation in order to partially complement one another, to partly assist, and partly to control. It is not easy for an observer to carry out on his own the study of a single sensory area or a more important side of it, successfully and exhaustively, partly because of the extension of the task, which makes a division of it just as necessary from another side is partly because, for some experiments, the direct cooperation of two observers or at least one observer and one assistant, for external reasons, belongs, at least in part, for a control of the results obtained by an observer by one or more others in our field is more important than elsewhere, because of the danger that the result depends essentially only on the observer's individuality. Thus, according to circumstances, partly a division of labor between the observers by division of the observation area, partly a joint participation of the same in the same experiments, partly the completely independent repetition of the same experiments by both can take advantage of place.

Perhaps it may be said in general that in our field no result obtained by any observer, no matter how reliable, may be considered certain if it has not been controlled by other reliable observers, because the observer's reliability is only a guarantee of fidelity and accuracy of his notes, but not of the generality of what he has observed in himself; although many conditions and laws are of the kind that one may presuppose from the outset, they are not merely a matter of particular individualities.

As important as the interaction of different observers is to a common investigation from a given point of view, it would be very impracticable to limit the possibility of psychophysical measurements to the involvement of a co-observer or assistant. Rather, as important as the control of any observations in this field is by independent observations, it is of the utmost importance that any kind of observations in this area be as undisturbed, as uniform as possible and under full control over time, the circumstances of the experiments, and the order in which they are attempted to carry out, in so far as only the danger can be excluded that the knowledge of the experimental circumstances, the influence of which one wishes to investigate, gives the imagination a clue, to distort the results. Thus, where an assistant is not needed for this or other reasons, he will also generally not be useful, as any complication of a machinery is harmful, which is not necessary. The special discussion of the methods of measurement will give many occasions the opportunity of coming back to this subject with special discussions relating to the nature of the circumstances and the experiences made, since a general apercu is not sufficient to support rules in particular.

c) considerations regarding the time and space of the experiments, constant errors. [7)]

Insofar as our methods are based on the comparison of two quantities, the successive conception of the simultaneous is preferable, and the latter basically hardly possible, in that the attention turns of its own accord to one and another quantity. The attempt should therefore be made immediately to observe the variables to be compared quickly, but each as undisturbed by the intervention of others, and to make the superposition of them only in memory. The ability to be able to compare quantities in this way is very strange, as EH Weber has already pointed out, and he expects his enlightenment to come from the progress of internal psychophysics. For now must be based on its fact.

7) Revision p. 130-138.

But the fact that the conception of the compared magnitudes does not coincide directly in time leads to successes that bring about the measure, just as they do not coincide directly in space, and thus enter into a different relationship with the comprehensible organs. I shall briefly describe such relations as relations of the temporal and spatial position of the compared quantities. The chief difficulties of a precisely comparable measure of sensitivity are based thereon, and the formation of the methods must chiefly be directed to the determination and elimination of them by method and calculation, in which one is nevertheless able to do more than for the first Sight may seem possible; but so far the attention has been less directed than the object deserves.

In general, with regard to the relationships of time, it should be noted that the following applies: 1) the time during which one conceives a quantity, e. For example, a weight trembles when weight tests are applied, a distance is envisaged, when it is a matter of judgment, and so on. 2) Intermediate time, which is allowed to flow between the conception of one and another variable; 3) the time sequence, whether one takes one or the other first; 4) the more or less frequent repetition of the comparative conception before deciding. In general, habit carries with it a certain uniformity in these circumstances, and the influence of small differences which occur in individual experiments is balanced out in a great number of them. But it may be appropriate to methodically hiring the experiments to make complete uniformity or comparability in these relations, by means of a counter, and to investigate by their deliberate alterations the influence of them, in which up to now very little has been done. In the weight tests I have done by the method of the right and wrong cases, however, I have strictly adhered to this consideration.

I will not enter into any generalities here concerning the conditions of the spatial situation of the compared magnitudes which are to be taken into account with such care, since they vary much more in method and field of experiment than in temporal position, and are only reminiscent of the fact that the double-sidedness of our sensory organs in this On the one hand, special consideration must be given, on the other hand, as far as it gives rise to a comparison of the degree of sensitivity of the doubly-existing organs, both in part and in part, and on the other hand, inasmuch as the cooperation of the mutual organs can not easily be in the same proportion to the quantities being compared.

In the measures of correctness, using the method of mean error, it makes a difference whether the normal distance, which one seeks to make the other equal, is found to the right or to the left of this, above or below to the same. In experiments on the tactile measurement of the skin, it is not a matter of indifference, if the experiments are carried out on themselves, with the use of pedunculated circles, whether the circle which determines the normal distance is grasped with the right hand, the other with the left hand , or vice versa, in that somehow the application method of the compass changes afterwards, etc.

Insofar as the conditions of a particular time and space situation, which are different for the different quantities compared, remain constant throughout a series of experiments, they justify to a certain extent what can generally be called a constant error.

In the case of the method of right and wrong cases in the field of weight tests, the constant error is that, if I put together a large number of cases where the vessel with the additional weights was the first-canceled one, with a large number, where it was the second-canceled one By the same token, the ratio of the right to the wrong cases is a very different one from the other, as well as otherwise, if I put together a large number of cases where the extra weight was in the left and where it was in the right vein. [8th)] In the case of the method of mean error in the field of measurements and tactile experiments, the constant error is that the mean of the distances which I have estimated at a given normal distance, after so many attempts, does not agree with the normal distance appreciably, but around a size, often considerable, that is legally dependent on the spatial and temporal position of the variables compared, deviates from this in the positive or negative direction, and, what is connected with this, that the sum of the positive deviations from the normal distance, the positive error sum, instead of the negative one absolute values fail the same, often very different from them, incomparably more than can be written on account of unbalanced contingencies.

> [8)] Renz and Wolf also note in their sonic experiments, according to the method of right and wrong cases, that one of them in general was inclined to regard the first-heard, the second the second-heard sound as the stronger, which proves that the influence of the various The temporal position of the influences also here, and in fact, according to circumstances changeable manner asserted.

It is perhaps possible to put suspicion in these statements, and to think that much of what has been observed is based on the influence of imagination, but only so long as one has not made experiments according to the methods in question, and one will soon convince oneself To get started as you like, to avoid constant mistakes. However, an influence of the imagination was so little in play with what I observed in this respect, that on the contrary the unexpected occurrence of the constant errors in these experiments was the thing which at first shocks me the most, and, before I too their elimination was most embarrassing; and even today, after having experimented for a long time in the field, especially of weight and measure, To me the last reason of it is for the most part unclear, and only the fact of it surely. Also, in other observers, which I prompted to repeat my experiments, quite similar found again.

Incidentally, the existence of constant errors only brings about a complication, not an inaccuracy, in the measure by our methods, insofar as they can be eliminated by means of suitable measures, and at the same time determined exactly by their size, as I show by special consideration of the methods.

Unfortunately, the constancy of constant errors does not take place in a strict sense. One day I am not so inclined as the other day to regard the vessel, which has been lifted or left, as the greater or lesser distance in a definite sense; but in external circumstances, the internal dispositions change in this respect, often in a highly striking degree. Our methods allow these changes to be easily followed, but also, when it comes to ultimate accuracy, find difficulty in it, as long as the variations of the constant errors in the method of the mean errors mingle with and contaminate the pure variable error, in the method of the correct one and false cases, but otherwise take the measure;

Apart from this, the complication of our methods by the constant error must not be regarded as a disadvantage of the same, but rather as an important advantage, as long as the determination of the constant error itself is a part of the psycho-physical measure obtainable thereby; the very fact that the influence of the sensation with the circumstances involved is thereby represented and measured, but at the same time there is the possibility of eliminating it from the measure of the sensitivity of difference, which, however, we are now only to do. The constant error should therefore not be thrown away as an idle refuse, but should be excreted carefully only by this measure, and, moreover, examined itself according to its relations, laws, relations of dependence in every field of experimentation, and by every method of experimentation. Indeed, our observational methods in this respect of observational art should be able to do so, not only by uncovering a general occurrence of constant errors such as one would scarcely have thought, but also to discover sources of them which have hitherto scarcely been thought of; but I refer to my "measurement methods" rather than this scripture.

At the same time, the sensitivity shown by the methods for the influence of the experimental conditions on the constant error is a proof of their fineness.

The above is far from exhausting everything that is necessary to know and to be observed by the one who wishes to make use of the foregoing methods for experiments himself. However, as I am obliged to reserve more detailed explanations on the "methods of measurement," I confine myself to mentioning, in part, the most important points of specialization in the last two methods, and then briefly outlining them, which will be discussed in more detail in the future. In doing so, I put down the weight tests for the method of the right and wrong cases, and for the method of the middle errors, the eye measures and tactile experiments, over which I have my own experience to command. The terms that I will use below

d) Special to the method of right and wrong cases, applied to the weight tests. [9)]

The experiments (begun in 1855), on the basis of which the following statements are based on the method of right and wrong cases, were first undertaken only with the simple intention of examining Weber's law more closely, and later in the interest of the training of the method itself continued and expanded, after it had been shown that the examination which I had in mind first demanded a preliminary investigation of the conditions of the accuracy of the method, an education of its experimental and calculation side, which at present was not yet present. For several years, I considered it a kind of daily work to make experiments in this interest for about 1 hour, and to continue such consistently with respect to the determination of this or that particular relationship for a greater number of days. This gives rise to material of experimentation which is by no means exhaustive in this document, of which the great number of trials occurring in some of the following chapters, and the repeated repetition of series of experiments to establish important points at different times and under different circumstances, testify There is also a great deal of practice in handling the method.

[9] Revision pp. 42-104, 358 ff . About the method of right and wrong cases in application to the sense of space. Dependent on the kgl, sächs. Ges. D. W. XXII, No. II. 1884. In the matter of the sense of time and the method of reputation F. Phil. Stud. III, p. 12 ff.

Insofar as our method depends on determining the ratio of the number of correct cases to the number of incorrect cases or to the total number of cases, I will, with the preferential application of the last ratio, determine the number of correct cases with *r;* those of the wrong cases with *f* , denote the total number of cases with *n* , that is, the ratio with which we shall have to deal mainly with , but so that if a test number is divided into equal fractions with respect to an observation value , and this is particularly taken into account are *r* and *n* to the number of correct and total cases of each fraction in particular is, however with*v is* the number of fractions, where then *vn is* the total number of cases for the observed value. Refers the whole series of observations, as is the case usually several to be compared with each other observed values, we must of course *vn* still the same be multiplied by the number to get the total number of cases for the whole series.

Where the verdict remains doubtful, such a case is remarkably half of the correct, half false cases. But in order to avoid half-cases arising out of this, I count, as the formation of the break depends only on circumstances, every correct case of judgment as two correct, every wrong as two false cases; and anyone, where the verdict remains doubtful, as one right, one wrong.

With *P* , the main weight, the weight of each of the relatively sophisticated vessels including load without is, di *D,* with *D* are referred to the additional weight (excess weight), which is applied in the experiments, with *h* a value which is directly proportional to the difference in sensitivity, and therefore the additional weights *D,* which is capable of delivering the same , is inversely proportional, or in short, the measure of the difference sensitivity by which it is to be done.

The method can be carried out in two ways: according to a first method, so that one decides only after repeated weighing back and forth of the loaded vessels, which is heavier or lighter; after a second so that one decides unbreakably after every single comparative annulment of both vessels, or in case of doubt puts the judgment to the undecided ones, which are counted half the right, half the wrong one.

In the past, I always used the first method; but later rejected all the attempts made with it, and held myself exclusively to the second, after I had convinced myself of the far greater excellence of the same. Not only can it be made more uniform than the first, but it can also be a precise elimination and determination of the contributions dependent on time and space, which constitute a constant error, only according to the second method, by adequate opposition of these influences against each other , as will be apparent below.

Of course, according to the second method, it is easier to make a mistake in the direction of the difference than in the first case, and the number of undecided and wrong cases is greater using the same *D* with the same total number of cases than after the first method, but the method does not make it more inaccurate, as long as it has to be based on the commission of errors, and what can be compensated by applying a larger *D* , not to small proportions which are just as little as too great an advantage for the measure. On the other hand, the second method, at the same time, yields many more cases than the first, and each duplication can be made in the same or comparable manner with the other.

Ignorance of the position of the additional weight, and hence the use of an assistant to determine the position of the individual to exclude an influence of the imagination on the judgment, is essential in the first method, and not only unnecessary in the second, as described below. but not even applicable. This will be motivated by a more precise explanation of the whole situation of the method.

According to the remark (see above), the uplifting of vessels is always to be carried out successively, and a double elevation of the second procedure, which establishes a verdict, thus arises by successive elevation of one and the other vessel, thus including two simple uplifts. But in so far as each judgment is counted in two cases according to the manner indicated above, the total number of cases is determined by the number of simple heavings, not double levies.

If I lift both vessels with the same hand, I call it a one-handed procedure; if I raise one with one hand and the other with my other hand, rather than two-handed. But the one-handed has always been carried out by me with both hands in so far as the left and right were applied in changing experimental departments. Here, in every major series of experiments, the rights have shown something, but less sensitively, than the left; but the one-handed process is not at all considerably more sensitive than the two-handed one. The constant influences of the time and spatial position of the vessels are very different according to one-handed, two-handed, left-handed, right-handed procedures. However, it is not the place to go into the specialties that I have at my disposal.

Special considerations required the establishment of the vessels, which together with the loaded load weights give the emphasis P ; and only after I have lost much time through experiments with imperfect devices have I stopped at the device to be described briefly below, with a revolving handle-roll and fixed weights, with the vessels, so to speak, forming a coherent solid body, which has sufficed.

Perhaps it is of some interest if, as an example - and indeed it is only one example - of how much small things one can embarrass and be stopped in experiments of this kind, mention beforehand some of those imperfect institutions.

Initially, I used simple hollow wooden cylinders as vessels, which I covered with my hand from above. But with heavy weights the hand had to be squeezed together so that the vessels did not slide out of the hand, while in feeble ones the hand was inclined to quietly grasp it. Nor could the uniformity of the version be well vouched. Then I provided the vessels with brass straps that rotated around pins attached to the opposite ends of a diameter of the vessel so that the vessels would self-orient themselves to gravity upon lifting. But this device soon became sloppy. Then I had the hangers annealed; but as they were of thin brass sheet, not to make the vessels too heavy by themselves, they withdrew, when I switched to larger weights and could no longer be considered comparable. After having substituted stronger ones, after rejecting all previous attempts, I have for almost a year made careful and tedious experiments with this apparatus, and at last all of them, though not downright, rejected, but considered needful of repetition and control, which has since been carried on by me so far that all those earlier attempts may be regarded as superfluous or, in their turn, only as an incidental control over the results of the newer ones; also in the following is completely abstracted from it. This depended on the following circumstances. The ones I used earlier, The load weights taken from the traffic and controlled only by weighing had different sizes according to their different severity. Since the vessels had to be wide enough to accommodate even the largest ones, the small ones, and even larger ones, were not secured from displacement when lifting the vessels. I presupposed that the pressure should always fall with the whole weight of the vessel on the same points of the hand embracing the stirrups, so that no disadvantage could arise from any shifting of the weights in the vessels, but omitted from the quantity otherwise to be investigated after the circumstances examined, which may be of influence in the method of making this the object of special investigation. This neglect has avenged itself. For when, at last, for the sake of safety, I began the investigation by deliberately making comparative experiments with weights fixed in the middle and at the sides of the vessel, it became clear that, by no means different size but different distribution of pressure, the results were both The vessel will appear heaviest when the weight is at the center of the air, and the difference is even not inconsiderable when comparing extreme situations in this respect. However, in my experiments only much smaller, and according to probability by the large amount of attempts in the main compensating displacements have taken place, This has partly been confirmed by the agreement of the individual larger fractions in the numbers obtained, and partly by the fact that the later experiments with the more perfect arrangement have essentially led to quite the same *results; however, those earlier attempts no longer pleased me, and their sharpness and binding force, if not in its entirety, but in their individual determinations, had become too precarious not to favor the effort of reassuming them with a new apparatus of reassurance among the former ,*

All the experiments to which I shall refer in the following, are carried out in very uniform circumstances according to the second method (see above), which I describe here as normal circumstances or normal conditions, passing on minor points which I reserve in the "measurement methods" , These normal conditions were deviated only insofar as the success of such amendments themselves should be made the subject of the investigation.

The vessels, according to the device in which I stopped at last, consisted only of a kind of framework consisting of four vertical brass rods connected at the bottom by a horizontal cross, between which were the perfectly fitting rectangular weights, different only in the thickness dimension (partly of lead, partly of zinc) were inserted, so that they had a firm position in it and could not move during the elevations. The vessel with the inserted weights and a lid placed thereon, on the middle of which a small open box was soldered, together formed the main weight *P,* which was carefully made equal for both vessels. In the box of the lid of one of the two vessels then the additional weight *D was*which thus retained its firm place, on the center of the main weight. The handle of the vessels was a wooden roller of 1 par, rotatable about a horizontal axis. Inch diameter covered with the whole hand.

Each vessel had, depending on the application of a lighter or heavier cover, together with it, 300 or 400 grams of weight, so that 300 grams was the lowest weight *P* , which could be applied, if no further load weights were added using the light lid. As the biggest emphasis I needed 3000 grams; The apparatus might not have tolerated a heavier load in the long run. Where it was not necessary to examine successes of application of various weights, I usually applied 1000 grams as the main weight.

The additional weights used were usually the sizes 0.04 P and 0.08 *P.*

In spite of the fact that both vessels were constructed in the same way, in order to compensate for an influence of an unnoticed difference, in each series of experiments *D was* just as often applied in one as other vessels under otherwise identical circumstances.

The height of elevation was limited by a horizontal board mounted at some height above the table, so that it was 2 inches 9 lin. Paris. cheat.

The uplifting happened with bare arms, in bare shirt sleeves.

The mode of uplifting was that if, for example, the left vessel was lifted first in a first double uplift, then in the second, this was done with the right one, and so on alternately. 32 double bumps in this way, or 64 simple bumps, which justify so many cases, are summarized as an experimental section, whereas *D* always remained in the same vessel. In the middle of each section, ie after 32 simple uplifts, each time the position of the vessels was changed from left to right. On the 4 times different time and space situation, which the extra weight*D* As a result of this, the so-called four main cases of the method to be discussed in detail below are based, each of which was therefore represented with 16 simple elevations or cases in each experimental department. In each of these departments, each of 64 cases, with changes to the conditions to be examined *(P, D,* etc.), were usually made 8 to 12 on each trial day after the other, and usually continued for 1 month in the larger test series.

The period of time of each vessel lift was 1 second, each time it was lowered, 1 second, the interval between lowing one and lifting the other, and also 1 second, which is the time of each double raise, which justifies a comparison or 2 cases 5 seconds. Just as much in between, ie 5 seconds, I left between one and the next double raise, during which the record of the result took place. In the one-handed process, the recording was always done with the idle hand; with the two-handed after the test days alternating with one or the other hand.

The application of attention soon becomes quite mechanical and uniform, so that, as I can prove myself from my experimental data, it is not noticeable at the end of the daily test-hour weakens shows; those judged by the extra weight *D,* the constant influences of the temporal and spatial position, and the irregular coincidences together in the direction of irregularly changing judgments: right heavier, heavier left, ambiguous, fall so to speak with objective character in the double elevations in the Hand, without the need for choice and reflection, which is the case with the first method.

How to set up the recording mode so as not to confuse oneself, and to be able to easily add up the correct cases obtained in the four main cases is specified in more detail in the "Measuring Methods".

So much preliminary of the external conditions of the experiments. After that I pass to the more general conditions of the method.

The general objective of the method under the different circumstances under which the differential sensitivity for weights to be comparatively examined for each of the compared conditions by a sufficient number of tests a value , or under division of the number of trials in *v* Group, *v* values to and to deduce from this the measure of the difference sensitivity, with which the secondary task can be related to determine the size and direction of the constant secondary influences involved in the experiments.

Now, from the outset, a fundamental difficulty seems to arise.

We know that, in otherwise the same circumstances, the ratio increases with the sensitivity to the difference in weight; but twice as large does not correspond to a difference sensitivity that is twice as great, if we wish to remain true to the concepts of proportion which we have established, but a half-weighted allowance *D,* which gives the same , corresponds to twice the sensitivity; and from a general point of view, the following can be noticed.

Although the sensitivity may be very small, the allowance *D may* always be so great in proportion to *P* that almost all cases or indeed all cases become correct, and it is clear that even the greatest increase in sensitivity then does not increase it can carry the ratio ; that, therefore, in this condition, since it may remain near or entirely constant with very different sensitivities, no suitable general measure of sensitivity should be sought; whereas in the case of very enhanced sensitivity, with a much lower allowable weight, the ratio will be equal to the approximation and to judge thereafter the intensification of the sensitivity, so that the nature of the thing points us to the measure which we have established. But how should it fit with our method?

Suppose, for example, I want to compare the sensitivity of the left and right hands for weight differences , and for the same main weight *P* and the same additional weight *D, set up*once with both arms with the left (L.), another time with the right (R.), In the first place, I only get a different one for L. and R., which makes me infer the greater or lesser sensitivity of one hand or the other, but there is no comparable measure of these sensitivities; and it begs, how can I find the various magnitudes of allowance *D* , which would give the same relation to L. and R.

Similarly, if I want to examine the sensitivity of one and the same hand, or both hands on average, at different *P's* . The same allowance *D* gives, according to experience, a greater proportion at low *P* than at larger, but it was rather to find the different *D* , which gives the same for the different *P's* , in the reciprocal of these *D* the measure of the difference sensitivity in the different To have values of *P.*

The method of right and wrong cases in the practice known since then, from this point of view, was in fact only capable of providing an indication of the more and less, but not a comparable measure of sensitivity. But the method can be trained to grant such a thing.

First, the way of the Tatonnements dar. One can change the weighting allowance in the circumstances compared until the same thing . But since only a great deal of experimentation can ever give a sure result even for one and the same *D* , this is a procedure which requires a great deal of experimentation for each of the tried *D's* , not only unspeakably lengthy, but also leads to the most laborious trial to no accuracy.

However, one can interpolate between obvious values; and for a long time I sought to help myself in this way; but the evils of circumstantiality and inaccuracy are only very incomplete. Fortunately, it is easy and complete to lift.

According to a principle which has been proved in principle, and which has been proved by experiments by me, based on mathematical analysis, but easy to translate into practice, it can be found from what has been obtained in a certain *D* , which *D* in the same *P* and the same way , would have been held may require any other to give, that is also what we want inferior as a solid, provided only that , after one concludes from a sufficiently large *n* is won. Yes one can directly, without calculation, from each *, to* which a sufficiently large *n* is subject [10]To find, according to a table, the measure of the difference sensitivity with which it is to be done, that it corresponds to the conception of this measure which we have established; and it should be shown immediately how this is to be done, after only a few words have been sent ahead of the way that led to it.

[10] If one goes down by fractionation of a large number of experiments up to small *n* in the individual fractions, one loses accuracy in the individual fractions, but regains these by merging the results of the fractions.

In a study of the theory of probability, to which I found myself constantly driven by the interest of the training of our methods, I found myself to consider: 1) that, according to the state of our procedure, the measure of sensitivity to differences by, usually *h* value, which, according to Gauss, offers the measure of the precision of observations, provided that, while the modality of the procedure is comparable, precision depends only on the sensitivity with which the difference is conceived; 2) that between the offered by the experiments and the product of that measure *h* in the allowable weight *D,* in which is found, di between and*h D,* must take place a mathematical relationship, which is a derivative of *hD* from , and thereafter by dividing by *D* , the degree of contrast sensitivity *h* must be found.

All that remained was to ascertain this relationship theoretically; secondly, to prove it by attempting to make a third use of our method of measurement. I believe that these three tasks have been solved satisfactorily, with which the method of right and wrong cases may have acquired the significance of a true measure method.

As far as mathematical deduction is concerned, I give it, since it is not necessary for the practical application of the method to take this into consideration, in the following intervention. The experimental proof comes out essentially to show experimentally that, if one has attained a certain value at a certain value *D* at constant sensitivity , the value calculated for another *D,* calculated according to our mathematical relation , which is to that in definite proportions, to find one's way through experimentation, allowing, of course, so little deviation, as to write on unbalanced contingencies; Or, which is only another form of the same probation, that, with equal sensitivity but different*D* by attempting ratios obtained after founded on our mathematical relationship table values of *hD* give, which is proportional to *D* are. [11] To prove this, however, I have very extensive series of observations to command, which I will communicate in the "measurement methods". Also, we will be guided by some of them in the ninth and twelfth chapters.

[11] Since the difference sensitivity in question is variable with *P* (but not with *D,* as long as *D* remains small), a constant *P is* required for experiments with constant sensitivity .

According to this, the subject matter can be represented in a purely practical way so that everyone can use the method even without understanding the reasons of the rules to be given and even without mathematical prior knowledge. It will also be possible to do so with confidence, after the theoretical derivation of it has had the control of a famous mathematical authority, and the control of experience has also been decisive.

Mathematical setup; and deduction of the bill rule of the method of right and wrong cases. [12]

However, until now there is no aprioristic principle, as depending on the size of emphasis *P* ratio is at constant additional weights *D* has to change, but this is only to be regarded as a matter of too constative by experiment law so against it is possible, according to the principles of Probability calculation *a priori* to indicate how the ratio must change (assuming a large *n*), if at constant main weights *P* and at all constant difference *sensitivity h*the additional weight changes, or the influence changes at all, which determines the apparent preponderance, and which here may be represented once for all by *D.* The same principles governing the change of the relative number of observation errors according to the changes in their size are presupposed here, assuming constant precision of observation. However, the relation between and *dh in* question is not represented by a finite expression, but only by an integral expression, which must be represented in tabular form for the practical utilization of the relationship, as will be done below.

[12] Revision p. 84-104.

The integral term to be denoted by Θ , which comes into play here, is the same by which the relative number or probability of errors in given limits of size is determined, except that the error , usually denoted by Δ, is half the extra weight occurs, namely

where π ισ the Ludolfian number, *e is* the basic number of natural logarithms, *t* = *h Δ* = , *h is* the precision measure in the Gaussian sense. The value of *t,* which belongs to a given value of Θ , can be found in tabular form in some places, such as Berlin. astronomer. Year f. 1834 p. 305 ff. Up to *t* = 2.0; and in a particularly published, now no longer in the book trade to have lithographed table to *t* = 3.0; so that if Θ after is given hereby at the same time *t* , or may hold given.

Now following, fundamental for our method, equations will be shown immediately, which means Θ from can be derived.

and after that

Of these relationships, it is sufficient to apply and consider the between and Θ as follows. From the observed value one derives the value Θ according to the equation , looks up the value in a table of the integral Θ , and divides it by to obtain *h* , or by *D* if, as we shall say, the *h* The method of right and wrong cases is only half as big as that of error theory. But in order not to have to first form the value out of that found by observation , I have the table of the integral Θ. where the relationship between and *t is* given, converted into one where it is equal between *t* and given. This gives the following basic table.

The mathematical derivation of the above relation between and Θ has passed the test of Professor Möbius, to whom I have submitted it, according to which mathematically it can be regarded as free of throw-in. But he has had the good fortune to substitute for my somewhat unwholesome derivation a shorter and more precise one, incidentally, to the same end, which I therefore prefer, instead of communicating mine in the following.

The Möbius derivative, as an example, substitutes the deviation of two parts of a straight line from equality for the deviation of two weights from equality. But the principle is one and the same thing otherwise.

It is general

the probability that the error in a measurement of a quantity falls within the limits of - Δ and + Δ , in which expression *h* as above the measure of the precision of the measurement, π the Ludolfian number.

Be now, for example:

A C B

three points in a straight line; *C* very close, but not quite midway between *A* and *B* located. In *n* observations according to the method of the right and wrong cases, I think *a* once that C is closer to the *A* than to the *B;* hence *CB*> *CA; n - a* = *b* times that *C is* closer to *B* than to *A,* hence *CB* < *CA* . Hereinafter, the probabilities for *CA* <*CB* and for *CB* <*CA* behave like *a*and *b*and these two probabilities themselves are and .

Be in line now

A CM B

M the real midpoint of *AB,* and *C* lie of *M* something few things by *A* to, it is *a* time my judgment was a real, and *b* times I was wrong. For I have believed *b* times the point *C* between *M* and *B* ; So have at each of these *b* the point by more than the small line estimates *CM* erroneous, through *M* addition to *B* to assume therefore have always a mistake> *CM* , after all the same side, committed.

The probability for this is on the one hand = , on the other hand, where *CM is to be* regarded as a positive size. Now is

+

therefore, therefore

,

Finally:

These two formulas and could be also so explained: When *n* times, consideration of the line *ACMB,* of but only the points *A* and *B* are visible, it is believed in *a* case that *M*between *C* and *B* is located somewhere (as the truth is); in *b* cases (falsely) that *M* somewhere between *A* and *C* is located. In the same

two sections *CB* and *AC* itself but also the limits of integration, as for which relate

··· -*H. CM* and ∞ , for ··· - ∞ and - *h. CM* are. Namely, when the direction *ACMB is taken* for the positive and *M* as the starting point, the abscissas of *C* and *B are* - *CM* and *MB,* the abscissas of *A* and *C* = - *AM* and - *CM; AM* and *MB* are to be regarded as infinite against *CM* .

As far as the Möbius derivative.

In order to reduce the example of the lines to the example of the weights, one will compare the one weight *P* with *AC,* the other *P* + *D* with *BC,* the

length with *P* + , and thus the piece *CM* with , thus

for *CM* in previous To substitute formulas. Furthermore, it is equal to ours

and equal to ours, resulting in the formulas for direct application to our method:

or if we have the integral

short with Θ designate

,

The fact that we take the precision or sensitivity measure of our method h equal to half the precision measure of error theory , as mentioned above , has no bearing on the applications within our method, since it depends only on ratios of t or h ; but it would be considered to compare, for instance, the results of the method of right and wrong cases of absolute value with those obtained by the method of mean errors, to which the integral Θ grants the mediation, as well as in the aprioristic calculation of the probable Error or uncertainty of or t , which we are not dealing with here.

Let us now turn to the practical:

The rule involved is simply that of finding the appropriate value $t = hD$ for the book values given by the experiments in the following table, which I call the fundamental table of the method of right and wrong cases (using an interpolation if the value is not exactly to be found in the table) and by dividing this value with D to determine the value h , which is the required measure of sensitivity, or even at constant D *to use* the value $t = hD$ thus found itself directly to measure, which is comfortable in many cases.

This rule is sufficient if, apart from the constant weight excess *D,* there are no other constant influences which can determine the judgment where the excess weight falls, or if such could already be considered compensated by the arrangement of the experiments . If not, the constant contributions are included in the value t ; it no longer depends only on h and D, if under D always only the additional weight is understood, but also on these co-influences; the simple division of the value t with D can of course be $h$can no longer be found correctly, and the value t , even at constant *D,* can no longer be used instead of h to a comparable degree, if not with D at the same time the co-influences are constant. But offers a well-established method with appropriate application of the fundamental table also here a simple way of remedial action, which will be particularly mentioned below.

Fundamental table of the method of right and wrong cases [13] .

	$t = hD$	diff.		$t = hD$	diff.		$t = hD$	diff.
0.50	0.0000	177	0.71	.3913	208	0 , 91	.9481	455
0.51	0.0177	178	0.72	.4121	212	0.92	.9936	500
0.52	0.0355	177	0.73	.4333	216	0.93	1.0436	358
0.53	0.0532	178	0.74	.4549	220	0.94	1.0994	637

0.54	.0710	180	0.75	.4769	225	0.95	1.1631	748
0.55	.0890	178	0.76	.4994	230	0.96	1.2379	918
0.56	0.1068	179	0.77	.5224	236	0.97	1.3297	1234
0.57	.1247	181	0.78	.5460	242	0.98	1.4531	1907
0.58	.1428	181	0.79	.5702	249	0.99	1.6438	∞
0.59	.1609	182	0.80	.5951	257	1.00	∞	
0.60	.1791	183	0.81	.6208	265			
0.61	0.1974	186	0.82	.6473	274			
0.62	.2160	187	0.83	.6747	285			
0.63	.2347	188	0.84	.7032	297			
0.64	.2535	190	0.85	.7329	310			
0.65	.2725	192	0.86	.7639	326			
0.66	.2917	194	0.87	.7965	343			
0.67	.3111	196	0.88	.8308	365			
0.68	.3307	199	0.89	.8673	389			
0.69	.3506	202	0.90	.9062	419			
0.70	.3708	205						

[13] Revision p. 66 f. Fundamental tables for extensive experiments see: About the dimensional determinations of the sense of space, Abh. Dks Ges. D. W. XX, p. 204 ff.

Remarks. 1) Insofar as it only depends on ratios of *t* or *h* , I use the numbers in the values *t of* the table instead of as decimal fractions to use as integers. This will always be the case for future quotations calculated according to the table. 2) It is only necessary to set the table to values above 0.5. If values of less than 0.5 prevail , as is often the case under given experimental circumstances in the case of not too large *D* for this or that of the main cases to be discussed below , then one has instead to look in the column of the table, and the associated value *t*with a negative sign in the equations to be *stated* later for the determination of *hD, hp, hq* introduce. 3) The table gives an infinite value for $t = 1$, ie for the case that all cases are correct . Strictly speaking, however, an infinite number of observations is required. In general, one must take *D* small enough and n large enough that that case does not occur.

The most convenient method is to use the previous table, assuming n = 100 once in a while , ie determining r for 100 cases each time , and dividing larger series of experiments into fractions of 100, and then adding the individual t-values obtained from them Sum or mean, since fractional treatment is in any case necessary or useful from another point of view. In fact, in the column one simply has to draw out the zero and the comma in front, in order to find the numbers r obtained by the experiment directly therein; and one not only spares the division for the formation of the values , but also requires no interpolation, since then all the experimental *numbers r* immediately right in the table.

If you choose another n than 100, you will always encounter values that are not exactly in the previous table. Then, with the help of the differences in the difference column, one can easily determine the associated t values by simple interpolation, whereby one can miss up to about 0.85 at most by 1 to 2 units of the last decimal in the t- value, which is irrelevant Obtaining the 4th decimal on observations of this kind can be considered a luxury anyway. At higher values, however, the higher these values are, the more erroneous in this interpolation; and I therefore add a few additional tables to supplement the last part of the table, wherein the values Closer to each other, and with their inclusion one will be sufficient in any case as the basis of a further interpolation.

Additional table I.

	$t = hD$	diff.		$t = hD$	diff.		$t = hD$	diff.
0.8300	.6747	70	.8825	.8397	91	0.9300	1.0436	133
.8325	.6817	71	.8850	.8488	92	.9325	1.0569	137
.8350	.6888		.8875	.8580		.9350	1.0706	
.8375	.6960	72	.8900	.8673	93	0.9375	1.0848	142
0.8400	.7032	72	.8925	.8768	95	.9400	1.0994	146
.8425	.7105	73	.8950	.8864	96	.9425	1.1145	151
.8450	.7179		.8975	.8962		0.9450	1.1301	
.8475	.7253	74	0.9000	.9062	98	.9475	1.1463	156
0.8500	.7329		.9025	.9164		.9800	1.1631	

.8525	.7405	75	.9050	.9267	100	0.9525	1.1806	162
0.8550	.7482	76	0.9075	.9373	102	.9880	1.1988	168
.8575	.7560	77	0.9100	.9481	103	.9575	1.2179	175
0.8600	.7639	78	.9125	.9591	106	0.9600	1.2379	182
.8625	.7719	79	0.9150	.9703	108	.9625	1.2590	191
.8650	0.7800	80	.9175	.9818	110	.9650	1.2812	200
.8675	.7882	81	0.9200	.9936	112	0.0675	1.3048	211
.8700	.7965	82	.9225	1.0056	115	0.9700	1.3297	222
.8725	.8049	83	0.9250	1.0179	118	.9725	1.3569	236
.8750	.8134	84	.9275	1.0306	120	.9750	1.3859	249
.8775	.8221	85			123	.9775	1.4175	272
.8800	.8308	86			127			290
		87			130			316
		89						

Additional table II.

	$t = hD$	diff.		$t = hD$	diff.		$t = hD$	diff.
0.970	1.3297	107	0.980	1.4522	150	0.990	1.6450	278
0.971	1.3404	109	0.981	1.4672	156	0.991	1.6738	304
0,972	1.3513		0.982	1.4828		0.992	1.7032	
0.973	1.3625	112	0.983	1.4991	163	0.993	1.7375	343
0.974	1.3740	115	0.984	1.5164	173	0.994	1.7764	389
0,975	1.3859		0.985	1.5345		0.995	1.8214	

0,976	1.3982	119	0.986	1.5337	181	0.996	1.8753	450
0.977	1.4110	123	0.987	1.5742	192	0.997	1.9430	539
0.978	1.4242		0.988	1.5961		0.998	2.0352	
0.979	4.4380	128	0,989	1.6195	205	0.999	2.1851	677
		132			219	1,000	∞	922
		138			234			1499
		142			260			∞

In itself, the number n = 100 has no special preference; and I have always used n = 64 as the basis for n = 100 , divided all my larger test series into fractions with n = 64 , and then added the t values that were specially calculated from the fractions , and used these sum values or the averages derived from them. The reason was that 64, as a power of 2, is capable of a larger subdivision of 2 than 100, and I initially wanted to keep it open for arbitrary fractionation. Later I stopped to compare all the experiments in this respect, since, as you will see later, the size of the *n,* what one bases on, has a certain influence on the size of the measures, which one must hold comparable everywhere. Therefore, in order to save the translation of the fraction into a decimal fraction and interpolation just as indicated for the above table, the fundamental table I usually use is equal to *r,* pertaining to n = 64; and I add them here if others want to use the same basic number.

Fundamental table for n = 64.

r	$t = hD$	r	$t = hD$
33	0.0277	49	.5123
34	0.0555	50	.5490
35	0.0833	51	.5873
36	.1112	52	.6273
37	.1394	53	.6695
38	.1677	54	.7143
39	.1964	55	.7619
40	.2253	56	.8134
41	.2547	57	.8696
42	.2844	58	.9320
43	.3147	59	1.0026

44	.3456	60	1.0848
45	.3772	61	1.1851
46	.4095	62	1.3172
47	.4427	63	1.5231
48	.4769	64	∞

In order to be able to treat my larger series, which always contain multiplas of 64 cases, comparatively with the fractional treatment, but also as a whole or in larger compartments, I have a larger table for n = 512, in which 64 is contained 8 times. constructed, which immediately also tables for n = 64, = 2 . 64, = 4.64. Incidentally, by dropping to the table of the Θintegral (see above) and using the equation between and Θ given above , the expert (with the help of interpolation) easily becomes tables for arbitrary basic numbers *n*can design. In any case, you will do well, what basic number *n* one also may choose to always maintain the same for all trials, always return with a greater number of experiments by fractionation in the same table and his once set out for all.

Prominent fundamentals tables you can now use to get out of what is given a *D* and *P* has obtained, to the *D* to close what at the same sensitivity *h* and thus the same *P* (because *h*with *P,* but not with *D* changes) would be required to give any other , just having to look for the other *t* in the table, the corresponding *t* and to set the following proportion: As the *t* = *hD of* both behaves, the *D* behavesthe same. Conversely, according to the table given *D* ' *s* , we can find the corresponding values if such is given for a *D* , as long as *h* remains constant. However, it will not be easily led to these applications practically by our method by the above determination manner of *h* or circumstances merely *t* is that upon which everything depends recently.

It must not be forgotten that the stated simple use of the table only takes place under the condition given, that the apparent preponderance, apart from the accidents, depends only on *D* ; but in reality it still depends on constant influences of time and space; and the value *t* to be derived from the table in this case is not merely = *hD,* but = *h (D* + *M),* where *M is* the algebraic sum of all constant contributions that are still outside *D* determine the apparent overweight. In view of this, the practical task is to combine the experiments and their computation so that *M is* compensated, and to return to the same value *hD* , which would be obtained without the existence of the contributions to the above simple use of the table.

As to the method of experimentation, our normal mode of execution, which was mentioned above, is calculated immediately for this purpose. Here, after a very regular mode, there is a change between four main cases of opposite time and space position of the additional weight, namely 1) where it is in the left-hand vessel, and where it is first removed; 2) where it lies in the left-hand vessel, and where this is lifted in two; 3) and 4) according to the right vessel; so, to keep the 4 main cases clear where it is:

1) in the left-most first vessel,
2) - on the left-hand side - -
3) - on the right-hand side first - -
4) - on the right-hand side - -

Briefly, I call these 4 main cases in the previous order

I>, II>, I <, II <.

The thereby obtained, for each main case especially together counted, correct numbers with

$$r_1, r_2, r_3, r_4$$

and their quotient by *n* corresponding values *t* of the fundamental table (which is no longer simply = *hD* are to be set) with

$$t_1, t_2, t_3, t_4$$

whereby for all main cases a same *n is* presupposed.

The way of fully compensating for *M* is then, as easily shown, to add the thus obtained *t's of* the *i* major cases and divide by four by having

after which dividing by *D* as before gives the pure value of *h* , instead one can again use *hD* or 4 *hD* for the measure if *D is* always kept constant.

This way of complete compensation of the co-influences *M* is based on the following points. According to (above), a co-influence dependent on the time sequence of the elevation and a dependence of the position of the vessels on the determination of the apparent excess weight takes place. The influence which depends on the time sequence of the uplift I shall call *p,* the space-dependent *q* . In opposite time and space *p* and *q have* an opposite sign. Which sign we want to use for a given position is arbitrary, except that we use the opposite in the opposite. So let's put *p* and *q* at the first main *trap*With a positive sign, *M* assumes the value + *p* + *q* for the first main *trap,* for the second - *p* + *q,* for the third + *p* - *q,* for the fourth - *p* - *q* , we obtain the following values for the 4 main cases For

$$t = h(D + M)$$
$$t_1 = h(D + p + q)$$
$$t_2 = h(D - p + q)$$
$$t_3 = h(D + p - q)$$
$$t_4 = h(D - p - q)$$

The addition of these 4 values and division with 4 gives *hD* ; Also, the addition of the first and fourth, as well as second and third equations, with subsequent division by 2, is sufficient alone to have *hD* found.

The same equations are suitable for giving , by additive and subtractive combination, the values of *hp* and *hq,* and consequently of *p* and *q* . You get so first:

,

Dividing the values thus obtained from *hp, hq* by the values obtained above , one obtains the ratio of *p, q* to *D* , and by multiplying this ratio by *D,* the value of p, *q* in grams, when *D* itself is expressed in grams , Also, *hp, hq* as well as *hD can* each be determined in two ways by the *t's of* two main cases, and a check can be made in the correspondence of the values thus obtained.

Depending on the direction of the influences *p, q* , they may just as well emerge with this sign of negative or positive sign, so that the magnitude of their direction determines their direction at the same time; where the sign is to be understood with respect to the way *p* and *q are introduced* into the fundamental equations.

The definitive solution of the whole problem with its secondary tasks thus leads to the determination of *h* , *p, q* by the following equations:

However, one often gets 4 *hp* , 4 *hq* for the *hD, hp, hq* or 4 *hD* values *,* or, in the case of a combination of the results from several, only the same number of fractions for any larger multiplis of those values can stand still, as the expert easily overlooks.

In this way, whereby at the same time one obtains a complete elimination and exact determination of the influences *p, q* , all of my later (in the 9th and 12th chapters) the following determinations of the sensitivity of difference in the field of weight tests are obtained; The results to be carried out there serve in multiple relation to the explanation and evidence of what is said here about the object in general. More complete and more coherent in this regard will be provided by the "Mass Provisions".

If I have to refer to it in the future, I will, in accordance with the choice of omens, take the influence p dependent on the time sequence of the upliftment as positive, if by virtue of this the first canceled, as negative, if the second-lifted vessel independently of D, the heavier appears to be the positive influence of the spatial position q, if by virtue of it the left-hand, as negative, if the right-hand vessel appears as the heavier one. So I say z. If, for example, the influence p weighed +10 grammes, this means that apart from the additional weight, the vessel which was lifted first appeared to be ten grams heavier than the second one. Chapter 12 will provide an opportunity to cite such provisions.

Even if the relations of the time and space of the vessels remain the same, p and q can nevertheless change for internal reasons, since those objective relations come into consideration only according to their subjective conception, which for very unknown reasons is very changeable.

However changeable the influences p and q are according to external and internal conditions, it has been found from the totality of my experiments, made under numerous modifications, that the influence p is increased by increased heaviness of the main weights, or by previous fatigue of the arms in one-handed and two-handed process has a tendency to change in a negative sense, ie to accept lower positive, or greater negative values, or to change from positive to negative values, further that p and q in the one-handed process and under otherwise equal circumstances larger positive or smaller negative Have values at the right as left; finally, that the size and direction of these influences does not depend essentially on the size of D. For more details is not here.

One might want to effect the compensation of the contributions *p, q* also by combining the *r of* the 4 main cases before the calculation of the *t's* ,

and deriving from the thus obtained common to the fundamental table a common *t* which one set = *hD* . This method may, under certain circumstances, provide services, but I designate this as incomplete compensation, by showing that it does not really return to the value *hD,* and hence h, which would have been obtained without the existence of the co-influences ,

If, for example, the influence *p is* in favor of the second-lifted vessel, and if we assume that it is exceedingly large, infinitely large, then it is self-evident that the addition of a finite *D*to the one vessel would become completely uninfluenced by the judgment and always and every time the second vessel lifted would appear heavier; that, therefore, if the vessel is annulled with *D* just as often first as a pair, as in our experiments, and if the cases of these two timings are taken together, one might be tempted to consider as amenable to the elimination of *p*the number of correct cases and false cases will be as equal as if the sensitivity for the weight difference were zero, where one also receives an equal number of right and wrong cases. The sensitivity to *D* appears to be so overwhelmed by the influence. Whereas, if the influence of the time sequence of the uplift were absent, *D* to assert its preponderance equally at both timings, and to justify an overweight of the right cases for the vessel in which it lies, commensurate with its size and the existing sensitivity. Thus, the combination of the correct cases at opposite times can not be set to be equivalent to the case that no influence of the time situation has ever been present. For, understandably, the stronger the co-influence, the more we approach that presupposed extreme. And what applies in this regard to *p* applies just as much to *q* and to the simultaneous existence of both. On the other hand, one gets through our method of complete compensation, where the numbers *r* for the different main cases are separated for the derivation of *t*are really attributed to the same result in terms of *hD* as if there were no co-operation *p* and *q* ; by eliminating the same.

As it is easy to understand, just as the influence of *D* on *p* or *q can* disappear, the reverse must take place as well. If *D is* very large, neither the influence of the successive abolition nor the influence of the hand position can be more noticeable, but the judgment depends only on the position of *D,* and, as far as *D* often takes on opposing temporal and spatial positions, as it does in our case study, the number of first and second cases, of the right and left cases, must be equal, or should approach more and more with increasing *D of* this equality.

Although all this results in a slightly theoretical manner, I admit that it was only through the experience itself that the influence *p* sometimes became so great that the kind of overpowering of the influence of *D was* already evident in the experiments without calculation and after the calculation the legal dependency relationships of the difference sensitivity appeared considerably changed, in the past I always combined the correct cases of the different time and space situation before the calculation of the *t*- values.

It is easy to see that the process of repeated reciprocation of the vessels (see above), which does not allow any distinction between the four main cases, can only afford this success of incomplete compensation.

Incidentally, the method of complete compensation can be dispensed with if it does not depend on an actual measure of the difference sensitivity, but only on judgment of more, less, and equal, and if there are no or no strong variations of the influences *p, q* in the course of time Has to presuppose examination. Then one will not only put together the numbers of all 4 major cases, but also keep it unnecessary to move from the correct numbers to the *t* values first, by taking an equal, larger or smaller number *r* given *n* using a given *D*, then an equal, greater or lesser difference sensitivity proves. But one must not forget that this remains tied to the condition of the constancy of the influences *p, q* . But, according to the above, a considerable quantity regularly has the same effect in the opposite sense of changing constant influences as after (i) the greater magnitude of irregularly changing contingencies, ie, to reduce the correct numbers *r* , so that with the same or even greater sensitivity of difference the combined correct numbers *r*The four main cases may be lower if the constant contributions are greater, so that false conditions can turn out in this way, which disappear only by way of complete compensation. Insofar as the great variability of these influences for internal reasons (see above), even with carefully comparable external relations, can never fully assert that they are really comparable in the values to be compared, the course of complete compensation becomes more complicated hereby the separation of the four major events and decrease the *t* values, always provide greater security, and the comparison of mere numbers *r* can only serve to more superficial and temporary provisions.

The methodical observance of the same observation number and the regular change of the 4 main cases, without which the exact elimination and determination of the constant influences *p, q* can not be achieved, presupposes a regular change of the position of the additional weight and thus constant knowledge of this situation. This knowledge would, in the given first method, where every decision which makes a contribution to the number *r be made* as a definite definite form only after repeated reciprocation of the vessels, would necessarily exert a determining influence on the judgment which it gives in the second, where the failure of each double levy contributes to *r*Since one knows that this failure is determined in an unpredictable manner by coincidences and by the spatial and temporal position of the vessels, the imagination loses no knowledge of the position of the *D* , loses a certain success of the individual double elevations then anticipate, but can only hold to the statement of the sensations as something objective. The sight of my watch tables confirms this. The failure of the individual judgments manifests itself quite irregularly and by the value and the relations of *p, q* on the whole just as much, and often more than determined by the situation of *D* , indeed the number of false cases, contrary to what the known location of the *D* would demand in many series of experiments in some main cases predominantly on the right one.

According to this, even in the second method, the use of what is indispensable in the first method, the position of the additional weight without our knowledge, is dispensable, and even here is not permissible, since rather a constant own control over the position of the extra weight and a whole undisturbed uniform tension of attention during the course of continued uplift is quite essential in this method.

After having done a few months by experiments after the first procedure, with careful observance of the ignorance of the position of the extra weight, before passing on to the second with knowledge of its position, I am well able to compare the ratios of the two modes of procedure, and would not have stopped at the second, if I had not sufficiently convinced myself that the necessary knowledge of the position of the additional weight was also safe.

If one does not find these explanations sufficient to exclude the suspicion of a co-operation of the imagination in my attempts made according to this method, then I must also refer here to the "measuring methods," where partly the more detailed description of the state of affairs of this experimental method, and partly the mode even how their results are expected to be more effective. In any case, I would allow objections from this point of view only on the basis of careful own examination of the procedure.

In the calculation, I not only divide the series of experiments into the four main cases, but also remarkably into fractions according to time and other circumstances, in such a way that every single *t*- value is subdivided into a fraction of 64 simple uplifts or cases; obtained fractions *t* to combine values to sum or average values, instead of the derivative of the *t* each main case from the total- *n* which gives the series adapted to perform, for reasons that are already indicated several times in general, and in the "measurement methods "will be discussed in more detail.

However, the calculation in this way, especially in larger series of experiments, rather cumbersome; but variations of the constant influences become less harmful.

It should be *noted* that the value *hD* for the derivation of fractions is averaged slightly larger than the totality obtained, the more, the smaller the fractions are taken, of which the reasons can be theoretically stated, but what I pass for now. Accordingly, it is necessary to compare the attitude of the derivative values always of fractions with the same *n* perform, and the *n* specifying to which has been fractionated. This *n* is therefore in the future to be leading me results where otherwise expressly stated, always been 64, with regard to simple elevations.

There are still practically useful remarks on the size of the *D* to be used in the experiments , which should not be taken too small or too large, about the safety provisions of the results and some minor points which I should discuss with the methods of measurement.

e) Special to the method of mean error, in application to the Augenmaß- and Tastversuche [14)].

As far as the experimental side is concerned, I notice that, in the case of straight measurements of better parallel threads or points or distant points than circle distances using thimble circles, the distances to which the estimate is to be applied are not included in the estimate of the angular width bring to; unless such is itself the object of observation.

[14)] Revision pp. 104-119.

For the palpation experiments I apply a stalked circle of females with inset English sewing needle points, and grasp the circles in the tests on the stems [15)] . The tips are not or slightly blunted in order to determine the distances on a scale with transversal accurate, but are set up very quietly and the experiments are not continued to irritation. I have done most of the tactile tests on myself, but comparatively also with the application of the compass by an assistant, with much less constant, but because of the more uneven application of the compass by a foreign hand, which increases the play of randomness, much greater variable Errors have been received, whose distinction and separation will be discussed immediately.

[15)] The application of unsadded circles with the compass on the thighs and self-application of the same, according to comparative experiments I have made, gives greater constant as well as variable errors.

Normal distance I call the distance, which is constantly added to the comparison in the Augenmaß- and Tastversuchen, misreading the, in general with a fault afflicted, distance, which has been estimated equal. The difference of a wrong distance from the normal distance gives what I call the raw error and denoted by ∂ , to distinguish it from the pure error Δ to be considered at the same time .

As already noted, the mean miscarriage distance derived from many observations generally deviates from the normal distance by an often considerable magnitude, and the positive and negative sum of the raw errors are not equal in absolute value, but mostly one outweighs the other , To take this circumstance into account, I consider the deviation of the mean error distance from the normal distance as constant error, and the deviation of a single error distance from the mean as pure variable error, and substitute the consideration of these two errors of consideration of the raw errors. As the raw error is algebraically composed of the constant and pure variable error, I call both the components of the raw error. The constant error I signify$c$, the pure variable error with Δ , and the sum of the pure errors obtained by a given series or fraction with $\sum\Delta$. Only pure mistakes should be applied to measure of difference sensitivity, and to draw only from them, not rough, errors of average errors serving for this measure. The constant error is based on constant influences on the time and space of the quantities being compared and on the way in which the subjective judgment determines how the judgment is affected.

The need to resolve the raw error into its components rests on mathematical as well as experimental grounds, which I discuss in the "measurement methods"; also there are mathematical relationships between the raw error and its components that it is useful to know in handling the method, which will also be an object of special discussions in the "measurement methods" by limiting myself to the essentials everywhere here to discuss the method.

Of fundamental importance to the method is the substantial independence of the pure variable error from the constant, which has been proved by the experiments, so that in opposite spatial and temporal position of the compared distances, with which the constant error changes in the opposite sense and the raw Error sum often very different, noticeably receives the same pure error sum; Unless the opposing constant space and time position carries at the same time a play of irregular coincidences of a different average size, which according to experience is not easily the case. According to this, it often does not seem necessary to repeat the experiments on the opposite distances and time positions of the distances compared in order to ascertain the conditions of the pure variable error. when it comes to determining the constant error. By suitably combining the values of the same given at opposite spatial and temporal positions, one can separate them into different components according to their relations of dependency, as I show in detail in the "measuring methods" and which the expert himself overlooks; a method which agrees substantially with the one in the method of the right and wrong cases for the separate determination of the influences show closer and the expert overlooks of its own accord; a method which agrees substantially with the one in the method of the right and wrong cases for the separate determination of the influences show closer and the expert overlooks by itself; a method which agrees substantially with the one in the method of the right and wrong cases for the separate determination of the influences*p, q* served.

In the case of the approximate measurements one must differentiate (depending on horizontal or vertical distances) the right and left position or the upper and lower position of the normal distance by the wrong distance; in the tests of the compass, the compass is replaced by the right circle, the circle of the left by the wrong hand, and vice versa, or, if one tries to do the other with one hand, and therefore holds both circles in the same hand, composing one circle with the upper one, the other with the lower part of the hand and vice versa. In addition, I have made in the Tastversuchen also a contrast to the time, depending on the normal circle or Fehlzirkel is applied with each comparison first.

When forming the average error from the pure variable error, one has the choice between two types of mean error. The one which I call mean error par excellence, or the particular distinction of the following simple mean error, denoted by ε , becomes by simple means the pure sum of error, according to the equation

won, if *m means* the number of errors that contribute to $\sum\Delta$. The other, which in astronomers bears the name of mean error par excellence, but here is called quadratic mean error and shall be denoted by ε_q , is obtained by making the errors singly to the squares, the sum of these squares $\sum(\Delta^2)$ divide by the number of the same m , and take the square root of the quotient, that is, the equation

,

In a word, he is the root of the middle squares. Both median errors, if derived from a large number of errors, theoretically taken, according to the probability calculus a constant relationship to each other, which is

when π the Ludolf'sche number, after which the square mean error noticeably exactly 5 / 4 is simple. By examining a large number of experimental series, I have convinced myself that experience corresponds very precisely to this situation, so that only random, and with a sufficiently large number of tests, very small fluctuations take place around this normal ratio. Evidence to s. in the measurement methods; also one can derive such from the results quoted in the 9th chapter about the eye-measure experiments. According to this, it does not matter whether one sticks to ε or ε_q . Meanwhile, a choice takes place according to which ε ισ much less cumbersome in the derivation, ε_q is somewhat safer in determination from an equal number of observations, so that (according to the principles of probability calculus) 114 observations are required to determine ε equally well, as ε_q *is determined* from 100 observations. After in-depth discussions in the "measurement methods" I do believe that the practical consideration for some great number of observations, as called for in our method, everywhere, mainly for ε speaks without the relatively small, and large *m* quite negligible preponderance of Security for ε_q compensated for the practical advantage. But the choice is left to everyone. Where one always μακεσ τηε same number of observations in order to obtain a result, one can use the pure error συμ ∑Δ directly to the same extent as ε , thus saving the division by *m* .

A special consideration deserves the fact that the pure error sum, just like the pure mean error, be it ε or ε_q , a somewhat different size, according to whether one determines the mean error distance against which the pure errors are calculated as the mean value of the totality of an error number, or divides the error number into fractions, in particular the mean error distance for each fraction, whereas the pure errors in particular, and then collate the results into sum- or averages, which is analogous to the circumstance noted for the method of right and wrong cases and has analogous reasons. In general, all other things being equal, the less the error has been driven, the greater the net error and the pure mean error. larger so z. For example, if one deduces a pure error sum from 100 raw errors at once, as if one divides these 100 errors into 2 fractions à 50, from each of these fractions in particular derives the pure error sum, and these two error sums merges. But this sum will again be greater than if you had merged the result of 4 fractions à 25, and so on. But the difference is very insignificant if you do not go down to very small fractions with the fractionation.

The reason is a double. One is that a small number of observations gives the mean error distance, and therefore the pure errors calculated against it, different from the true values, for which we should consider those obtained under the same conditions of observation from an infinite number of observations, and it is possible from the probability calculus, and confirms in the experience that the quadratic mean error thereby becomes necessary, the simple average (also necessary with normal error distribution) too small. The other reason lies in the variations of the constant error that can never be completely excluded in longer series of tests, whereby the pure error sums are contaminated and enlarged when observations,

Because of the first of these causes, a correction can be made, which I call the correction because of the finite *m* , which reduces the error sum or mean error to the case that the true mean error distance determines from an infinite number of observations, and against this the pure errors be counted. This correction has been already applied when the square mean error, if it served to accuracy requirements in physical and astronomical observation, and is that one ε_q instead = rather

= accepts, from which we already have seen that it is so insignificant, and the easier it is to neglect the larger *m* is. For the simple mean error ε , the corresponding correction has hitherto not been developed because no practical use of it has hitherto been offered. I find that after an analogous course, as the derivation of the correction

of the quadratic error , that it is multiplied by the factor , if π ισ the Ludolfian number. It is simpler and more precise enough to do this , which approximates a little more than the first offering , as one finds through the execution itself [16] .

[16] Also, the correction factor is only an approximate, which stands for an integral expression that can not be represented in finite form, but deviates only imperceptibly from it.

An expert mathematician has had the courtesy to control the derivative of this correction to be communicated in the measurement methods. The same correction factor has to be used to correct the error sum of finite *m* , if one stops at the error sums without taking the mean error ε ουτ οφ them. If one observes a series of observations on a fractional basis, ie if the pure errors are determined by the mean error distances of the respective fractions, then the correction due to the finite *m must be applied* to each fraction, in particular to the *m of* the fractions, not the total number of observations of all fractions , Examples of this s. in the 5th section of the 9th chap.

There where it just depends on money provisions, you will be the correction due to the finite *m* can save by reacting always the same *m* sets basis, or if one hires a different number observations, always on the same *m* fractionated by then from the Finiteness of the *m* resulting reduction of the mean error or error sums all in the same conditions applies.

Because of the second cause, no correction can be made; but to eliminate it considerably by sufficiently strong fractionation. Now that the first cause by a correction or an always equal *m* can be rendered harmless, so I prefer to also make the second harmless, generally before a strong fractionation of unfractionated treatment of larger trials. In my Tastversuchen I have always up to *m* fractionated = 10 (what ε and AΔ the correction factor $^{31}/_{30}$ there), and the 10 observations of each faction, where it could happen without too much irritation, are always made immediately behind each other. However, some parts, such as the forehead, do not tolerate so many successive attempts in the same place.

In any case, according to the present method, as with the method of right and wrong cases, it will be necessary to state in the results whether and to what *extent* we have fractionated it in its derivation. In this respect I shall use *m* and μ in the method of mean errors , as *n* and *v* in the method of right and wrong cases, that is, if fractionated, *m* for the number of observations entering a fraction , μ for the number of fractions, so that μ *m* the total number of observations is that contribute to a result with respect to one and the same observation value which is then made of μ will be merged individual results.

For error sums that give a very small midpoint error, it may be necessary to take into account two other corrections, which I call the correction because of the size of the intervals and the estimation of the classification. The first refers to the fact that one always records only errors which are spaced apart by certain finite intervals, which are the greater, the less the scale of the scale by which the errors are measured, and the subdivision is driven by decimals by estimation which reduces infinite errors but to the neighbors of this scale. This has an influence on the mid-level error. The second one refers to the fact that errors are again measured when measuring the errors on the scale.determine *a priori* ; the second demands experimental investigations as to how the estimation errors of the division behave at the various fractions of a degree of division, as an interesting investigation by Volkmann in the reports of the Saxon Soc. Jahrg. 1858 p. 173. However, I am abstracting here all the more from taking on these corrections, as they are almost always negligible.

Of greater importance are formulas and rules by means of which it is possible to determine the uncertainty of the mean errors and error totals according to the size of the observation number, as well as rules according to which the obtained individual results can be connected to the most probable result. Everything that is necessary to know in this respect can be drawn from the theory of probability and made practical for use; However, in order to do so adequately, it would require some preliminary discussion, which is likely to go too far here.

An intelligent handling of the method of mean errors requires a knowledge of the main points of mathematical error theory, which is part of the theory of probability. I think that in this respect the essentials in the "methods of measurement" can be understood even for those who do not want to delve into this doctrine themselves; but this can not be understood here.

f) Mathematical relationship of the three methods.

One may raise the question of the relationship between the values obtained by means of the three methods. Given the same difference sensitivity in a given sensory area, the just noticeable difference, the mean error, the ratio, and herewith t = *hD have been* determined. The question is, how will they relate to each other? The answer has to be based on the following aspects:

Strictly speaking, one would have to say: the just noticeable difference of a quantity is that which, applied as a difference of the quantities to be compared in the method of right and wrong cases, gives no false cases, but can not be reduced, without such give; for the fact that it is still perceptible excludes its occurrence and consequently every wrong case, and it is said that it can only be felt at the slightest reduction by the fact that it is only noticeable. But in reality, if you do not want to have wrong cases with a given difference, you have to take it high enough that it is not coincidences that reduce it to curiosity, and how high this is, or how many wrong cases, under a vast majority, are more correct wants to allow

The mean error, on the other hand, is necessarily smaller than the just noticeable difference, if it should allow no or only exceptionally wrong cases. Because if in the method of mean error a difference z. If, for example, two circular distances are still noticeable, the distance is changed until it becomes imperceptible; and in general, to determine the mean error, all errors from zero, which are smaller than the just noticeable difference, contribute. A fixed ratio of the just noticeable difference to the mean error, however, can not be specified for given reasons.

On the other hand, there is such a mathematical relation between the method of right and wrong cases and the method of mean error bound by the main integral of the probability calculus that it can be stated what proportion of right and wrong cases will arise if the size of the simple or square mean error as difference size *D used* in the method of right and wrong cases under otherwise comparable circumstances. And when using the simple average error is, as I will show in the "measurement methods", as the difference between size (extra weight in the weight attempts) the ratio noticeably $^{2}/_{3}$ precisely 0.658032.

In the meantime this theoretical relationship has yet to be proved by experiments, which may have some difficulty in so far as it is necessary to make the circumstances comparable for the methods to be compared in such a way that the coincidences acquire the same influence.

2) Measurement methods of absolute sensitivity.

The field of these methods has hitherto been almost ruptured with regard to the intensive sensations, and apart from a determination by Schafhäutl of still audible absolute sound-intensity, the determinations of EH Weber, and those of Kammler on still-noticeable pressure quantities, of which provisions in Chapter 11 I would not know what could be moved here. In the field of the sensation of light, even a pure determination of the absolute sensitivity is not even possible, because one can not eliminate an inner source of the sensation of light, of which I speak in chapter 9.

In contrast to this, the methods of absolute sensitivity have found a very extensive application in the area of extensive sensations, as long as it has been frequently studied to determine appreciable sizes or distances on the retina or skin. In the last analysis, Eber Weber's well-known and for the whole of psychophysics groundbreaking experiments on the just noticeable distances on the skin are known, by which is designated a mode of behavior which one can apply to the absolute measure of sensitivity, a method of behavior which is the method of noticeable differences for the degree of difference sensitivity is analogous. The other two methods of this measure, however, are analogous to the absolute measure of sensitivity.

Volkmann has found on the remark that can easily be stated that the width of the compass points, which gives a very noticeable distance, is not absolutely solid, but fluctuates within certain limits, by the same distance in the case of successive attempts as distance, sometimes not can be perceived as such, as long as an upper limit is not exceeded, from which distance is always perceived as a distance, or a lower one under which it is never perceived as such, but which limits themselves are not capable of absolutely exact determination. This is not prevented, as the experience itself has taught us, by a majority of contacts of the skin with the compass points under altered width, close to the upper limit, according to the previous method, or distance falling between the upper and lower limits as just noticeable average distance, comparable in various attempts to gain a measure of it. If this were not the case, Weber's experiments and other confirmed results would not have been possible. But it can be based on that remark a modification of the Weberian method, whereby it becomes an analogue of the method of right and wrong cases, and has in fact been founded on it by Volkmann, consisting in that in repeated experiments at a given circle distance between the specified upper and lower limits, the result of each circular application is noted, and the number of cases counts where the distance and unnoticeability of the distance takes place; that 2) this procedure is repeated at different circle distances within those limits. The greater the extensive sensitivity of the skin area, the greater the number of cases for a given circle size, which represent the correct ones, ie where the actual distance is perceived as noticeable, and the smaller the distance can be to deliver the same number of correct cases. Now one would be able to use any ratio of the right ones to the total number of cases as a standard of sensitivity; by going to the circle of the various parts of the skin, where they give the same relation; Perhaps, however, the most favorable aspect of this is the relationship favored by Volkmann in this respect, where the refinement occurs just as often as the unnoticed. Since the associated circle widths are not going to be absolutely accurate, one will be able to achieve exactly enough by interpolating the neighboring widths that have defeated the experiment. The experiments made by Volkmann on the course of the exercise of tactile sensitivity according to this method are contained in the reports of the Saxon Societ. 1858, p. 47 ff., And have proved the applicability of the method by their interesting results.

Another modification of Weber's method, which I call the method of equivalents, has been applied and developed in the tactile realm of myself in connection with the method of mean error whose analogy it represents; in the meantime EH Weber has already applied it earlier to experiments on the absolute sensitivity of different parts with regard to the pressure sensation [17)].

In essence, when applied to the tactile measurement, it consists in alternately placing, instead of a circle on a skin site, the two, respectively, *A, B,* on two different skin sites *A, B,*whose extensive sensitivity is to be compared, and firm *A* distance of *A* circle at *A*- site *B* distance of *B* circle on *B*After the feeling of the skin, the distance seems equal in both places, although in reality it may be exceptionally different, depending on the different sensitivities of the skin. Thus one obtains equivalents of equally estimated distances for both skin sites, whose reciprocal value, determined as the mean of a larger number of experiments, can serve as a measure of the extensive sensitivity [18)].

[17)] Program coll. *P. 97th*

[18)] On the determinations of the sense of space, etc., Abh. Of the kgl. Sächs. Ges. D. W. XXII, p. 273 ff.

It will be easy to see that this method is very fine and accurate, in that, as far as the sensitivity of the patches of the skin remains constant, there are very constant results with vanishing uncertainty, the first of which is the comparison of the various experimental fractions the easily calculated probable error of the mean result is proved; but in so far as that relation varies, allowing these variations to be pursued in a fine manner. As a matter of fact, in trials which have been continued for months on the same parts, I have found the most constant relationship to be maintained if only a few tests were made every day; just as decided but also several times, where many experiments were made every day, whereby a considerable influence of practice arose,

An advantage of this method over the two previous ones is that it does not confine itself to comparing the sensitivity of the skin in the limits of the just noticeable distances, but makes it comparable at any arbitrary distance; whereas it has its drawbacks in that it gives only comparative data of absolute sensitivity, whereas the value of a distance which is just noticeable, or an equal number of notable and notable cases, can be regarded as a datum indicating the absolute sensitivity of given areas of skin characterized in an absolute way. So each of these methods will have to apply in their own way.

As can easily be overlooked, the procedure which is adopted in the method of equivalents is essentially the same as in the method of mean error, except that the equalization of the two circadian distances for sensation is not effected on the same but on different parts of the skin, and not on the difference, but has the ratio of the compared sizes eight. But nothing prevents, in the method of mean errors, also the relation of the comparison quantities, ie the normal distance and the error distance, and in the method of the equivalents the deviations of the individual *B* distances from the middle *B* distance as well as pure Error Δ Considering this consideration, and taking into account this consideration, the method of equivalents is basically only the more general of the method of mean error, and this a special case of the method of equivalents, namely, where among all possible passages one is given *A*- position to the *B*- position, which makes *A-point* itself, whereby the *A*- distance in the normal distance, the *B*-Distance in the wrong distance passes. This is shown by the fact that the ratios of the constant error and the pure variable error of the method of mean errors in the method of equivalents only recur in a more general way. Thus, just as the method of mean errors, so does the method of equivalents require many considerations and precautions, which are related to those of the method.

Especially essential is the reversal of any comparison. If you have z. If, for example, the equivalent of *B*- lip against *A*- chin is determined, one must determine by an equal number of experiments the counter-equivalent of *B*- chin against *A*- lip, note both results, but finally take the remedy, not one with to obtain a one-sided result with a constant error. My "measurement methods" will provide sufficient evidence and explanation of how essential this caution is. The size of the constant error can also be found here by a simple calculation.

Fundamental laws and facts.

IX. Weber's Law [1)].

The law expressed in Chapter 7 only in general, as the chief underpinning of psychic measure, to which I give the name of Weberian, shall now be discussed in more detail on the part of its meaning, its reasoning, and its limitations, as far as it is so far Investigations provide a clue to it.

One can pronounce the same thing in various forms, which in the matter come to the same thing, but of which, according to circumstances, one or the other may be more expedient for the reference to it.

[1)] In the case of p. 42 ff. Revision p. 146 ff.

First and foremost, one can say that a difference between two stimuli, also tangible as positive or negative growth for one or another stimulus, is always perceived as equal, or gives the same sensation difference, sensation growth, if its relation to the stimuli between which it exists, or as long as it is taken as a growth, if its relation to the stimulus to which it grows remains the same, whatever its absolute magnitude changes. So that z. For example, an addition of 1 to a stimulus whose strength is expressed by 100 is perceived as strongly as an increase of 2 to a stimulus of strength 200, of 3 to a stimulus of strength 300, and so on

The following shorter expressions are equivalent to the preceding expression: the difference in sensation, the growth of sensation remains the same when the relative stimulus difference or relative stimulus growth remains the same; and: the sensation difference, sensation growth remains the same, if the ratio of the stimuli remains the same; It should be remembered (compare Sect. VI.) that with the constancy of the relative stimulus difference or stimulus growth, the constancy of the relation of stimuli, as, conversely, is given by itself, which allows the last mode of expression of the law to be substituted the first to put.

Finally, with reference to the conceptual discussion of the sensitivity to difference in chapter 6, the law can also be expressed as follows: the simple sensitivity of difference stands in inverse proportion to the size of the components of difference; the relative remains the same for every quantity of difference.

The law may be envisaged in the sphere of intense and extensive sensations, and in the first place, in terms of strength and height (insofar as a high-quality quantum of sound is given), without being justified from the outset. To regard the probation of the same in some special field of sensation as valid for another at the same time, rather, demands a special investigation in every sphere.

In the question of whether the law is confirmed in the domain of extensive sensations, one must substitute for stimulus and stimulus difference in the expression of the law the size of the extent and the difference in expansion, which are understood by the eye or organs of touch. One will find the law confirmed if z. For example, in the case of two lines twice as long, the difference must be twice as great as to be noticeably more pronounced or, more generally, equal in size.

With regard to the height of the notes, it is the number of oscillations that is responsible for the size of the stimulus.

With the correctness of the law, the correctness of many conclusions is automatically established; and to show that these conclusions are confirmed in experience, therefore, to be regarded as part of the probation of the law. Instead, however, of entering into this in the abstract, I prefer to be led to it in the special jurisdictions of the law in the various fields, and refer in this respect especially to the field of sensation of light.

With regard to the historical, I have already noted that although EH Weber is not the first to express and prove the law, he is the first to express it in a certain generality, to prove it and to present it from a point of general interest , He relies on experiments on just noticeable differences in weights, lines, pitches, which, as one may observe, are examples of the three main sides of sensation, intensity, extension, height, which can be considered at all more justifies that we designate the law by its name. To be sure, after the merely incidental interest in what was previously bound by the law, he did not subject it to much in-depth investigation. but to say so the attack points of all further investigation given by his. I therefore make his statements verbatim before proceeding to the further inquiries into the law, which became necessary after claiming to be the underpinning of psychic standards, since documents must be strengthened and extended, in accordance with there are heavier and more to rely on. By the fundamental importance which the law has for us in this respect, I shall as far as possible become acquainted with everything which has informed me of earlier and newer, foreign and personal facts relating to the proving as well as to the limits of the law completely inform. before proceeding to the further investigation of the law, which became necessary after claiming to be underpinned by psychic standards, as documents must be strengthened and expanded, as more serious and more must be based thereon. By the fundamental importance which the law has for us in this respect, I shall as far as possible become acquainted with everything which has informed me of earlier and newer, foreign and personal facts relating to the proving as well as to the limits of the law completely inform. before proceeding to the further investigation of the law, which became necessary after claiming to be underpinned by psychic standards, as documents must be strengthened and expanded, as more serious and more must be based thereon. By the fundamental importance which the law has for us in this respect, I shall as far as possible become acquainted with everything which has informed me of earlier and newer, foreign and personal facts relating to the proving as well as to the limits of the law completely inform.

After a preliminary overview it is to be admitted that much still lacks a thorough proving and even examination of the law. Much in this regard is in terms of intense sensation of light, sensation of sound and pitch, sensation of heaviness of weights, and in the field of judgment. Certainly, everywhere the law is more or less wide. In terms of temperature sensations, it is still considered problematic; in the sphere of extensive tactile sensations, the experiments speak against it rather than for its validity. With regard to other areas of sensation, no experiments are yet available.

The own data Weber's.

In general, Weber speaks as to the fact of the law in his treatise on the sense of touch and the common feeling p. 559 under the heading: "About the smallest differences of the weights, which we have with the sense of touch, the length of the lines, which we with the face, and the sounds that we can distinguish with the earpieces, "according to some special provisions," I have shown that the success of weight determinations is the same, whether you take ounces or solders, because the number of grains does not matter The question is whether the preponderance is the 30th or the 50th part of the weight, which is compared to a second weight, and so is the comparison of the length of two lines and the height of two notes [2)], It makes no difference whether one compares lines that are about 2 inches or 1 inch long, considering first one and then the other, and not both at the same time, next to each other, and yet the piece is about which one Line over the other, in the former case once again as large as in the latter. Of course, when both lines are close to each other and parallel to each other, one only compares the ends of the lines and examines how much one line surmounts the other, and then it depends only on how large the towering piece of the line is is, and how close both lines are to each other. - Even when comparing the height of two tones, it does not matter whether both tones are higher by 7 pitches, or lower, if they are not just at the end of the pitch, where then the exact distinction between small differences in tone becomes more difficult. Therefore, it is not the number of vibrations that one tone has more than the other that counts, but the ratio of the number of vibrations of the two tones that we compare "......

[2)] Delezenne in *recueil des travaux de la soc. of the sc. de Lille* 1827 in the Ausz. in *Bull. of the sc. nat. XI, p.* 275 and in Fechner's Repertor. of experimental physics. Leipzig 1832. Bd. I. p. 341st

"The conception of the relations of whole magnitudes, without having measured the quantities by a smaller scale and acquainting them with the absolute difference between the two, is an extremely interesting psychological phenomenon." In music we summarize the sound relations without knowing the vibrational numbers In architecture we have the relations of spatial sizes without having determined them according to customs, and so we take the sensitivity quantities or force quantities in such a way in the comparison of the weights. "

On the basis of the experience on which Weber bases his law, the general information about it is given with regard to the relations of tone and linear relations, but in the case of the unconditional fidelity of this observer it is permissible to enclose the weight of observed facts. With regard to the weight ratios, his experiments in s. *Program collect. p.* 81. 86 f. to find.

Weber distinguishes two types of experiment, one in which only the feeling of the skin is claimed by the pressure of stronger and weaker weights on the resting hand placed on the table, and the other, where the feeling of the muscular force to be applied when lifting the weights at the same time is claimed by the hand is lifted with the weights. Now, if 32 ounces or 32 drachmas were used as a larger weight, the relative difference to the smaller weight remained almost the same in both tests, and averaged 4 persons and both weights in the first test 10.1 (Ounces or drachmas), the second 3.0.

The detailed description of his experiments (Progr. Coll., P.

In piurimorum hominum manibus, mensa quiescentibus, pondera duarum librarum collocavi, tabulamque papyraceam interposui. Postea, insciis illis, pondus alterutrum imminui, manusque pondera ferentes mutavi, levius nimirum pondus nunc ad dextram nunc ad sinistram transferendo. Saepe etiam pondera a manibus ablata denuo iisdem manibus imposui, ita quidem, ut homo non suspicari potuerit, sed tactu tantum percipere, in quonam latere pondus gravius collocatum esset. Manusque gravius pondus a leviori recte discernebat, notavi. 'Turn si homo iteratis periculis et mutatis saepe

»Postea eadem experimenta in iisdem hominibus iterabantur, hoc modo tamen, ut manus, et manibus simul pondera, extollerent, et pondera manibus pensitarent. Quo facto, si inventum a me est, in quanta ponderum differentia diversitas eorum certe cognita frit, iterum notavi, numerosque, differentiam ponderum exprimentes, inter se comparavi. «

After communication of different series of experiments, which refer to other conditions, as its law, drives Weber p. 91 continued:

»Non silentio praetereunda sunt alia experimenta, quibus probatur, tactum et coenaesthesia etiam in observandis ponderibus multo minoribus eadem inter se esse ratione, quam si librae duae seu. triginta duae unciae cuilibet manui imponuntur. Eorundem enim hominum manibus, quibus antea duo pondera triginta diiarum unciarum imposueram, nuno pondera triginta duarum drachmarum ie octavam ponderis illius partem imposui. Etiamsi suspicatus eram, fore, ut

difierentiam ponderis duorum corporum octies minorum non tam clare sentirent, camouflage experimentis probatum est, differentiam minorum ponderum tactus non minus subtiliter distingui, quam differentiam and majorum ponderum.

Quatuor afferam experiinenta hoc probantia. Postquam nimirum quattuor homines, quos numeris signet lubet, pondera majora, triginta duabus uncis constantia, aequalia, manibus immotis imposita, comparaverant, alterutrum pondus magis magisque imminuere coepi, usquedum bomines illi differentiam ponderum animadverterent. Qua differentia notata experimentum idem hoc modo repetii, ut pondera manibus tollertur, adeoque simul ope tactus et coenaestheseos musculorum aestimarentur. Quo facto differentia ponderum, quae illorum observationem fugiebat, iterum notata est. «

»Nunc loco majorum ponderum minora pondera, triginta duabus drachmis constantia, eodem plane modo adhibui, differentiasque ponderum in experimentis non observatas, sensum scilicet fugientes, annotavi.«

"Lam si differentias ponderum graviorum et leviorum observationi nostrae subtractas comparas, easdem paene esse observabis."

Numerus hominum, Differentia minima unoiarum vel
in quibus experimentummarum, manibus imposita-
menta instituta rum, in qua diversitas ponderis
sunt. percipiebatur.

1. tactu. .... 32 unc. 17 unc. differs 15 unc.
tactu et coenaesthesi 32-30 $^{1}/_{2}$ - - 1 $^{1}/_{2}$ -
tactu. .... 32 drachm. 24 drachm. - 8 drachm.
tactu et coenaesthesi 32 - 30 - - 2 -
2 tactu. .... 32 unc. 22 unc. - 10 unc.
tactu et coenaesthesi 32-30 $^{1}/_{2}$ - - 1 $^{1}/_{2}$ -
tactu. .... 32 drachm. 22 drachm. - 10 drachm.
tactu et coenaesthesi 32 - 30 - - 2 -
3. tact. .... 32 unc. 20 unc. - 12 unc.
tactu et coenaesthesi 32 - 26 - - 6 -
tactu et coenaesthesi 32 drachm. 26 drachm. - 6 drachm.
4. tactu. .... 32 unc. 26 unc. - 6 unc.
tactu et coenaesthesi 32 - 30 - - 2 -
tactu et coenaesthesi 32 drachm. 29 drachm. - 3 drachm.

1) light. [3)]

In the treatises of the Saxon Society of Sciences, math.-phys., I have described in detail the validity of our law in the field of intense light-sensation. Cl. IV. Pp. 457 ff., Entitled "On a Basic Psychophysical Law, and Its Relationship to the Estimation of

Star Sizes," with a supplement to it in the reports of the same society 1859, p. 58 ff., From which essays I give here the essentials with a few additions.

[3)] In matters p. 149-160, 178-186. Revision pp. 152-168. Psych. Maßprinzipien, p. 181 ff.

The law in the field of the sensation of light has already been stated by earlier experiments of Bouguer, Arago, Masson, and Steinheil, occasionally in connection with other investigations, more recently by myself and Volkmann; without, however, much attention paid earlier.

All previous verifications of the law are based on the method of just noticeable differences, apart from that by Steinheil, which is based on the principle of the method of mean error, and the indirect probation which the law found in the estimation of the star-sizes.

Since my own experiments, though not the strongest but most simple proof of the law, and the first experiential knowledge of the law, have attached themselves to me, I shall make a start here, and make the general explanation of the law.

With half-covered skies, one can usually easily find a few neighboring cloud shades that only show a trail-by-eye difference for the eye, or a little cloud that only differs noticeably from the heavens. After putting two such components of a just noticeable difference of light in the sky into my eye, I put some gray glasses in front of my eyes, as they are now available to optometrists for use by people with dim eyes, each of whom, just before that eye taken after but only superficial photometric testing, about $^1/_3$, both together for no more than $^1/_7$of the light. We simply take in front of the eyes taken glasses the light of each component on $^1/_7$ reduced, so herewith the difference was the same at the same time on $^1/_3$reduced, and it was natural to assume that the so greatly weakened difference, as it previously was only noticeably unnoticed by the weakening, or, if, for example, the limit of the fuzziness were not attained before the application of the glasses, it would at least become noticeably more indistinct. But it did not show that way. The difference remained at least as noticeable as before, and others whom I made the experiment declared in the same sense.

The same experiment was repeated with just folded glasses using an eye at the end of the other, where the components together with their differences to a maximum of $^1/_7$ came down, the difference still remained at least as markedly.

Finally, weakening through colored glasses, with which I sometimes went down to considerably greater darkness, gave the same result. In this case, of course, not differently colored cloud shades or a cloud against the blue sky must be taken in the eye, since color glasses express a different proportionate absorption to different colors.

If we now observe that in weakening the absolute difference of the components in previous experiments, the ratio of the components, and hence their relative difference, remained unchanged, we shall see in the unimpaired distinctiveness of the difference an affirmation of our law.

For the first sight it might indeed seem very obvious and in contradiction with everyday experience that one, on $^1/_3$, $^1/_7$and, indeed, much more reduced photometric difference should be at least as noticeable to sensation as without weakening; as we diminish daily differences in light by weakening and disappear. But one must not overlook the condition of the law, under which it alone demands success, and under which it takes place alone, namely, that the difference in light, when weakened, retains an unchanged relation to its components, in the same proportion as weakened components. Hot the case of fulfilling this condition the first main case. But the difference can still be weakened in another way, in that the stronger component is countered by the sole weakening, or the weaker by the sole exaltation of the other. In this case, which is called the second main case, the difference with its absolute weakening at the same time experiences a weakening in proportion to its components; and then, indeed, as can be easily proved by later experiments in accordance with general experience, the distinctiveness of the difference diminishes, and disappears altogether if the components approach one another sufficiently.

To these two main cases may be added a third main case, which is able to provide direct confirmation of our law by the first indirect confirmation: that both components, instead of changing them in equal proportions, added an equal plus or deducted the same amount becomes. In this third case, in contrast to the first, the absolute difference remains the same, the relative changes. It decreases, if we add equal benefits to the components, it will increase if we deduct the same amount at the same time. Insofar as the law does not attach equality to the equality of the absolute, but of the relative difference, we shall have to expect, on the condition that it is correct, that in our third principal case the distinctiveness of the difference does not remain the same. despite the fact that the difference remains the same in absolute terms; rather, it decreases or increases as the same plus is added or the same deducted.

To prove that this is really the case; It does not require a specially thought-out attempt, even if the confirmation by tests is easy. But it offers us the same field of observation that has served us so far in a day-to-day experience.

At night everyone sees the stars, in full daylight he does not even see stars like Sirius and Jupiter. But the absolute difference in brightness between the places of the sky, where the stars are located, and the surrounding areas is just as great as at night. It has only been added an equal plus to the intensity of both by daylight.

Perhaps one could have interpreted the success of our first experiments with the clouds as follows: the difference between the dark glasses, however, was weakened in very strong proportions, but still absolutely existent, and so he still had to consider his absolute It is necessary to perceive one's existence without the necessity of making the progressive perceptibility dependent on maintaining the same relative size. But it is seen from the above experience that the absolute existence of a difference in light is by no means sufficient to make it perceptible, and that even very considerable absolute differences completely vanish from the eye when they show a very small relative magnitude. No one will be able to diminish the luminosity of the stars Sirius and Jupiter from the surrounding sky at night, and no one will be able to discover these stars in the daytime with keenest attention; so that the assertion may seem conspicuous, that the difference in brightness of the surroundings is as great during the day as it is at night. In fact, it is physical, whereas it is absolutely zero to the sensation, and indeed less than zero, insofar as it first needs a certain enlargement before it reaches the point from where it becomes noticeable.

Incidentally, one should not limit the phenomenon merely to points of light. The further experiments with shadows, on the other hand, give the most convenient opportunity to observe the same phenomena on surfaces of light of arbitrary extent with considerable absolute differences; but also experiences of everyday life can be reiterated in this relationship.

As is well known, the figures are based on original oil paintings, on daguerreotypes, painted plates, lacquered tables and the like. Like. By reflecting lights quite unrecognizable. Now, as we know, the intensity of the specularly reflected light does not depend on the color or darkness of the surface from which it is reflected, but on the same substance only on the smoothness of it and on the angle of incidence; thus adds an equal plus to the darker and lighter areas of the figures and the ground, thereby making the differences between them unknowable.

The foregoing is likely to suffice to prove the law in general. But can it really apply for exactly?

With diligence, I have said that the difference of the cloud shades when viewed with the darkened glasses seemed at least as noticeable as with the naked eye. For some of those whom I have repeated the experiment found with glasses even a little sharper than without glasses, and to me it often appears, though not always, so. In any case, one can be sure that a difference in light, as one would most easily like to expect, does not lose any of its functionality by weakening its absolute size while retaining its relative size. But even a gain in terms of clarity in this case would be a deviation from the law, which establishes a constant remoteness to the constant difference between them.

Apart from the fact that this could possibly involve altered altercation conditions, one could also think of a subjective delusion, the way in which one would be inclined to consider an equally noticeable difference to be more noticeable, provided that it is proportionate to the weakened one Impressions of the components is. In order to achieve a result that is as independent as possible from subjective illusions, I combine the following experiment with the previous experiment.

With the glasses in front of my eyes, I look up the weakest possible difference, just as noticeably prized, in the sky, and then take the glasses away from my eyes. If the clarity has been increased considerably by the glasses, then the difference with removal of the glasses, which is only noticeable with the glasses, must disappear. But

I have never been able to find any difference, however slight, in the repeated repetition of the experiment, with the simple and doubled-together lenses, which I could not recognize even after removing the lenses, if only the first impression of a momentary glare had passed. from which the eye finds itself struck by the suddenly stronger light when the glasses are removed.

In all the experiments mentioned, it is essential to use only very slight differences which bear the character of the image. For if the law, as we continue to show, allows for expansion to greater differences, it is not easy to prove it directly in such cases. The judgment as to whether those with and without glasses are equally clear is very uncertain and vacillating, and is undoubtedly co-determined by a majority of circumstances. Even when only just noticeable differences are applied, as we have seen earlier, the judgment of equality can be subject to the same deceptions, even if, in absolute terms, they can not be so significant as to apply greater differences. But the main advantage of using very small differences is that that the combination of the experiment with the countermeasures makes it possible to become completely independent of the judgment of equality or inequality, and to base the conclusion solely on the existence of the difference for the sensation, which one can not easily deceive about Equality. If the weakest possible difference, which is still recognized without glasses, is still recognized even with strongly darkening glasses, and conversely, if the weakest possible difference, which is recognized by strongly darkening glasses, is still recognized, there is some kind of objective proof that the difference through the glasses can not gain or lose any appreciable degree of refinement. to become completely independent of the judgment of equality or inequality, and to base the conclusion solely on the existence of the difference for the sensation, which one can not be so easily deceived as about equality. If the weakest possible difference, which is still recognized without glasses, is still recognized even with strongly darkening glasses, and conversely, if the weakest possible difference, which is recognized by strongly darkening glasses, is still recognized, there is some kind of objective proof that the difference through the glasses can not gain or lose any appreciable degree of refinement. to become completely independent of the judgment of equality or inequality, and to base the conclusion solely on the existence of the difference for the sensation, which one can not be so easily deceived as about equality. If the weakest possible difference, which is still recognized without glasses, is still recognized even with strongly darkening glasses, and conversely, if the weakest possible difference, which is recognized by strongly darkening glasses, is still recognized, there is some kind of objective proof that the difference through the glasses can not gain or lose any appreciable degree of refinement.

In any case, the combination of the experiment with the counter-experiments makes it possible to deviate from the validity of the law in the limits of the intensity of light in which the probation has hitherto held, which did not go as far as approaching complete darkness, or even very dazzling lights even trapped within very narrow limits. In the meantime no unlimited validity of the law is asserted or demonstrated, but rather a deviation from it, at least for the attempt, upwards as well as downwards, certainly. And before we go into the further trials, it will be useful to speak of these limits of the law, since the trials themselves can only take place and be understood with regard to the limits.

Certainly no one, even if the observation could be made without danger, recognize the spots in the sun (at least at a high level) with the naked eye, while each one perceives them with darkened lenses. But if the law should reach to the highest degrees of light, the spots with the naked eye would have to be so easily distinguished from the surrounding light-ground as with the use of dark glasses. It is not disputed that even at much lower light intensities there is a deviation from the law, probably everywhere the eye feels blinded, although certain attempts to do so are completely lacking.

It may therefore also be possible that in the case of very bright cloud illumination a small profit in clarification of the differences in the cloud shades is actually achieved by the dark glasses, but it can only be a very small order after the failure of the combination of trial and experiment I have not been objectively informed of attempts with moderately bright cloud illumination; Of course, because of the great irritability of my eyes, I was not able to do experiments with very dazzling lighting calmly and often enough to be able to say something certain about it.

On reaching the lower limit, it is clear from the very beginning that, if one wanted to go to extremes with the obscuring of the glasses, there was nothing left to be seen anywhere, and therefore no difference his; and that, according to the principle of continuity, one must already feel a diminished distinctness, if one approaches this limit only very closely, as the experience confirms. In fact, a difference may be as great as it pleases, so one will always be able to find a degree of obscuring the glasses in which it appears more indistinctly than without glasses. The same sunspots, which become clear in moderately dark glasses, become more indistinct in very dark glasses, and finally become completely unrecognizable.

Thus, instead of being able to assert an unlimited validity of the law, we can only assert, according to the testimony of the experiments, that in the, rather broad, limits of intensity in which ordinary seeing moves, it is so far confirmed that there is a deviation from Laws are not detectable.

But its validity in middle limits itself can in some way be deduced from the opposite direction of deviations from above and below. In intense light the clarity increases by weakening, in very weak by amplification of the components in the same proportions. Thus, from a mathematical point of view, there must be a certain middle interval, where it remains unchanged through amplification and weakening. Only that the great extent of such an interval could not be foreseen from a mere mathematical point of view.

I have preceded the previous experiments, not only because they were the ones that I first fell for examining the law before I knew what had been done earlier, but also because they were particularly convenient, easily accessible to everyone and, as a matter of fact, are as much evidential for the general fact of the law as all others are. But if one has neither the determination nor the uniform preservation, nor the alteration of the shades of light in its power, then one can not arbitrarily produce all three main cases with it; and from this point of view, however, it is advisable to include other types of behavior, which add the experiment to the observation.

Now there are very different ways. To produce shades of light of varying gradation up to the very noticeable difference between them, after which the experiment may take various forms. It is very easy to produce with ink the weakest possible shades on vellum paper, which, just as little as the cloud shades afford a measured difference, but nevertheless have the advantage of uniform preservation, arbitrary gradation and handling.

In fact, I have recently repeated the experiment and the counter-experiment on such and the corresponding success as before in the cloud shades. Even with darkening glass combinations that after careful photometric measurements only $^{1}/_{100}$ transmit light, I realize after I looked through a short time, nor the schwächstmöglichen shades that I only just recognizable find with the naked eye. Only the attempt must be made in good daylight; for if I replace it with the light of the study lamp in which I am accustomed to writing, the shading with the same obscuration becomes quite unrecognizable; however, a darkening to $^{1}/_{12th}$ and more, makes it appear as clearly as without darkening.

Another simpler and more convenient, at the same time certain measurements of permitting, and capable of modification in all three main cases, is the application of two neighboring shadows, produced by two lamps or lights from the same object, not by the photometric ratio of the two shadows only easily regulated, but also, with the use of equally bright light sources, easily measurable by the reciprocal ratio of the squares of the distances of both sources is given by their shadows, while the photometric equality of the sources by the same brightness of the shadows at the same distance from the same readily available , and by cleaning the lights or screws of the lamps is manufacturable. However, on the whole, it is even more appropriate

Let the two light sources *L, L '* and *L' be the* one whose shadow one wants to contemplate. This shadow is still illuminated by the other light *L,* the surrounding ground of both lights,*L, L '* . If one withdraws the light *L '* farther and farther from the tablet catching the shadows, while *L* stands still, then the reason surrounding the shadow receives an ever smaller illumination supplement by *L',* and finally it becomes so small that it becomes imperceptible to the eye, that is, the shadow disappears in the surrounding ground. Once this point has been reached, a slight displacement of one of the two lights or screws of either lamp in the right direction is sufficient to make it noticeable again.

Now one can first of all repeat trial and experiment with the dark glasses; and thus it will be able to establish the law as well as the lower limit of the law.

Instead of weakening the two components by the same conditions by means of dark glasses, the same attenuation can then be effected by displacing the two light sources *L, L '* into ever larger, but the same ratio, distances from the shadow-collecting panel. This is what happened in the following experiments. At the same time, the direction of the process was reversed in such a way that, instead of observing the constant distinctiveness of the difference as success of the relatively equal weakening of the components, conversely, this was observed as a success of producing the same feature, as more clearly apparent from the following will emerge. Instead, the new method of experimentation becomes a complement and control rather than a repetition of the previous ones.

As my very weakened eyes could not engage in such attempts, in which the most attentive attention and the keenest eye-sighting are necessary in order to perceive traces of the shadow that is in the process of disappearing or reappearing, Volkmann, with the assistance of a few co-observers, has the same position with good eyesight accepted. The following is the essence of the procedure and the successes.

A staff placed vertically in front of a vertical white board threw two shadows on the board under the influence of two light sources *L, L '* . The one light source *L* , a burning stearin candle, was obtained at a given distance from the panel, and the other, the same light intensity with which was detected photometrically in two ways, now moved back so far from the panel by one of the co-observers until that of Shadows sharply contemplated by the observer, which she threw, just ceased to be noticeable. For Volkmann's eyes, the distance between the candle *L 'and* the shadow had to be 10 times that of the candle *L,*ie the difference in lighting, where the shadow ceased to be just noticeable, $^{1}/_{100}$ of the absolute light amount. But the same relation of the distances, and hence the illuminations, where this point occurred, was also found in quite different absolute intensities of illumination, which was remarkably partly obtained by altering the intensity of the flames themselves, partly by making the flame *L* larger or smaller Distance from the board was offset. Always the distance of the flame *L 'had to be* noticeably 10 times as much to bring the shadow to the point of disappearance. So the attempt became of an intensity of lighting *L*equal to 0.36 is varied by intensities = 1, = 2.25, = 7.71 to 38.79, where as 1 is the illumination by a stearin orb in 3 decimeters distance from the white board, without the ratio of the distance of the other Light source to the board noticeable or significantly different. Only at the weakest intensity (0.36) was there a noticeable small drop, ie the distance of the light *L '* had to be slightly less than 10 times the distance of the light *L* (according to the table of the results 9-, 6-fold). In order to make the shadow disappear, it is undisputed that the lower limit, which has the validity of the law for the experiment, began to be exceeded.

For the sake of brevity, in this account I have referred only to the point of disappearance. In reality, however, around the point of disappearance, the light source *L 'was* alternately moved back and forth, so that between the point of disappearance and reappearance of the shadow, the point of discernment was obtained as accurately as possible; and since the obstruction of the light source *L* by an assistant was only due to the call of the observer, who was completely focused on the apperception of the shadow, the definite fixation of the distance took place without his knowledge of the observer and could not therefore such knowledge is influential, making the result of these experiments all the more ambiguous.

These experiments are by Volkmann under the direction of Prof. Knoblauch, Dr. med. Heidenhain in Halle and Dr. med. I was a young employee from Berlin, and partly also in my own presence. And remarkably found himself a little to all observers mentioned $^1/_{100}$ again the absolute lighting fluctuating value than just noticeable difference.

However, this method can no great sharpness in individual experiments to by the light *L '* within a certain width, which is about to Volkmann $^1/_{10}$ can may be the total distance dislocate, without knowing exactly where the point of Ebenmerklichkeit of the shadow to fix; therefore, for each observer, the means of several experiments was generally considered authoritative; however, the individual results often fluctuate very little about the means, and the uncertainty left over after the remedies is very small.

This experiment with the shadows corresponds to the first main trap; but it is also easy to understand the second by approaching one light alone without the other, or making it lighter, or darker, from the other; the third by illuminating the two shadows which make a difference, or a shadow and the ground, together with a third sufficiently bright light, whereby one is able to make a very distinct difference for the eye disappear.

As far as my own and through me induced attempts. In spite of the fact that they are not essentially new after the preliminary remark, their guidance could still be useful to the former, provided that they are independent of them and with some modifications, thus contributing to the safeguarding and clarification of the law. Now, however, the essentials of what I have gradually come to know of the earlier trials should also be added. First of all Bouguer has to s. *Traité d'optique on the gradation of the lumière par Lacaille.* 1760. p . 51 describes the attempt with the disappearing shadow in a similar manner as Volkmann [4)] and describes it under the heading:*"Observations faites pour déterminer, source force il faut qu'ait une lumiere pour qu'elle en fasse disparaitre une autre plus faible."*

[4)] I gather from Masson's account of the literal rendering of his words in *Ann. de Ch. et de Ph.* 1845. *T. XIV. p.* 148; since Bouguer's writing itself was not available to me.

Although it is merely the result of an experiment with a single distances both lights, after which a shadow at about 1 / 64 difference (instead of 1 / 100 disappears at Volkmann); but further says that this degree of sensitivity must vary according to the eye of the observer; but he had believed that he was independent of the power of light for his eye.

According to an oral statement by Masson, [5] Arago repeated Bouguer's experiments and also used colored lights. Arago himself, in his popular astronomy, [6] affirms positively the lawfulness of the law, saying, after arguing the Bouguerian method, "which is also the absolute magnitude of *M* and *L* (the two lights of Bouguer's experiment), always the attempt will lead to the same result (the same just noticeable relative difference). " But here he does not make his own experiments regarding the subject.

[5] *Ann. de Chim. et de Phys.* 1845. *T. XIV. P* . 150.

[6] Edited by Hankel, Th. IS 168.

Even in his Mémoires sur la photométrie (p.256) he does not return to the law, but, it seems, presupposes, under the presupposition of the law, experiments which prove an influence of the movement on the visibility of the difference, and which I below will lead.

Masson [7] came casually on his attempts to prove the law in an extensive investigation of electrical photometry. His method is ingenious and simple, and his statements make the probation far more acute and complete than the statements of Bouguer and Arago. In essence, it was this: A white disc of about 6 centimeters in diameter, on the one sector, for example, $1/_{60}$ of the circular area betragend, to some parts *mn* was blackened in beige recorded manner was in rapid rotation

offset so that by virtue of the Nachdauer the visual impression, the black part expanded to a ring or wreath on the white disk, which according to the known, this prevailing laws about the brightness ratios rapidly moving bodies to 1 / 60 was darker than the white disk base. An eye on what is still able to distinguish the wreath from the bottom will hereinafter be able to make a difference that does not have 1 / 60 , to perceive is the intensity. Masson now made a number of such slices customize, in which the ratio of the angular size of the sector to the circular area relative 1 / 50 , 1 / 60, 1 / 70 and as progressively to 1 / 120 whereby it was put in a position to determine boundaries between which the limit of sensitivity was, was. The following results for consistent results with this method, which, at the same time, has the interest, compared to the previous one, to show that instantaneous light behaves in the same way with the light remaining with respect to the law.

[7] *Ann. de Chim. et de Phys.* 1845. *T. XIV.* P. 150th

As is well known, if one rotates a disk, alternately divided into white and black sectors, illuminated by daylight or a lamp, it appears uniform gray. If one illuminates them instead with the instantaneous electric spark, then one sees all sectors completely differentiated. Applying both types of illumination at the same time, it depends on the ratio of the intensities, whether one sees uniform gray or distinguishes the sectors; The former, when the electric light is too weak, the latter when it is sufficiently strong. According to Masson, according to Masson, the ratio of the two intensities of illumination, in which the uniform gray occurs, is different, but remains the same for the eye of the same observer. The sectors disappear and the uniform gray occurs when the instantaneous illumination of the white sectors by the electric light (the blacks do not throw back a considerable light) no longer gives them sufficient preponderance over the uniformly gray coloration that would occur without the electric light that it can be distinguished from the eye; and, depending on the relative width of the black and white sectors, with which the gray changes, a different degree of electrical illumination is therefore required for the same fixed illumination. Can the eye after the previous trial still if the instantaneous illumination of the white sectors by the electric light (the black ones do not throw back a considerable light) no longer gives it sufficient preponderance over the uniformly gray coloring which would occur without the electric light, that it can be distinguished from the eye; and, depending on the relative width of the black and white sectors, with which the gray changes, a different degree of electrical illumination is therefore required for the same fixed illumination. Can the eye after the previous trial still if the instantaneous illumination of the white sectors by the electric light (the black ones do not throw back a considerable light) no longer gives it sufficient preponderance over the uniformly gray coloring which would occur without the electric light, that it can be distinguished from the eye; and, depending on the relative width of the black and white sectors, with which the gray changes, a different degree of electrical illumination is therefore required for the same fixed illumination. Can the eye after the previous trial still Accordingly, this requires a different level of electrical illumination at the same fixed illumination. Can the eye after the previous trial still Accordingly, this requires a different level of electrical illumination at the same fixed illumination. Can the eye after the previous trial still $^1/_{100}$ to distinguish, so in case of equality of the white and black sectors, the illumination of the white sectors by the electric light is $^1/_{200}$ its illumination must be through the permanent light by this illumination by the rotation of the disk to a gray of half photometric brightness is attenuated. The experiments according to this method have been made by Masson for purposes other than to prove our law, and have been widely varied, but the agreement of their results with those of the previous method has been established.

The details of his results are given by Masson, first with regard to the first method of observation, afterwards turning to the second, as follows : [8] *:*

"En essayant différentes vues, j'ai trouvé que pour celles que l'on considere comme faibles, la sensibilité a varié de $^1/_{50}$ *à* $^1/_{70}$*. Elle a été de* $^1/_{80}$ *à* $^1/_{100}$ *pour les*

vues Ordinaires, et pour les Bonnes vues de $^{1}/_{100}$ à $^{1}/_{120}$ et AudeLA. J'ai deux personnes rencontré apercevant continued distinctement la couronne produite sur un disque donnant le $^{1}/_{120}$ "

»En faisant varier l'intensité de l'éclairement, j'ai trouvé que, quand il était suffisant, pour qu'on pût facilement lire dans un inoctavo, la sensibilité ne variait pas pour un même individu. Ainsi, comme Bouguer l'avait reconnu, la sensibilité de l'oeil est indépendante de l 'intensité de la lumière. J'ai fait varier de plusieurs manières la puissance du rayon lumineux réfléchi par le disque. J'ai pris la lumière d'une carcel placée à diverses distances du disque, l'éclairement par un temps sombre et couvert; j'ai opéré à la lumière diffuse après le coucher du soleil; j'ai employé la lumière solaire réfléchie par un héliostat, et quelquefois j'ai rendu le faisceau divergent au moyen d'une lentille. La distance de l'oeil to disque est sans influence sur la sensibilité,

»Les résultats n'ont pas été modifiés, quand j'ai changé le rapport entre le diamètre du disque et la largeur de la couronne. J'ai employé des disques, dans lesquels la surface parcourue par le secteur noir était le tiers ou le quart de celle du cercle. J'ai placé la partie noire au bord du disque, au center, et entre le center et la circonférence. Enfin j'ai disposé sur un même cercle plusieurs portions noires appartenant à of the secteur ayant avec le cercle des rapports différents et et j'ai employé le disque no. 5 [9] . Dans tous les cas, la limite de la sensibilité est restée invariable. «

»En éclairant le disque mobile par lumières colorées, j'ai pu déterminer si la sensibilité de l'oeil variait avec la nature of the rayon lumineux. Sauf quelques restrictions dont je vais parler, j'ai trouvé que la limite de sensibilité est indépendante de la couleur. Ainsi, the vois aussi distinctement la couronne au $^{1}/_{100}$, soit que le disque j'éclaire par la lumière naturelle, soit que j'emploie of rayons colorés. "

»J'ai produit des lumières de miscellaneous couleurs en faisant passer au travers de verres colorés les rayons du soleil ou ceux d'une lampe de Carcel. Je me suis servi des couleurs d'un specire, et enfin de l'appareil photométrique de M. Arago. «

[8] The fact that, as far as I know, Masson's work did not turn into any German scientific journal will justify the somewhat longer verbal communication.

[9] This disc contains a broken black sector part.

»Les verres que je dois à l'obligeance DE M. Bontemps ont tous été essayés au spectre. Excepté le verre rouge, qui la lait passer que l'extrémité rouge du specter, tous les autres laissaient passer toutes les couleurs en quantités variables. Quelques-us, le rouge par exemple, absorbaient une telle quantité de lumiere, qu'on voyait difficilement la couronne. «

»Dans les essais précédents, l'observateur ayant l'oeil fixé sur le disque pendant un temps plus ou moins long, nous ne pouvons affirmer que les limites de sensibilité, ainsi déterminés, resteront les mêmes quand l'éclairement sera instantané. Je me suis *assuré par le moyen suivant que, dans ce dernier cas, la limite de sensibilité éprouvait peu de variations. «*

»Après avoir éclairé les secteurs du photomètre [10] *by a lamp Carcel, j'ai placé une lumière électrique à la distance limite, puis j'ai fait varier, soit la distance de létincelle, soit celle de la lampe, de manière à rendre très-sensible les secteurs. J'ai opéré pour diverses intensités d'éclairement. En comparant ainsi la variation de distance nécessaire pour produire l'apparence des secteurs à la distance absolum des lumieres, j'ai trouvé, et cela résulte aussi des expériences que je citerai plus loin, qu'on pouvait prendre pour limite de sensibilité dans mes expériences photométriques les nombres obtenus pour les lumieres fixes. «*

»En soumettant à mes expériences plusieurs individus, j'ai constaté un fait de la plus haute importance pour la photométrie absolue, each veux dire pour la comparaison des lumières fixes à un lumière instantaneise pour unité. J'ai trouvé que deux personnes, qui avaient la même sensibilité, donnaient, après avoir acquis suffisamment l'habitude des expériences, les mêmes nombres au photomètre électrique. «

»J'ai substité aux papiers blancs éclairés par des lumières colorées, paper coloré éclairés par de la lumiere naturelle. La limite de sensibilité m'a toujours paru plus petite dans ce dernier cas, et un peu variable avec la couleur des papiers. Je ne pense pas cependant qu'on doive regarder ce fait comme une exception a la règle que j'ai établie. Il est en effet à peu près impossible de procurer des papiers uniformément colorés; la lumière qu'ils réfléchissent est toujours très-faible, et le noir qu'on dépose à leur surface adhère difficilement et réfléchit lui-méme une quantité de lumière blanche qui varie dans des limites assez étendues relativement a la lumière réfléchie par les disques colorés , Cependant, pour des papers rouges et bleus,

»Ayant remarqué qu'à la limite de la couronne décrite par la partie noire du secteur, il y avait toujours un certain contraste qui, rendant la couronne plus apparente sur ses bords, aidit à sa vision, j'ai terminé la partie noire du secteur par une bordure frangée no. 6 et 7th fig "(see original).

[10]. Masson refers here to a photometric device described in his original treatise, consisting of a rapidly rotated circular disk to be illumined by the electric spark, divided into white and black sectors. Comp. P. 153.

»É résulte aussi des expériences, que j'ai faites sur plusieurs individus, que la sensibilité de leur organs restant la même pour toutes les couleurs, ils éprouvaient, en fixant le disque éclairé par le rouge, une fatigue, un malaise qui indiquaient chez eux une espèce de répugnance pour cette couleur. Il serait curieux d'examiner si cet effet n'est pas produit sur quelques yeux par une couleur au re que le rouge. «

I finally come to Steinheil's experiments. In his famous treatise on the prism photometer [11, he found] occasion to investigate whether the error committed in the estimation of the equality of light intensities varies according to the magnitude of the intensities, and gives (p.14 of his essay) the result of the above observations made briefly then: "They show that you can see with great precision the point at which two faces are the same light, the uncertainty of each estimate of the kind is not over. $^{1}/_{38}$ of the total brightness, this may be big or small. "

[11] Elements of the brightness measurements in the starry sky of Steinheil, in the Abhandl. the mathematics. Phys. Kl. bair. Akad.1837.

This statement includes the statement of our law. For the uncertainty in the estimation of the equality of two light intensities depends understandably on the magnitude of the difference still discernible, and if at different intensities an error is made about an equal proportion by means of a plurality of experiments, then the limit of the distinctiveness of a difference must also be lie equal proportions of these intensities.

Steinheil himself sums it up by saying (p.71) with reference to the same observation: "In Section B it will be shown ... that the estimation of equal brightness always lacks an aliquot part of the total amount of light It follows, therefore, that if one weakens the surfaces of light to the point of intensity where they are no longer distinguishable from the heavens, then they have an intensity proportional to that of the heavens. "

The absolute indication $^{1}/_{38}$, the former of $^{1}/_{64}$ bis $^{1}/_{120}$ relative to stand out, and it remains questionable whether it depends on a difference of the eyes or of the method; but that does not do the law for what it is here to do. It should be noted that the fraction $^{1}/_{38}$, which measures the uncertainty to Stein Hail, the just noticeable differences, which the fractions $^{1}/_{64}$ bis $^{1}/_{120}$after the other observers, although proportional, but not to be considered as coincidental; though this remark does not explain the size and direction of the difference between the results.

Steinheil's experiments (pp. 73 ff., Of his treatise), insofar as they are considered comparable for the purposes of proving our law, refer, however, only to a scale of three intensities, which behave as 1,000, 1,672, and 2,887; So they do not have a big extent; yet they are very estimable and important, not only because they stem from one of the most excellent observers practiced by photometric means; but also because they are based on a different principle of probation than the previous ones, and thus all the more testify that the law passes every kind of test.

In fact, it is easy to overlook the fact that in Steinheil's proof the principle of the method is subject to mean error, whereas the earlier proofs are based on the principle of the method of just noticeable difference.

Since the representation and calculation of Steinheil's experiments could not happen without circumstance, I refer to the original or to my essay p. 477 where I after a slightly modified calculation and the exclusion of one with the other not quite comparable test series instead of the fraction $^{1}/_{38}$ find $^{1}/_{40}$. Only the combination of the found, and the calculated according to the validity of the law calculated simple mean error of the square roots of the intensities proportional, observational quantities may follow here.

Obs.	Ber.
2,517	2,426
1,712	1,846
1,471	1,428

The previous verifications of the law were based on very small differences, which, as seen in Chapter 7, is essential for a psychic measure to be established on this basis. The direct proof of this for more than just noticeable differences has some difficulty, since the judgment about their equality is not quite sure, and the combination of trial and experiment is not just as significant as it is in mere existence same feature can be concluded. But in my essay, p. 489, I have the experience that when one eye is covered, a light shadow is placed over the field of vision, which one is not inclined to keep lighter or darker, may one consider a fire or a wall than such a which, from a certain point of view, comes under our law and can be interpreted as a proof of it for something more than just noticeable differences. The discussion of this experience may be looked at in the essay itself.

But there is another, and much more unequivocal, proof of the law in more than just noticeable differences, and at the same time the first that exists for the law, and again on that high field of observation, the first mentioned proofs It has to be assumed that the trained astronomer's eye has happily overcome the difficulty of estimation in the sense of our law.

The estimation of star sizes has been known since old age (Hipparchus) not according to their photometric light values, but according to the impression they make on the eye, in such a way that astronomers are the stars 1, 2, 3 usf have sought to distinguish them by the same apparent differences in brightness, but let the numbers of the star sizes decrease, while the apparent brightnesses increase. According to our law, the perceived brightness difference between the successive size classes can only be the same, provided that the photometric relationship between them is the same, hence the mathematical series of star magnitudes, a geometrical star intensities listened to briefly with star intensity the photometric value of a star.

However, this contradicts the fact that according to a statement based on J. Herschel's investigations in v. Humboldt's cosmos, which is the series of star intensities belonging to the successive star magnitudes instead of a geometric series, rather a quadratic power series, Should it be a geometrical series, then each number would have to be obtained by multiplying it by the same number from the next preceding one, and, if possible, one should have the same number in simple numbers as the previous series

This contradiction seems all the more important for the first sight, since Herschel's quadratic power series itself is preferred to the geometrical series, and its most careful and most important documentation is its highly careful revision of the stellar magnitudes and their comparisons with the star intensities according to their own photometric determinations One can even be sure of the present question with some certainty. In the meantime, in my essay, I have argued, as I believe, without reproach, that the contradiction is merely apparent, and on closer examination rather dissolves into the full affirmation of our law. Here are the most important points:

A significant deviation between the above two series and finds only at the l. Size class instead. In this, however, the intensity of the individual stars varies from simple to about 16 times, so that, if one arbitrarily chooses the intensity of a star of this class as a representative of the intensity of the whole class, one can arbitrarily bring it into agreement with this or that series; and indeed such arbitrariness has taken place in Herschel. For it had a predilection for the quadratic power series of intensities, in that, assuming that the relations of the numbers which denote magnitude, at the same time denote the relations of the distances in which they stand of us; and, accordingly, as the representative of the stars of the 1st magnitude, chose the one that best suits this assumption, α Centauri, whereas Herschel himself expressly designates in another place a different star, α Orionis (Beteugeuze), which occupies a middle place under the stars of the first magnitude, as a "typical specimen" of the stars of the first magnitude, as one Star "of an average first magnitude." Actually, the same takes this place after Herschel's own observation data, according to which among the remaining 14 of it photometrically determined and ranked by fractional stars stars 1. Size 8 a smaller, 6 have a greater intensity, 6 a smaller, 8 a larger size number, as α Orionis.

It follows that, if one seeks a mean or typical value for the stars of the first magnitude without arbitrary adaptation to any assumption, one does not have to choose α Centauri, but αOrionis for it. Now, α Orionis stands for α Centauri according to Herschel's own photometric determination in the ratio of 0.484 to 1. If we substitute 0.484 for 1 in the quadratic power series, it passes over into

0.484 but differs little as 0.5 or $^1/_2$ and $^1/_9$ of $^1/_8$, that, in consideration of the self highlighted by Herschel difficulty of accurately determining the size and intensity, considering also that by itself the quadratic power series is not declared to be exactly true to the observations, the difference can be considered small enough to be geometric for the quadratic series

to find substitutable. For larger size classes, of course, both series would diverge further, but Herschel's photometric determinations do not go beyond the fourth magnitude class, and so there is no need for further comparisons.

A more thorough calculation, in connection with which I must refer to my paper, has further shown that the geometrical series of star intensities is not only compatible with Herschel's observational data, but better represents them with appropriate reference to them and suitable definite exponent of the series the quadratic series of exponents, in that the combination of observation and calculation according to Herschel's formula, based on the presupposition of the quadratic series of powers, leaves a sum of squares of error 2,719, according to our formula, 2,2291, based on the presupposition of the geometric series.

However, J. Herschel's investigation, if one of the most important, is not the only thing that can be summed up in this subject; and the conduct of the geometrical series of stellar intensities to the arithmetic series of stellar magnitudes is beyond doubt by various other thorough investigations, all of which, independently of each other, have led to the same result, as Steinheil, Stampfer, Johnson and from Pogson. The compilation of these investigations can be found partly in my larger essay cited above, and partly in the supplement to it in the reports of the Saxon Society.

The exponent of the geometric series does not vary very significantly by 2.5 or 0.40, according to the results of these various investigations; depending on the series of intensities ascending or descending, it is determined as follows:

	asc.	**new to old.**
after J. Herschel's Datis	2,241	.4427
- Steinheil [12] (1). ,	2,831	.3588
(2). ,	2,702	.3705
- Rammer [13] (1). ,	2,519	.3970
(2). ,	2,545	.3929
- Johnson [14] (1). ,	2,358	0.424
(2). ,	2,427	0.412
- Pogson	2,400	0,417

[12] (1) According to Steinheil's own calculation, (2) after a slightly modified calculation, see my first paper, p. 518 ff.

[13] (1) After determination on fixed stars, (2) after determination on planets.

[14] (1) After own revision of the star sizes, (2) with use of other size estimates.

The deviations between these determinations of the exponent are explained by deviations partly between the size estimates and partly between the photometric determinations of the different observers. It may also have had some influence on the absolute value of the determinations that the intensity of the celestial ground has not been taken into due consideration, as I discuss in detail in my second essay. Here, however, it would not be in the field to deal more closely with the subject by satisfying the general agreement of these investigations in the results which are essential to us, that is, the validity of the geometrical series of star intensities.

From what has been said above, there must be a passing contradiction which is found in J. Herschel's statements against our law, and which we should not disregard, as stemming from such a reliable observer, even if it is in contradiction with the result of what from the other side of Herschel's investigation, as discussed above, and can not annul the result of all previous investigations.

In describing his Astrometer (Capreise, p. 357), Herschel notes in a note that it is useful to use an equilateral prism in order to use its reflective effect to make the line connecting two stars to be parallel to the horizon, and adds in addition: "Occasionally, too, it may be used to enfeeble the light of almost equal bright stars, by external reflection in an eqaal ratio (by bringing the line joining their reflected images in parallel to that. In this state of affairs, it would seem apparent which would otherwise escape detection. By increasing or diminishing (equally) the angles of incidence, the reflected images may be more or less enfeebled. A plain metallic mirror may be used for the same purpose. «(For this a parallel passage in Outlines p. 522.)

In any case, whatever happens with this contradiction, it seems to me impossible to see in the deviation noted by Herschel more than a deviation of small order, which under certain circumstances enters into observation. It appears that he has observed this deviation only occasionally, without making any definite experiments, and since he himself elsewhere speaks of "innumerable" causes which "determine our judgment in an incomprehensible manner in such experiments," one may occasionally say Observations should not be considered sufficient to justify the same particular experiments as they have been before. From the other side, however, one can imagine that an eye so studied and practiced in such observations, as that of Herschel, at last attains a sensitivity to subtle differences, and hence subtle deviations from the law, which represent only a very small quantity for the measure, at intensities where it is still for an inexperienced eye do not become noticeable; and it is not improbable that Herschel's statement refers preferentially to very bright stars, where the deviation may already be felt everywhere because of the upper limit of the law, since Herschel himself emphasizes the difficulty of accurately determining the brightest stars, and thus preferably here may have applied indicated means. Unfortunately, nothing can be decided on it because of lack of specific information. But he demands that, certainly based on something actual, So far, the proof has been that the law exists within certain limits without specifying these limits; which has not happened yet. But part of the discussion about the circumstances, the reason, the nature of these limits will be discussed in some of the discussions; In some cases, the quotation of certain points may be appropriately connected here, which, without prejudice to the validity of the law, have an influence on the distinctiveness of the differences in light, that is to say they are the same or comparable in experiments on its validity; Incidentally, here, where they first come into consideration, should also be treated adequately for later reference to it.

The upper limit of the law, when the eye feels dazzled, is undoubtedly related to the fact that the eye is thereby injuriously affected. In a sense, an upper limit of the kind is self-evident. It is undeniable that the internal movements to which sensation depends can not be increased beyond a certain limit without destroying the organ and eliminating the possibility of its further enhancement. Even two unequally strong stimuli, which reach and exceed this limit of excitement, will only be able to bring it up to this same maximum of sensation, and thus can no longer give a difference of sensation. In any case, the approach to this limit already leads to a deviation from the law.

It makes sense to push the upper deviation from the law simply by making the eye less susceptible to differences in light by blunting it, and this seems to be strikingly confirmed by the fact that after a sudden passage from full daylight to a dim chamber In the first moments, nothing distinguishes, but gradually learns to differentiate better and better. But now the same phenomenon is asserted in the opposite direction. Anyone who, after long stays in the dark, suddenly leaps into the light, is at first unable to distinguish objects, and only gradually learns them. If dulling were the reason that one does not recognize differences in very bright light, At first sight one would have to make the most of the entrance from the darkness to the light, and gradually differ less and less. Already at the beginning of the experiment and the counter-experiment with the cloud nuances, this double case made itself felt.

One might be inclined to distinguish immediately the inability to enter the light from the light into the darkness, but rather to an after-duration of the light impression, as a blurring against the impression, and the corresponding inability to enter from the darkness into the brightness of the omnipresence to push impressions. If the eye-black remains bright for a while after passing through darkness, then light-faint impressions could not be perceived according to the principle of the disappearance of the stars during the day, and in the reverse transition, the stronger impression prevailed at a slower rate the weaker, so differences between strong light initially could not be perceived. In fact, I presumably put this explanation in my essay on "A Psychophysical Basic Law," p. 487. However, on closer consideration, both sides of the statement seem untenable to me. For, according to all previous experience, the phenomenon of after-life is too fast to ignore other difficulties; and the assumption that a strong light impression is relatively slower than a weak one, contradicts positive attempts by Swan[15] .

[15] Sillim. J. 1850. IX. p. 443rd

If I am not mistaken, the initial inability to look into the darkness after entering from the light is far from anything that will be discussed in Chapter 12; but explain the same inability to cross from the dark to the light, and perhaps it is therefore necessary to explain that, just as the previous action of a strong light-stimulus against the absolute sensation of a weak one makes more or less blunt for a while, so too previous effect of a strong difference in light against the subsequent sensation of a slight difference for a time more or less dull. Be it that we pass from very bright and too weak light, or vice versa, this is a strong difference of light, which, though successively conceived, but also against differences in light, which are to be understood at the same time, for a while more or less dull. However, even this explanation is still very problematic.

However that may be, it always remains, according to the reasons to be stated in Chapter Twelve, that if the eye becomes dull for both components, it will have no other result than if both components were externally attenuated in the same way where the difference still remains noticeable, so that a disruption of the law can not take place from here.

As for the lower limit of our law, on closer inspection it is not to be regarded as a true limit, and what has hitherto appeared to be a deviation from the law is, strictly speaking, an inference of the law. In order to show this, a preliminary discussion, which is also important for the sequence after many relationships, is necessary.

Abnormally, in all areas of the senses, inner causes (internal stimuli), independent of external stimuli, can give rise to sensations known as hallucinations; a proof that a fortune exists independently of external stimuli in all sensory areas. Insofar it can not have anything conspicuous in itself, if such also manifests itself constantly and normally in a given area. An example of this is the visual sense, where we have to recognize a normal hallucination. In fact, black, what we see in the dark and with our eyes closed, is a sensation of light that takes place without external stimulation, not to be confused with the nonsense that takes place with the finger or the back of the head, and not unlike hearing in the absence of external noise. Rather, the black that we have in the closed eye is only the same sensation of light that we have at the sight of a black surface, which can pass through all the gradations into the strongest sensation of light; yes, the inner black of the eye, even through purely internal causes, sometimes turns into bright light, and contains light phenomena, so to speak.

On closer inspection one discovers in the black of the closed eye a kind of fine dust of light, which in different persons and in different states of the eye is present in varying degrees of opacity, and may, in a morbid state, increase to lively phenomena of light. In my eye, there has been a strong continuous flicker of light since a prolonged eye disease, which increases as the stimulus state of my eye, which is subject to great fluctuations, increases. Incidentally, such lively subjective phenomena of light can take on very different forms in different individuals, which I will not go into any further detail here, but refer to writings on eye diseases and *chapters on subjective phenomena of light in physiological writings. Comp. z. B. Rüte's Ophthalmol. 2nd edition p.*

The black ground of the eye can also increase in depth and decrease. The proof is easy to guide. If one has a sharp and persistent view of a white disc on black papers, a deep black afterimage of the disc appears in a relatively light-colored ground, even in the closed eye with held hands (to exclude light from entering through the eyelids). at the same time the net-skin, in the place of the after-image, becomes dulled against external light; for if the eye in which one has the after-image is set open upon a white surface, one sees there a spot dark against the white ground. So the eye-black deepens through fatigue of the eye and brightens relatively by rest.

The same success that fatigue has here is that paralysis, which is partial or total, imperfect or perfect, temporary or permanent, may affect only the retina or the central parts of the visual system. Not infrequently, only some parts of the retina are paralyzed, and then the patient sees with the naked eye gray, black spots, or stains on the objects corresponding to the paralyzed spots (in view of the sensitivity, which is differently weakened for different color rays) [16)]. For some patients such occurs temporarily. Even the whole field of vision can be darkened as well as permanently, temporarily from internal causes. Rüte [17)] "Observed a lady in whom, under constant light, often a complete darkness suddenly poured over the eyes, from which the visible objects appeared only occasionally like phantoms, and disappeared immediately when the patient tried to fix them."

16) Rüte, Ophthalmol. II. 458.

17) ophthalmol. I. 156.

If not only the retina but also the central parts of the sense of sight are completely paralyzed, one would expect that not only the field of vision darkens, but the black of the visual field itself shrinks (as it disappears at the boundaries of the visual field in the closed eye) and with the eyes no more than with the finger or a dead Nervenstrange is seen. I have not found anything about it, and I have not been able to obtain any definite information from famous ophthalmologists as to whether such things have really been observed in a total and lasting way; it does not seem that way; temporarily and partially but according to Rüte's [18)] In the case of nervous subjects, it sometimes happens that, in momentary paralyzes of individual parts of the retina, the piece of the external world corresponding to the paralyzed areas appears to them to be absent in space [19.] "Probably the central conditions of the facial sensation in the brain, too much connected with the conditions of life, so that a total and permanent cessation of one could take place without the others.

18) ophthalmol. I. 154.

19) A Treatise on Graves "On the Disruption of the Visual Field in Amblyopic Affects" in Gräfe's Arch. F. Ophthalmol. II. Abth. 2. p. 258 seems to concern *only cases in which pieces of the field of vision have not both disappeared and been darkened.*

On the condition of the unlimited validity of the law downwards, even the photometric intensity of the black eye can be determined by quite analogous experiments, as hitherto carried out to prove Weber's law. For this purpose, one only has to remove a single light from a shadow-giving body at night, until the shadow, blackened only by the eye, is only indistinguishable from the ground illuminated by the eye-black and the external illumination , Specifies the found one of Volkmann book value 1 / 100 at that distance below, as is the illumination, the light which inflicts the eye black, 1 / 100 the intensity of the eye black.

This attempt has really been made, albeit only casually, so far. For Volkmann's eyes, the shadow disappeared on a ground of black velvet as the light, a usually-burning stearin candle, had moved back to 87 feet in a long dark corridor still elongated by some rooms. Insofar at this distance, the illumination, the light inflicted the eye black, $^1 / _{100}$ was of Illumination by Black, they would at $^1 / _{10}$ that distance, which at 8.7 feet distance have been the same. The experiment, then, says that a black tablet receives just as much illumination from an ordinary stearin candle burning about nine feet away as from the black eye alone without external illumination, and hence the photometric intensity of the last illumination of the first is.

Perhaps one might find such a brightness value of the eye-black even more conspicuous and much too large, if it is supposed to be equivalent to the illumination of a surface by an ordinary candle at a distance of about nine feet. But one must not overlook that it is the illumination of a black surface, which equivalency claims, because proved by the attempt becomes. But even an absolutely black surface would not be enlightened, even by the flame, which was still so intense, by swallowing all light, and only the fact that there is no absolutely black body makes one even speak of a slight degree of enlightenment of a black ground , So, after all, the black ground cast back little, but little, light in the shadow's surroundings, so that the brightness of the eye-black could well be commensurable in the way it was proved by the experiment.

I have only cited the result for Volkmann's eyes in the most careful experiment which he has hitherto made; two other persons, whom he brought to the experiment, recognized the shadow at that distance of 87 feet, over which the experiment could not be driven according to the nature of the locality, which proves that either the brightness of its eye-black or its sensitivity is different was. Volkmann intends to give these experiments even more precise determination, further execution and consequence. For the present, the result obtained by him is sufficient to show that the photometric intensity of the black eye is neither immeasurable nor immeasurable; and that's what matters here first.

Inasmuch as the black of the field of vision, with all the above exclusion of external light, can still be regarded as a real sensation of light, it must not be neglected in the examination of Weber's law. Suppose we look at two cloud shades close to each other or shadows with our naked eyes, the brightness of the black of the eye adds to both cloud shades or shadows. If we now dampen the light of both cloud shades or shadows by providing a gray glass in a given ratio, the brightness of the black remains undamped, and still adds with its constant intensity to the two clouds or shadows, which are not truly the same ratio and hereby does not retain the same relative difference as before, but a lesser thing, which according to the law must carry a diminution of the difference in sensation. Yes, if we go on and on with the darkness of the glasses, the eye-black remains at last, instead of the nuances alone, and all difference disappears. The black in the eye, in effect, in this experiment, no matter how strange this may seem, much like the bright illumination of the sky in which the stars disappear. Thus, in mere relation to the external light stimulus, Weber's law can only be confirmed in so far and as long as the inner eyesight is vanishingly small compared to the outer, just as Masson claims the validity of the law only from the point on, where you can read ordinary pamphlet; whereas, when one goes down to great darkness with the trials, the difference of the nuances must become more indistinct. The same applies to all mentioned modifications of the experiment and is confirmed throughout the experience.

A good explanation of what has been said above is that a gleam of light, which differs only a little from the eye-black, can be made to disappear by a seemingly completely opposite means, but according to the same principle.

In the evening, when one looks at a star that can only be distinguished from the black background, one can just as well make it disappear if one puts a darkening glass in front of one's eyes, as if the eye were the lamp of approaching the page. A corresponding experience could be very nicely done on the magnificent comet of the year 1858 at the beginning of October. Both gray and colored glasses, the approach of a bright lamp from the side shortened the tail exceptionally, and a dark red glass, through which I recognized the finest cloud nuances in daylight, even made the comet disappear. The first is explained by the fact that through the glasses the light of the star or comet, not that of the black in the eye, is considerably weakened, the second by that

On the one hand the light shines through Sclerotica and Chorioidea with a reddish color, on which depend some remarkable phenomena of objective and subjective coloring of the pictures, the bridge in Poggend. Ann. LXXXIV. P. 418 has studied very carefully; secondly, from the picture there is a direct, scattering reflection on the remaining parts of the retina, as well as backwards to the cornea, from which the light is partly reflected back to the retina, which points, with reference to the following, especially Helmholtz in Pogg. Ann. LXXXVI. Pp. 501 ff. thirdly, because of the microscopic composition of the ocular media of cells, fibers, pellicles, there is an irregular dispersion of light, as it seems, according to the principle of diffraction, from which the colored courtyards visible around light flames hang, which Meyer in Pogg. Ann. XCVI. P. 235 has been the subject of a special study. By *virtue of the ultimate cause, as well as by the direct dissipative reflection of the light source's image on the rest of the retina, the illumination of the retina is most near the picture, but it is indeed waning over the whole base of the eye.*

By the combined action of these causes, the very faint light of the star or comet's tail, like the starlight through the daylight, is all the more easily drowned the closer it is to the image of the light source, since the illumination of the eye-ground in its surroundings is strongest.

Hence the statement Brewster's [20] :

"When the light of the candle's flame, which is held close to the right eye, acts on one part of the retina, it makes all the other parts of the retina to a greater or lesser degree insensitive to all other light impressions. The insensitivity reaches its maximum close to the enlightened spots, and decreases with the distance from it. Moderately lit objects really disappear in the area of the highly charged parts, and bodies of vivid colors not only deprive everything of their luster, they also change their color. "

On the same ground it is based on the fact that, according to Helmholtz Method [21,] the so-called violet rays of the solar spectrum, which are not seen by the usual method, can be seen even without the use of fluorescent substances, if they are so arranged that they are distinctly isolated from the remaining parts of the spectrum, which they can be subdued by scattered light, can be understood.

[20] Pogg. XXVII. P. 494.

[21] Pogg. LXXXVI. P. 513

A general conclusion from the foregoing is that, notwithstanding intensified illumination, the amount of reflected light on black and white areas grows in the same proportion, but the difference in whiteness from black appears greater with increasing illumination because the brightness of the black always accounts for the majority the brightness of the black retains. This is z. For example, the simple reason that one can read better in the light than in the dark.

Apart from the limits of the law, which are related to the degree of the intensity of light, it must not be forgotten that confirmation of it by observation is to be expected only insofar as the remaining circumstances remain the same except for the intensity relations Conception of the difference in light can express an influence. Now the study of the circumstances, which in this respect may be of considerable influence, is still highly incomplete; but some points are to be thought of, which deserve attention according to previous experience preferably.

Already (where) it was stated that Arago recognized an influence of the movement of the components on the perception of their difference. Volkmann has also noticed this influence. In order to grasp the subtlest traces of the appearing or disappearing shadow, the shadowing light had to be moved, with which the shadow moved at the same time; and the just noticeable difference $1/_{100}$ is determined under the influence of the movement.

In Arago's experiments on this, the components did not consist of two shades, but were obtained in such a way that with a telescope, which inside had a Rochonian prism (whereby a double image is produced), and in front of the lens a Nicol prism was attached by whose rotation the one image in arbitrary and measurable proportions against the other can be weakened, was aimed at an opening in black cardboard, which projected on the covered sky, where then the position of the main sections of Nicol's and the Rochonian prism against each other causes the relative intensity of the two images produced by the latter to be determined.The rectilinear motion of the Rochonian prism in the telescope in the direction from the ocular to the objective caused the weaker image to be set in motion, so that from the position where its band passed through the center of the thicker, it passed to the one in measured time. where its edge touched with its edge.

In three series of experiments using several observers in this way, at a speed of motion of the image of 12 angular minutes in the time second, the disappearance of the weaker over the stronger superimposed image for the eye occurred as the intensity of the weaker following fractional value the stronger one was:

	In the silence.	When moving.
I.	$1/_{39}$	$1/_{58}$
II.	$1/_{51}$	$1/_{87}$
III.	$1/_{71}$	$1/_{131}$

With regard to the great difference shown by the absolute numbers of these three series of experiments, Arago merely notes: *"Je ne chercherai pas ici à expliquer, comment la sensibilité de l'oeil correspondant à l'état de repos a étési différente dans ces trois séries d 'expériences. C est là un phénomène physiologique , sur lequel il aura à revenir. «.* The difference may not have depended on the diversity of the observers, as Arago says: The above are the *résultats à très-peu concordants, obtenus par M. Laugier, par M. Goujon et par M. Charles Mathieu;*just as little by a difference of absolute intensity, which partly contradicts the explicit recognition of our law in popular astronomy, and partly by the general statement which it *appends* to all experiments: *'Ajoutons, comme renseignement propre à faire juger de l' obscurité du champ , que l'image faible, lorsqu'elle se projetait en dehors de l'image forte, a disparu quand son intensité était de* 1 / 2100 . "

Of interest in relation to the influence of the movement are also the following remarks by Förster [22) , which he makes regarding the application of his photometer:

At that time, in a heavily darkened room, we looked at sheets of paper a few feet apart, on which there were black numerals, isolated by larger gaps, and it was important for us to keep our eyes quite still. The room was so dark that the numbers *just appeared to us as black spots. If I fixed one of these, it did not take long - with a certain very weak illumination - until both the fixed number and all the others in the gray of the paper sheet, which was getting darker, completely disappeared. When that moment had come, fixing became impossible, an unpleasant feeling appeared in the orbit, the eyes made a small movement, and immediately the whole arc of numbers was visible again.*

[22] *About hemeralopia p. 13th*

It is still unclear what the influence of the movement is based on. It has been sought in that the difference falls into a new, not yet fatigued position, but since the components of the difference are not changed by the movement, but only the place of the very small difference is lost, it does not seem that the Fatigue condition could be significantly reduced by the movement.

In the near future it would be possible that it is rather the multiplied conception of the difference by a majority of points than the freshness of these points, which causes the greater distinctiveness of the difference in the movement, in so far perhaps a summation of the impression of successive points in time up to certain limits. Finally, the following could be at play, although not yet explained, but in a sense a reason for its explanation by its generality. Any comparison of two different quantities succeeds better if we regard them successively with the same parts of the organs, as simultaneously with different ones, as EH Weber has emphasized and proved by experiments, and as already stated (chapter 8). Thus we recognize a small difference of two weights more easily by successive weighing with the same hand, as simultaneous with different hands. However, the movement of the components in our light experiments translates the simultaneous difference for different retinal points into a successive one for them. At the same points, to which even stronger light fell, soon weaker and vice versa, and the more rapidly the movement takes place, the more points enter this succession in a given time. However, even this explanation is only a presumption so far. However, the movement of the components in our light experiments translates the simultaneous difference for different retinal points into a successive one for them. At the same points, to which even stronger light fell, soon weaker and vice versa, and the more rapidly the movement takes place, the more points enter this succession in a given time. However, even this explanation is only a presumption so far. However, the movement of the components in our light experiments translates the simultaneous difference for different retinal points into a successive one for them. At the same points, to which even stronger light fell, soon weaker and vice versa, and the more rapidly the movement takes place, the more points enter this succession in a given time. However, even this explanation is only a presumption so far.

Further, among the circumstances which influence the discernibility of a difference, is the extension of the components, but without altering the law of intensity, if the extension remains comparable every time, as is immediately apparent from stars good as having been valid on stretched shadows. But a point of light, with equal intensity, is not so easily distinguishable from the ground as a surface of light. Since this subject is now covered in more detail in Chapter 11, I will not go into it here.

Third, it has been found that a given relative difference in light is more readily recognized when its components are dark in the light ground than bright in the dark ground. Here is above not only based on experience explicit indication of Arago regarding the choice of one or the other at a ratio given by him photometric equipment before [23] ; but Hankel has also found the same on the occasion of other photometric experiments, which are not yet published.

[23] Arago's Works, edited by Hankel.

Finally, the following remark: In the analogy between pitches and colors, which is otherwise usually regarded as valid, it is a noteworthy circumstance, quite out of this analogy, that Weber's law in the realm of colors does not consist just as well as immediately In the range of pitches, that is, the equally noticeable differences in the numbers of vibrations are by no means proportional to the numbers of vibrations of the colors. In fact, at the boundaries of the spectrum at intervals of a small or even major third, the eye scarcely observes a change in color, while in the area of yellow and green the noticeable color transitions follow so rapidly that all transitional steps between yellow and green fall within the interval of a small half-tones are crowded 24), Incidentally, there are also other points, which are not discussed here, in which the analogy between tones and colors fails. 25)

[24] Helmholtz in the reports of Berl. Acad. 1855. p. 757 ff.

[25] In the matter of p. 166 ff.

2) sound [26] .

In the realm of sound it is necessary to distinguish mere noises, which have no definite pitch, where then only strength can be considered as measurable, and tones in which the amplitude, which is dependent on the amplitude of vibration, is proportional to that of the squares; Vibration factor dependent, by the same physically measured, height are particularly taken into account. The proportions of strength are found in the same way in others as in others, which can only examine the height of the latter. First, let's take the strength in the eye.

[26] In the matter of p. 160. Revision p. 367-419.

Renz and Wolf 27) have under the direction of Vierordt, according to the method of right and wrong cases, made experiments on the sensitivity of the ear to differences in the sound intensity at the tiktok of a clock, which was placed at various distances from the ear under appropriate measures. The main result follows from their experiments:

[27] Vierordt's Arch. 1856. H. 2. p. 185. Poggend. Ann. XCVIII.

"If two sound quantities of absolutely but rather weak intensities are perceived directly behind one another, the certainty of the judgment increases with increasing difference of the sound intensities in such a way that sound quantities in the ratio of l00 **:** 72 were clearly distinguished from one another under all circumstances. behaving like 100 **:** 92, the number of right decisions exceeds by a small amount the sum of false and undecided. "

These diligent attempts deserve attention as explanatory examples for the application of the method of right and wrong cases, and, as far as they indicate a comparatively small certainty in the knowledge of differences in sound power, which will also be considered for the following but not suitable for deciding the validity of our law, since they have not been directed to the equality of perceived difference at different absolute levels of sound. The following tests relate to this.

When I talked to Volkmann after his execution of the photometric experiments on the great importance of a more general proof of Weber's law, he improvised to the provisional proof of the law for sound-intensity equal to the following apparatus, which was manufactured at not significant cost that day.

It simply consists in a pendulum hammer, which strikes against a plate of any sounding or non-sounding substance. The axis of this pendulum was a strong knitting needle, turning in brass holes, between two pillars fastened on a board and connected at the top by a cross-wood. It is understandable that as the hammer is made heavier or lighter, dropped from the greater or lesser height against the plate, or as it approaches or gets further away from the apparatus, the sound will be physically stronger or weaker. Since the apparatus in its raw version did not have a graduation for determining the elevation of the hammer every time, The hammer was of wood and struck against a square glass bottle. Now two elevations of the hammer were sought, which gave sufficiently distinct sounds, that an observer standing directly by the apparatus was not mistaken, if, without knowing the elevations, he advised which sound was the stronger; but little enough to distinguish that, if one reduced the difference by about half, the judgment was uncertain and gave partly correct, partly false cases. ThereonThe observer gradually withdrew to 6, 12, 18 steps, so that the initial distance of the observer from the apparatus was at least twelve times that. At each of these intervals, the same experiment was repeated several times, with those two elevations which had presented to the observer nearby a difference which was still clearly recognizable but only very slight. Since 12X distance of the observer, the physical sound intensity to $^{1}/_{144}$ has descended [28]so close by would not have had to vanish much beyond the just noticeable difference, if at all, it depended on the absolute strength of the sound. But at all three distances of the observer his judgment remained just as sure and correct as in closest proximity.

[28] Of course, this would only be true if the experiment was carried out in the open air. The above was employed in a closed room.

As crude as it was in some respects the apparatus and the attempt, the essential seemed to be sufficiently taken into account and the result so decisive that it was foreseeable that a more exact execution with a carefully constructed apparatus would not lead to any other results. In fact, this has been demonstrated on a very large experimental scale, from simple to many hundreds of times in Volkmann's later experiments, but which were not made with a falling pendulum but with steel balls falling freely onto a steel plate and some of which I took part in. In these experiments, both drop height, as gravity of the falling balls, as the distance of the observer were changed within wide limits; but the drop heights and their differences are precisely determined on a vertical scale along which the fall took place. Incidentally, the manner of employment and the success of the experiments were in accordance with the previous one. For the most different absolute sound intensities the ratio of fall heights 3 appeared**:** 4, which corresponds to the following derivation of an equal ratio of the sound intensities, just sufficient to make a safe distinction for two observers with good discrimination, which probably agrees with the results obtained by Renz and Wolf.

Here follows the detailed description of the experiments taken from Volkmann's observation journal.

"A prismatic rod is graduated and erected vertically on a board, which can be obtained by 3 screws in the horizontal, on this rod two runners are attached, from which horizontally two arms α , β go out from the height, which the two arms A bullet is dropped on the board, and the bullet was grasped between the thumb and forefinger, and the tip of the forefinger touched the arm α or βand then the fingers were carefully removed from each other to drop the ball. I had two bullets of equal weight, holding one with my left hand, the other with my right hand, so as not to pick up the ball for the second attempt or even search for it after the first drop attempt. "

"The closest proximity of the listening observer to the fall instruments was 1 meter, the largest distance being 6 meters."

"The absolute fall heights that came to compare differed like 3 **:** *11.0."*

"The weights of the falling balls differed as 1.35 grams **.:** *14.85 grams ...."*

"Numerous experiments within the breadth of these differences in sound showed that Heidenhain and I are capable of distinguishing with certainty sound powers that behave to each other as 3 **:** *4. If the difference is reduced to the ratio 6* **:** *7, then some will already come Mistakes and more often undecidedness in judgment. "*

Fechner, on the other hand, was already very much mistaken in the ratio of 3 **:** *4, but exercise had an influence on increasing discriminating power, for at the end of a very long series of observations, he always distinguished sound powers in a ratio of*

3 : 4, whereas at first he was more likely to err heard as correct and still longer to try $^1/_3$ false information when $^2/_3$ made right. "

The previous attempts are based on the principle of the method of just noticeable differences; and since, for reasons stated earlier, this method can not be used to achieve the same sharpness as by the method of right and wrong cases and mean errors, it is indisputable that experiments by these methods are still desirable. But with the exceptionally large variation of the absolute sound levels, which was found in the attempts made, they are decisive enough for the validity of the law in general, and at most a small order deviation of the same within the limits of the attempts made could be possible. without this justifying a probability.

It may be useful to add a few things about the apparatuses and their theory to be used in experiments of this kind.

Schafhäutl [29)] has previously reported a falling-sphere instrument for measuring the sensitivity of sound power but using it merely to measure absolute sensitivity.

No less, the sound pendulum has already been used for this purpose before. Itard [30)] has used such a device for the examination of the sensitivity of the hearing in hearing diseases under the name Akumeter, which consists of a beaten copper rings hanging from a rod free of the machine erected by a column on a pedestal, and against which Pendulum strikes whose elevation is measured on a degree arc.

[29)] Abhandl. d. baier. Akad. VII. 2. Abth.

[30)] Gehler's Wort. Art. Hearing. P. 1217.

I have myself made a double-tone pendulum-shaped pendulum, where two pendulums of equal construction strike from two sides against a thick slate plate [31] ; but so far not found time to try it.

[31)] With wood, I have not been able to achieve the same sound for both pendulums.

The following about the theory of instruments:

One can find easily that when a body it was falling by free fall or pendulum to another body, the strength of the sound arising from the process is in the composite ratios of drop height and the weight of the falling body [32)] as far as the Neglect the influence of air resistance and any other disturbing influences on the fall speed.

[32)] Schafhäutl sets the sound intensity proportional to the square root of the drop height of the sound-emitting body (Munich, Abhandl., VII, p. 17), which I can not correctly find from the above derivation.

In fact: the strength of the sound is proportional to the squares of the vibration amplitude of the sounding body; the vibration amplitude of the sounding body is (according to known formulas) proportional to the speed at which the particles pass through their equilibrium position, ie the same with which they are removed therefrom. This is in a composite ratio of the speed with which the falling body impinges and its weight. The velocity with which the body hits, ie the final velocity of its fall, is proportional to the square root of the fall height according to the laws of gravity. Thus, the square of this final velocity is proportional to the height of the fall, hence the square of the velocity at which the particles move away from the rest position, and so on proportional to this height of fall. Since it is known that there is no difference in the terminal velocity, whether a body falls by free fall or by a curved path through a certain height, so you can just the previous consideration on the hammer of the pendulum (the friction on the axis as a precondition) as a free-falling body. It is only necessary to beware of communicating a speed to the falling body at the beginning, if the specified dependence of the sound intensity on the fall height should remain valid. Air resistance is likely to be neglected all the more given the low fall heights and velocities with which one operates in general when one uses lead as a falling body. Whether a body falls by free fall or by a curved path through a certain height, one can apply the previous consideration just as on the hammer of the pendulum (the friction on the axis as negligible assuming) as a free-falling body. It is only necessary to beware of communicating a speed to the falling body at the beginning, if the specified dependence of the sound intensity on the fall height should remain valid. Air resistance is likely to be neglected all the more given the low fall heights and velocities with which one operates in general when one uses lead as a falling body. Whether a body falls by free fall or by a curved path through a certain height, one can apply the previous consideration just as on the hammer of the pendulum (the friction on the axis as negligible assuming) as a free-falling body. It is only necessary to beware of communicating a speed to the falling body at the beginning, if the specified dependence of the sound intensity on the fall height should remain valid. Air resistance is likely to be neglected all the more given the low fall heights and velocities with which one operates in general when one uses lead as a falling body. Thus one can apply the previous consideration to the hammer of the pendulum (assuming the friction on the axis as vanishing) as a free-falling body. It is only necessary to beware of communicating a speed to the falling body at the beginning, if the specified dependence of the sound intensity on the fall height should remain valid. Air resistance is likely to be neglected all the more given the low fall heights and velocities with which one operates in general when one uses lead as a falling body. Thus one can apply the previous consideration to the hammer of the pendulum (assuming the friction on the axis as vanishing) as a free-falling body. It is only necessary to beware of communicating a speed to the falling body at the beginning, if the specified dependence of the sound intensity on the fall height should remain valid. Air resistance is likely to be neglected all the more given the low fall heights and velocities with which one operates in general when one uses lead as a falling body.

From the above, it is evident that the sound of the sound at the sound pendulum is not in proportion to the angle of elevation φ oϕ the pendulum, but to the vertical height, by which the hammer is raised above its lowest point, di in proportion to whereupon the instrument could be equally graduated. For example, since the cosine of 45 ° equals 0.707 and the cosine of 90 ° equals zero, then the ratio of the sound intensities at these two elevations is that of 1-0.707 = 0.293 to 1 or nearly as 3 to 10. The elevation angles 60 °, 90 ° °, 180 ° correspond to a ratio of the sound strengths ½ **:** 1 **:**2. As long as the elevations do not exceed 60 °, the sound power can be approximated proportionally to the squares of the same, so that a double elevation equals four times, a triple nine times the sound intensity [33] .

33) This follows from the known formula - etc.

Here are two small tables giving the sound intensities associated with the elevations of a sound pendulum from 0 ° to 90 ° and vice versa when the power at 90 ° is set equal to 1.0000 (in Table I) or 10 (in Table II). At 180 °, it is twice as large as at 90 °, and all strengths at elevations between 90 ° and 180 ° fall in between, but it will not be easy to apply a pendulum at elevations over 90 °.

Tables about the relation between the elevations of a sound pendulum and the sound intensity.

I. II.

Elevat.	Strength	Elevat.	Strength	Strength	Elevat.	Strength	Elevat.
90 °	1.0000	45	.2929	10	90, 00	3	45, 57
85 °	.9128	40º	.2340	9	84 °, 26	2	36 °, 87
80 °	.8264	35 °	.1808	8th	78 °, 46	1	25 °, 84
75 °	.7412	30 °	.1340	7	72 °, 54	½	17 °, 19
70 °	.6580	25 °	0.0937	6	66 °, 42	¼	12 °, 97
65 °	.5774	20 °	0.0603	5	60 °, 00	1 / 8	9 °, 07
60 °	0.5000	15 °	.0341	4	53 °, 13	1 / 16	6 °, 41
55 °	.4264	10 °	0.0152				

50 °	.3572	5 °	0.0038				

With regard to the pitches in which the frequency of oscillation is responsible for the size of the stimulus, except for Weber's general statement, there are the statements of Delezenne, cited by Weber; but his observations, as I have learned from his original treatise, are mainly concerned with what deviations from purity are still distinguishable in this and that kind of interval (harmony, octave, fifth, and the like) Whether the deviation from the equality of two tones at the same ratio of the vibration numbers or at different levels of the tone scale appears to be the same, which is the real question of Weber's law. In the meantime, confirmation of the law in this respect requires not only special attempts since it is the simple and, so to speak, notorious statement of the musical ear, that the same ratios of the numbers of vibrations correspond to a difference of tone perceived as equal in different octaves, so that the law can be proved to be more direct than elsewhere, even for large differences , Euler, Herbart, and Drobisch have also found their way here in their mathematical consideration of the tone relations.

I interviewed several persons with good musical hearing, occasionally making experiments with a wooden sound pendulum, which struck wood whether they could draw a comparison of the ratio of the sound strengths at 45 ° and 90 ° with a pitch ratios. Some declared themselves incompetent; Strangely, however, most of those who embarked on a comparison (independently of each other and without knowing anything about the other's judgment) agreed to compare this ratio with that of a fourth. But I will give to these attempts in their hitherto only rough and casual employment all the less, as that agreement was not without exception, and even consider it very questionable whether to make a direct comparison between ratios of strength and height by feeling altogether. In any case, the result of these experiments confirms that of Renz and Wolf, as obtained by Volkmann, according to which one is not inclined to make quite significant differences in the sound powers (3**:** 10) as high.

In the same respect it was interesting to me to hear from a musician (the violin virtuoso v. Wasilewski) that it had been learned at the Rhenish Song Festival that a chorus of 400 male voices did not impress much more than 200.

3) weights. [34)]

Weber's results, obtained by the method of just noticeable differences, by which the first confirmation of our law is offered in the field of weight experiments, have already been quoted. His experiments have the special merit that in a part of them the sensation of pressure of the skin is distinct from the sensation of the muscles, and the results obtained in this way have been compared with those obtained by common claims of both sensations; The method of correct and false cases, which will be discussed in more detail below, refers to the natural connection of the two sensations which takes place in the lifting of the weights compared.

[34)] In matters p. 164, 186-199. Revision pp. 168-173, 358-367.

For the understanding of the following it will often be necessary to refer to that which is said in the chapter on the execution of the method, without it being necessary for me to return to it in detail here. From the other side, some documents and explanatory examples for what is said there will be found below.

My main series of experiments on the object in question are two-handed, one-handed and one-handed (with right and left), which, both comparable, are performed by a series of 6 main weights, 300, 500, 1000, 1500, 2000, 3000 grams very consistent results. The one-handed series is hired in October and November 1856, the two-handed in December 1856 and January l857. The circumstances of the experiments of both series were generally the stated normal circumstances. Specifically, the following should be noted:

Each of the two series comprises in 32 experimental days à 12 divisions of 64 elevations in total 32 · 12 · 64 = 24576 simple uplift or fall. For each major weight *P* , two specific proportions were used as the additional weight *D* (periodically alternating therewith) , namely 0.04 P and 0.08 *P.* The latter additional weight may appear large, but gives, as can be seen in the following test tables, still enough false cases, which is related to the establishment of the procedure described, to base each comparison (calculated on two counts) on a simple double levy, instead of justifying repeated weighing, where a *D*= 0.08 P would still like to deliver incorrect cases. On every trial day of 12 · 64 = 768 lifts, all 6 major weights, each in 2 divisions of 64 heaves, were all tested with the same proportional *D* , and this was changed only days or weeks, as indicated below. In addition, after days in ascending ( ) and descending (↓) sequence of main weights. Thus, in each of the two series of experiments, each of the six main weights comes to 32 in all · 128 = 4096 lifts or falls; 2048 with *D* = 0.04 *P* and just as much with *D* = 0.08 *P* ; 1024 each and just as much ↓ . In the two-handed series, with each major weighting, the 128 upliftings of each day were *incontinuously made* , in the one-handed sequential 64 with the left, 64 with the right, alternating after days the left or right began. In the two-handed series was to two days, in the one-handed only after every 8 days between *D* = 0.04 *P* and *D* = 0.08 *P* changed. This has led to the difference that in the two-handed series the sensitivity values for both *D's* are quite similar, so that this series after which may with used for a confirmation of the law, the ratio of the right to the total number of cases of the size *D* at a constant sensitivity *h* depends [35] , whereas this is not exactly the case with the one-handed series where the weeks at 0.08 *P* *give* proportionally smaller sensitivity values than those at 0.04 *P* , which goes into the remark (chapter 8). But in terms of the influence of the greatness of the emphasis on the measures of importance which alone matters here, the one-handed is quite as comparable in itself as the two-handed.

[35] This law is expressed after the discussions in Chapter 7 by the relation of the values to *t* = *hD* in our fundamental table, according to which a double *D* gives a double *t* when the influences *p, q are* eliminated.

First of all, in order to start with the simplest, if not the most accurate, way of using these series of observations, I give the total number of correct cases r for the different weights P, specified for some main circumstances, but without any specialization of the 4 main cases and no reduction to the exact ones Measures, ie the values $t = hD$, which can be calculated from them, even *if,* without such calculation, the main results in question are obtained from the ratios of the correct numbers r will let go, according to which the stricter treatment of the series will be able to do more than the same results will make it a little sharper.

The used weight unit below is everywhere the grams.

In order to leave no doubt about the meaning of the numbers in the tables below, I expressly state the same for the first number in the first table. The number 612 at P = 300, D = 0.04 P, n = 1024, says that with a main weight = 300 grams and an additional weight = 0.04 of the main weight, ie 12 grams, the number of correct cases of all days, where the main weights in ascending order (612 was, while the total number of cases, right and wrong together, under the same circumstances was 1024, according to which the number of false ones was 1024 - 612 = 412. Hereinafter, the meaning of the remaining numbers will be self-evident. The numbers r Of course, the vertical sum-sum column includes 4 times n *of* the numbers in the special columns, ie, 4096, as indicated by the way in which the r *of* the 4 vertical special columns in the vertical sum-sum column are added together; on the other hand, the numbers r *of* the horizontal sum-sum column belong to 6 times n *of* the special numbers, ie 6144, since the r belonging to the 6 $P's$ in the same vertical column are added in the horizontal final sum column.

I. Number of correct cases *r of* the two-handed series .

P	n = 1024				total (n = 4096)
	d = 0.04 P		d = 0.08 P		
		↓		↓	
300	612	614	714	720	2660
500	586	649	701	707	2643
1000	629	667	747	753	2796
1500	638	683	811	781	2913
2000	661	682	828	798	2969
3000	685	650	839	818	2992
Total (n = 6144)	3811	3945	4640	4577	16973

II. Number of correct cases *r of* the one-handed series.

P	n = 512								total (n = 4096)
	d = 0.04 P				d = 0.08 P				
	left		right		left		right		
		↓		↓		↓		↓	
300	352	337	344	318	387	372	386	342	2838
500	339	332	348	335	383	402	413	366	2918
1000	325	343	382	388	383	412	389	422	3044
1500	353	358	371	383	406	416	435	430	3152
2000	378	353	369	382	413	418	414	421	3148
3000	367	343	364	386	426	433	429	438	3186
Total (n = 3072)	2114	2066	2178	2192	2398	2453	2466	2419	18286

I pass over, as not belonging here, any discussion of the difference in the results of these tables according to the different circumstances (as the different size of the *D,* application of the left and right, and ↓), which one can easily notice for oneself, and what will be discussed in more detail in the "measurement methods". Here, the specification of the results according to these circumstances would be principally only for the purpose of showing that the number of correct cases *r* in relation to the principal weights *P*In all circumstances it follows essentially the same course, that is slowly increases with the main weights, and with the highest main weights, 2000 and 3000 grams, changes only little more. If this agreement has been envisaged for the various experimental circumstances, one can further adhere to the vertical final sum columns, which in both tables give the correct numbers for the individual main weights for an n = 4096.

If the law is found to be directly and accurately proved by these experiments, then, in consideration of the fact that everywhere the same ratio of the additional weight to the main weight exists, all the numbers *r*not only approximate, but exactly the same for the different weights. This is not the case. It is, however, the deviation, which in such a way remains for the attempt still of the law, to regard just as little as a true deviation, as the deviation, which we found in the region of the sensation of light at the lower limit, rather just so and from a whole analogous aspects as a requirement of the law. In the same way as we have to take into account the internal excitement of light, even without access to the external light, so here also without external weight *P* , when lifting the same with raised weight of the arm and any covering garment (at my attempts only a light shirt sleeve [36]), And as the law in the former domain can confirm itself by experiment only in proportion as the inner eyesight can be neglected against the outer, so in the other field only in accordance with the moment of weight of the lifting and at the same time lifted arm against that of the lifted one Weight can be neglected.

[36] In addition, it remains questionable to what extent the pressure of the air on the skin does not have to be taken into account with a certain value, but it seems to be charged into the organism.

But in fact, in our highest weights, we see only a very insignificant deviation from the requirement of the law, and the general course of deviation in the sense in which it is expected to occur, ie, the correct numbers grow somewhat with *P.* Let us think namely, to the principal weights *P* increasing in size, always adding the same absolute addition *A* by virtue of the weight of the arm, while *D* grows only proportional to *P* , as is the case with our experiments, then, of course , the greater the proportion to which the correct number depends more *A* against *P* disappears in the divisor, ie the larger *P is* itself, and becomes noticeably constant from the point where *P* has become large enough that one can regard *A* as no longer objectionable; as the experiment shows.

In view of the not inconsiderable weight of the arm, it may at first only be noticed that the increase which the main weights of 300 to 3000 grams receive as a result does not manifest itself in an even greater increase of the correct numbers with ascending *P* , and especially that this is not at the transition between the two smallest weights 300 and 800 grams is more noticeable, where even in the two-handed row shows rather a small decrease. But at first this latter anomaly, to which I shall return later, is by no means to be taken for granted that the burden on the arm by its own weight must be brought in the same way as an external weight added thereto; secondly, consider that the lifted weight *P*but at the end of the lever arm, which the lifting arm forms, the weight of the arm acting in the center of gravity of the arm acts on a shorter one, which comparatively reduces its moment; third, that the addition of this moment to the moment of *P* comes into consideration only for the feeling of the muscles, but not for the feeling of pressure, since only the weight *P*but not the weight of the arm pressing on the skin; Fourthly, finally, that the correct numbers of the previous tables do not yet provide an accurate measure of the sensitivity, but merely indicate the rate of sensitivity in ascending with the main weights anyway. In fact, all that has been said in Chap. and, in particular, the circumstance mentioned makes allowance for the fact that the influence of the time sequence of the elevation *p*at strong increase of weight increases, and thus according to the comment in chap. 8 makes the sum of the correct cases somewhat smaller than would be the case without this disturbing influence, so that without this disturbance, in fact, the correct numbers for the highest weights would have been somewhat larger, and thus somewhat more differentiated from the lower ones. This is especially the case with the smaller additional weights 0.04 *P,* whereas against the larger 0.08 *P* the influence *p* disappears relatively more. Thus, in the two-handed series for the main weights 1500 and 3000, one has correct numbers at 0.04 *P* in Summa 1321 and 1335, at 0.08 *P*in contrast, 1592 and 1657; in a one-handed case at 0,04 *P* the numbers 1465 and 1460; 0.08 *P* 1687 and 1726. The difference therefore is much larger at 0.08 in both series *P* 0.04 *P* .

The perturbation by the side effects *p, q is* completely eliminated by the complete compensation of the same, discussed earlier (chapter 8), which is based on the separation and separate calculation of the four main cases. The first is the specification of the *r* -values according to the four main cases in a first table (III) and the *t*- values derived by means of the fundamental table (still without fractionation) in a second one (IV). In the "measurement methods" I will also announce a corresponding specification for the one-handed series; Here I wish not to pile up too many numbers. The definitive results to be considered below in the discussion of the question of our law are in columns 4 *hD*and *8hD* of Table IV; the remaining columns of this table and the whole of Table III are taken into consideration for our present purpose only as a record of those definitive results; but, incidentally, they may be useful in explaining the way in which such are to be gained, and in many points of the method in general, as I enclose.

For the sake of clarity I will again discuss the first numbers of the following two tables:

The number r_1 = 328 at P = 300, D = 0.04 P, n = 512 in Table III says that at P = 300 grams, D = 12 grams, 512 cases of the first major case, ie where *D is* the first-canceled there are 328 right cases.

The corresponding number t_1 = 2547 of Table IV is found after the fundamental table

by taking to the associated *t* value. The statement n = 512, v = 1 above means in Table IV that each *t* value is derived from 1 by 512 cases (ie without fractionation).

III. Values *r of* the two-handed series specified after the 4 main cases .

P	n = 512								total
	D = 0.04 P				D = 0.08 P				(n = 4096)
	r_1	r_2	r_3	r_4	r_1	r_2	r_3	r_4	
300	328	304	328	266	404	358	372	300	2660
500	352	274	321	288	399	339	364	306	2643
1000	334	318	335	309	377	365	410	338	2796
1500	346	323	308	344	408	402	399	383	2913
2000	296	365	309	373	404	385	439	398	2969
3000	244	393	265	433	392	447	390	428	2992
total	1900	1977	1866	2013	2384	2296	2374	2153	16973

IV. Values *t* derived from the previous table *t of* the two-handed series.

$n = 512$, $v = 1$.

P	$D = 0.04\ P$					$D = 0.08\ P$					
	t_1	t_2	t_3	t_4	total 4 *h*	t_1	t_2	t_3	t_4	Sum 4 *hD*	Total 8 *h*
300	2547	1677	2547	346	7117	5679	3692	4260	1535	15166	22283
500	3456	624	2290	1112	7482	5444	2958	3932	1749	14083	21565
1000	2769	2181	2807	1856	9613	4469	3973	5971	2920	17333	26946
1500	3224	2363	1820	3147	10554	5873	5584	5444	4726	21627	32181
2000	1394	3973	1856	4301	11524	5679	4813	7558	5397	23447	34971
3000	- 416	5168	312	7200	12264	5123	8067	5034	6915	25139	37403
Sat.	12974	15986	11632	17962	58554	32267	29087	32199	23242	116795	175349

You see how much on the nature of the four major events, the numbers *r* to change and how these changes are influiert on the size of emphasis with. At $P = 3000$, the correct number $r = 244$ is even smaller than the wrong 268 (which is obtained by subtracting the correct number from the total 512), which then has a negative value *t* (in Table IV) (see Chap. 8th). Incidentally, such cases occur enough in my other observation tables.

One can now use Table III, which is described in chap. 8 given rules for the complete compensation and determination of the influences *p, q* itself to exercise in which provision here to enter into no interest.

Further, by comparing the sum values of the *T's* table obtained at $D = 0.04\ P$ and $D = 0.08\ P$, one can convince oneself that these sum values are remarkably proportional to the *D's* , namely that noticeably doubled at 0.08 *P* at the rate of 0,04 *P* , which guarantees the legality of invoicing according to the remark made (see Chapter 8) [37] . However, all this is not discussed here.

[37] By the way, there are also other test series for me.

What matters here is that we see to what extent the sum values 4 *hD* , 8 *hD* , which result from the addition of t_1, t_2, t_3, t_4, are constant at the different *P's* , which is exactly the case. as the sum of the numbers *r* , from which they are derived, would be if our law is valid, and if the arm weight not *P* zuträte.

The numbers of the columns 4 *hD,* 4 *hD,* 8 *hD* namely, the former for each of the two relative additional weights in particular, the latter adds up for both, the actually considered here quantitative measures, which without the co-influences of the time and space position of the vessels p , q *were* obtained, ie products of the measure of the difference sensitivity h in the 4- or 8-times additional weight D, from which by division with 4 or 8 D [38] the difference h *could be found* even for the different main weights . This would have to be done without the weight of the arm according to our law of weights$P$ and consequently the additional weights D proportional to it are found to be inversely proportional, and accordingly the products 4 hD or 8 hD are found equal at the various main weights . Since it is easier to assess deviations from equality than proportionality, the products have stopped at 4 hD , 8 hD , without *going* back to h itself.

[38] D is on average 0.06 P for the column *8hD.* For a more accurate calculation, however, the calculation of h for $D = 0.04\ P$ and $D = 0.08\ P$ must be made, especially from the columns 4 hD , and from this only the most likely to seek mean of h .

For the sake of clarity, we now put the values of the three main columns, by division, respectively, with 4 or 8, back to the simple value hD , in the following table. The terms $v = 4$, $v = 8$ above the columns state, according to the in Chap. 8, that each number of columns is derived, respectively, from 4 or 8 times n observations; *But* $n = 512$.

V. *hD* values two-handed series.

$n = 512$.

P	$D = 0.04\ P$ ($v = 4$)	$D = 0.08\ P$ ($v = 4$)	medium ($v = 8$)
300	1779	3792	2785
500	1871	3521	2696
1000	2408	4333	3368
1500	2639	5407	4023
2000	2881	5862	4371
3000	3066	6285	4675
total	14639	29200	21918

In order to translate the abstract meaning of the numbers *hD,* with which we have here only to do with the establishment of our law, into a meaning for the experiment, it is this: if, instead of applying the same relative additional weight to each of the applied weights As was done, this divided by the number hD or a given multiplum or fraction of *hD,* would have applied, so everywhere would be an equal number have been obtained. So you will z. For example, in the two-handed series following page, according to the statements of the experiments, the additional weights belonging to the main weights 2000 and 3000 grams, which are in the same ratio as these main weights, have to divide, respectively, with numbers which are proportional to 4500 and 4909, thus they fall equally into the sensation.

If these results are not yet the definitive ones, to which I come in the following, I have diligently given them here, because they do not diverge significantly from the definitive ones, so that at least one could follow them, and because without Great extensiveness let all the documents of the same give, from which one can reproduce them after the rules specified in the 8th chapter itself. In this case, all the correct numbers obtained for the same main case, the same *P* and *D* during the entire month of observation, are taken together, and without fractionation to derive the *t*Values from the fundamental table have been used. However, as noted (see Chapter 8), in all my experiments I preferred to more surely eliminate the variations of the influences *p, q* , and especially to calculate the *t*- values for each main case from fractions of $n = 64$, and to merge them to sum or averages. So it happened in the following two-handed as well as one-handed series. However, reproducing the correct numbers of these fractions à 64 and the individual *t*- values derived from them would cost too much space, so I limit myself to the following, which is composed of all the fractions with the number *v* divided the fractions to give definitive results for both series, at which it is finally necessary to stand still.

VI. *HD* values of the two-handed series.

$n = 64$.

P	$D = 0.04\ P$ ($v = 32$)	$D = 0.08\ P$ ($v = 32$)	medium ($v = 64$)
300	2023	3918	2971
500	1965	3705	2835
1000	2530	4637	3584
1500	2774	5910	4342
2000	2966	6034	4500
3000	3296	6520	4908
total	15554	30724	23140

VII. Values *hD of* the one-handed series.

$n = 64$.

P	left	right	L. and R.

	D = 0.04 P (v = 16)	D = 0.08 P (v = 16)	medium (v = 32)	D = 0.04 P (v = 16)	D = 0.08 P (v = 16)	medium (v = 32)	Total center (v = 64)
300	3916	4845	4381	3658	5360	4509	4445
500	2876	5246	4061	3349	5584	4467	4264
1000	2906	5649	4278	5103	6230	5667	4973
1500	4016	6426	5221	4638	7647	6143	5682
2000	4700	6515	5608	4517	6821	5669	5639
3000	4455	8084	6220	4551	7616	6084	6152
total	22869	36765	29769	25816	39258	32539	31155

Occasionally, I refer here to a point in the method that is explained by comparing Table VI to Table V. Both tables, which are valid for the two-handed series, are based on the same observation values and differ only in that in Table V the values *hD are derived* without other fractionation than after the four main cases, using n = 512, in Table VI but with strong fractionation using n =64. It depends on this, according to the remark (chapter 8), that all values of the last table are somewhat larger than those of the first one. The deviation would be indifferent if the ratio of the magnification were the same for all values, since in this case only ratios are important. But some values are magnified in different proportions than others. This depends, as can be seen from a special discussion of the series of observations, on the fact that p and q did not remain completely constant during the course of one month, but varied in an irregular manner. By dividing the series into fractions so small that the variation can be neglected during each of them, the elimination of $p$and q resulting disadvantage, and therefore the values obtained in Table VI are preferable to those of Table V. In the meantime, there is no significant difference in the course of the values between the two tables, so that one could well have stopped at the first, much shorter, derivable one. In any case, the comparison of these tables may indicate how, approximately, the degree of fractionation may change the absolute values.

Comparing the definitive means obtained with the one-handed and two-handed series, we find that

P	einhänd. / zweihänd.
300	1,496
800	1,504
1000	1.325
1500	1,309

2000	1,253
3000	1,254

according to which the ratio of the two values slowly diminishes with increasing emphasis, but seems to approach constancy.

Looking at the course of the values *hD* in previous Tables VI and VII, it is well to teach that it leads essentially to the same thing as the course of the numbers *r* in the first tables, except that the growth of the values *hD* with *P* for the given reason is highest *P* values is more noticeable than that of the numbers *r* . Still, however, the increasing approximation to equality with growth of *P is* clear enough.

Thus, for the three highest *P* values = 1500, 2000, 3000 grams in the two-handed row, on average, the values *hD* = 4342, 4500, 4908; in the one-handed 5682, 5639, 6152. While *P increases* from 1500 to 3000, that is, from simple to double, *hD* increases relatively little, namely from 1.13 times to 1.08 times.

It now seemed to me of interest to again note in particular that approximative equality of *hD* , which is most important for the proof of the law, at higher weights for the two highest principal weights of 2000 and 3000; and I took this opportunity in a series of experiments which was at the same time intended to compare the one-handed and two-handed procedure in alternate experiments, in that the previous two series, when put together as a whole, afford no certainty of such a comparison (see above); By the way, the proportionality of the *t* values with the applied *D* , which has already been established by other experiments, must also be proven.

This series, also 32 days old, is in Dec. 1858 and Jan. 1859 under the normal conditions given below, thus, although much later, quite similar to the earlier ones. Each test day comprises 8 departments of 64 lifts, the whole series therefore $32 \cdot 8 \cdot 64 = 16384$ elevations. Between the two main weights was changed from one day to another, between one-handed and two-handed procedure after every two days, also changed every day after every two divisions between an additional weight *D* = 0.04 *P* and 0.08 *P* , which therefore at *P* = 2000, respectively 80 and 160, at *P* = 3000, respectively 120 and 240 grams. In addition, in the one-handed procedure, as I have always thought, Left and Right changed after each department of 64 elevations.

To distinguish it from the previous one, I call this series of experiments the two- and one-handed. First, in Table VIII, I give the added numbers *r of* the 4 main cases to a provisional apercu; in Table IX, however, the values *hD* calculated in a manner comparable to Tables VI and VII, classifying the 4 main cases from fractions of 64 , without again being able to communicate the documents of this calculation in Spezie because of their sufficiency.

VIII. Number of correct cases *r of* the two- and one-handed series.

p	Two-Hand	one-handed	
	n = 2048	left	right

			n = 1024		n = 1024	
	D = 0.04 P	D = 0.08 P	D = 0.04 P	D = 0.08 P	D = 0.04 P	D = 0.08 P
2000	1280	1503	708	840	681	863
3000	1297	1536	737	882	703	847
total	2577	3039	1445	1722	1384	1710

The sum r at P = 2000 is 5875
- - - - - = 3000 - 6002.

IX. *HD* values of the two- and one-handed series.

n = 64.

p	Two-Hand (v = 32)		one-handed (v = 16)			
			left		right	
	D = 0.04 P	D = 0.08 P	D = 0.04 P	D = 0.08 P	D = 0.04 P	D = 0.08 P
2000	2461	5018	3456	7078	3709	9464
3000	2702	5326	4270	8310	4212	8028
total	5163	10344	7726	15388	7921	17492

The sum of hD at P = 2000 is 31186
- - - - - - = 3000 - 32938.

If the external circumstances and the method of calculation of this series are strictly comparable to the previous series, the numbers in Table IX should be in agreement with those of Tables VI and VII for P = 2000 and 3000. They are, however, considerably smaller in the two-handed series, and although close to the numbers for 0.08P in the one-handed , they are considerably smaller for 0.04P , recalling that the numbers of the one-handed series at D = 0 , 04 P and

0.08 Pthemselves are not comparable among themselves, as evidenced by the fact that the latter are not appreciably double the first, and, as already remarked, depends on their being preserved in different weeks. This may give a clue to what has been said that one can not count on comparability of measures obtained in different epochs, even if the external circumstances were the same. In the meantime, the comparability of each series in relation to the circumstances in question does not make any entry.

That in our present series, where the change between the two D's took place on the same day, consisting of r calculated values hD noticeably proportional given D's show is one of the confirmations of our accounting rule.

According to the definitive results , hD *increased* from $P = 2000$ to $P = 3000$ only in the ratio 31186 to 32938. The deviation of both numbers from the equality signifies the deviation from the direct demand of Weber's law, which is explained by us by the entering arm-weight. However, the number 9464 in the previous table is undoubtedly too large by chance, after comparison with all other figures; and thus the deviation turned out a little smaller than it would be anyway. For the rest, the essential result of this series is the perfect confirmation of the result of the earlier ones.

Since the ratio part, with which the moment of the lifted arm is to be attributed to that of the lifted weight, can not be determined from the outset, partly because the moment on the living does not want to be measured precisely, partly because it is not exactly known in In which proportion the effect of the feeling of the muscles enters into the total effect, one might think of determining the value attributable to P from our values hD according to the presupposition of the validity of our law; but some consideration shows that they are not well enough.

Based on the principles used in the probability calculation of the errors relating to the interaction of independent precision conditions, if the muscle sensation alone would *equal* the value $t' = h'D$ and the pressure sensation alone the value $t'' = h''D$ at some Additional weights D would have produced, from the interaction of the same a value t =

to expect, and thereafter an approximately unsure account. Now, according to our law , t' stands in the inverse relationship of $P + A$, if A is understood as above, t'' merely in the inverse relationship of P, and consequently one has

if c 'and c *are* constants. The three unknowns c', c'', A would then have to be determined from our values hD obtained for the various P's , but even if the difficulty of this calculation were to be overcome, the anomaly would be determined at low P, which will be discussed immediately. a precise calculation in the way.

The fact that, in the transition from P = 300 to P = 500 grams, t instead of increasing, rather showing somewhat diminished, is an anomaly which can not be explained from the above. I hardly believe that it rests on contingencies because of insufficient observation, even if the possibility of this is not absolutely excluded, since it is only small, and very large numbers of experiments are required to keep small differences safely established; but apart from the agreement of both series of experiments, each of which counts a large number of experiments, even with the smallest values of P the increase of t with growth of $P$to be comparatively strong, in that here the increase to the moment of P by the moment of the arm is comparatively greatest here. And if there are no particular disturbing circumstances with the smallest weights, which are outweighed by their influence in the larger ones, I think that would have necessarily been necessary.

Although I can not give any definite information about this anomaly, whose secure conclusion by new experiments would rather be desirable, the following seems to me to offer some probability in the event that it really exists in nature.

It can be imagined that an increasing pressure, apart from the reduction of the sensitivity, which according to our law is proportional to the increase of the stimulus, has a diminishing influence on the sensitivity by the mechanical compression of the nerve endings or in the apperception of the pressure who disappears at greater weights against the influence which follows our law, which must have a more general and profound reason; but for smaller ones could predominantly assert. This would explain the diminution of t at the onset of growth of P. I am not averse to relating the circumstance, which otherwise also seems puzzling to me, that we feel a slight tickling touch stronger, and are more strongly stimulated to reflex movement than a somewhat stronger pressure, although the preponderance of the sensation is always for one very strong pressure remains. But I gladly admit that these are just thoughts that may require further consideration and suggestion.

It is very probable that, just as the field of weight tests divides the lower limit of the law with the field of light experiments, this will be the case with respect to the upper limit, only that the experiments do not continue to such a limit from me where the burden begins to be detrimental and, of course, not by the method of right and wrong cases, which requires a tremendous amount of experimentation, could be continued for a sufficient length of time without lasting harm to obtain reliable results. But, according to the method of just noticeable differences, perhaps demonstrative experiences in this respect could be made without being able to obtain a disadvantage; since the degree of accuracy that can be achieved with this method is less dependent on the number of experiments.

If we overlook the above, the study of the validity and limits of our law in the field of weight tests is still a long way from being completed; and my own attempts at the solution of the task, according to which Weber's is only a second step to the first, which many will have to follow with new modifications of the method. From what has been achieved so far it can only be said that the observations generally agree so well with the law that one can not doubt its approximate or exact validity within certain limits; but the anomaly at the lower limit, the question of the upper limit, the more exact determination and determination of the influence of the arm-weight, the perfect and exact separation of the feeling of pressure and muscular feeling, are still points, which await completion by future trials. Weber's experiments have generally proved the law in general, without the method being able to ascertain the deviations therefrom; my experiments have revealed such things without sufficing to eliminate them by giving due consideration to the circumstances on which they depend.

While there can be no doubt that the isolation of the pressure sensation is achieved by a Weber's experiment, where the weight is placed on the last phalanx of the hand resting on the table; The isolation of the muscular sensation, however, seems to me not so sure of the other, as indicated by him in the treatise on sense of touch and common sense, where the observer with his hand covers the united corners of a cloth in which a weight hangs; since the weight necessarily has to act all the more to let the corners slide through the hand, the heavier it is, if it is not countered by a stronger intake, and consequently a greater pressure. But should the pressure be kept constant,

A method to isolate the muscle feeling exactly in the experiments, I do not want to applaud at all. To isolate the sensation of pressure, it may be better to use balls or hammers, which fall from the given height on the skin, while maintaining our method, than the application of resting weights; and a comparison of the results thus obtained with those which afford the elevation of weights, may be of no little interest.

Regardless of the question of our law, the direct result of the experiments reported in the foregoing can be expressed as follows.

If one compares one given weight with another, to which a certain excess weight is added against the former, then the greater the weight, the more must be the more must the more weight be added, and the greater the difference between the two weights in the sensation to fall.

If the excess weight is allowed to grow proportionally with the principal weight, so that its absolute and relative magnitude with respect to the principal weight are not always the same, the fidelity of this relative excess weight increases somewhat with the rise to higher principal weights; However, it tends more and more to equality, so that the difference in the clarity of the same relative additional weights at main weights of 1500 and 3000 grams is only small, about the ratio of 10 **:** 11 corresponds. That is, the relative weights at 1500 and 3000 grams of weight would, rather than equal, be approximately 11 **:**10 behave in order to appear even more noticeable, that is, to give an equal ratio more correctly to the wrong cases in the corresponding method.

However, this ascending course of the distinctiveness of relative relative weights with the size of the main weights suffers an exception at very low main weights, in that the noticeability increases slightly from 300 to 500 grams; whereas over 800 grammes the ascending course is kept going.

The reason for the exceptional gait in lower weights is as good as unknown, and only (so) a casual conjecture therefore expressed; the reason for the deviation from the similarity of relatively equal weights when ascending to higher weights can be probed with probability that the moment of weight of the lifting and in the uplifting arm comes into play as an increase of the principal weight which indicates the relative equality of the actually relative to this increased head weights relative to putting, extra weight entry.

When applying different weights to one and the same main weight, the noticeability increases with the size of the extra weight. This increased peculiarity results in an increased ratio of true to false cases as well as in the total number of cases, if one uses the method of correct and false cases to compare the weights. However, the number of correct cases does not increase proportionally to the size of the extra weight, but in smaller proportions.

The rule (chapter 8) of finding by means of the fundamental table how the correct number changes according to the additional weight is confirmed in the experience.

These results are by experiments with main weights = 300, 800, 1000, 1500, 2000, 3000 grams and additional weights equal to 0.04 and 0.08 of the main weight, coinciding in lifting the weights with only one hand and with both hands, with the excretion of found constant errors, which depend on the time and spatial position of the lifted weights.

4) temperature. [39)]

The question of how our law is applied to temperature sensation still includes dysfunctions. EH Weber [40)]is inclined to accept; Rather, we can perceive the act of rising and falling of the temperature of our skin as the degree to which the temperature has risen or fallen, for example, we do not feel whether our forehead or our hand is warmer until we have Place a hand on the forehead, where we then often perceive a great difference between the two, and at some times the hand, to others the forehead warmer, "to which other, asserted by Weber experience, which just point. In the meantime it seems that we are also able to perceive a prolonged warmth as heat, and persistent cold as cold, if it deviates sufficiently from the ordinary or middle temperature.

[39)] The touch. and the public. P. 549.

[40)] In the matter of p.165.

Be that as it may, considering the question of Weber's law concerning differences in temperature, the temperature of an absolute zero can not in any case be taken to be the stimulus, but merely the difference from a temperature at which we feel neither heat nor cold, because the size of the sensation of warmth and cold depends only on this. This difference can now increase and decrease, and the question of Weber's law will be whether an equally large relative enlargement does not cause the temperature sensation to be appreciably or more generally equal to the temperature sensation than to the absolute temperature but to this temperature difference.

After some, but by no means sufficient, experiments I have made on this issue, it seems to be within certain limits of medium temperatures, while it is definitely no longer the case in very cold and very hot temperatures.

My experiments on this are made on six days (in December 1855) by the method of just noticeable differences, using the Weberian method of alternating immersion of two fingers of the same hand always to the same depth into two vessels with unevenly warm water , For observation we used a few very precise and precisely compared Greiner thermometers with Reaumurian scales of the Leipzig Physical Cabinet, in which half of a tenth of a whole degree can still be estimated. As the one of them to Hankel's statement, which was kind enough to let me have them for the tests so as to own Konstatierung by 0 °, 05 or $^{1}/_{20}$Degree higher than the other, every observation has therefore been corrected. On the other conditions of the series of experiments I will give the necessary results according to the results.

Within the temperatures of about 10 ° to 20 ° R. I found the sensitivity for the temperature differences so great that the just noticeable differences did not allow an exact determination. At any rate, a maximum of sensitivity, where vanishing or almost vanishing differences are apperceived, lies within these limits, without permitting an exact direct determination. Above 20 ° to the heat of the blood, beyond which my experiments do not go considerably, I found the results to be quite in accordance with Weber's law, if I measure (empirically) the temperature excess over the mean temperature between freezing cold and blood heat = 14 °, 77 R. [41] assuming that the just noticeable temperature difference was proportional to this elevation above the mean temperature. This is followed by the temperature differences *D,* recorded as just noticeable before all calculation, with the temperatures *t* at which they were observed, indicating these as the mean between the two temperatures between which the difference *D was* observed and the values of *D* calculated on the assumptionthat the just noticeable differences are proportional to the temperature surpluses above 14 °, 77. The first page (I) of this table, because the observed differences here are too small at all, is disregarded, and can merely serve to prove the insignificance of just noticeable differences in the limits of the temperatures of this part of the table; whereas the second page (II) of 19 °, 13 ° R. may be considered according to their correspondence between observation and calculation.

I				II			
Date of verse	*t* ° r.	*D* ° R. Obs.	*D* ° R. calc.	Date of verse	*t* ° r.	*D* ° R. Obs.	*D* ° R. calc.
Dezbr.				Dezbr.			
2	15,03	0.19	0.009	26	19.13	0.15	0.16

26	15.40	0.10	0.023	26	20.45	0.20	0.21
26	15.55	0.09	0.028	26	20.63	0.15	0.21
26	16.18	0.15	0,051	26	21.20	0.20	0.23
21	16.70	0.20 **	0,070	26	21.73	0.25	0.25
26	16.71	0.09	0,070	21	23.30	0.30	0.31
21	16.75	0.10 **	0.072	21	25.35	0.40	0.39
21	16,88	0.25 *	0,076	21. 26	26,80	0.40	0.42
21	17.00	0.00 **	0.081	21. 26	28,80	0.60	0.51
26	17,20	0.20	0.088	26	30.50	0.60	0.57
21	17.30	0.10 **	0.092	26	31.35	0.60	0.60
26	17.69	0.23	0.106				
26	18.78	0.15	0.145				

[41] This temperature is based on the thermal conditions of the human body through Lichtenfels and Fröhlich in the Abhandl. the Vienna. Akad.

The calculated values of the table are obtained by multiplying the excess temperature by 14 °, 77, ie *t* - 14 °, 77 by 0.03623. This constant is derived only from the observations of *t* = 19 °, 13 to 31 °, 35; but the observed and calculated values of *D* over 14 °, 77, and under 19 °, 13, which, as I said, are only traces, are also included on the first page of the table. The observations of the table belong to only 3 of the 6 test days; by referring to the observations at the other 3 merely to temperatures below the mean temperature I give below especially.

As for the values of *D on* the first page of the table denoted by asterisks , they are those which are recorded not merely as noticeable, but as marked (1 star) or as distinct (2 stars) in the observation register, which is more than just noticeably. One of these sensible differences (at 17 °) could no longer be detected on the thermometer (the required correction made by 0.05 ° in this case). In fact, one might be inclined to assume the mean temperature of greatest sensitivity according to these values at 16 ° to 17 °, rather than 14 °, 77, and it is possible that it lies there. But you can look at the almost vanishing values of *D*Building anything safe near the mean temperature, considering that apart from the variations in sensitivity, the scale of the fuzziness, the errors of reading, a very small deviation between the temperature of the water and the thermometer is sufficient to cause or obscure such differences although it was as much a concern as possible to reduce these sources of error to the smallest. The result of the calculation of 14 °, 77 corresponds on the whole better to the observations.

Incidentally, if even those traces of D below t= 20 ° almost in the order of errors of observation, they can not be considered purely as such, because the test was generally done without knowing for which water an excess of temperature took place, and decided only after repeated alternating immersion, if I thought I was quite sure of the result, which occurred to such an extent that in a very large number of experiments I was mistaken only once near the middle temperature, where the just noticeable differences become almost vanishing, in which, in the subsequent statement, the when, just as noticeably assumed, difference was found in the opposite vessel, as where I accepted it, against which cases were very frequent, where I could find no difference between the two waters,and afterwards there was always no or one below the boundary of the noteworthy part of the thermometer, which by a kind of mutual control proves at the same time that the indications of thermometer and that of feeling were generally reliable.

However, with the range of the just noticeable differences above the mean temperature, which can be seen from the table, which corresponds to Weber's law, insofar as it can be judged from the smallness of the differences, no symmetry took place below them. Up to about 10 ° down, the just noticeable differences were still too small to give anything to their circumstances, but further down they increased without comparison more rapidly with increasing cold than is compatible with the course above and with Weber's law; so that they were empirically fairly well represented assuming a proportionality of them with the cube of $T - t$, where T = 14 °, 77, $t$the temperature at which the just noticeable difference was observed, and 0.002734 is the value with which one has to multiply $(T - t)^3$ in order to obtain the just noticeable difference at the thermometer, which indisputably depends on a sharp decrease in the Sensitivity based on the cold. Probably a similar deviation would be found if one approached the temperature beyond the heat of the blood, where the burning sensation occurs, but it is always striking that the deviation above the mean temperature begins only in higher degrees, but below the middle temperature starts.

Here follow the formula

$$D = (14.77 - t)^3 \cdot 0.002734$$

calculated values in combination with the observed within the temperature limit + 10 °, 5 and + 4 °, 5 R. Lower downwards I got in a few days too divergent values to build something on it.

date of tries	t ° r.	14 °, 77 - t °	D observed	D calculated	difference
December					
5th	4.60	10.17	2.80	2.88	+ 0.08
23rd	5.32	9.45	2.54	2.31	- 0.21

23rd	5.43	9.34	2.40	2.23	- 0.17
21st	5.65	9.12	2.00	2.07	+ 0.07
23rd	5.69	9.08	2.54	2.05	- 0.49
5th	5.73	9.04	2.22	2.02	- 0.20
Second	5.81	8.96	1.62	1.97	+ 0.35
5th	5.85	8.92	1.80	1.94	+ 0.14
Second	5.88	8.89	1.75	1.92	+ 0.17
Second	6.11	8.66	1.55	1.78	+ 0.23
1. 25.	6.98	7.79	1.06	1.29	+ 0.23
25th	7.15	7.62	1.40	1.21	- 0.19
23. 25.	7.18	7.59	1.49	1.20	- 0,29
25th	7.20	7.57	1.30	1.19	- 0,11
Second	7.21	7.56	0.91	1.18	+ 0.27
23rd	7.64	7.13	0.93	0.99	+ 0.06
26th	8.18	6.59	0.75	0.78	+ 0.03
5th	8.20	6.57	0.80	0.78	- 0.02
23rd	8.43	6.34	0.65	0.70	+ 0.05
23rd	8.56	6.21	0.61	0.66	+ 0.05
23. 26.	8.71	6.06	0.53	0.61	+ 0.08
23rd	8.73	6.04	0.45	0.60	+ 0.15
2. 15.	9.15	5.62	0.48	0.49	+ 0.01
2. 25.	9.77	5.00	0.40	0.34	- 0.06
5th	10.5	4.27	0.40	0.21	- 0.19
			33.38	33.40	

In view of the many difficulties presented by these fine experiments, and in particular by the fact that the values of quite different days are taken together, and these are not sure of complete comparability of the sensitivity and partly exact preservation of the same subjective criterion of being observable Results are a match of the calculated with the observed values and a change in the positive and negative differences between observation and calculation, which you can probably be satisfied with. Of course, the match would have increased a lot if I wanted to skip some not very fitting values, but I gave everything that was just noticeable before the calculation. But I am far away, to regard the given formula as more than an empirical one, within certain limits. For the sake of completeness I finally add the values above 10 °, 5 up to 14 °, 20 observed values of*D* , if nothing else can be seen with certainty from it, than that they are very small. However, they are still slightly larger than they should be after the calculation, if one applies the previous formula on it, as the compilation of the calculated afterwards with the observed values shows.

date of tries	t ° r.	D	
		Obs.	calc.
December			
25th	10.88	0.15	0.161
23rd	11.36	0.13 *)	0.108
5th	11,45	0.30	0,100
5th	12.15	0.30	0049
5th	12.40	0.20	0,036
25th	12.50	0.15	0.032
21st	13,30	0.20	0.009
21st	13.40	0.25	0,007
5th	13.50	0.15	0,006
5th	13,90	0.25	0,002
5. 21.	14,20	0.15	0.001

*) Significantly instead of just noticeable.

Although these attempts have been made with great attention, they still make it desirable to repeat from the point of view that the experiments have been carried out below the mean temperature with the temperatures only rising above the mean temperature, which may make some entry for comparability. Also, to ensure Weber's law above the mean temperature, a much larger number of observations would be necessary than are present here, so that after all I can give the result of these experiments only as a preliminary, which may still be subject to modification. I designate it expressly as such, and consider that it is quite likely that Weber's law within the limits set by it, but by no means proved. It was my intention to complete or renew the attempts after these relationships. But I've been interrupted and have not found time to come back to it.

About the modality of the experiments, I add the following:

The two vessels in which the water of different temperature contained were large clay harbors to slow down the temperature changes as possible. They were so filled with water that when the index and middle fingers of the right hand dipped down to the floor, the water reached just to the joint between the 1st and 2nd limb of the index finger (calculated from the palma). So always the same contact size was made with the water. The thermometers, fixed in suitable racks, dived with the bullets into the middle of the water, which was well stirred before each observation. The temperature of the water was partly altered by stirring with ice, partly with metal or clay dishes, which stood on the hot stove. The two fingers making the experiment became in one of the two vessels for so long, until immersed in the ground, left until they assumed a constant temperature, then alternately dipped in one and the other vessel until a judgment had formed. If the temperature sensation was above that which I have just described as marked, the temperature was changed by stirring in the opposite direction, so that I did not know whether the excess of the temperature had passed to the other vessel or not, and the observation repeated until , usually only after repeated repetition of this amendment, a just noticeable difference, a procedure that is quite tedious indeed admitted. The temperature was read immediately after judgments were made. until a judgment had formed. If the temperature sensation was above that which I have just described as marked, the temperature was changed by stirring in the opposite direction, so that I did not know whether the excess of the temperature had passed to the other vessel or not, and the observation repeated until , usually only after repeated repetition of this amendment, a just noticeable difference, a procedure that is quite tedious indeed admitted. The temperature was read immediately after judgments were made. until a judgment had formed. If the temperature sensation was above that which I have just described as marked, the temperature was changed by stirring in the opposite direction, so that I did not know whether the excess of the temperature had passed to the other vessel or not, and the observation repeated until , usually only after repeated repetition of this amendment, a just noticeable difference, a procedure that is quite tedious indeed admitted. The temperature was read immediately after judgments were made. until, especially after repeated repetition of this amendment, a just noticeable difference was found, a procedure that is quite tedious indeed. The temperature was read immediately after judgments were made. until, especially after repeated repetition of this amendment, a just noticeable difference was found, a procedure that is quite tedious indeed. The temperature was read immediately after judgments were made.

Although I have only accepted the value of sensation as decisive, in my observation register I have also recorded the following values of sensation, as constant as possible, according to the ascending series of their size.

Imperceptible, barely perceptible, just noticeable, noticeable, distinct, decisive, strong, very strong. Of course, a sharp divorce of these values is not to be expected. The values barely perceptible were those where I was not quite sure not to deceive myself, and although this could be controlled by observation, then a fortuitous agreement was possible; Therefore, I have used such values only insofar as, when I came close to a noticeable or clear point on the same or different observation days, the means of these provisions were taken into account as just noticeable, which had happened several times.

It is indisputable that attempts will be made in this field, by the other methods, to those by the method of just noticeable differences.

Volkmann has Mr. Lindemann, stud. med. to make experiments according to the method of median error, and to write his doctoral dissertation on it, which he wrote under the title: "De *sensu caloris, Halis*But from these experiments can not be concluded much, because the temperature scale has been passed from 7 ° and 14 °, 6 ° to 45 °, 55 ° C., ascending twice and descending twice, but so on Only a few experiments come in the interval of temperature, which allows no use according to the principle of the middle error method: the right hand always plunges to the wrist, always in the beginning warmer in the ascending row, always in the beginning in the ascending row colder water, which was then made equal to the sensation by the addition of colder or warmer water to the other.

In the two ascending series, ie where Lindemann successively brought about the adjustment of the temperatures of both water masses for the sensation in ever higher temperatures, a positive error was always made, with the two descending invariably always a negative error. It may be questioned whether this was due to the fact that in the ascending and descending series, the temperature scale was reversed in the opposite sense, or that in each individual experiment the transition took place in an inverse sense between the initially warmer and colder water. But from the circumstance that the first experiments of each of the four rows show the given relation, the latter is to be concluded. Incidentally, here, as in many respects, there is no need for more precise information on the circumstances in question.

So here we have results that are essentially affected by constant errors, and according to the regularity with which the individual errors in each individual series change as they ascend or descend through the temperature scale, the errors appear to be almost constant, as variable Errors necessarily large irregularities in detail would have to show. The smallness of the same is striking.

Between 26º, 4º and 38º, 8º C. [42] (both inclusive) gave 23 attempts of the 1st ascending series regularly + 0.05 as errors except for only 5 attempts. At higher and lower temperatures the error increased, but little, and somewhat irregularly upward, so that in the interval from 39.4 to 45.5 only errors 0, 5; 0.6; 0.7; 0.8, more down (+ 0.5 at 14.6, which started the ascending series, + 0.4 at 16 ° and 18 °, 2, etc.). In the second ascending series, from 31.35 to 42 °, 9 in 14 ascending trials were invariably found to be 0.05; this rose higher to 0.1 at 44.8 and 45.1, and lower down to + 0.25 at 7 °, 9 and 8.4. In the first descending series, the error - 0.05 was observed from 41.5 to 19.5 in 22 descending trials except three, increasing to - 0.1 at 44.7 and to 0.29 at 7 °;

[42] I always quote only the lower of the two temperatures between which the difference existed.

These experiments are in agreement with mine in that, from an interval where the defects almost disappear, the errors towards the freezing side increase more rapidly or in greater proportion than after the heat side. They give much smaller errors here than I and earlier Weber have found the just noticeable differences, which, however, is not a contradiction, since according to the remark (chapter 8) the errors everywhere must on average be smaller than the just noticeable differences; Partly it may also be due to the fact that only two limbs of my two fingers, and Lindemann's whole hand, was immersed in it. A more significant difference is that Lindemann finds the interval of smallest errors around the blood heat, instead of me having the interval of the smallest noticeable differences around the mean temperature. In the meantime, since his mistakes are evidently essentially constant, it can not be judged whether there is a real contradiction in this; and new experiments on this subject are needed anyway. It is clear from the experiments so far that the insignificance of the differences which can still be recognized, as well as the errors which are committed on average, present great difficulties to precise measurability.

Perhaps the most appropriate approach to experimenting with this subject would be to apply analogously the method of right and wrong cases, as happened in my weight tests. Of course, it will not be easy to obtain such constant temperatures and temperature differences, as can be obtained weights and differences in weight; if you now reduce the causes of temperature change as possible, and z. If, for example, after every 10 observations the temperature is re-recorded and, if necessary, regulated, it seems, especially in view of the reductions which the fundamental table permits, that viable results must be obtained.

5) Extensive sizes. [43)]

(Good judgment and tactile measurement.)

Apart from the general statement of Weber's, F. Hegelmayer has taken the measure of good fortune [44)], stud. med. in Tübingen, an approximate confirmation of Weber's law was given by the method of right and wrong cases, but which both in regard to the far too small number of experiments, as the lack of comparability, which took place between several drawn from which funds 'leave too much to be desired to be considered as authoritative. In essence, the experiments consisted of comparing lines of given length, partly horizontal, partly vertical, with other previously seen lines that differed by certain larger or smaller fractions thereof, changing the interim time to examine their influence the main observer's intention was, and counted, how often the estimate, whether larger or smaller, was correct, erroneous or *in suspenso* remained. As much as his observations allow to conclude, the relation of the right and wrong cases was not essentially dependent on the absolute, but only on the relative size of the fractions, which Hegelmayer himself draws; but the results are very irregular at all, and I pass over their more specific communication.

[43)] In matters 174-178. About Proof Revision p. 334-358. Over measure. Pp. 423-427. About the measures of the sense of space, Abhandl. the Royal Saxon Ges. dW XXII, II., pp. 111 ff., on the validity of Weber's law in the domain of the sense of the time Revision pp. 419-423, Abhandl. the Royal Saxon Ges. d. W. XXII. No. I, p. 9 ff.

44) Vierordt's Arch. XI. p. 844. 853.

My own and Volkmann's attempts by the mean-error method, with distances observed between small points or parallel threads, give a very definite confirmation of the law for any considerable distance, that is, from 10 to 240 millimeters at an eye distance of 1 foot to 800 Millimeters, in that the pure error sums or mean errors that have been obtained in this case go as precisely proportional to the distances as one can always expect. In contrast to this, Volkmann's experiments, as well as the attempts he made by Appel (a student with particularly acute eyes) with micrometric distances of 0.2 to 3.6 millimeters, do not find this proportionality at eye distances which are beyond ordinary sight;[45) , constant at the different normal distances, the other, which I call the Weberian variable, proportional to the distances in the sense of Weber's law. Probably the former is also involved in the experiments with the longer distances, but it is so small that it disappears markedly against the latter, the distance-proportional, component at greater distances, and in the uncertainty of its determination goes down, while it at all small distances forms the larger part of the variable error sum. At the very smallest distances of 0.2 and 0.3 millimeters for Volkmann's eye, the error was also abnormally increased by irradiation, as it seems.

[45)] It is not a constant error in the sense of Ch. 8, but grows as well from variable errors as the other component, and therefore called constant opposite it, because, as determined above, it remains constant with variation of the normal distance, not, like the Weberian variable, itself with it changes.

It can be seen, therefore, that we are dealing here with a lower limit of the law for the experiment; and probably very large distances would also make an upper one.

The main results are included in the following. They all refer to the pure variable error Δ in the sense given earlier (chapter 8), and everywhere give the pure error συμ ∑Δ , and mostly (where I have determined it) the pure συμ οφ squares $\sum (\Delta^2)$, for every distance in particular derived from μ fractions of m observations [46)] made according to the time period , so that the total number of errors contributing to each special sum is μm . The numbers μ and $m$are specified for each series of observations. For the horizontal sums, the double μapplies , provided that the sums therein are always composed of two special sums, respectively for L. and R. or O. and U. In fact, there were always the same observations of the left and right positions of the normal distance (L. and R.) when the distances were horizontal or in the upper and lower positions (O. and U.) when they were vertical, for which the results are specified.

[46)] It goes to chap. Figure 8 shows a certain difference in the absolute values of a pure error sum, whether it is derived from fractions or from the totality in the context.

Only the micrometric series V is made with vertical distances, ie between horizontal threads, all others with horizontal distances, ie between vertical threads (where threads have been applied).

The proportionality with the distances can be τεστεδ directly on the simple sum $\sum\Delta$, without deriving the mean error from it.

The least-squares of sums can, if you will, serve to derive the quadratic mean error , according to which one can convince oneself of the constancy and the behavior of the normal relation as far as the contingencies permit, but which investigation I pass over here. Just as they can serve to prove that is easy to infer from the previous conditions, and that the sum of the error squares is elsewhere considered in more detail from me, $\sum(\Delta^2)$, divided by the square of the error sum $(A\Delta)^2$ and multiplied with twice the number of observations, that is, here with $2\mu m$, approximately the Ludolfian numberπ gives. Only that the observations of the series I and II at the smallest distance are not suitable because of a circumstance to be passed here. However, these conditions are not closer to us here.

All the series cited here have more or less constant errors, which are of no interest in this place, but will take place in my "measurement methods."

Series I Fechner (9 Dec. 1856 to 17 Jan. 1857).

5 horizontal distances, determined by the little protruding (Nähnadel-) Spitzchen two concealed by me, next to each other on the table, lying circle, and from the clearest Sehweite of about 1 par. Looking at the foot. Distance determination by means of a scale with transversals, the tenths of a, here the unit forming, half par. Decimal line (which equals 0.72 duodecimal line). The covering of the circles was done to exclude the influence of the angle in the estimation. However, the process retains the small defect that the circular points projecting from the ceiling are slated at greater distances than at smaller distances, a deficiency which is avoided in the subsequent series of experiments by the use of parallel threads.

In order to leave no doubt as to the interpretation of the numbers in this table, I give special mention to those of the first, according to which all the rest may be easily interpreted.

At the distance D = 10, which according to the previous 10 half paris. Decimal lines, = 3,6 duod. Lines was, the position of the normal distance to the left (L.) a pure error συμ $\sum\Delta$ = 20,27 ωασ obtained; that is, when (L.) all at D = 10 together expects obtained positive and negative of the sum according to the same error pure by absolute values, the sum of half 20,27 is par. Decimal lines out. The indication of m = 60, μ = 2 on the table then it means that this error sum, just like all the others in the columns L., R., from 2 ·60 = 120 individual errors composed; but that every such sum of error is not derived in the context of the 120 observations, but especially from two fractions of 60 observations; for each of which the mean error distance and against it the pure errors were specially determined.

m = 60, μ = 2. Unit ½ par. Dezimallinie.

D		10	20	30	40	50	total
AΔ	L.	20.27	35.98	60.42	85.29	85.85	287.81
	R.	18.37	40.87	60.49	69.19	99.55	288.47
total		38.64	76.85	120.91	154.48	185.40	576.28
∑ (Δ^2)	L.	4,621	17,36	50.56	88.41	105.99	266.94
	R.	4,056	23.06	47.11	57.74	122.47	254.44
total		8.677	40.42	97.67	146.15	228.46	521.38

Series II Volkmann (March 22 to April 1, 1857).

Eight horizontal distances, determined by three parallel strands stretched by weights and displaceable on a horizontal scale, vertical, against white, against a black background, threads of 220 mill. Length in 800 mill. Distance of the eye. The scale gives directly millimeters, between which is estimated.

Here we give the sums ∑Δ here by double calculation, for m = 48, μ = 1, and m = 16, μ = 3, which gives υσ αν οππορτυνιτψ το χονϖινχε ουρσελϖεσ οφ τηε πενδινγ differences (see chapter 8).

1) m = 48, μ = 1 **.** Unit 1 mm.

D		10	20	40	80	120	160	200	240	total
AΔ	L.	7,552	7.914	26,95	39,90	75.05	102.30	87.11	117.96	464.7
	R.	5,050	10,800	24,50	42.89	58.70	93.82	96.63	145.82	478.2
total		12.602	18.714	51.45	82.79	133.75	196.12	183.74	263.78	942.9
∑ (Δ^2)	L.	1,657	2,558	22.66	48.67	199.96	371.83	229.63	394.45	1,271.41
	R.	1,021	3,406	18.11	60.47	117.37	314.56	331.57	612.95	1,459.46
total		2,678	5,964	40.77	109.14	317.33	686.39	561.20	1,007.40	2,730.87

2) m = 16, μ = 3. Unit 1 mm.

D		10	20	40	80	120	160	200	240	total
AΔ	L.	7.13	7.59	20,08	39.79	75.45	103.65	86,40	108.92	449.01
	R.	4.86	11.06	23.58	42,10	58.45	77.23	96,20	140.20	453.68
total		11,99	18.65	43.66	81.89	133.90	180.88	182.60	249.12	902.69

It can be seen that the difference between the two methods of calculation is very small for most values, but very significant at D = 40R and D = 1 60R, which is related to a strong variation of constant errors, shown in detail in the series, which took place here had [47] . Since this variation is better eliminated by fractionation, the calculation 2) merits 1) preference.

[47] The fact that some of the values in Table 1 are slightly smaller than in Table 2 depends on the particular distribution of the errors.

Series III. Volkmann (6 and 17 Dec. 1857).

This is a later repetition of the previous series under the same circumstances, only with the omission of the two smallest distances.

$m == 16, \mu = 3$. Unit 1 mm.

D		40	80	120	160	200	240	total
ΑΔ	L.	21.1	42.4	57.0	90.0	81.4	98.2	390.1
	R.	8.4	32.1	63.5	63.2	106.3	117.9	391.4
total		29.5	74.5	120.5	153.2	187.7	216.1	781.5

Not without interest will one see in previous series the great agreement between L. and R. in the vertical final sum columns; a proof that the pure variable errors are independent of the position L. and R., whereas the constant errors, which are not mentioned here, were very dependent on it, and brought great differences into the raw error sums L. and R. ,

The three series consistently show the proportionality of ∑Δ with the distances, which is most easily overlooked when dividing the sums by the distances where each row shows a noticeable constancy of the quotients. From the sums for L. and R. (in the second row after m = 16, v = 3) one obtains the following values for

in

I	II	III
3,864	1,260	0.738
3,843	0.936	0.932
4,030	1,286	1,004
3,862	1.035	0,958
3,708	1,114	0.939
	1,226	0.900

	0.919	
	1,099	

In order to obtain the average error made for the unit of distance in an observation, or the fraction of the distance that the error forms on average in an observation, one can divide the average of previous values for each row by the number of observations. which contributed to a value to which one has to take twice the product of the *m* and *μ* over the observation tables, since the μ ισ especially valid for L. and R., but here both are summarized. Since, however, the larger error sums promise more exact values for larger distances than for smaller ones, one proceeds more precisely [48]if one adds all the error sums, which addition is already made in the vertical final sum columns, divide the sum of them with the sum of all distances, thereby obtaining the error sum for the distance unit, and divide this by 2 μm . That's how you get

I.

II. 1)

2)

III.

[48] The method of the least squares provides a more or less precise, but more complicated method of determination, the result of which, however, deviates so little from the above, that it is not worth while to consider it.

After this I guess even average a distance by about 1 / 60 Volkmann in his previous attempts (II) by about 1 / 90 , in his later (III) by about 1 / 100 wrong, and this ratio is for the various distances equal. If one wishes, one can derive from this mean error the probable error by simple multiplication by 0.845347, ie the error which has just been exceeded as often as is not reached, which is therefore smaller than the mean error, because smaller errors to be made more often than large, but the given normal ratio has to what about details in my "measurement methods".

One can see that the accuracy of the estimate was considerably greater for Volkmann than for me. This may have been due either to the fact that the distances between three parallel threads may be more easily compared than between the tips of two adjacent circles, or to a really greater sharpness of proportion than to me, which indeed appears to occur, or on both together; which would have required further comparative experiments to be decided. And it is undisputed that a more extensive study of extremes and averages of sharpness is made for a greater number of individuals and for different circumstances of observation, according as one passes the lateral thread, as in Volkmann's experiments, or the middle thread, according to one or both eyes used in the observation Depending on the vertical, horizontal or angular distances between points, between lines, and the size of circular, square rings and surfaces, etc. subject to the experiments, a not small interest; being careful to take care of the size and nature of constant mistakes everywhere. Here, however, it was merely a question of pursuing the object in relation to the law, which concerns us now.

If Volkmann's second series of observations has produced a not insignificantly lower average error, and thus greater precision than the first, then the difference can be written on the success of the exercise; there between the l. and row 2 have even tried a series of dimensional tests, including all the following micrometric ones, although in the fractional treatment of the first row such progress has not been shown, as the special investigation of the fractions shows.

Perhaps it may be of interest that Volkmann's average error for the extensive side of the facial sensation agrees markedly with the just noticeable difference of the intense side in him; but you can not see anything universal in such a match.

I have the numbers $^1/_{60}$, $^1/_{90}$, $^1/_{100}$ indicated in the Raw, by the previously stated precisely appearing figures $^1/_{62.5}$ u. sf themselves as can not be accurate and are quite comparable because a different *m* has succumbed in their derivation, and that *m* is a finite everywhere. After the point (ch. 8) but is obtained the smaller the error sums, and therefore mean error, from a smaller depending *m* is carried out the discharge. The proof to it gives row II, where on 1) 0,01187 or , on 2) 0,010808 = as the mean error for the distance unit. Both determinations are subject to the same observations, but were divided in 1) in fractions of *m* = 48, in 2) in fractions of *m* = 16 observations, from which the derivation took place. As you can see, the difference in the results is not significant, but at least present and considered.

To return all values to the normal case that the observations would be infinitely many, one has, according to the correction formula, which I briefly cited (see Chapter 8) and theoretically grounded in my "measurement methods," each of the values previously obtained to be multiplied to obtain, since *m* relative 60, 48, 16, 16 is obtained:

I.

II. 1)

2)

III.

If this correction were to be perfectly sufficient, then in the second series the results 1 and 2 would have to be brought into perfect agreement. In fact, we see that they are so close that one will no longer find the difference very noteworthy, and might be inclined to write to him that this correction is not an absolutely exact and certain one, but only a law of probability , which can leave small differences by chance. However, the difference in fact is not accidental, as taught me a sufficient examination of other analogous cases by always found in the same direction [49] and this depends, as I can also prove, and has already briefly pointed out in the eighth chapter, that our correction does not coincide with the never entirely missing variations of the constant error, which at larger *m* contaminate the pure variable error. In this regard, the corrected value at *m* = 16 will be preferable to the value at *m* = 48.

[49] If we therefore put distrust in the above correction, then I notice that the correction of the quadratic mean error accepted by all mathematicians and astronomers, which I stated (see chapter 8), shows the same insufficiency for the same reason as I am also able to prove and prove sufficient experience.

Since the constant error in my observations in series I did not have a considerable size, any variations thereof will not have much influenced the result, so that the corrected value can be considered accurate enough. I did not go into a very special investigation.

I now pass on to the presentation of the results of the micrometric series. All these rows are made with a micrometric screw apparatus, which gives by readings on the screw head parts of 0.01 millimeters, between which tenths have been estimated, which form the unit in the following tables, so that 0.001 million following everywhere is the unit, and z. B. a distance equal to 300 a real distance = 0.300 millimeters, an error sum equal to 265 such equal to 0.265 millimeters means. Where there are still fractions which, incidentally, are rather idle, they have been created by reducing the raw errors to mere ones.

The distances in this apparatus [50] are determined by three fine parallel silver filaments of 0.445 millimeters thickness and 11 millimeters long, which were viewed at different distances, indicated throughout in millimeters, against the frosted glass of a lamp or the bright sky.

[50] Further described in the reports of the sächs. Soc. 1858. p. 140th

In Volkmann's series the values are found bracketed at the very shortest distances, as those which emerge from the law of series, and therefore are not taken into account in the subsequent calculation. The reason for this deviation lay in the fact that irradiation was so strongly asserted here, and that the threads came so close to flowing that Volkmann, during the experiments, himself felt the uncertainty of estimation incomparable to the other distances. In Appel's very keen eyes, which are very little affected by irradiation, such an exclusion did not appear necessary.

In addition to the micrometric series reported here, there are two more which I pass over, because they were employed with too few and too close distances, and contained in them too discordant values.

Series IV. Volkmann (March 22 to April 1, 1857).

7 horizontal distances. Vision 333 Mill.

$m = 30$, $\mu = 4$.

D		200	400	600	800	1000	1200	1400	total
AΔ	L.	(694.5)	534.0	630.6	740.5	824.2	1,023.2	1,057.6	5,504.6
	R.	(630.5)	611.3	672.3	801.0	952.8	1,097.6	1,218.1	5,983.6
total		(1325.0)	1,145.3	1,302.9	1,541.5	1,777.0	2,120.8	2,275.7	11,488.2
$\Sigma(\Delta^2)$	L.	(13439)	8327	11721	14344	16561	29964	31144	125500
	R.	(11134)	10968	12504	17655	22564	32419	38835	146079
total		(24573)	19295	24225	31999	39125	62383	69979	271579

Series V. Volkmann (April to June 1857).

6 vertical distances. Sehhe 333 Mill. These experiments with vertical distances are made with the glasses, as otherwise difficult to estimate here instead of sufficient clarity, while all attempts are made with horizontal distances without glasses.

$m = 96$, $\mu = 1$.

D		(400)	600	800	1000	1200	1400	total
AΔ	O.	(1429.2)	1,645.3	1,618.9	2,417.4	2,388.2	2,993.6	12,492.6
	U.	(1563.0)	1,335.0	1,998.7	2,070.0	2,810.3	3,150.0	12,843.3
total		(2998.2)	2,980.3	3,617.6	4,487.4	5,198.5	6,143.6	25,338.9
$\Sigma(\Delta^2)$	O.	(28170)	42981	45016	97527	89314	155248	458256
	U.	(50708)	27011	72011	73199	128531	176638	528098
total		(78,878)	69992	117027	170726	217845	331886	986354

Series Vl. Appel (May and June 1857).

7 horizontal distances. Sight 370 Mill.

$m = 48$, $\mu = 2$.

D		200	300	400	500	600	700	800	total
AΔ	L.	592.44	508.00	653.02	643.90	726.64	739.12	716.00	4,579.12
	R.	594.20	679.00	681.00	575.50	719.52	649.00	778.61	4,676.83
total		1,186.64	1,187.00	1,334.02	1,219.40	1,446.16	1,388.12	1,494.61	9,255.95

Series VII. Appel (October 1857).

6 horizontal distances. Sehweite 300 Mill. The calculation of ∑Δ follows here twice, for $\mu = 2$ and $\mu = 6$.

$m = 33$, $\mu = 2$.

D		200	400	600	800	1000	1200	total
AΔ	L.	442.6	647.8	661.9	929.2	941.9	1,070.8	4,694.3
	R.	450.6	623.9	715.8	720.5	838.8	1,027.0	4,376.6
total		893.2	1,271.7	1,377.7	1,654.9	1,780.7	2,097.8	9,070.8
$\sum(\Delta^2)$	L.	4773	10046	9805	18422	19899	23595	86540
	R.	4385	8620	11895	13149	15810	22901	76960
total		9358	18666	21700	31571	35709	46496	163500

$m = 11$, $\mu = 6$.

D		200	400	600	800	1000	1200	total
AΔ	L.	422.8	646.7	661.9	848.4	901.3	1049.1	4,530.2
	R.	455.2	620.4	688.8	691.2	812.0	976.0	4,243.6
total		878.0	1,267.1	1,350.7	1,539.6	1,713.3	2,025.1	8,773.8

If one surveys the results of these series, for which the bracketed values may, for some reason, be disregarded once and for all, then one sees not only between the series of the same observer, but also the two consensus observers, ie, an increase in the error sums Distances, but much slower than in proportion, and also the two series that have been left out here, in this general result, are quite unanimous with the others. However, as noted, one can represent the error sums as resultants of two components, one of which is constant at the different distances and under the name of Volkmann's constant with *V*is to be denoted, while the other is proportional to the distances, and is to be designated by the name of Weber's variable for the unit of distance *W* , whereafter *W is* still to be multiplied by the distance *D* to be proportional to the distance for each distance To give value *WD* .

However, according to the theory of how to combine the effects of sources of error, the composition of the pure error συμ ∑Δ for any given distance from both components V and WD is not to be represented by a simple addition of both components, ie one can not set

$$\sum\Delta = V + WD$$

but the sum of the squares of both components is equal to the squares of $\sum\Delta$, so that one has

$$(\sum\Delta)^2 = V^2 + (WD)^2$$

therefore

,

Since the square of an error sum , di $(\sum\Delta)^{2}$, has an *a priori* determinable ratio to the sum of the error squares $\sum(\Delta^2)$ according to the error theory , then instead of the squares of the error sums, sums of the error squares in previous equations can be substituted. The physiological interest, to which I shall refer below, is likely to be more in the former form, which I have therefore initially based on the following.

In fact, it can easily be proved theoretically and proven by experience that, if two error sources independent of each other are given, one of which would have produced an error sum A for itself *and* the other an error sum B , then their collision does not yield an error sum $A + B$but only a smaller sum of errors, because on average errors of opposite sign as well as of the same sign from both causes meet on average, but only the latter give a resulting error equal to its sum, the former equal to its difference; However, the theory of errors shows that the sum of the squares of the components normally (ie, errors strictly obtained for an infinite number under similar circumstances) is equal to the sum of the resulting squares of errors, such that the sum of the squares of the simple error sums is normally the squares of the squares resulting error sum is equal, and a confirmation of this result of the theory by experience can be found easily by composing the errors of two independently derived error series as components by algebraic addition to resulting errors, thereby obtaining an equivalent for the coincidence of the success of independent sources of error. In fact, in this way I have convinced myself of the confirmation of the theoretical result both in terms of the sum of squares of the error and of the square of the error sum by multiple samples and will give the evidence elsewhere.

Insofar as an error source independent of the distances and dependent on the distances in the specified sense should exist for the eye, the above will also have to be applied to the components dependent thereon, and the above equations will thus be justified. But whether the presupposition of such sources of error is valid is to be ascertained from the observations themselves, insofar as in the case of validity such values V, W can be calculated from the fact that the observation values can be represented backwards by the above formulas.

For such calculation of V , W observations at two different distances are sufficient. If we take the error sums in row IV for D = 800 and D = 1400, respectively 1541.5 and 2275.7, by combining L. and R., then we have to start with

$$V^2 + 800^2 W^2 = 1541.5^2$$
$$V^2 + 1400^2 W^2 = 2275.7^2$$

from this, $V2$ and $W2$ are easily to be found as two unknowns determined by two equations, according to which there is a root extraction V and W itself.

Insofar as experiments are available for determination at more than two distances, one can compute V and W from several such combinations, where then the validity of the assumption must be justified even before the backsighting of the error sums according to V and W by the values of V, W, which is obtained from the various combinations, agree closely enough to be able to write the deviations remaining on unbalanced contingencies of observation. By drawing back from several values determined in this way, one can determine V and W more precisely.

This method has only the misfortune that the choice between the observation values, which one wants to combine for every two, is arbitrary, and every other way of combining one finds a somewhat different definitive means; although, if the observations are really accurate enough, the differences in the definitive result are so small that one value can be used as well as the other. In the meantime, however, the calculation according to the method of the least squares, which excludes all arbitrariness, and prefers to find the most exact possible result, which is to be inferred from the series of observations, remains to be preferred. It has, according to the combined sums for L. and R., without reduction to the same $m$and to the same range of sight, which follows below, immediately given the results [51], where the probable error of the determination with ± ισ added, and μ ισ the valid for the summary of L. and R., that is twice as large as the μ is above the watch tables.

[51] The same was applied so that the equations were set in the form $V^2 + D^2 W^2 = (\sum \Delta)^2$, which is directly linear if V^2, W^2 are sought as unknowns. From the thus obtained values V^2, W^2 are then V, W derived by Wurzelausziehung. The probable errors of V, W are for the deviations of the calculated ovεσ from the found $(\sum\Delta)^2$ for V^2 andW^2 is calculated, and according to the principles of the error calculation by dividing relative to $2V$, $2W$ of the of V, W reduced.

Values of V and W for the unreduced error sums, after the Equation $V^2 + D^2 W^2 = (\sum\Delta)^2$.

line	m	μ	Visual distance.	V	W
IV. Volkm.	30	4	333 million	974.36 ± 34.34	1.5008 ± 0.02628
V. Volkm. (Vert.)	96	2	333 -	1398.20 ± 49.35	4.2411 ± 0.01332

VI. Appel	48	4	370 -	1169.90 ± 33.76	1.1603 ± 0.10008
VII. Appel	33	4	300 -	1008.60 ± 121.97	1.5668 ± 0.051576

Now, to decide whether our assumption of Volkmann's constant and Weber's variable is confirmed in the stated sense, we may first of all pay attention to the probable errors of their destiny, which are generally very small relative to the values of *V, W* demonstrate. Secondly, according to the values of *V, W* in the previous table, ωε can calculate the values of $(\sum\Delta)^2$ or $\sum\Delta$ associated with the various *D's of* the experimental tables, the former according to the equation $V^2 + D^2 W^2 = (\sum\Delta)^2$, the latter after Equation , and can compare the result of the calculation and observation, where a very satisfactory agreement is shown. I give following the compilation for $(\sum\Delta)^2$, whereby I pass for the sake of brevity the indication of the distances, which can be supplemented with bracketed values from the observation tables.

Compilation of the observed and the values of *V* and *W.*

values calculated in the previous table $(\sum\Delta)^2$.

IV		V		VI		VII	
Obs.	calc.	Obs.	calc.	Obs.	calc.	Obs.	calc.
1311700	1309780	8882200	8430000	1408000	1422444	797820	1210970
1697600	1760270	13087000	13466000	1409000	1489750	1617300	1392280
2376300	2390980	20137000	19940000	1779600	1583980	1898100	1861120
3157700	3201880	27023000	27855000	1486900	1705130	2738700	2517500
4497800	4192980	37744000	37208000	2091500	1853200	3170900	3361400
5178800	5364280			1926800	2028200	4400700	4392900
				2233900	2230110		

The correspondence between the calculation and the observation is very satisfactory, except for a few slightly different values of the series VII. And after this it may be said that Weber's law is confirmed to the smallest extent in the range of eye size, only that it is subject to a complication which one must first dissolve in order to recognize it.

It has seemed to me of interest to make the calculation of the observations in a few modified ways, which do not lead to values substantially different from those of the previous ones, but which can serve to show how the possible choice between these different ways of calculation makes no material difference justified in the results. These different calculation methods were applied to the series IV.

1) Instead of calculating L. and R. in unison, as above, I have separated both, and calculated the same way; so i got

	V	W
Left	436.82	0.7540
Right	500.23	0.8005
	937.05	l, 5545

2) Perhaps one holds in principle the approach of the equation

for the least-squares calculation more ρεασοναβλε than the one above, since $(\sum\Delta)^2$, but $\sum\Delta$ ισ νοτ directly observed. But as a result, the equation loses its linear form, and one must expect corrections, which, as the expert will easily find, leads to great expansiveness. Meanwhile, for the series IV, I have specially executed this bill for L. and R. what has given

	V	W
L.	441.1	0.7517
R.	502.0	0.7970
	946.1	1.5487

These values differ only insignificantly from those found under 1), and it would not be worth the effort to follow the more complicated path.

3) Instead of the square of the error sum $(\sum\Delta)^2$ I have the sum of the error squares $\sum(\Delta^2)$ for the calculation of the constants V', W' according to the equation $V'^2 + D'^2 W'^2 = \sum(\Delta^2)$ based on. So I got for L. and especially

	V'^2	W'^2
L.	6084	0.013591
R.	7429	0.016234
	13513	0.029825

Now according to the error theory $\sum(\Delta 2)$ with $(\sum\Delta)^2$ by the equation

linked where π the Ludolf'sche number, which approximate to the previous values of V and W returns.

Also for the series VI and VII I made the calculation according to the first (below 1) used approaches for L. and R. especially. I received for VI

	V	W
L .	507.88	0.69166

R. 609.47 0.41611

1117.35 1.10777

for VII

V W

L . 515.66 0.81175
R. 447.48 0.75605

963.14 1.56780

The table of values *V, W* given above gives the same for the error sums obtained in each series of errors in species, and hence proportionally for these sums. But since the different series are subject to a different number of errors for each distance given by the product μ *m of the* same table, the values of the different series must be comparable with each other with their number of errors, see 120, 192, 192, 132, giving the values *V* , *W* as they are on average for 1 observation. It must also be remembered that since a different *m* in the observations, because of the finiteness of the *m* , the correction by multiplication , which by the way is only very slight, should be added; Finally, it must be remembered that the range of sight, from which the distances were conceived, was not the same everywhere, which indeed can have no influence on the value *W* , provided that the distances appearing smaller at greater distances will always give the same proportion *error W* ; the error *V* which, in order to be comparable when obtained from different points of view, must be reduced to the same field of vision by reciprocal conditions of the range of vision which takes place every time; but to add to each, measured with respect to the cornea, eye distances 7 millimeters as the distance of the crossing point of the direction lines of the cornea, so that z. For example, for the range of vision 333 mill. In the series IV. 340 mill. Is to be taken in the reduction, and so on

[52] Cf. In matters p. 216 f. Revision p. 110 f. 339th

Taking these three reductions or relative corrections by reducing all values to the case of a single observation as the mean of an infinite number of observations at 333 + 7 milli-widths, we obtain the following instead of the values in the table above.

Corrected and reduced values of *V* and *W* for an error at 340 million distance from the crossing point of the visual rays.

Line.	*V*	*W*
IV. Volkm.	8.210	0,01265 =
V. Volkm. (Vert.)	7,319	0,02220 =

VI. Appel	5.5331	0.00608 =
VII. Appel	8.5476	0,01172 =

The values of this table show some interesting and some conspicuous. The value of the Volkmann series IV with micrometric horizontal distances is not very different from the values of the series II, which were obtained just before by Volkmann at much larger distances , which is nothing more than a value *W* derived from a series where the complication with *V* disappears. The difference that still exists between the two values can very well be written on the exceptionally different experimental circumstances.

In contrast, between the micrometric series IV with horizontal distances (between vertical filaments) and the micrometric row V with vertical distances (between horizontal filaments), the most noticeable difference in the value of *W,* notwithstanding that both are not very separated in time by *W being* vertical Distances is almost as big as in horizontal; that is, the estimate for the former much inaccurate, which was also felt directly in the experiments themselves. Also, the constant errors not listed here were much larger at the vertical distances than at the horizontal ones. Appel's values *W*in the two series of observations VI and VII for horizontal distances differ so far from each other, and the value at VI is so small that it gives rise to mistrust. In the observations themselves, however, nothing is found which certainly grounds mistrust, or explains the difference. The value in row VII is very close to Volkmann 's; without any knowledge of the results of Volkmann being obviated and contributing to agreement. A very special interest is claimed by the Volkmann constant *V.* Apart from the somewhat more deviant Appel's row VI, which also has something suspicious regarding *W* , the three other values of *V* for two very different observers, so close to each other for vertical and horizontal distances, that one can assume that there is an absolute constant established in nature; for the difference of 7,319 and 8,210 between *V* at horizontal and at vertical distances is not greater than that it is randomly observed, and that the complication of magnitude *V* with the constant error and *W* in the raw errors [53)] exact excretion of the same difficult, probably even as accidental. The Appel's value of 8,546 is surprisingly in line with Volkmann's 8,210. A complete assurance of the constancy of this size in different individuals and under different circumstances of observation would, of course, demand an even greater multiplication and further extension of the experiments than are presently available.

[53)] The raw error δ are composed of the pure error variables Δ and the constant error *c* as components together, and *V* and *W* are again of the pure components error variables Δ .

It wonders what significance this size can have. In anticipation of his own future presentation, which Volkmann will give of his investigation, I will briefly mention here the point of view which, from the outset, gave Volkmann the presumption of such a constant, and conducted it at the onset of his arduous experiments. For, indeed, the existence of it was a presumed one in advance, if it has remained doubtful whether what has been found is really what has been supposed.

If Weber's view is correct that the size of a distance is estimated according to the number of retinal elements that it holds between them, a line or distance on the retina must appear the same size, their ends may be the nearest or farthest points of two Retinal elements meet, and a smaller line may therefore appear the same size, as a larger; so in the cases expressed by the following scheme; where the circles represent the sensory retinal elements thought to be circular,

which scheme easily overlooks the fact that a line or distance, which is larger or smaller than another by a noticeable two diameters of one retina element, may under some circumstances be considered as large, an error which, in the case of larger lines or distances, which many retinal elements to understand in oneself, but is negligible, but not so in micrometric lines and distances. In the case of micrometric experiments according to the method of mean error, therefore, a noticeable error in the equalization of the distances must depend on this; the size of the mean error dependent thereon must be related to the diameter of the retinal elements; Volkmann's constant could represent this mean error, and hence allow a conclusion on the dimensions of retinal elements,

In order to investigate the question more precisely whether this mean error can be represented by the Volkmann constant, it was necessary to 1) determine the relation of size which the mean error flowing from the given circumstance must have to the diameter of a retinal element; 2) to investigate whether the latter may be exactly or sufficiently constant for the different normal distances in order to be able to identify a constant, as what *V has* shown, 3) whether the magnitude of this constant is sufficient with respect to that relationship true to the anatomically determined dimensions of the retinal elements.

The first and second questions are in themselves matter of probability, and very well indicate the principle of calculating the execution but even too difficult for experienced mathematicians [54] . One can, however, supplement it by a route of experiment, which establishes outside the circumstances which are presupposable in the eye, and which Volkmann has taken. The third question suffers from the difficulty that the last perceiving retinal elements may not yet be known exactly. However, I do not enter into any further discussion of this whole subject in order to avoid too much anticipation of Volkmann's own communication, which is the property of this investigation. The foregoing must have sufficed to direct the interest to the constant in question.

[54] Apart from my own judgment, I can appeal to the judgment of Prof. Möbius in this regard.

To conclude the preceding consideration of Volkmann's constant, let us mention only the calculation by which the size observed in the experiments is due to that which it represents on the retina itself, a reduction which is, of course, necessary if one wants to examine the question of their relationship to the size of the retinal elements.

According to the table (point V), Volkmann's constant at a field of vision = 340 millim., Calculated as the point of intersection of the directional lines, was 8,210 di 0,008210 millim., If the unit in which all the results of the micrometric experiments were expressed are, 0.001 million. Assuming now the distance of the crossing point of the directional lines of the retina in round number to 15 mill., Then the quantity representing *V* on the retina behaves to the observation V as 15 **:** 340, ie the constant *V*in the given row represents on the retina a size = 0.0003621 mill. This assuming that the linear size, which occupies the image of a distance seen on the retina, is given by the distance of the retina, which from the boundaries of the outer Route between the lines drawn by the crossing point of the direction lines between them. Such is the ordinary bill.

One wonders, of course - a remark I owe to EH Weber, - whether the point of intersection of the directional lines is determinative. In general, we measure distances by eye movement, passing the axis of the eye from one boundary point to the other, and it appears that the point of rotation of the eye is assumed to be the one by which the rays must be drawn from the boundaries of the outer line , which takes the picture of the same on the retina to determine. But this is [55] 5.6 Lin. = 14.224 millimeters behind the foremost point of the cornea, that would be 7.778 mill. In front of the retina, which would reduce the previously calculated size by about half. I have to put the decision of this question on my side.

[55] After Volkmann in Wagner's Wörterb. Art. See. P. 234.

One might think that Volkmann's constant depends on the fact that the error of estimation of the division is made, which, of course, can not depend on the magnitude of the observed distance, and therefore gives a constant mean error at all distances. But our *V* is much too large for that because the direct reading on the micrometer screw gave us 0.01 or 10 thousandths of a millimeter; *On* the average, however, it amounted to about 8 thousandths of a million. In the mean, that estimate can not be far too much. It is undisputed that Volkmann's constant has received a small increase through this source.

Should it really have a fixed organic reason in mind, then here in the field of extensive sensation of light we have found a remarkable analogy with what we have found in the field of intensive, provided here too Weber's law only insofar as we take into account a constant quantity added by internal organic causes of external variable action.

Before applying the method of mean errors, I have also made some experiments according to the method of just noticeable differences over the distance measure with the eye, which I, though they are actually antiquated by the more accurate and sure after that method, here only want to lead because otherwise there are no definite ones according to this method.

After a few tentative experiments on the acuity of my judgment, a circle became a span of l par. Duod. Customs, another of 1 plus $^1/_{40}$ Inch and confused the circles so that I did not know which one was the next. By means of mere judgment I sought to discover which one was the other. I decided correctly each time, but only after a long test for the next. The circles were held next to each other in the clearest visual range in front of the eye, so that the distances to be compared were the compass points in the horizontal. But the same difficult but definitely correct decision was made after the span and the difference doubled and was quadrupled, so that in the latter case the span of one circle was 4.0 and the other 4.1 inches. This small series of three experiments has been repeated by me three times with equal success, twice in one day, once a day. Nor did it make any difference in the feeling of the difference in the span whether I held the circles at a greater or lesser distance from the eyes, except that the limits of accommodation of the eye were not exceeded. I would probably be a little finer than that$^1/_{40}$ have always distinguished correctly. But I have already remarked that, if one does not take the limit of what is just obvious a little higher, one gets into the method of correct and false cases, which in many observations is more precise but lengthy and unsafe in few attempts. The difference was small enough that, if I cut it in half, no more reliable decision was made, and with my then untrained eye demanded great attention to be recognized.

As well as Weber's law has been confirmed in the domain of circumspection, it is necessary to revalue the question of what this confirmation actually means for the extensive sensations. In the sense of Weber's view of the mediation of the magnitude of extensive sensation, the fundamental question which we wish to answer to the significance of Weber's law in this field would be whether differences of spatial distances appear equally large or equally noticeable, if the numbers of the senses of perception in the distances differ relatively equally, and whether, accordingly, the magnitude of the stimulus in the case of intense sensations can be represented by the number of active senses in extensive ones for our law. But all these experiments give no indication as to the sense of proportion, since, according to the natural mode of use of our eye, they are all carried out under the influence of the motion of the eye, whereby the smaller and larger distances do not depend on the different number of sensory circles which they have contained but by the circumstance that the same point of clearest vision has been passed through a greater or less distance. Indeed, in the case of the density of the distribution of nerves, diminishing from the axis of the retina, one would not even have expected the direct confirmation of the law by our experiments, if they were not carried out with motion. the smaller and larger distances being compared not by the different number of sensory circles which they have conceived, but by the circumstance that the same point of clearest vision has been passed through a greater or less distance. Indeed, in the case of the density of the distribution of nerves, diminishing from the axis of the retina, one would not even have expected the direct confirmation of the law by our experiments, if they were not carried out with motion. the smaller and larger distances being compared not by the different number of sensory circles which they have conceived, but by the circumstance that the same point of clearest vision has been passed through a greater or less distance. Indeed, in the case of the density of the distribution of nerves, diminishing from the axis of the retina, one would not even have expected the direct confirmation of the law by our experiments, if they were not carried out with motion.

From this we might suppose that our affirmation is based more on the muscular sensation of the movement, insofar as it helps to convey the distance estimate, than the number of sensory circles concerned with the estimated distance, and also in this respect if justified, the probation of the law would always be important; but in any case the above basic question remains unsettled, and also the relationship to the feeling of muscle is subject to difficulties, for which I will not go into detail for now.

Now the way to try to answer the question on the skin, whose analogy with the facial organs in relation to the conception of extensive magnitudes of EH Weber has been so well emphasized, and where one has nothing to do with the influence of the movement , Except that here, too, one can not count on a uniform distribution of nerves. In the meantime, it seemed useful to see how the successes in this respect are put on various parts of the skin, and there are corresponding attempts by myself on the forehead, which seems to offer the most favorable field of observation because of its large, smooth surface with a hard surface on the foreleg of the left middle finger and on the back of the hand according to the method of the middle fault. But the consistent result of these experiments is that no even approximate proportionality of the pure errors takes place with the distances, but in general they do not take much more slowly, and even beyond certain limits or at larger intervals, with the distances, so that it is not even possible to think that they are By analogy with what was found in the micrometric eye-size experiments, can be represented by composition of a proportional to the distances and a constant component with respect to the distances. After all, these experiments, though they can not be considered as pertinent to the investigation of our question because of the unevenness of the distribution of nerves, do not leave the slightest probability of the existence of the law in this region, if we seek it in this way.

In the meantime, the new question arises as to whether, in the field of extensive sensations, one really has to seek in this way; which, of course, seems self-evident for the first sight, if an analogous validity of the law is demanded in the domain of extensive as intensive sensations, insofar as it is found in the latter in this way. But one must not overlook the fact that the distances which we determine in the eye and on the skin are merely delimited in the given field of sight and touch, and the growth of this field does not increase thereby, whereas the intense light stimulus is not merely limits in a given Determines intensity, but only generates a previously non-existent intensity of irritation, which sets the conditions differently. In a chapter of the following part, Where, in particular, I return to the extensive sensations with some remarks, I shall return to this point; but the series of experiments on which the above-stated negative result is based, I will communicate in the "measurement methods".

6) Fortune physique et morale.

One can still follow Weber's law into a more general area. The physical goods that we possess (*fortune physique*) have no value and meaning for us as dead masses, but only, if they are external means, to produce in ourselves a sum of valuable sentiments (*fortune morale*); with regard to which they take the place of the charm thereafter. A thaler, in this regard, has much less value for the rich than poor, and when he makes a beggar happy for a day, he is not noticeably sensed by him as a millionaire's fortune. This can be subordinated to Weber's law. To the same increase to what Laplace the *fortune morale*To grant, the increase must stand to the *fortune physique* in the proportions of this *fortune physique* .

This principle is first presented in a paper by Daniel Bernoulli in the Comment. Acad. scient. imp. Petro Polit. TV 1738, which bears the title: "Specimen theoriae novae de mensura sortis." Later it is from Laplace in s. Theory analytique des probabilités p. 187. 432 reproduced and further developed in inferences, and by Poisson in s. Recherches sur la probabilité has been mentioned and accepted with its implications.

The terms fortune physique and fortune morale are not yet used by Bernoulli, but first by Laplace. Bernoulli, after some preliminary discussion, says: "Nempe valor non est aestimandus ex pretio rei, sed ex emolumento, quod unusquisque inde capessit, Pretium ex re ipsa aestimatur omnibusque idem est, emolumentum ex conditione personae, Ita procul dubio pauperis magis refert lucrum facere mille ducatorum, quam divitis, etsi pretium utrique idem sit; " and further (p.177) "Ita vero valde probabile est, lucrulum quodvis semper emolumentum afferre summae bonorum reciprocae proportional." Hereupon he founds p. 181 the differential formula and p. 182 the logarithmic formula, which we later base more generally on Weber's law.

Laplace says (p.187): «On doit distinguer dans le tuen espéré, sa valeur relative, de sa valeur absolute: celle-ci est indépendante of the motif, qui le font désirer, au la que la première croit avec ces motifs. On ne peut donner de regle générale pour apprécier cette valeur relative; cependant il est naturel de supposer la valeur relative d'une somme innniment petite, en raison directe de sa valeur absolue, en raison inverse du bien total de la personne intéressée. En effet, il est clair qu'un franc a très-peu de prix pour celui qui en possède un grand nombre, et that la manière la plus naturelle d'estimer sa valeur relative, est de supposer la raison inverse de ce nombre. «P. 432: "D'après ce principe, x étant la fortune physique d'un individu, i'accroissement dx, qu'elle recoit, produit à l ' individu un bien moral réciproque à cette

fortune; I'accroissement de sa fortune morale peut donc être exprimé par , k étant une constante. Ainsi en décignant par y la fortune morale correspondante à la fortune physique x, on aura

$$y = k \log x + \log h,$$

h étant une constante arbitraire, que l'on déterminera au moyen d'une valeur de y correspondante à une valeur donnée de x. Sur cela, nous observerons, que l'on ne peut jamais supposer x et y nuls ou negatifs, dans l'ordre naturel des choses; car l'homme, qui ne possede rien, regarde sonhood comme un bien inoral, qui peut etre comparé à l'avantage, que ceci procurerait une fortune physique, dont il est bien difficile d'assigner la valeur, mais que l'on ne peut fixer au-dessous de ce, qui lui serait rigoureu-sement nécessaire pour exister; car on contoit, qu'il ne consentirait point à recevoir une somme modique, telle que cent francs, avec la condition de ne prétendre à rien, lorsqu'il l'aurait dépensée. »

Poisson says p. 72: "Comme l'avantage, qu'un gain procure à quelqu'un dépend de l'état de sa fortune, on a distinguished cet avantage relatif, de l'espérance mathématique, et on l'a nommé espérance morale. Lorsqu'il est une quantité infiniment petite, on prend son rapport à la fortune actuelle de la personne, pour la mesure de l'espérance morale, qui peut d'ailleurs être positive ou négative, selon qu'il s'agit d'une augmentation ou d 'une diminution éventuelle de cette fortune. Par le calcul intégral, on déduit ensuite de cette mesure des conséquences, qui s'accordent avec les règles, que la prudence indique sur la maniere, dont chacun doit diriger ses spéculât! ons. "

X. The fact of the threshold. [1)]

A sensation, a difference in sensation, generally grows with the size of the causative stimulus, difference of stimulus, and it seems natural to assume for the first sight that the point from whence the sensation that sensation-difference begins to become noticeable, with the zero-point of the stimulus, Contraction difference coincide. But the fact contradicts this premise; On the contrary, it is shown that every stimulus, such as stimulus-difference, must have reached a certain finite magnitude, before its peculiarity begins only just, ie, before it produces a sensation which noticeably affects our consciousness, or establishes a noticeable sensory difference. Conversely, the peculiarity of the stimulus, the difference in stimulus, disappears earlier than it has come down to zero values. The zero point of the sensation,

[1)] In the matter of p. 7.82 ff. Revision p. 177-180. Psych. Maßprinzipien, p. 196 ff. On the Mixture Threshold (see below p. 330 f.): In the matter of p. 105

f., Revision p. 179f. Psych. Maßprinzipien, p. 204 f.

The point at which the peculiarity of a stimulus or a stimulus difference begins and fades we shall briefly mention the threshold, which expresses as well the sensation and the sensory difference at the boundaries of the notability as the stimulus or stimulus difference, or the stimulus relation, which the sensation or the difference in sensation can be referred to this point, so that we can speak of both the threshold of a sensation or a sensory difference, as the threshold value of a stimulus or stimulus difference or irritation ratio, short stimulus threshold, threshold of difference, ratio threshold of the stimulus. Since, given the sizes of two stimuli, the difference follows their ratio and vice versa, it is generally sufficient to

Insofar as extensive magnitudes and differences of such require a certain value, to be understood by the skin or the eye as expansion or expansion difference, we will be able to transfer the concept of threshold to it, and mean that the threshold relating to intense sensations is the intense one for extensive sensations concerning the extensive call.

Insofar as finally other sensations than other, more general and higher phenomena of consciousness, for. For example, if the overall consciousness of man, depending on sleep and waking, the consciousness of individual thoughts, attention in a given direction, have a point of extinction and emergence, we will be able to generalize the concept and expression of the threshold. In these cases there is no longer any threshold of an external stimulus which causes the raising of consciousness to the threshold or to which it corresponds; but the question arises whether we should not assume for it a threshold value of the underlying psychophysical movement, and whether the stimulus threshold, threshold of difference, ratio threshold, sensations merely exist insofar as they can be translated into such a question, which will be addressed in the beginning to internal psychophysics. For now, however, it will be concerned only with the discussion of purely empirical relations which can be directly established, and that in this chapter I will seek to explain and explain partly the generality of the fact of threshold and threshold, and in part to discuss the implications and applications. which carries the existence of the threshold in the field of experience; but below to special provisions on the thresholds. In this chapter I will try to explain and explain partly the generality of the fact of the threshold and threshold of difference, and in part to discuss the conclusions and applications which the existence of the threshold entails in the field of experience; but below to special provisions on the thresholds. In this chapter I will try to explain and explain partly the generality of the fact of the threshold and threshold of difference, and in part to discuss the conclusions and applications which the existence of the threshold entails in the field of experience; but below to special provisions on the thresholds.

l) The intense threshold.

a) Threshold.

In the realm of intensive sensation of light, the direct proof that it first requires a certain strength of the light-stimulus to arouse sensation, that is, a threshold for the sensation of light at a finite value of the light-stimulus, can not be conducted, because the eye, as repeatedly discussed , through an inner excitement, is always above the threshold, to which every external light stimulus gives only one subsidy. The facts which teach that this subsidy requires a certain strength to be noticed are more in the section on the threshold of difference.

But with regard to the modification, which we call color, the following conditions for visibility can be established: 1) that the refrangibility, and thus the number of vibrations, exceeds a certain limit; 2) that the intensity or amplitude of the vibrations exceeds a certain limit; 3) that the color works in a sufficient extent, which must be the greater, the more the more lateral portions of the retina fall the color; 4) that she was not mixed with too much white.

As for the first, one knows that beyond the red border of the spectrum no colors are seen anymore, or are somehow to be made visible; notwithstanding, heat phenomena prove the existence of rays beyond this limit. Now the red rays have the slowest vibrations, and the inability to perceive ultra-red rays seems to be written on anything other than their vibrations being too slow. On the other hand, the so-called ultra-violet rays, which, when ordinary prisms are used, can not be made visible under ordinary measures, and whose existence was formerly inferred only from their chemical effects, have recently been made visible by appropriate measures, and only necessary is to bring them to perception with sufficient strength,

In fact, the use of rock crystal prisms, which allow the color jets more abundant, than the glass prisms, the ultraviolet part of the prismatic spectrum is still visible, where it is no longer detectable when using glass prisms, especially when Spectrum designed by means of a quartz prism isolated by a screen with gaps and viewed through a telescope made of glass lenses with an attached second glass prism. Proof, however, that the violet rays pass through glass prisms, and are no longer recognized only because they are too weak, is that they can still be visualized in the spectrum produced by glass prisms by the fluorescence discovered by Stokes.

On the other hand, as EH Weber notes [2)], a green space is no longer seen as green through a very narrow gap, and he concludes that a colored area must have a certain perimeter to make its specific color impression. On the other hand, one can assert that some fixed stars are still a little bit colored; but the coloring is very little salient, and it must be borne in mind that the image of the stars, as indeed of the slit, always extends somewhat through irradiation, so that it can not be regarded as a very punctiform one.

[2)] Müller's Arch. 1849. p. 279th

Extensive and careful experimentation with the subject with regard to the behavior of the lateral parts of the retina has been undertaken by Aubert [3] , whose special results, however, are not well placed in this general account.

[3)] Gräfe Arch. F. Ophthalmol. Ill. 38 ff.

As for the fourth, it is always possible to dilute a color liquid to such an extent, or to mix a color substance with so much white that the color becomes imperceptible to the eye. This case will be discussed in more detail in the chapter on mixing phenomena.

With regard to the intensity of the sound, the fact of the threshold is easily ascertainable.

When a sounding body moves away more and more, we finally do not hear it anymore, notwithstanding the sound waves that strike our ear, they have not become zero. The approximation of the sounding body has merely the effect of making noticeable by impression, which is imperceptible because of its weakness, but not because of its absence.

So we do not hear a bell too far away. But if 100 bells, which we do not hear individually, ring together in the same distance, we would hear them. So every single bell in this distance must make its contribution to hearing, which alone is not sufficient to produce a noticeable sensation of sound.

One does not hear a caterpillar in the forest, but when general caterpillar feeding is in the forest, one hears it very well; but the noise that many caterpillars make is only the sum of the sounds of the individual caterpillars. So every single caterpillar, regardless of being heard for itself, must contribute something to the hearing of the entire caterpillars; but this is not strong enough to create noticeable auditory sensation.

At all times of the day a certain noise fills the air, but if it does not exceed a certain strength, we believe we hear nothing.

In homeopathic dilution you will not taste the bitterest substance anymore. It is enough to concentrate the dissolution, and the taste becomes noticeable.

It is undeniable that there are always many odorous substances in the air that we do not smell because they are too dilute. But the dog and the savage really smell the trace with his sharpened organs, which we would no longer smell, but would also smell as it intensified.

A single galvanic plate pair gives no appreciable sensation, while the column consisting of individual plate pairs gives a hit.

Every pressure on our body just needs to be distributed sufficiently to become imperceptible without it being nothing.

b) Difference threshold.

That a difference in stimulus must have a certain size in order to be perceived as a difference is generally not doubted, and the method of just noticeable differences applicable in all sensory areas rests entirely upon it.

However, the existence of the threshold of difference can not be said to be more beautiful, simpler and more striking than in the field of the sensation of light through the attempted shadow, which we cited to prove Weber's law. Let us remember the circumstances of the experiment:

You put two lamps next to each other and in front of them a shadowing body. Each of the two lamps gives a shadow that is merely illuminated by the other lamp, while the surrounding ground is illuminated by both lamps. If you now lower the wick of one lamp more and more, or if you remove it further and further from the shadowing body, you can see the shadow that it casts become weaker as the illumination of the surrounding space becomes less and less different, and finally this one Shadows disappear, as if absorbed by the general enlightenment of the ground, notwithstanding that both light sources are still there. I was astonished when, for the first time, I noticed that two lights were merely shading. Both lamps are burning clearly, but only a shadow is there. In a word, when the difference between the enlightenment of the one shadow and the surrounding space goes below a certain limit, the difference totally disappears for the sensation and can no longer be perceived.

This attempt is particularly striking, because one has the components here in the eye at the same time, and the eye can focus sharply, calmly, and steadily on the borderline of the same while making its difference disappear; Thus, there can be no question either of forgetting the earlier impression, of ignoring the difference, to which one might be inclined, in other types of experiment, to postpone the non-perception or the disappearance of the difference.

The experiment allows for some modifications. General: if one of the shadows is noticeably noticeable, one only needs his lamp a little lower, or the other to screw a little higher, then it becomes imperceptible; and if it is imperceptible, one only needs his lamp to be higher, or the other to be screwed deeper, then it becomes noticeable. Instead of higher and lower screws, the same applies: more approach and removal.

The same as this experiment teaches the experience, already asserted, that we can not see a star in the sky of the heavens with the utmost attention.

Just as common as the fact of the threshold of difference is the fact of its increase with the size of the stimuli. As far as Weber's law is concerned, the size of the difference that is just noticeable, and therefore the threshold of difference, is in direct proportion to the size of the stimuli, the difference of which is to be understood; if it does not exist, there is still a dependency on the size of the stimuli. which is no longer that of simple proportionality.

Insofar as the size of a relative difference in stimulus remains the same, if the ratio of stimuli remains the same and vice versa, then it can be said that the differences of sensation which are just noticeable occur with the same relative difference of stimuli as with the same stimulus, irrespective of the size of the stimuli. But if both are in fact based on the same thing, from a formal point of view it can sometimes be more convenient or more appropriate to stick to one than another. Accordingly, we denote in the future with absolute difference threshold, coincident with just noticeable differences, the absolute stimulus difference, with relative difference threshold or difference constant the relative stimulus difference,α , ω , *v*for that. Thus, the difference χονσταντ ω for the light intensity according to Volkmann's method is the ratio constant .

Generally one has

$$v = 1 + \omega \text{ and } \omega = v - 1.$$

The logarithm of *v will be* used several times in the sequence . Unless now ω in the expression $v = 1 + \omega$ is always a very small size, the higher powers against the first can be ignored, you can according to known mathematical sets for log (1 + ω substitute) $M\omega$, where *M* is the modulus of the logarithmic Systems is, so put

$$\log v = M\omega .$$

It is important to keep in mind that if the relative stimulus difference and the stimulus ratio, and hence the difference constant and ratio constant, are always constant together when the stimuli are changed, then by no means, when one of these values changes, the other becomes proportional changes. On the other hand, the logarithm of the ratio constant according to the above equation grows in proportion to the difference constant, and it can be substituted wherever *v* only for ratios log ω for *v* .

2) Extensive threshold.

If a white circle is too small on black ground, or vice versa, or is viewed too far away, it will not be recognized. If two points or parallel threads are too close to each other, or are viewed too far away, they fade away from the eye, and their distance becomes imperceptible. The boundary where the first occurs may be called the threshold of recognizable magnitude, where the last occurs, as the threshold of discernible distance.

It is well known that two too close circular tips flow together on the skin to a common impression, and so there is also a threshold of recognizable distance here.

No less do two impressions flow away into an indistinguishable impression when they happen too fast after each other. So there is also an extensive threshold in terms of the time distance.

If an object, such as the hour hand of a clock, a star in the sky, moves too slowly, the movement is not recognized, with sufficient acceleration, it is recognized. So there is also a threshold of recognizable speed.

Here time and space come into consideration at the same time. Probably the speed begins to become recognizable when the time threshold coincides with the threshold, ie when in the smallest time that does not flow for the soul at a time, a space is described that does not flow into a point of space for the eye.

3) More general considerations regarding the threshold.

The fact of the threshold is a paradox from the beginning. The stimulus or stimulus difference can be increased to certain limits without being felt; From a certain point of view he will feel and will feel his growth. How can something that does not work in consciousness, when it is weak, begin to work in something through reinforcement? It seems as if summation of null effects could give a bit of the effect. But if this relationship can be difficult for a metaphysician, then from a mathematical point of view it has no difficulty, and this may suggest that the mathematical point of view, according to which the size of the sensation as a function of the size of the stimulus (in consequence of the internal movements induced thereby). It can also be considered the right metaphysical one. In fact, if *y is* a function of *x* , *y* can vanish, go into negative or imaginary at some values of *x* , but it is sufficient to increase *x* beyond that value to *y* to see positive values again.

With the fact of the threshold depends on itself following fact. The lower the size of the stimulus or stimulus difference falls below the threshold, the less the stimulus or stimulus difference can be perceived, the greater it will require before its sensation occurs. As long as the stimulus or stimulus difference remains below the threshold, the sensation of it, as one says, remains unconscious, and the unconsciousness deepens more and more, according to the extent of the magnitude of the stimulus or stimulus difference descending below the threshold. Thus the distant sound, the odor-stimuli in the atmosphere below the threshold, and with it the sensation thus awakened, remain in the unconscious until the intensity of those stimuli exceeds a certain magnitude, the threshold. Of itself offers itself here,

The subtlety of small differences automatically raises a subtle and not unimportant question for the method of measuring sensitivity according to the method of right and wrong cases.

Supposing that the difference of weights or, more generally, of the stimuli taken in the experiment is so small that it falls below the limit where it can be consciously recognized, one wonders whether he is counting on the number of right and wrong ones Cases can ever gain influence; whether it is not as good for the relationship of the two as if there was no difference at all, until the difference has crossed the border where it can be felt as such, and whether from that time on the influence, not the post the absolute magnitude of the difference, according to the difference of which from the value where it really begins to be felt.

This seems obvious at first, for how can a difference not affecting our consciousness determine our judgment? Nevertheless, it can not be accepted without invalidating, with the principles of the method of measurement in question, the principles by which the probability of errors with respect to their size is calculated, and on which that method of measurement is wholly founded; a more detailed examination also leads to a completely self-evident, completely opposite result. In spite of the fact that a difference is insignificant in itself, it will, in a sufficient number of comparisons, find an excess of real cases in favor of the heavier weight, generally the greater stimulus.

It must be borne in mind, however, that at the same time as the difference which it is to be considered, there are accidental influences which, with equal weights, would determine the judgment on average as often in favor of one as the other. The difference, however, adds itself to the influences which determine the judgment in favor of the one; and, in part, that such influences, which without that would have been imperceptible, become noticeably in favor of this side, partly he intensifies the already noticeable influences to this side, and makes the less so than the opposite influences. This does not prevent the fact that in many cases the influence of the additional weight, together with the disturbances which are present, remains under the notice, in which case the judgment remains ambiguous.

One sees in this way, as an intrinsically imperceptible difference in that it adds up with other influences, but can give noticeable effects; and the probability, ie, proportionate number of correct and false cases in a very large number of trials, depends on the size of the difference in a way that allows one to derive a measure of the sensitivity from it, as shown earlier.

The threshold of both stimuli and stimulus differences is capable of fatigue, habituation, exercise, internal causes of excitement or paralysis, remedies, the periodicity of life, individual constitution, etc., of the greatest and most varied modifications, that is, to be considered as constant only. as these relationships produce no change in it. The investigation of these relations of dependence belongs to the most important tasks of psychophysics, and coincides with the general investigation of the relations of dependence of absolute and difference sensitivity or irritability and excitability, in that the absolute sensitivity of the stimulus threshold, the difference sensitivity of the threshold of difference, is reciprocal.

If the fact of the threshold can be transferred from the stimulus to the psychophysical movement thus triggered, which is later tried to prove, then, according to our general assumption that there is a fixed relationship between bodily and psychic changes in us, the threshold of the psychophysical Activity which corresponds to the beginning of a certain sensation, must be regarded as immutable, so that the sensation certainly begins when the activity to which it is attached reaches the threshold level. But since, depending on the changing state of the organism, a stimulus may find it easier or harder to induce psychophysical activity in this magnitude, the threshold value of the stimulus, which is linked to this point, is not invariable.

This distinction as to whether we refer the threshold at which the sensation disappears to the stimulus or the movements induced thereby must be kept in mind; in that only the threshold in the latter sense can actually be constant, while in the first sense it changes with the stimulus-susceptibility and manner of attachment of the stimulus.

4) Consequences of the existence of the threshold.

Many consequences of interest and importance are linked to the existence of a stimulus threshold and threshold of difference.

If any smallest stimulus is ever recognized, we should, since minima of stimuli of all kinds always play around us, feel an infinite mixture and an incessant change of quiet sensations of all kinds, which is not the case. That every stimulus must first exceed a certain limit before it awakens sensation, assures man of undisturbed state to a certain extent by external stimuli. He need not reduce the charms to zero, which he is not able to remain undisturbed by, but to withdraw from those who are weakened by the distance, only to a sufficient distance, or generally bring them down to a certain limit ,

Just as the imperceptibility of every stimulus, when it falls below a certain limit, secures a state undisturbed by strange perceptions, so the imperceptibility of each stimulus difference, when it falls below a certain limit, gives us a uniform state of perception.

Because of internal and external causes, stimuli will never act uniformly through time and space, but this does not prevent us from seeing light and colored surfaces at the same time, hearing uniformly sustained sounds, and so forth

The well-known experiment of the rotated disk with white and black sectors gives a simple proof. Turned with enough speed, it seems uniformly gray. However, the intensity of the impression on both edges of a sector can not really be the same because in passing a black sector is progressively lost from impression and won in advance of a white one. But as the difference at both edges becomes smaller than the difference threshold, the appearance of uniform gray occurs. Namely, the uniformity appears to be perfect at a sufficiently rapid rotation, so that it is not possible with keenest attention to discover a change.

An analogous case occurs when one holds the finger on the edge of a rapidly turned gearwheel (spur gear). However, if one differentiates the individual teeth with slow rotation, this is no longer the case with faster ones. Valentin [4)] has made extensive experiments on this. U. a. he observes that if the widths of the teeth present only insignificant variations, there is no substantial disturbance, whereas if in a wheel of 160 teeth 3 or 5 are three or four times as narrow as the rest, the Uniformity can no longer be fully achieved even at high speeds.

[4)] Fourord's Arch. 1852. p. 438. 587.

Just as a disk with white and black sectors appears uniformly gray on rotation with sufficient speed, a surface of regularly alternating white and black squares appears uniformly gray from a sufficient distance. This can have a double reason; either that distances can not be understood any more from too small a point of view, then the appearance would depend on the extensive threshold; or that the irradiation of the white squares at a very small angle of view allows them to flow into one another; then the appearance would be attached to the intense threshold of difference. Maybe both causes can work together; nothing seems to have been decided by the observations so far.

Let us throw away from these facts of external psychophysics a glimpse of the meaning they can gain for the inner. If the stimulus is translatable into psychophysical movement, then the soul, in spite of a presence and play of psychophysical movement, will be able to move in a senseless and uniform state, if only certain limits are not exceeded. The first case, as I show in the future, is realized by sleep, the second by the fact that psychophysical movements can not be uniform in nature. They are probably oscillatory in nature. But the changes of the psychophysical movement are not felt unless they exceed a certain limit;

This also makes it easier for us to grasp an idea of what a different quality of sensation ties in with. While the non-uniformity of the psychophysical movement is not perceived as a non-uniformity of sensation, its quality may depend on its mode. However, an execution of these hints does not belong here; and also in the inner psychophysics can be addressed for now only with great measure and support.

The fact that the eye, in view of the intense sensation of light, is always above the threshold by virtue of a weak internal excitement, gives rise to a special teleological remark.

If it takes a certain amount of external light-stimulus to lift the inner movements to which our sensation of light is attached to the threshold, weakly illuminated and black objects would not be seen at all, and here the effect of the blind spot of the retina would arise , which would undoubtedly be very annoying. If, on the other hand, the eye were raised far above the threshold by inner excitement, then, according to Weber's law, slight increases of light would no longer be clearly recognized. The black in our outwardly unruly eye, so far as it represents a very weak degree of light, is undoubtedly the most advantageous thing that could take place in the establishment of our sense of sight.

There is no corresponding teleological motif for the ear; rather, it may seem rather annoying here, if every smallest noise should be heard. In fact, even if we direct our attention to the ear, in the normal state we have nothing analogous to black-eye, but only the feeling of silence.

But as the ear may be abnormally elevated above the threshold by internal irritation, and then, when showering, blasting, etc., occurs, it may also have sunk deep below the threshold by its inanity. Here belong the experiences which now find their true interpretation, that persons who suffer from torpor of the auditory nerve only understand the speakers with a sound, as drumming, driving in the carriage. Evidently the strong sound must serve to raise the ear to the threshold, after which also the growth of the sound, which by itself would not have been sufficient to cause this, can be heard.

Nor do I want to commemorate next applications of the fact of the threshold of difference.

If, as is now quite generally accepted, the so-called irradiation in the eye depends rather on the optical aberrations of the eye and diffraction phenomena than on the retina assumed the effect of light on the retina, this physical irradiation with increased intensity of light can not Expansion, but only increase in intensity. But Plateau's experiments have [5] demonstrated that perceived by the eye irradiation not to a certain maximum can not be exceeded by far grows in proportion to the intensity of light, but not insignificant with this intensity.

[5] Pogg. Ann. L. Supplement. P. 412 ff.

For, according to his experiments, the following light intensities *i* correspond to the following visible irradiation widths *j* on a black ground; where the maximum *i* = 16 was the intensity of a bright sky mirrored by a mirror below 30 ° (against the mirror surface):

i = 1 2 4 8 16

j = 40 ", 9 47", 6 55 ", 7 56", 0 56 ", 0.

With regard to the threshold of difference, the result found by Plateau is that the visible irradiation grows in extension with the intensity of the light, but in lesser proportions, and not beyond a certain limit, as a necessary constant for a physically constant radius of radiation.

The furthest possible limit of visible irradiation must necessarily be at the limit of the physical. But in the case of the weak intensity of the irradiating light, when in the immediate vicinity of the irradiating edge it must have become so close to the black of the ground that it can no longer be distinguished from it, the limit of visible irradiation must come nearer to the irradiating edge, the weaker the irradiating light is.

In a treatise on the tightness of the comet mass, Babinet [6] points out that astronomers of the 10th, the 11th magnitude, and even below, have been observed by the reliable, designated astronomers, through the comet nucleus, without noticeable weakening of their stars Splendor, whereas, according to Valz's observation, a star of magnitude 7 almost extinguished the brilliance of a brilliant comet. Afterwards he makes the following consideration with reference to the Bouguer's difference constant:

«Puisque l'interposition d'une comète éclairée par le soleil n'affaiblit pas sensiblement l'éclat de l 'étoile devant laquelle elle forme un rideau lumineux, il s'ensuit que l'éclat de la comète n'est pas le soixantième de celui de l 'étoile, car authority l'interposition d'une lumière égale à un soixantième de celle de l' étoile eût été sensitive. On peut donc admettre tout au plus, que la comète égalait en éclat le soixantième de l'lumière de l'étoile. Ainsi, dans cette hypothèse, rendant la comète soixante fois plus lumineuse, elle aurait eu un éclat égal à celle de l'étoile, et si on l'eût rendue soixante fois plus lumineuse qu'elle n'était, c'est- à-dire trois mille six cents fois, la comète eût été alors soixante fois plus lumineuse que l'étoile, et, à son tour, elle eût fait disparaître l'étoile par la supériorité de son éclat »....« On peut admettre que le clair de lune fait disparaître toutes les étoiles au-dessous de la quatrième grandeur; ainsi l'atmosphère illuminée par la pleine lune acquires assez d'éclat pour rendre invisibles les étoiles de cinquième grandeur et au-lingerie. »

[6] *Compt. rend.* 1857. *p.* 357th

Babinet makes further observations of this, whereby he finds a really small size for the density and mass of the comets, which, however, from other considerations is to be assumed to a lesser extent, which, however, do not concern us here any further. Also, I give the above consideration only as an example of possible application of the difference constant, but consider the application of Bouguer's value to stars not permitted, for reasons which I will discuss in the following chapter; whereupon the whole calculation result Babinet's becomes precarious.

XI. Details on the size and dependence of the thresholds in the various sensory areas.

Absolutely firm and generally valid provisions on the size of the threshold and threshold of difference are not possible in any sensory area, as long as the threshold depends very much on the external mode of attachment of the stimuli and the state of the sensitivity of the organs, which are very changeable elements, to which the difficulty arises. accurately determine the value at which a sensation or sensory difference begins. In the meantime, what has been said in this respect (see Chapter 6) concerning the measures of sensitivity in general applies. The determination, even if only approximate, of mean values for ordinary conditions on the one hand, extreme values on the other hand, always retains its interest, and in many cases can not be dispensed with.

The lower the threshold, the greater the sensitivity under otherwise similar circumstances. It is undisputed that, after the establishment of the human organism, there is a limit which can not be exceeded in this respect; whereas many circumstances, some abnormalities of constitution, organs, contingencies of all kinds, can raise the threshold; Therefore, all the thresholds actually obtained are to be regarded as upper limits, below which, so to speak, is the ideal threshold, which would be found under the most favorable circumstances. The smallest thresholds, insofar as they are based only on good observation, therefore have the most interest in bringing the upper limit of the true limit closest.

It is common ground that the following does not provide a complete summary of what is available in various areas of threshold information; however, the following information will provide a starting point for further completion. However, most of these data will only be able to refer to the difference threshold, since there is currently little available about the absolute stimulus threshold.

1) Intensive threshold.

a) light and color.

That a stimulus threshold with regard to the sensations of brightness can not be included in the experiment has been discussed earlier (Chapter 10). With regard to the difference threshold, the previous information in the 9th chapter is communicated and the following is the summary.

Bouguer found by experiments with shadow, questionable whether with or without agitation, the difference threshold is equal to $^1/_{64}$ of the intensity; Arago without movement in different individuals $^1/_{39}$ bis $^1/_{71}$, with movement of $^1/_{58}$ bis $^1/_{131}$ (see chapter 9..); Volkmann by experiments with shadows in different individuals with agitation about $^1/_{100}$ (see chapter 9..); Masson by experiments with the rotated disk at different individuals $^1/_{50}$ bis $^1/_{120}$ and over it (see chapter 9).

According to Masson, the value for different colors remains the same, but is different for the eyes of different individuals.

In the experiments by which the above provisions have been obtained, light or shadow areas of a certain extent and direct vision have been used everywhere. But it is certain that the threshold of difference depends, at least to a certain extent, on the extent of the visible quantities, and behaves differently on the lateral parts of the retina than on the central.

In general, a small black area on a white ground, or vice versa, disappears more easily in the ground, that is, is not distinguished from it, as it is seen from a smaller angle of view, and meets the laterer parts of the retina. Lines are still recognized at the same thickness with points where they are no longer recognized. Also, the color makes a difference.

As far as the influence of size is concerned, irradiation must be such as to cause objects of very small dimensions, with the same distance of the eye, to disappear more easily than larger ones, which is not always considered necessary. It should be noted that a black line, or a black point on a white ground, spreads so well by irradiation, with diminution of the black, as a white on a black ground with diminution of the brightness; of which the fact and theory has been more accurately stated and developed by Volkmann [1)] .

[1)] Reports of Leipz, Soc. 1858, p. 129 ff.

In fact, of course, light that dissipates through the irradiation is thus diluted, or black, which is thereby overrun with light; less easily distinguishable from black or white ground; and this circumstance must concern points in stronger proportion than lines. It is therefore not disputed that the threshold of difference of the fixed stars is very much greater than the Bouguerian value, which Babinet based on a calculation; ie a fixed star is already at a much greater intensity differences

than $^1/_{64}$ can not be distinguished them against the sky reason, and it would be important for some astronomical proportions most to him directly by experiments on artificial stars
determine [2],

[2] It will be useful to refer to Stampferí's related-task attempts in the session. d. Vienna. Akad. 1852. p. 504. 511 consideration.

The foregoing suffices to show that intense and extensive thresholds of sensation of light can only be determined with reference to each other. For this reason, I leave this subject for the present, in order to come back to it under 2), considering the extensive threshold, and then to further discuss the influence of the irradiation.

Thus it has been remarked that colors too, in order to be recognized as colored, must be presented to the eye to a certain extent. Even with direct vision, it is the case; even more with indirect. It is not disputed that irradiation conditions and induction conditions (in the sense of the bridge) play a role in the disappearance of color against small colored surfaces, but this is still completely unexplained. Aubert has made the most careful observations of the facts [3] , but, to draw more definite conclusions, his observations on the behavior of colored squares on black and white ground in the lateral parts of the field of vision would be analogous to the behavior of white and black squares on a colored background to be added yet.

[3] Gräfeís Arch. F. Ophthalmol. III. P. 38 ff.

b) Sound and pitch.

Schafhäutl [4] set about the limit of audibility of the sound when using the same device by dropping a bead [5]from a measured height to a right angle plate of ordinary mirror glass fixed to its node by screws and fixed against the record fixed ear position. The horizontal distance of the center of the disc, where the bullet hit, from the center of the opening of the ear, which was intended to hear the sound, was 55 mill., The vertical 74 mill., The linear 91 mill. "Experience me the author says that this is the best distance in which the ear certainly hears the slightest sound which it is yet able to affect. " The essential result of these experiments (not described in detail) is given by the author as follows:

"In my attempts to determine the sound quantity which is still audible to my ear, I have found that the sound of a 1-milligram cork-ball drops 1 millimeter in height, still averagely for my ear in complete rest, that is, at night In 30 experiments of this kind, at 12 o'clock in the morning, with complete calm, I have with full determination heard the sound produced by the above experiment 25 times, a similar relationship also occurred in some musically educated ears of younger people few who could still hear this sound if they had not practiced their ear, but some, after several exercises, succeeded in hearing the above sound with certainty. "

[4] Abhandl. d. Munich. Acad. VII. P. 501.

[5] Until it falls, it is grasped by tweezers, which are opened by means of two pushers.

"Therefore, I do not expect the sound size, caused by the fall of a 4-milligram cork bead from 1 millimeter height, to assume as acoustic dynamis, which denotes the average limit of sound parameters still audible to the healthy human ear under the influence of our civilization."

It would be undisputed that experiments with more significant sound parameters would be desirable with greater distance from the ear, since, of course, small disturbing influences and measurement errors would lose their influence. It should also be borne in mind that according to the circumstances of the previous experiments, hearing was noticeably only possible with one ear, whereas we generally use both ears for hearing.

According to the experiments of Renz and Wolf cited above (see Chapter 9) and of Volkmann, there is a much lower sensitivity for differences in the sound intensity than for differences in light intensities, provided sound levels that are approximately 3 : 4 still apply certainly different, but more uncertain as they come closer.

As far as pitch is concerned, it is generally accepted that there is a lower limit to the absolute audibility of sounds, and it is usually assumed to be 30 vibrations (Chladni) or 32 vibrations (Biot) in seconds. Meanwhile, would go to the more recent experiments of Savart [6] a tone still be audible with the rod Irene corresponding to 14 to 16 cycles per second, and he is inclined to believe that it arriving just waiting to extend necessary, the individual impressions to to make even deeper sounds audible, so that there is no real limit in it. However, Despretz contradicts [7]who has diligently repeated Savarti's experiments with certainty, and concludes: "that at present it is not proved that the human ear can hear and determine sounds of less than 32 simple vibrations." Savart was probably led astray by the great intensity of the tone of his apparatus, which in fact gave very strong notes, no longer musical or determinable according to their height, which would therefore rather have the character of the noises.

[6] *Ann, de Chim. et de Phys.* XLVII. p. 69 or Pogg. Ann. XXII. P. 596.

[7] *Compt. rend,* XX . p. 1214; Pogg. Ann. LXV . p. 440th

In fact, if Despretz is right, then it was the noise that every single stroke of the staff siren gave to itself, because of the *continuous* duration of the beats *in continuo* , which gave the illusion of a sound.

As it may be with the difference between Savart and Despretz, it would be absurd not to accept any lower limit of the tones for a human ear. Of course, a sound produced by vibrations lasting one hour could no longer be heard by humans as sound. Maybe from differently organized beings, but certainly not from humans.

The audibility of sounds seems to have not only a lower, but also an upper limit.

Sauveur in the *Mém. de iíAcad. Ann.* 1700 sets the upper limit at 12400 pph in the second. Wollaston believes the voice of the bat and the field home formed the limit of the highest perceptible tones. From the lowest tones of the organ to the highest of the insects, the vibrations would be 600 to 700 times faster, which would bring the upper limit to 19,000 to 22,000 simple oscillations. Biot even accepts only 8192, Chladni 12000, Olivier [8] 16000, Young 18000 to 20000 as the upper limit.

[8] Urstoff of the m. Spr. P. 12.

Savart, meanwhile, found that if one produced only the high notes of sufficient strength, for which a toothed wheel served him, whose teeth touched a thin body, nor notes which corresponded to 48,000 simple vibrations (= 24,000 beats), and Despretz draws from his experiments with small tuning forks the result that the ear can still hear, determine, classify sounds (up to 73,000 vibrations) *(entendre, apprécier, classer)* , "but that the hearing of very high notes does not happen so rapidly that one could introduce them to the musical scale. "

After all, it can still be questioned whether the limit of the audibility of high notes has already been reached, and that even with greater amplification even higher tones would be audible. On the other hand, it is very possible that either the nerves themselves are incapable of hearing too high sounds, or the eardrum with its annexes unable to pick them up.

The above concerned the absolute audibility of sounds. As far as the differentiation of pitches is concerned, the sensitivity for it seems to be greater without comparison than for the differentiation of sound intensities.

A. Seebeck [9] was able to notice two tuning forks, which were almost exactly in harmony, so that the one 1209, the other made 1210 oscillations per second (determined by means of the collisions with simultaneous sounding) [10]that one was one track lower than the other. "This small interval has not yet been distinguished from total harmony," says Seebeck. "It need not be remembered that this distinction demands a well-practiced ear, but although I have reason to believe that my ears are quite sharp in that direction, I can not doubt that the ear of a tuner, a violinist, and so on, can go even further in. Two excellent violinists, to whom I presented just these two forks, were not in the least doubtful, which of them was the higher In this case, the two tones were the same in sound, may well be favorable for a more precise distinction of their height, and perhaps the same sharpness can not be achieved at all heights. "

[9] Pogg. Ann. LXVIII. P. 463.

[10] It is undeniable that he has sounded them one after the other, although this is not expressly stated.

Earlier information about the sensitivity of the ear to differences in tone is far from that high. W. Weber 11) occasionally observes that under favorable circumstances the ear is able to determine the sounds so accurately (ie, without the help of shocks and without averages) that the error on 200 vibrations is never more than 1 vibration.

[11] Pogg. XIV. P. 398.

Delezenne [12] has not merely determined the just noticeable deviation from the purity of harmony, as is the case with the previous provisions; but also of other intervals, as octave, fifth, major third, great sixth. It may be remarked that this means not determining the just noticeable deviation of one sound from another, but a sound difference or pitch from another; in that every pure interval between two tones struck one after the other represents a difference, and the impure one represents a somewhat different difference. But the case in which one determines the just noticeable deviation from the purity of the harmony, may be regarded as a special case of the general case, namely, namely, where one determines the deviation from a zero difference of two tones.

[12] *Recueil des travaux de la soc. de Litte.* 1827. *p.* 4th

The experiments were done this way. One on a monochord (sonometre) stretched over two bridges string whose length between the bars was exactly 1147 millimeters, and made the 120 oscillations per second, was divided in a point of their length by a subordinate movable bridge, that both parts of the String through their tones one of the above sound intervals gave. The movable bridge was sharpened: it was put under the string so that it did not increase its tension, and pressed against it by another sharp edge. Delezenne first assured herself of the purity of the sound interval. Then the movable bridge was moved a little, up to 1 or a few millimeters to the right or left, and judged by the observer when a deviation from the purity of the interval became noticeable; other times too,

Although these experiments seem to be done with great care and diligence, unfortunately there is no such thing as an exact method, so that one can not give too much confidence to the comparability of the numbers found. It is therefore to be hoped that these determinations, which are equally important for the musical exercise as for the theory of musical sensations, will be based on the method of right and wrong cases, partly on the average error, with each time precisely maintaining comparability, with different persons of bad and good obedience, since the method used by the author of just noticeable differences or the marginal errors one commits can not afford a sufficiently sharp result.

The following is the result of Delezenneí's experiments.

If, with a string of 1147 millimeters in length, which gave 120 vibrations per second, a bridge inferior in the middle was a little crazy, disturbing the harmony of the two parts of the strings, then very subtle ears were required to make a difference To perceive the successive tones of both parts, when the bridge was only 1 millimeter from the center, which was one part of the string, that is , the other mill., and therefore the ratio of their lengths and at the same time their numbers of vibrations . In one circumstance the difference was recognized even by untrained ears.

"I am on the place of the mobile home of the millimètres à droite ou à gauche, the différence devient sensible aux oreilles les moins exercées, ainsi que je m'en suis assuré sur plusieurs personnes. Si le déplacement du chevalet n'est que d'un millimètre, il faut avoir l'oreille assez délicate pour s'en apercevoir immédiatement. La personne soumise à cette épreuve ferme les yeux, soit pour n'être pas distraite par les objets environnants, soit pour ignorer les déplacements finés ou réels du chevalet et éviter ainsi de se prévenir dans le sens du changement qu'elle verrait opérer. Une oreille trèsdélicate est donc sensitive à cette légère différence. Admettons que ce soit la limite extrême de la sensibilité de l'oreille humaine, et calculons les rapports entre ces deux sens si peu différents. Nous verrons

l'oreille la mieux organisée est donc sensible à une différence de 4 vibrations sur 1149 !! «

«Pour comparer cet intervals à celui réprésenté par le comma , et que nous prendrons partout pour unité, nous dirons, que l'oreille est à peine sensible à un quart de comma, sur l'unisson.«

»Nous avons vu, qu'an déplacement de 2 millim. était sensible aux personnes qui n'avaient jamais essayé de comparer des sons. ó Nous trouvons, pour sons ainsi comparés, l'intervalle

Ces personnes là sont donc sensible à une différence de 3 vibrations sur 1151, ou à un intervals et pé supérieur au demmi-comma. «

According to Delezenne, if we add the corresponding definite results for the other intervals, a very sensitive ear may just as well distinguish a deviation from the following intervals, if the ratio of the numbers of vibrations is the oncoming one, and the tones are heard after each other.

harmony

Octave [13]

Quinte

big third

great sexes [14]

or

As you can see, the deviation from the fifth is felt most clearly.

[13] For persons who were quite untrained in the comparison of tones,

[14] was found to move to the right or left, depending on the way in which the bridge was displaced.

c) Weights.

With the smallest possible absolute weights, the pressure of which is still felt on different parts of the skin, Kammler has set up a series of tests in connection with a few employees (Aubert, Förster, Trenkle), and the results in s. Dissertation: Experimenta de variarum cutis regionum minima pondera sentiendi virtute. Vratislaviae published in 1858. The experimental method was that light weights of elderflower, cork, card paper, 9 millimeters in size, and various weights, enlarged by circumstances by a support, were lowered very slowly and as perpendicularly as possible to the part to be tested, for whatever purpose fine bow-shaped brass wire or a pig's bristle was attached to two diagonally lying corners so that the weight would take the form of a stirrup,

The special message of all the results would go too far here, since the whole surface of the human body of various observers is through experiments. I notice only the following: The order of the sensitivity of the parts has nothing in common with that which Weber has set up after his experiments with compass points over the sense of touch. It was close to the 4 observers, but not completely consistent. The most sensitive parts included forehead and temple, eyelids, dorsal side of the forearm, where mostly 0.002 grams were felt; the fingers were generally much less sensitive.

The following is the special information for the most sensitive parts, where the lightest weights were just felt so.

0.002 grams were felt from Aubert: forehead, temples, right and left forearms with the joints on both sides on the volar and dorsal side, outer parts of the metacarpal of the thumb and back of both hands. ó From Kammler: forehead, temples, dorsal aspect of the right forearm, back of both hands. From ranger to forehead, temples, upper and lower eyelids, nose. ó From Trenkle: nose, lips.

0.003 grams, from Aubert on: outer parts of the metacarpal of the right thumb. From Kammler: on: Volar part of both forearms and dorsal parts of the left forearm; outer parts of the metacarpal of the left thumb.

0.04 grams, from Kammler: outer parts of the metacarpal of the right thumb.

0,05 grammes, from Aubert to: nose, lips, chin, lower and upper eyelid, middle of the abdomen, etc. ó From Kammler to: nose, lips, chin, lower and upper eyelid, middle of the abdomen, etc. ó From forest ranger to: lips , Belly, etc. From Trenkle: forehead, lips, lower and upper eyelids, abdomen, forearms, etc

The heaviest weight, which was just felt, is 1 gram on the nails of the fingers and (at Aubert) the right heel.

With regard to the differences in weight, the results have already been communicated (see Chapter 9), which EH Weber occasionally obtained on the basis of Weber's law. His essay[15)], however, contains even more detailed experiments on the smallest discernible difference in weight depending on the relation of the mere sensation of pressure or the sensation of pressure and muscle in connection, and on the difference of the parts to which the pressure is expressed.

[15)] *Programmata collecta* p. 81 sq.

In the following experiments, the two weights to be weighed against each other were on the two different hands, and their difference was determined comparatively according to the method given (see Chapter 9): a) by the mere feeling of pressure, while the hands were resting on the table rested; b) by the combined feeling of pressure and muscle, by raising hands. While each time 32 ounces of weight were assumed on each hand, the difference was noticeable when the weight on one hand was reduced by the following sizes:

a

bóóóó

1) businessman, untrained. 6 1

2) scholar, mathematician. , 6

2.5

3) EH Weber himself. , , , 16

2

4) Businessman,

untrained 8 4

5) Virgo 16 2
6) Woman 16 4
7) Woman ....

12 2

8) Student 8 3
9) Student 12 2
10) Student ....

8 1.5

11) 15 1.5
12) 10 1.5
13) 18 8
14) 12 6
15) 6 4
16) 8 1
17) 8 4

óóóóóóóóó
means 10,88 2,93

In the following experiments, 16 the observer alternately weighed two weights with the same hand, which hung in two folded cloths, the united corners of which were embraced together by the hand. "Of 10 persons, half male, comparing 78 and 80 ounces in the manner described in weights for lifting the weights, were only two who could not distinguish the heavier weight from the lighter, 7 of them determined at 3 Each of them made 4 to 7 attempts and in each case determined the weight correctly One of the 10 observers determined seven times correctly and 8 times in 8 of them one wrong time. "

[16)] Tasts. and public. p. 546th

Weber thinks that in this experiment alone muscular feeling comes into consideration, in which I would not quite agree with it after the comment (see Chapter 9).

In the following experiments [17)] was placed on the eponymous parts of both sides of the body (the last two parts on the median line) the constant weight of 6 piled on each other Speziestalern, each weighed a little less than 2 ounces, so that the total weight on each side was near 12 ounces. One by one these were taken away on one side, until the difference in weight was palpable. The following table (p.96) gives the number of specie thalers that had to be taken away so that the difference became noticeable (the subject of the experiments is not designated).

Volar surface of the fingers 1
Volar surface of the hand [18)] .... 2 Dorsal

surface of the fingers 2
Inner arm surface .... 4 Foot
sole in capitulis metatarsi. 1
concave part of the sole of the foot. , 4
heel of the foot 3

[17] *Progr.* Coll. P. 96.
[18] Superficies volaris metacarpi manus.

Gastrocnemii 4
side parts of the forehead l
hairy part of the back of the head 4
front part of the chest 4
shoulder blade 2
side parts of the abdomen ... 1
median line of the back of the
shoulder blade .... , , 5
Median line of the abdomen ... 5

With these experiments one can still relate those according to the method of equivalents, which are mentioned in Chapter 12.

d) temperature .

On the magnitude of the just noticeable differences in temperature, EH Weber [19] has given some information, according to which the method of alternating hand-immersion in two vessels with unevenly warm water, with great attention, can detect with the whole hand the difference of two temperatures which only $^1/_5$ bis $^1/_6$ degrees is R.; but he has not determined the temperatures at which these differences are noticeable. I have found that even smaller differences can be seen in medium temperatures, and that they vary greatly according to the temperature. Compare this with what has been reported in the ninth chapter, pp. 202 ff.

[19] The sense of touch and the common sense, Wagnerís Wört. P. 534.

On the degrees of heat and cold, which are able to cause pain, the experiments and discussions of EH Weber are to be found in the same paper, p. 571 ff.

2) Extensive threshold.

a) sense of sight.

In fact, all expansions that we perceive on the retina are delimited in the general field of vision, and one may raise the question of what number of sensory circles would be to produce a field of appreciable extension of the sensation at all, a question that is well from which one must distinguish: what fraction of the number once exists belongs to one part of the general field of vision as distinct from the rest, when it is excited in different ways from the rest. There is no way of deciding that question, and so I abstract here from it, notwithstanding that it is actually the fundamental question of an extensive threshold, to return to it only with some in a later chapter from a theoretical point of view.

What are the smallest sizes, distances, size and distance differences still discernible with the eye?

The task of determining the smallest still recognizable distance actually coincides with the determination of the smallest still recognizable quantity, provided that the diameter of any still recognizable quantity at the same time as a still recognizable distance between its limit points, and vice versa can still regard recognizable distance as a still recognizable quantity. However, the experiments are divided into those in which a point, a line, a thread, a small surface on a broad, uniform basis was observed and watched, at which distances of the eyes, and thus at which angle of view, this small size was still recognizable or disappeared, and in those where two or more distant points, lines, threads, small areas were considered on a given ground, and watched at which point of view of their mutual distance uniform melting occurred. The former experiments may here be referred to as the smallest identifiable quantities, the latter as such over the smallest recognizable distances. The experimental conditions are different in so far as the former involves the considerably radiant participation of the success of the experiments, only of two limits, and of the latter four boundaries.

Any quantity which is yet to be recognized by the face of vision will in general appear on a certain basis, and thus be recognized only in accordance with the difference of the reason that the question of the extensive threshold for the face is connected with the question of the intense threshold of difference, and already mentioned in this regard. A visible variable is the more easily distinguished with the same extension, the greater the relative difference in light, on the other hand (the same relative difference, the larger it is, up to certain limits). If the reason is black and the area to be distinguished is white or vice versa, then this will remain valid.

Twining [20] has deliberately directed experiments on the determination of a legal relationship in this respect by certain in which distances an illuminating lamp black round to regularly appear minor stains on a white ground, which received its light only from this lamp, as separately when the eye was brought to a different distance from it, it led to the law that as the distances of the eye decrease in geometrical progression, the associated lamp distances grow in arithmetic progression [21] .

[20] Twining, Inquiries concerning Stellar Occultations by the Moon and the Planets, o Experiments on Light and Magnitude in Relation to Vision, in American J. of sc. 1858. July, VC XXVI. [2]. p. 15th

21) The author himself expresses the result of these experiments (p.

In the words of "the distance of the light is a logarithm of the linear magnifying effect." "One remarkable result of this law is that of a small fractional change of a faint light possesses a great efficacy to balance a given magnifying effect as a large fractional change of a much brighter light."

If one sets the illumination intensity *J* the squares of the distances lamp *L,* and the apparent diameter *D* of the black spots eyes intervals *A* reciprocal, it can be

for *L* substitute and for *A* substitute ; according to which the expression of the law translates into equal relations being equal differences . Such a law is not in itself probable, and the assumption made by Twining that *A be* reciprocal with *D* It is undisputed that it is not admissible because of the influence of irradiation, which is impossible to ignore in the circumstances of these experiments, and which will be mentioned shortly. Regardless of the author's attempts, as can be seen below, to agree very well with the given law, it is probably only an empirical expression whose universality under other experimental circumstances can still be doubted as a real law of nature. In the meantime, these experiments are not without interest, as long as it is clear that the intensities of illumination at which the clear cognition of the spots begins or ceases to increase in very strong proportions, if a large eye relief is increased in given proportions, but in a small proportion, when a small eye relief is increased in the same proportions. Thus correspond to the two largest eye distances 107,29 and 134,11 Engl. Customs applied by the author whose ratio 4**:** 5 is, the lamp spacings 29.5 and 15.5 Engl. Inches, ie a ratio of illumination intensities of 1 **:** 3.62, but the two smallest eye distances which he applied, 28.12 and 35.16 inches, whose ratio is also 4 **:** 5, the lamp spacings 131.6 and 110.5, ie a ratio of illumination intensities 1 **:** 1.419. And here you will always find a general basis.

The essence of the apparatus employed by the author consists of a box, blackened inside out, by the way, everywhere sealed, but at its front has a square opening, through which light enters from one side, while looking in from the other side the illuminating lamp and the eye are only so far laterally (on the opposite side of the opening), so as not to hinder themselves in the illumination and the eyes. On the back wall inside the box is a piece of paper with small round black spots equidistant from each other [22]*which receives the illumination and is seen on. While the eye is now housed in different experiments in different distances from the rear wall of the box, the lamp is shifted each time moved closer or further away until the black spots to be clearly start but, or the clear separation of the same just stops* [23]*, The lamp was shrouded to the aperture required to exit the light, and the eye looked through a 0.16 inch circular aperture and 3 inches long eye-tube. Tube and the air were displaced on graduated long boards or battens, which converged at a small angle to the box, and indeed was the board on which the eyes tube was moved to a geometric progression with the exponent* $^5/_4$ *divided. In front of the square opening in the box,* *there was a blackened screen with an opening corresponding to that opening, to keep out scattered room light.*

> [22] *"A regular paper with small black round spots equidistant and regularly arraoged." About size and distance of the spots from each other is not specified.*
>
> [23] *"Till the cluster of black spots has just been resolved" or "just ceasing to be visible as a distinct cluster."*

The following table shows the results of observations 24) . At each eye distance four pairs of observations were made according to the original, but the table gives only 4 numbers, which are probably means of two each. The successive distances to eye are in the geometric relationships 4 / 5 , and the last column gives the lamp calculated distances calculated according to the condition that that geometric conditions of the eye distances, an arithmetic difference supplied hear 16.0 inches of the lamp distance.

Eye Relief	lamp distance				Avg. lamp distance	calculated
134.11	14.5	14.8	18.2	14.5	15.5	14.8
107.29	34.3	29.5	27.6	26.4	29.5	30.8
85.83	40.5	51.7	50.6	46.5	47.3	46.8
68.66	57.4	69.2	61.9	64.7	63.3	62.8
54.93	74.9	77.1	74.7	79.1	76.5	78.8
43,95	99.0	90.5	88.3	90.2	92.0	94.8
35.16	114.1	106.5	110.0	111.4	110.5	110.8
28.12	138.4	122.6	132.1	133.4	131.6	126.8

> [24] With regard to the last, smallest, eye-distance, the author observes "occasional brief paroxysms of distinct and magnified vision."

It has previously been asserted that the distinctness of small visible quantities is fundamentally dependent on the effects of irradiation. This influence will now be considered more closely. Under irradiation, we summarize the physical light propagation of the impression on the retina, which depends on optical aberrations and diffraction.

In all experiments on the smallest recognizable size or distance one goes down to such a smallness with the size or distance, or moves away from it so far that apart from the Irradiation the picture on the retina becomes a point or a line of a very insignificant diameter, and in general one has, except for Volkmann in his recent treatise on Irradiation [25], the diameter of the smallest recognizable image or the smallest recognizable distance calculated irrespective of irradiation. But Volkmann's fine experiments, the results of which follow below, have established beyond doubt that even in the best and bestaccommodated eyes there is a noticeable and measurable spread of the light impression through irradiation; and if one holds together his data about the size of the circle of irradiation with the clearest possible vision, whether with his or with the information given by other observers about even the smallest identifiable magnitudes, one finds that the diameter of the circle of irradiation is the diameter of the images of the irradiation smallest identifiable quantities (or at the smallest discernible distances) not only gains a very significant ratio,

[25] Reports of the Saxon Soc. 1858, p. 129.

In fact, according to Volkmann's measurements below, the edge of a shiny silver filament on black ground widened to the black side by 0.0012 to 0.0032 [26] Mill. = 0.000532 to 0.001418 Lin. in min. and max. of 6 persons with the best possible accommodation of the eye, and, if the thread is seen black against a light background, by 0.0003 to 0.00185 mill. For example, according to Hueck, the angle of vision under which a white line disappears on a black ground, that is to say the limit of the still recognizable line width, is 2 seconds, which represents 0.000145 million on the retina

[26] As half of the value indicated later *R*.

Since, according to the remark made (see Chapter 10), the extension of the irradiation dependent on physical circumstances can not grow with the intensity of light, the light of an intense and a faint point is thereby spread over the same space, but that of the faint one It can thus be weakened to the point of imperceptibility, while the intensity of the light remains appreciable.

If a point of light is not intense enough to be different in the center of the circle of irradiation from the threshold of difference from the ground, it can no longer be recognized. If a black point is applied on a white ground instead of the other way round, analogous considerations apply, provided that the surrounding points of light dilute by irradiation and allow the black point to be overrun with light, thus causing the black point to spread by weakening it Blackness arises as a spreading of white dots on a black ground, as Volkmann has discussed (aa OS 120) and proved by experiments.

Bergmann [27] observes that the points or lines used for experiments on the smallest recognizable quantities appear very pale at maximum distance, so that the straining eye can easily confuse it with any slight shadow, and that every time one does a grid drawing of millimeter-wide black and white stripes from the distance where they were first recognized, gradually approached, the white of purity, the black of depth. These are circumstances which are easily explained by the fact that, at a greater distance than the clear range of sight, the propagation of the light increases as a result of the optical aberrations.

[27] Henle and Pfeufer Zeitschr. III. F. Bd. 11. p. 93.

Easily explained because of the great, and after the above, influence which has the intensity of light on the recognizability of very small sizes, you have the experiments on recognizable smallest distances declared suitable to test the sharpness of the room meaning [28]), In the meantime, the influence of irradiation is only more complicated, not absent. If two bright points or lines come so close together that their circles of irradiation intersect, and the minimum of the brightness in the space no longer differs from the maximum in the center of the points of irradiation around the difference threshold, they can likewise no longer be distinguished. Here again, experience shows that there is a certain influence of the intensity of the light. For I find in Steinheili's photometric treatise (p.17) the remark that glasses of low blackness express a surprising success with regard to the separation of very close binary stars; I confess, however, that I can not derive this influence from the conditions of irradiation known to me. Because it seems to me, provided that by increasing intensity the expansion of the circle of irradiation does not grow, that the ratio of the minimum and maximum ordinates of the intensities must remain the same in strong and weak light, which would leave the peculiarity of the difference unchanged; However, in view of the oncoming intensity of the reason, there must be a certain advantage of distinctness with greater intensity.

[28] Weber, reports of the Saxon Soc. 1853. p. 141.

The above, taken together, shows that the previous experiments on the smallest recognizable sizes and distances on the retina are not usable to draw firm conclusions about the fineness of the sense of space or the degree of extensive sensitivity, so long as not the question how much proportion has the irradiation thereby, and this proportion is eliminated, and that the calculation of the size of the smallest images on the retina from the size and distance of the objects and the assessment of the relationship in which such to the last elements of the retina standing, illusory and erroneous insofar as irradiation is not taken into account. In this connection, Volkmann concludes his treatise on irradiation (p.48) as follows:

The question arises whether the influence of the dimensions of the components on the remoteness of their difference is entirely due to the influence of irradiation, which would presuppose that it soon finds its limit with increasing magnification. Unfortunately, there is still a lack of deliberately penetrating attempts directed at the object. Only a few, but not particularly concerning, Försterís experiments [29] would I be able to refer to the question which seems to show that the influence of magnitude on the clarity really goes beyond what is to be written on irradiation. These attempts were made as follows: "An internally blackened, parallelepipedic box, closed on all sides, about 36 inches in length and about 8 inches in width and height, forms the dark chamber in which the object to be illuminated is placed, at one square end two round openings of 2½ inches center distance for the eyes and next to it at the same height a larger, 25 ⌡ Zentim. squarely holding for the light source. The latter opening is covered on the inner wall of the box with fine white office paper, and at 1½ inches from it there is a burning wax candle [30] (obtained as uniformly as possible ⁾ . The paper square illuminated in this way serves as a light source for the objects to be mounted on the wall vis-à-vis inside the box. The size of the light source was changed as desired by diaphragms (map rails with opening) of certain dimensions, which were pushed close to the same. "

[29)] About hemaralopia. 1857. p. 5. 10.

[30)] 12 pieces per pound, 4½ ″ long, ¾ "in diameter.

Now the author notes (p.10): "The weakest illumination required for the distinction of 1n2 centimeters wide and 5 centimeters long (with the long side vertical) black rectangles on a white ground (at a distance of 12 inch = 32.5 centimeters from the eye) is represented by a size of the light source of 2ñ5 sq. millimeters. If the light source falls below this number, the objects must be significantly larger. "

It can be calculated that the image of a 2 centimeters. wide stripe, at the indicated eye distances, is 0.9 mill. on the retina, which far exceeds the value of irradiation in accommodated eyes according to the above data. If even larger objects become visible under weaker illumination, the influence of size can not depend solely on the irradiation. In the meantime, however, more extensive experiments with a change in size and absolute brightness, especially directed at the object, still have to be desired.

All that is said above is that the previous determinations of the smallest recognizable quantities and distances, irrespective of the irradiation, are not conclusive, but do not assert that there is no threshold of extension for the eye independent of irradiation. It may be true that the extension of an impression on the retina or skin may diminish as desired, and yet a sensation may arise if only one living nerve end is hit and the impression exceeds the intense threshold; but this does not mean that this impression is really perceived as extended, that is, in such a way that a majority of points are distinguishable in that the extension of the impression falls below a certain limit, as well as what is essentially connected with it.

In fact, it is a generally valid assumption in the physiology of the nervous system that impressions are distinguishable only if they are made on different sensory circles, if sensory circles are the end, or in the case of branching the totality of the ends of a primitive nerve fiber , A circle of sensation, whether belonging to an unbranched or branched fiber, necessarily has a certain diameter, and afterwards impressions falling upon the same sensory circle can no longer be distinguished. In the realm of the face, of course, the experimental proof that it really is so is subject to insuperable difficulties. because the circle of irradiation of a point of light should always be greater than the diameter of a circle of sensation; but in this we may look over the skin, which is analogous to the retina, for extensive sensation. It is true that irradiation also plays a role in the tactile experiments, provided that the pressure of a touching tip necessarily has to transplant more or less on the neighboring sites. But neither is it possible for Weber to observe the flow of two liners. Distorted compass points on the back, upper arm, and thighs in a single impression, yet to make the differences observed between different parts of the skin in relation to the just noticeable distance dependent thereon.

The above shows that for the evaluation and interpretation of the experiments on our object, on the one hand, the knowledge of the absolute magnitude of the irradiation, which can occur with the best possible accommodation of the eye, is important, on the other hand, the knowledge of which retinal elements one has as representatives of the sensory circles. and what dimensions they have. First and foremost, I follow the results that Volkmann received in himself and some other persons. In the last respect I notice briefly that the so-called cones are now regarded as the sensory retina elements, and that, according to Köllikerís measurements, the diameter of a pin at the yellow spot, where the clearest vision takes place, is 2ñ3 thousandths. miner[31)] found that the smaller of these data was confirmed when measured on the fovea lutea to the outside.

[31)] Henle and Pfeufer Zeitschr. III. F. II. BS 37.

Volkmann's experiments on irradiation [32)] .

Silver threads of 0.445 mill. In diameter, of the following clearest range *S* (in millim.), A) as black threads against a light background, like the sky, or b) as white threads against a bright background black background appeared, on the average of the following number *Z* experiments, of which the first number goes to a), the second to b) Irradiationskreise of the following diameter *R* in millimeters.

observer	Z s		R	
			a	*b*
AW Volkmann (Author). ,	39. 24	333	0.0035	0.0046
His son Otto V., 23 years, good eyes	10. 15	250	0.0037	0.0064
His son, Edmund V., 26 years, good, very experienced eyes. , ,	15. 12	250	0.0024	0.0058
Dr. R. Heidenhain [33)]	? 40	100	?	0.0051
E. Appel, student, very keen eyes	20. 20	300	0.0006	0.0025
Young girl, 16 yrs, very short-sighted, otherwise good eyes. ,	10. 15	112.5 [34)]	0.0017	0.0024

[32)] Reports of the Saxon Soc. 1858, p. 129.

33). (A) Heidenhain gave no uniform, and therefore not quoted, results; but the majority of the attempts to accept irradiation were not favorable, which Volkmann describes as an exceptional case that did not occur to him again.

34) For a) $S = 115$, for b) = 110.

Volkmann notes that in addition to the series of experiments, the results of which are given here, there are many (occasionally occasioned by him) individual observations by the professors Knoblauch, Hankel, Ruete, Czermak, and others. A., which has all the corresponding results (ie a proof of existing irradiation). These results were obtained as follows. The observer brings the micrometric instrument (discussed in chapter 9) with parallel silver filaments of 0.445 millimeters in diameter to the distance from the eye, in which he sees the filaments sharply, and by turning the screw he seeks the parallel threads to give a distance which is equal to the diameter of the threads. It turns out, however, that every time he makes the distance much larger than the actual diameter of the threads, because this diameter appears widened by irradiation. This can be done by calculation: Thus, with reference to the date that the point of intersection of the directional lines in the axis of the eye is 9 millimeters behind the foremost point of the cornea, and 15 millimeters in front of the retina, after the removal of the micrometer threads from the eye Eye and their mutual distance not only the diameter $2r$ *of* the retinal image of each micrometre, but also calculate the distance ε οφ the axis of one wire from that of the other in the retinal image, except for the scattering by irradiation; according to which, by a simple consideration for the case realized in the experiment, that the distance δ between the widened wire images appears equal to the diameter 2 ρ οφ a widened wire image, the diameter of a circle of irradiation is found; namely by $\varepsilon = \delta + 2\rho$ and

$2\rho = \delta =$, On the average of 39 experiments Volkmann found a clear distance = 0.207 mill., Appearing equal to the dark thread of 0.445 mill. Width at a distance of the eye = 333 mill., After which $= 2\rho = 0.0055$ mill .; $2r = 0.00199$ Mill., And consequently $- 2r = 0.0035$ Mill. As a check, Volkmann made ten attempts to make the distance between the threads seemingly twice as large as the diameter of the threads. According to the results of the previous experiments, it can be calculated (p.144) that this distance should be 0.328 mill., With which the average result resulting from the ten experiments was remarkably equal to 0.367 mill. which proves that this attempt deserves confidence.

The following points deserve attention: Irradiation behaves differently in the vertical and horizontal directions. If Volkmann regarded the wires in a horizontal position from the same distance as in previous attempts at perpendicular, the picture was highly indistinct, so that he had to draw weak convex glasses to maintain the same range of vision, 333 millimeters, in light In the background, on the average of

10 observations, the diameter of the circle of irradiation = 0.0047 mill., Instead of being 0.0035 mill. In the perpendicular position (without glasses).

For Volkmann, the specification of the 5 test days for the *a* method with respect to brightness resulted in the following distances *D* between the micrometer threads, at which apparent equality with the wire diameter takes place (the number attached to *D* means the number of experiments):

1st day of experiment (not specified) $D_9 = 0.1897$

2nd day, cloudy weather ,. , , $D_{10} = 0,2271$

3rd day, clear sky ... $D_{10} = 0.2153$

4th day, very bright sky. $D_{10} = 0.2074$.

Heidenhainís experiments on the method b) gave on

1st day (not specified). , , , $D_{20} = 0.111$

2nd day, very bright light. , $D_{20} = 0.153$.

A certain influence of the degree of illumination does not result from this.

Special provisions for the smallest recognizable sizes.

If, according to the above, the previous determinations of the smallest identifiable quantities and distances do not seem suitable for letting pure results of any kind be drawn, they are nevertheless important in that they partly determine a limit under which, at any rate, the power of the eye is insufficient have a practical interest; therefore a compilation about it should not be unwelcome.

Unfortunately, this compilation shows only a small agreement of the results obtained by different observers. And since the precarious value, which can only be settled afterwards, would disappear altogether, unless the circumstances of the observation were precisely specified, I do so much of the same with the observers' own words.

Insofar as it is necessary to translate facial angles into magnitudes on the retina, or conversely, on the basis of Listingís provisions, the distance of the crossing point of the main rays from the retina is 15.1774 mill. = 6.735 par. Lin., And of the cornea = 7.4696 mill. = 3.315 par. Lines are assumed, and thereafter for 1 sec. Visual angle 0.00007357 mill., Or 0.00003265 par. Lin. substituted.

Almost most often one uses the following statement in Smithís optics, which I here after the French translation of his work (TI p.

"Le dr. Hook nous assure que l'oeil le pin subtle ne peut pas bien distinguer une distance dans le ciel, comme une dache le corps de la lune, ou la distance de deux étoiles, qui comprend dans l'oeil un angle moindre qu'une demi-minute (Voyez ses remarques on the machine céleste d'Hevelius p.8). Si l'angle n'est pas plus grand, les étoiles paraîtront à l'oeil nud, comme une seule étoile. J'ai assisté à une expérience où l'un de mes amis qui avait les meilleurs yeux de la compagnie, pouvait à peine distinguer un cercle blanc sur un fond noir, ou un cercle noir sur un fond blanc ou opposé a la lumière du jour , lorsqu'il comprenait dans son oeil un angle moindre que les deux tiers d'une minute; ou ce qui revient au même, lorsque sa distance à l'úil surpassit 8156 fois son propre diamètre; Dr. Hook. "

Tobias Mayer [35] gives the result of several experiments as follows:

"Prima experimenta facta sunt in loco umbroso, apertis fenestris a sole tum meridiano aversis; objectis atramento sinico, ink vocant, in charta plana et albissima pictis. 1) Punctum nigrum, rotundum, diametri ¼ lineae Paris. oculo myope, sed convenienti lente munito spectatum, cum distaret oculus 10 feet Parisienses, adhuc satis bene distingui poterat. In distantia 12 pedum dubia videbatur, in distantia vero 13 pedum jam prorsus evanuerat. ó 2) Simile punctum, sed cujus diameter 0.44 lineae, adhuc videbatur distante oculo 14¾ pedes; distante autem eodem 17 pedes, vix vestigium ejus adparebat, sicut remoto oculo ad 18 pedum distantiam omnino evanuerit. ó 3) Punctum aliud diametri 0.66 lin. cernebatur adhuc in distantia 24 ½ pedd., aegerrime autem ac dubie in distantia 26 pedd. et oculo paulo plus remoto nihilum eius spectari potuit. «

[35] Comment. Soc. Gotting. T. IV. 1754. p. One hundred and first

After a few more experiments with lattice figures, which will be discussed below, he adds:

"Puncta et figurae, quae in superioribus adhibitae sunt, quamvis luce solis aestivi et meridiani, atque adeo fortissima collustrata, in iisdem tamen quam proxime distantiis, subquibus supra, incipiebant confusa apparere; discrepantia certe, si qua erat, repetito saepius experimento modo majores distantias, modo minores arguere videbatur. «

This result, that the degree of illumination has no influence on the recognition of the points, is in definite contradiction with the results of Plateauís experiments below.

If the distance of the eye, which according to Mayerís terms (p.101) was able to constrict the points in the three experiments e conspectu eripere, is set to 12.17 and 26 feet with it, then the diameter of the image in the eye was 0, 000973; 0.001126 and 0.001186 par. Lin .; the visual angle 30, 35, 36 seconds, so at these different distances noticeably the same size.

Plateau [36] considered in his experiments both color and degree of illumination. Small colored paper disks of 1 centimeter were attached to an outdoor vertical panel. From these plateaus moved successively until the colored disc appeared only as a small, barely perceptible, cloud, and disappeared a few paces farther, then measured the distance from the objects and then calculated the angle of vision. The results in two cases were as follows:

In the shade. In the sunshine.

White 18 "12"
Yellow 19 "13"
Red 31 "23"
Blue 42 "26"

the difference that the color makes here is probably due only to their different degrees of brightness.

[36] Pogg..Ann. XX. p. 327th

Hueck 37) made experiments in the following way: a clearly seen point is sharply fixed by a normally formed eye; Gradually, the observer moves away from the object until it disappears, and the board on which the dot or line is located appears quite pure.

[37] Müller Arch. 1840. p. 85.

"From several hundred observations made by different individuals, it now appeared that a white, non-glossy spot on a black field disappears at 10 sec. Of visual angle," which is 0.00033 par. Lin. or 0.00074 million on the retina. A white line on a black field is still seen under 2 seconds angle of vision; on the other hand, the visual angle under which black dots disappear on white field was 20 seconds. The first corresponds to 0.0000652 par. Lin. or 0.0001470 million, the latter 10 times that on the retina. Details of the number of experiments and the degree of agreement that has been obtained are not given with regard to the latter information. Also on the lighting conditions in the experiments, nothing is said.

Volkmann [38] was able to perceive a single thread up to a distance of 21 inches, and another person induced by him to observe the same thread up to 22 inches away. Volkmann recognized a 0.002-inch-thick hair [39] inches wide. A pupil of Bari recognized [40] a hair of thickness still at a distance of 28 feet.

[38] Volkmann Beitr. P. 202.
[39] Wagner's Wort. Kind .. See. P. 331.
[40] According to a statement by Volkmann in s. Art. See p. 331.

More detailed information with some interesting secondary provisions are Ehrenberg [Ü], which I find rarely considered. They do not refer to observations with altered distances of the eye, but to the clearest visual range (after Ehrenberg 4ñ6 inches), in which very small objects are recognizable. I tell her here with his words:

To be quite certain, not to be deceived by the politeness or shame of those who do not like to see something not to be seen, I have often recorded the objects seen by the observers, or letting me describe them cumbersome, which taught me with conviction, that they saw completely the same and just as sharp as I had seen, and mostly without the former having to change the microscope. Observed attentively by a large number of persons of various senses, I was likely to find that there was a fairly firm general limit to the vision of the unclouded and healthy human eye, which must permit a conclusion as to the highest power of the microscopes. I made many observations to find out To what extent the differences of myopic and presbyopic eyes have an influence on the general expression of that force, and have often convinced me that the not uncommon opinion that myopic persons are more or sharper than others is unfounded. The result of my experience is a double:

1) There seems to be a normal force for the human eye with regard to seeing the smallest parts, and the deviations from it seem to be much rarer than one usually thinks.

[Ü)] Pogg. XXIV. P. 35th

It can only be said of those who are able to see clearly at any distance. Among more than 100 people whom I have observed, those who are most keenly seen in ordinary sight were not able to distinguish more than I saw, and those who were weak-minded or long-sighted were usually able to see what I was but, in the case of seeing with the naked eye, they usually needed a slightly greater approach or distance of the object from their eye than I did.

2) The smallest usually achievable for the natural human eye | size is both the white color on a black ground, as for the black color on white or lichthellem basically $^1/_{36}$ of a Parisian line in diameter. Is still possible by Lichtkondensierung largest and tension of the attention the sizes between $^1/_{36}$ and $^1/_{48}$ to recognize a line, but without focus and doubtful [41)] .

[41)] "that $1/_{49}$ to maintain the effort would not be worthwhile, understood well. The next trouble-cost ratios were $1/_{60}$ or $1/_{72}$ line, and about I can not make any experiences that they seen by anyone would. "

This is the limit of the power of the natural human eye for colored bodies, which anyone can easily verify, as I have tested, by casting very fine black specks on very white paper, e.g. B, of dryer ink, ink and the like, brings., And the smallest of which receives with a very fine tip and laying on a glass micro-meters, which is at least 1 / 48 Line directly indicates. Sun and lamplight also allow, with or without mirrors, to look at black bodies, etc., on the glass micrometer in the light. Bodies smaller than those given can, irrespective of effort, not be recognized singly, but still in a simple straight row with the naked eye. Further, if such several are together in close proximity and in multiple series, they make a communal impression upon our eye and deceive us, as if we saw a larger simple body or surface 42)., The usual distance, which good eyes observe, when they want to recognize these smallest bodies, I found by measuring 4-5 inches, sometimes 6 inches, which latter is the usual distance for very sharp-sighted ones. Myopic persons rarely approach the same objects more than 4 inches, more rarely 3 inches, etc., and usually become the same as the rest. Someone whose sharpest vision is 4 inches, can not increase his eyesight by closer approach of the eye to the object, but feels pain and sees indistinctly. Once the object has been fixed, it can be removed much more without losing sight of it. I myself can 1 / 24 I did not see a line at 12 inches black on white, but I looked at it at 4ñ5 inches, so I can remove it to 12 inches and see it clearly. This phenomenon is based on the well-known power of the eye to adapt to the distance. Often you can see even small objects in the distance, as soon as you are aware of their location, or if they move. Similar appearances are given by a balloon in the clear sky and a ship on the horizon, easily seen as soon as one is alert, but the ability of quick orientation is based on habit and on mental sharpness, without allowing a conclusion on vision in general. If someone is more energized by facial impressions than another, he orients himself faster, but he does not see any more than another, who, because he takes these impressions less vividly, orientates himself more slowly. I often use the means of looking for very small objects only with the magnifying glass, if I want to see them with the naked eye, to give them another situation with a fine point. Even this appearance is only for the orientation with respect to the place of the bodies, and only promotes the speed of this orientation. Myopic eyes orient themselves more and more easily, because they are less distracted, because their field of vision is a smaller one. It is probable, finally, to add a higher potency to the absolute sight of the human eye, which is that for the recognition of luminous corpuscles. Small bodies glowing in the dark appear to be always much larger,1 / 48 line, depending on the light intensity, nor of affecting the human eye. I have never had the opportunity to observe even luminous magnitudes of such a small diameter, so that I could call attention to a limit in this respect ..... Metallic luster, which is a very powerful light reflex, is mine observations made at Goldstäubchen with the naked eye at ordinary daylight down to 1 / 100 recognize a line, that is twice as far as colors. "ó

". The situation is different with lines Opaque yarns of $^1/_{400}$ to line thickness recognizes against light with the naked eye Spinnenfäden measure. $^1/_{300}$ bis $^1/_{2000}$ lines; threads of the silkworm $^1/_{200}$ The latter are twice in the cocoon.."

> [42] "I am accustomed to discerning in this manner very fine eyelashes of the infusoria, which, when moved, form a small apparent surface, which is visible, but as soon as they rest, their delicacy is often so great that the eyesight brings them with them not reached the microscope. "

Considering the size 1 / 36 Lin. at 4ñ6 inches eye-space, which Ehrenberg defines as the limit of vision for nonlinear bodies, is transformed into lines on the retina, one finds 0.0039 to 0.0025 Lin, which is strikingly larger (10 times larger) than Hueckís 0 , 00033 Lin., Notwithstanding that both results are derived from a large number of experiments, and Mayerís result exceeds by more than double. In the same way, Hueck and Ehrenberg differ in that, according to Hueck, black dots on a white background require a larger visual angle than vice versa, whereas Ehrenberg considers both indifferent.

A difference in the circumstances may be that Ehrenbergí's attempts to observe tiny particles are very close, but that of Mayer and Hueck with points of considerable dimensions from a greater distance, since both observers progress from the point of clearest vision to the disappearance of the observer Have point removed. Now, according to Mayerís experiments, the distance does not make a significant difference, but since its distances are 12 feet and above, a much greater closeness, as happened in Ehrenbergí's experiments, could make a difference; which is even closer to examine.

If it is not only the existence of a visible size to recognize, but also to determine the shape, a larger visual angle is required. According to Hueck [43)] , a square of 1.2 '"diameter at 11' distance, ie at a viewing angle of 2 ′ 35", was still recognized as a square. Just as a slate stroke of 1.5 '"length was on 13', under 2 '45" still recognized as crooked. Document (double agent) with a width of the letters 1.5 '"and spaces within the letters 0.5"' Las Hueck using a suitable for his eye glasses at 13 'distance.

Bergmann [44)] found "that short lines were seen less widely than equally broad ones."

EH Weber, [45)] notes: "According to my experiments, a white line on black ground can be seen from a distance more than three times greater than an equilateral square of the width of the line, and the brightness of the illumination of the line and a very sharp background can make that distance even bigger. "

[43)] Müller's Arch. 1840. p. 88.

[44)] Henle and Pfeufer, Zeitschrift III. F. Bd. II. P. 92.

[45)] Reports d. Saxon Soc. 1852. p. 142.

Special provisions for the smallest recognizable distances.

The experiments on this are done under different forms, according to which the results also change.

α) Two distant points or squares.

The statement Smithís regarding two stars is already (so) communicated.

Volkmann [46] dropped on a small thermometer ball of 0.15 inches in diameter the flame images of two lights, which were 4 inches below each other, 8 inches from the sphere .... He recognized, with the help of the glasses, complete separation of the images in the Glase up to 20½ inches and touching in the middle, but clearly duplicate, pictures down to 26 inches .... One of his friends repeated the experiment, and recognized the pictures to 37 inches away. In order to fully recognize the duplicity without glasses, Volkmann had to approach the glasses to 12 inches.

According to Hueck [47] , two black spots on white backgrounds, spaced 0.45 "apart, merged with each other at 10 feet distance of the observer, giving the angle of view of their distance 1 '4". The same result was given by strokes that were just as far apart.

EH Weber [48] adds to his statement of proportion as to the distance in which white lines and squares on black ground vanish from the eyes, as follows: "On the other hand, two white equilateral squares on black ground are interspersed by a black space of divorced from each other, which is just as wide as the squares, from a distance still distinct as two, being nearly equal to that from which two white lines on black ground are still distinguished as two, which are as broad as those quadrilaterals and be separated from each other by a black linear space of the same size. "

[46] N. Contrib. P. 202.
[47] Müllerís Arch. 1840. p. 87.
[48] Reports of the Saxon Soc. 1852. p. 142.

β) Two distant threads.

Volkmann [49] put two spider-web threads in parallel direction and at a distance of 0.0052 "next to each other, and found that he recognized them as double at 7", but no further. The most discerning among his friends recognized the duplicity at a distance of 13. "Two black parallel lines on a white ground at a mutual distance 0.016 ″ Volkmann recognizes with the help of the glasses at a distance of 27íí.

Valentin [50] was able to distinguish between two lines, if their pictures on the retina were only 0.0009 '"apart.

Hueck found the same result for strokes as for points (see above).

γ) Streaky and dicey figures.

Tobias Mayer [51] describes experiments in widespread daylight as follows:

l. Figura striata, cujus nigri ductus, aequales albis interpositis, lati erant 0.36 lin. Paris. specta directe in distantia pedd. 11 jam aliquantum confuse videbatur, ita ut vix liceret alba intervalla a nigris discernere. In distantia 12 pedd. omne discrimen inter strias aberat. Certe nonnisi aegerrime sentiebatur. Paulo plus remoto oculo tota figura eundem colorem quasi cineatium mentiebatur.

2. Figura itidem striata, sed cujus atrae striae duplo crassiores erant, quam albae, harum enim latitudo erat 0.2 lin. Illarum 0.4 lin., Incipiebat videri confusa distante oculo 9 vel 10 pedes.

3. In eadem distantia alia figura striata, cujus albi ductus duplo latiores quam nigri, inverso nempe praecedentium ordine, desiit distinete videri; latitudo striarum albarum erat 0.4 lin., lin. nierarum 0.2.

[49] Wagnerís Wörterb. Art. See. P. 331.

[50] Valentin, Lehrb. d. Physiol. II. P. 428, here after Weheri's article Tastsinn p. 634.

[51] *Comment. soc. Gotting. T. IV. P.* 102nd

Notandum, binas has figuras (no 2 et 3) etiam in experi-mentis sequentibus [52] and semper oculi distantiam requisivisse. Quare commodum erit, deiis in posterum conjunctim referre.

4. Figura cancellata lineis nigris, quarum latitudo 0.44 lin. Eadem, quae interstitium alborum, spectata e distantia 15½ pedd. incipiebat mentiri aequalem ubique nigredinem, ut dubium esset, number aibi quid in illa contineatur.

5. Figura aleae similis, aleatam dicere brevitatis causa liceat, quadratulis nigris albisque varia, quorum singula latera aequalia 0.52 lin. Distante oculo 12 pedes, extremam visionis speciem praebebat, paulo enim plus remoto oculo confusa alba cum nigris apparebant. "

[52] Employees with candlelight

After comparison of No. 1 with no. 2 and 3, and from no. 4 with no. 5 Mayer concludes that inequality of white and black intervals facilitates cognition.

The experiments with the streaky and dicey fig. 1 to No. 5 were then repeated in the dark under direct illumination with a tallow at different distances *L of the* same from the figures, where the following distances *A of* the eye resulted from the figures as limits of knowledge *(termin. Visionis)* (unit of the paris foot):

L A

Distance Distance of the eye
of light no. 1st no. 2 and 3rd no. 4th no. 5th

½ 7½ 6½ 12 9½
1 6½ 5½ 9½ 7½
2 5¾ 4½ 7 6
3 4¾ 4¼ 6½ 5¼
4 4½ 3¾ 6 4½
5 4¼
6 4

7 3¾
8 3½ 2¾ 4½ 3¼
13 3 2½ 3¾ 3

Mayer represents the law according to which *A* changes with *L* by the following formula

.

where *n is* a constant dependent on the character of the figure, which he assumes in the different figures as follows:

No. 1 2 u. 3 4 5
79 52 73 99

He gives a compilation of calculation and observation, according to which the formula is close enough within the limits of these experiments.

Hueck [53] considered the coins, medals, and gems engraved in dashed lines by means of a machine in the trésor de numismatique et de glyptique. Paris 1834, and was able to distinguish intervals of 0.0727 "'at 22" 3' "distance, ie at a viewing angle of 56.8"; yes, some quite clean with very sharp lines printed on quite plain smooth white surface still under 44.3 "visual angle At a greater distance the dashed area appeared gray Yellow stripes on red surface appeared orange at a retinal image of 0.001", just yellow Stripes on blue surface green.

Marie Davy [54] drew black lines on white papers in such a way that the interspaces were just as wide as the lines themselves. He made such leaves with strips of various widths, and then tried how far he had to remove each of them from the eye. to no longer see the black and white stripes, but a monotonous gray. He found that this occurred at such intervals in all leaves, that the width of the retinal image of a stripe, calculated by him, was nearly 0.0011 millim. revealed. Namely, it was 0.00109, 0.00113, 0.00113, 0.00112 millimeters at intervals of 5.8, 0.75, 0.53 and 0.41 meters. ó The type of calculation is not specified.

EH Weber [55] applied black lines, which were drawn very tightly and uniformly by machine-stitch and printed on white papers. They were 0.025 par. Lin. wide, and just as wide were the spaces between them. His son Th. Weber still recognized the lines at a distance of 9 par. Inch 2½ Lin., Where the angle of view, under which a gap was seen, was 45.3 sec. He had the same experiments done by several others; where the greatest sharpness was found in twos, one of which (No. 9) also recognized the lines at 9 inches, the other (No. 8) at a distance of 11 inches, which was 45.3 and 36.5 seconds, respectively Gap or 0.00148 and 0.00119 par. Lin. equivalent.

[53] Müllerís Arch. 1840. p. 87.

[54] Instit. XVII. p. 59.

[55] Reports of the Saxon Soc. 1853, p. 144.

Bergmann 56) used lithographed lattice drawings whose bars and spaces were each 1 millim. are wide, in the following way. A hole of about 20 millimeters in diameter was cut in the middle of the lid of a round cardboard box and the lattice panel was fixed against the lid from the inside, so that only a circular part of it appeared from the outside. "This gives the possibility of being able to give any inclination to the bars by rotation of the lid, so that the one whose eyes are checked, by giving the direction in which the lines run, can prove that he really sees them . "

[56] Henle and Pfeufer, Zeitschr. III. FUBS 94 f.

"The success of a considerable number of experiments was that the selected good eyes of several individuals never needed a closer approximation than the one corresponding to experiment No. 8 at EH Weber: the strips with their millimeter-wide intervals were always at a distance of 8.5 meters recognized. " ....

"Frequently, moreover, even at great distances, even from 7 meters away, the direction of the lines was repeatedly recognized correctly, and the experimenters often remarked that if they knew the direction of the lines, they would have the same direction in these lines In a previous experiment it even came to pass that a boy of ten years old, to whose excellent eye the author had occasionally become aware, correctly indicated three times in succession the direction of the lines (each time changed) at a distance of eight meters a false statement. "

Bergmann points out (p.97) that "at the distance of 5.5 meters, in which quite good eyes regularly detect with certainty the direction of the lines, the images of them are slightly wider than half of a peg diameter, from which one always assume an essential relationship between these dimensions. "

In the distances exceeding 5.5 meters, in which often a correct recognition of the direction of the strokes already occurred, the fading errors often showed the peculiarities that the direction of the strokes was indicated just perpendicular to the real ones. At the same distances, the lattice panel often appeared piebald. A man who did not know the object thought it was diced at a distance of about six feet; a second, who stood about two feet behind him, explained that he already saw that from there.

Bergmann intimately relates these circumstances to a probable presupposition about the shape and arrangement of the pegs as sensory retinal elements; still it would be too cumbersome to enter here.

In certain directions of the bars, the recognition of the grids seems to be easier than in others, but this varies according to the individuality of the eyes (see Bergmann p 1041).

Behavior of the side parts of the retina in recognizing the smallest sizes and distances.

In the previous data, the assumption of visible magnitudes and distances with the most clearly visible, central parts of the retina was assumed. After the periphery, both the recognizability of the quantities as distances decreases, but by no means in the same proportions in all directions. Observations on this are available from Hueck, Volkmann and Hüttenheim, some from Bergmann, the most detailed of Aubert with Förster, in which, among other things, it is specifically proved that the impossibility of distinguishing two points at some distance from the axis of the eye does not in any way indicate optical aberrations of the eye can be pushed; and that numbers or squares of different sizes, viewed at the same angle of vision at different distances from the eye,

In order not to extend the detail of this chapter too far, I believe that I have to refer to the original treatises with regard to the details of these experiments.

Hueck in Müllerís Arch. 1840. p. 92.

Volkmann, in Wagner's Wort. Art. See. P. 334.

Aubert and Förter, in Gräfe Arch. F. Ophthalm. III. P. 14 and Moleschott Unters. IV. P. 16.

Bergmann, in Henle and Pfeufer Zeitschr. III. F. Bd. II. P. 97.

Hereby one can relate the experiments for the determination of the size of the part of the retina, with which one sees sharply enough to be able to read pamphlet, about which EH Weber in the reports of the Saxon Soc. 1853, p. 128 ff., Aubert and Förster in the abovementioned treatises.

Distance differences (good judgment).

On the fineness of the good measure EH Weber [57) has the] following information:

"Dissecui chartam papyraceam scriptoriam magnitudine maxime consueta in octo parts aequales et cuilibet parti lineam rectam et aequalem inscripsi, curans simul, ut omnes lineae aequali crassitie et nigritie, diversa autem longitudine essence Brevissima linea 100 millimetris alia quaedam linea longior 100½ mm, alia longior 101 mm constabat Ita diversae lineae usque ad longitudine 105 millimetrorum ductae sunt.

[57)] *Progr. Coll. P.* 142nd

lam duae chartae iuxta se positae homini proponebantur, cuius subtilitatem visus examinare cupiebam. Homines arti delineandi operam navantes, ideoque visu exculto gaudentes, lineam perpendicularum 100 mm longam a linea perpendiculari 101 mm longa discreverunt, et experimenta ter, quater et quinquie iterato semper longiorem lineam recte indicarunt. Accidit camouflage et his ut defa-tigati nonnunquam errarent. Plures vero homines lineas 100 et 104 mm longas non, certo sed non nisi lineas 100 et 105 mm longas distinxerunt. His experimentis intellectum est, a nonnullis centesimam, from aliis vero vigesimam lineae partem, qua altera linea altera parallela maior est, satis certo visu cognosei. "

I have quoted some of my own experiments on the just noticeable differences of circular distances (see Chapter 9).

b) sense of touch.

It is well known that EH Weber investigated the size of the just noticeable distance of circular tips on the skin and found that this distance on different parts of the skin was exceptionally different. The greatest he found the sensitivity on the tip of the tongue, where still at ½ par. Lin. Distance the circle tip was recognized as double, soon at the volar side of the last phalanx (l par Lin.), At the red part of the lips (2 Lin.), At the Volarseite the 2nd phalanx (2 Lin.) Etc, least on the upper part of the spine and on the middle of the upper arm and lower leg (30 par. Lin.). His watch table is the most detailed in s. *Progr. Coll. P.* 50 *sequ.,* shorter summarized in the article sense of touch and common sense in Wagnerís Wört. P. 539 and in s. Abhandl. in the reports of Leipz. Soc. 1853 p. 85 ff. [58) has been] reproduced; at which latter place he gives various supplements concerning the general conception of the sense of space and the methods of determining its fineness. The observations of Weberi are first published by Allen Thomson (in *Edinb. Med. And Sug. Journ.* No. 116), later by Valentin (Lehrb. D. PhysioL 1844. Bd., US 565), and finally by Czermak physiol. Studies or Meeting the Vienna. Acad. XV. P. 425, XVII. P. 563, Moleschott, sub. P. 183) and extended by the latter according to several relationships.

[58)] Extract from it, but without the tables, in Fechnerís Zentralbl. 1853. no. 31. The table is also of Czermak in its physiolog. Stud p. 54 given.

Very interesting experiences, after which chloroformation and anesthesia greatly increase the just noticeable circadian distances on the skin, has Lichtenfels in the meeting papers of the Vienna. Acad. 1857. VI. p. 338 made known. Palsy-like conditions of the skin have the same success, what Landry's experience in archive. *gene. de méd. XXIX. Juill. Sept.*(Cannst., Anniversary of 1852. p. 189) and especially Wundt in Henle and Pfeufer Zeitschr. 1858. p. 272, also Brown Sequard in Cannst. Jahresber. 1853. p. 202, which also reports a case of the reduction of the just noticeable distance by a hyperaesthetic state. Experiences, according to which the just noticeable distance is reduced by exercise, Hoppe has in s. medic. Letter. 1854. Issue 2, Czermak in the above essays, and especially Volkmann in the proceedings. d. Saxon Soc. 1858. p. 38 made known.

Theoretical discussions on the conditions of the skin-space sense can be found in the mentioned treatises of Weber and Czermak, in Lotzeís medicin. Psychol., 1852, in Meissnerís contributions. Anatomist. u. Physiol. of the skin. Leipzig 1853 and in Wundtí Abhandl., Which contains a detailed compilation of this subject.

c) perception of time and movement,

If two impressions are made too quickly behind each other, they flow into a uniform one for the sensation, and one can ask how great the interval between two impressions must be in order to be able to interpret them as different.

A purely experimental answer to this can not be given for an analogous reason as to the extensive threshold of space. For, just as every impression has an irrational circle around it, every impression leaves an echo. If the echo which the first impression leaves is still strong enough, at the onset of the second, that the difference from the second does not reach the intense threshold of difference, then the one impression must flow uniformly with the other.

It may be asked whether the impossibility of conceiving of two impressions that occur too rapidly as distinct from one another depends on this circumstance. Nothing has been decided on this experience, and it is difficult to decide for sure. Probably, however, according to the analogy of space relations, there is an impossibility, as regards time relations, of perceiving each other too close impressions as differences.

However, one can not assert the existence of sensory circles in the sense thus discussed; but perhaps there is something in it for the fact that the subjective measure of time is just as different in time from psychophysical oscillations in us, as the subjective measure of the dimensions of sensory circles, and everything that falls within the duration of such an oscillation, can be distinguished as little as temporal falls into the extension of a sensory circle, spatially. In the meantime, it would be pointless to pursue this hypothesis without the possibility of a more detailed explanation.

The question in question is, among other things, due to the attempt of the rotated disc with white and black sectors. While a white sector passes by, the impression grows as a black passes by, he picks up. Is the phenomenon of uniformity merely due to the fact that the difference between minimum and maximum does not reach the intense threshold of difference which exists for calmly perceived impressions of light, or is the phenomenon of uniformity promoted by the minimum and maximum of the impression occurring so rapidly behind each other, that we can not separate each other in time; and, accordingly, can the difference be greater without jeopardizing the appearance of uniformity, than in the case of calm impressions?

There seems to me to be some possibility to decide on the basis of an attempt on this the first question, if one has determined some dates before.

In connection with the question of the time threshold, the question of the time which is necessary to understand given impressions with a given clarity stands. On this I find some remarks and attempts by Valentin in his Lehrb. the physiol. II. P. 471:

"What minimum of time," he says, "belongs to a satisfactory conception of familiar objects, is best seen from the reading of familiar letters." If I read only one line of greater pressure of this work (the Valentinian textbook), then 10 attempts at one letter at a maximum of 4.21, at a minimum of 2.34 and at an average of 3,330 thirds, reading a whole page that had no paragraph, and printed only with a larger step, found 2629 letters and punctuation marks 1 minute 32 seconds .This gives on average 2.10 tenths for 1 image.I did the same experiment with a continuous page of petit print of this work, so for 3944 letters and other characters I needed 2 minutes 12 seconds, hence for an impression 2.01 Terzien.We may, therefore, generally assume that in the rapid reading we require only two to four thirds on average for the interpretation of each individual character. "

About the smallest movements still perceptible, I find the following information in Gehleri's dictionary, Article Face p. 1457, by Muncke:

"From the determination of the duration of the light impression on the eye in conjunction with the above size of the visual angle G. G can explain why some very slow movements are not perceived. . Schmidt [59)] In order to make this clear, he chooses the example that the stars, even in the equator, where their movement is the fastest, still seem to stand still. If one sets the duration of the light impression in the eye high to 0.5 sec., Then the star goes through an arc of only 5 sec. In this time and since this is smaller than the smallest angle of view for a spatial object, it seems still to stand. On the other hand, when the star is viewed through a telescope at only 100x magnification, the angle of view is 50 sec., And its movement becomes difficult but barely noticeable, but the larger the magnification that is applied, the faster it appears. Here, however, the vivid light impression of the star comes into consideration on the eye, for when observing the movement of the minute hand of a pocket watch, Schmidt received a different result. The latter, when applying a tenfold magnification, perceived it just the same way. But by the length of the pointer 4.8 par. Lin. was, and the visual angle of a department of the same (for l0 Z. Distances of clear vision at the observer) 13.5 min., the movement of the same in one second = 13.5 sec., and with 10x magnification 135 sec. or 2 min. 15 sec. In the meantime, this ingenious method of measuring the smallest movements, and much more, in particular the sharpness of the face and the illumination of the observed object, has come into consideration, which is why the two statements given differ so much from one another. To test the latter, I myself watched the minute hand of my pocket watch, which was 9.1 Lin. long and steel blue moving on a dazzling white dial. As long as he moved over the latter, I could see his back with his unarmed eye, and with a face width of 8 o'clock, but he seemed to stand still when he was over a black graduation, so that this movement as the Borderline of those that my eye can still perceive. So you can only double that specified size of 13.6 sec. And in the ratio of 10 but he seemed to stand still when he was over a black line, so that this movement is to be accepted as the limit of what my eye can still perceive. So you can only double that specified size of 13.6 sec. And in the ratio of 10 but he seemed to stand still when he was over a black line, so that this movement is to be accepted as the limit of what my eye can still perceive. So you can only double that specified size of 13.6 sec. And in the ratio of 10**:** 8, in order to obtain for my eye the smallest optical angle of nearly 34 sec., [60] which, however, under less favorable conditions, especially if the measurement of the distance of the point of the pointer between the two minutes of the minute, would not be so small. This explains why the movement of the stars in the telescope becomes visible at an optical angle of 50 seconds, partly because of their strong light in the relatively dark space, partly because the field of view of the telescope is somewhat illumi- nated, because of its interior black tube, however, is completely dark, and thus in this way the variable distance of the star from the edge of the visual field can be measured. "

[59)] Manual and textbook of natural science. Giess. 1826. 8. p. 471.

[60)] The more exact calculation gives 34 "50" '.

For this one can still the following information from Valentin in s. Lehrb. II. P. Add 465:

because a large number of secondary conditions, which can not always be exactly calculated, contribute significantly. Not only the strength of the light, the brilliance and the color of the object considered, the distance, the range of vision and the acuity of the eye, but also the nature of the neighboring objects have a significant influence on the perception of the slightest movements. If the pointer of the clock z. For example, as the result of subtle strokes, its slightest movement is more easily perceived than otherwise, because those fine lines applied as ornaments serve as fixed points of comparison, and the slightest dislocation of the point of the pointer is all the more noticeable. " the brilliance and color of the object under consideration, the distance, the range of vision, and the acuity of the eye, as well as the nature of the neighboring objects, have a significant influence on the perception of the slightest movements. If the pointer of the clock z. For example, as the result of subtle strokes, its slightest movement is more easily perceived than otherwise, because those fine lines applied as ornaments serve as fixed points of comparison, and the slightest dislocation of the point of the pointer is all the more noticeable. " the brilliance and color of the object under consideration, the distance, the range of vision, and the acuity of the eye, as well as the nature of the neighboring objects, have a significant influence on the perception of the slightest movements. If the pointer of the clock z. For example, as the result of subtle strokes, its slightest movement is more easily perceived than otherwise, because those fine lines applied as ornaments serve as fixed points of comparison, and the slightest dislocation of the point of the pointer is all the more noticeable. "

XII. Parallel Law to Weber's Law [1)].

It is a fundamental question to which we have been led on the occasion of the probabilities of Weber's Law, and which will be considered here in more detail as to whether and how the sensitivity to differences is parallel with the sensitivity to absolute stimuli, namely, whether the modification of the sensitivity to stimuli, which arises due to the action of the stimuli themselves, also involved the sensitivity to differences of the same.

[1)] Revision p. 180 f. 240 ff.

A white disk on black paper covers itself with constant observation with a grayish veil becoming darker and darker to a certain extent, a proof that the sensitivity to the light is dulled by its action, which can be supplemented by enough other evidence. On the other hand, after fatigue by carrying or lifting loads, a load is felt to be heavier, whereupon the sensitivity to weights is increased by the prior action of the weights. There is a greater light stimulus, here a lesser burden to be felt equally strong.

The question arises: will this show at the same time the difference of light which is even noticeable or even noticeable, the difference in weight being increased or reduced, or will the tired organ still feel the same difference of physical action as without fatigue?

For the first sight, it may seem quite natural, that if each of these stimuli is felt to be weaker or stronger for one another, the difference of the same is felt to be weaker or stronger. But, as Weber's law has taught us, that when two stimuli are really weaker or stronger, the difference is felt as strongly as before, provided that it is at the same time toned down or grown with the stimuli; Thus the impression of the stimuli, which is altered from an internal point of view, could possibly have the same result, as the real objective alteration of the stimuli, and the difference afterward, are felt just as strongly.

In fact, what does psychophysically mean: the sensitivity to a stimulus is altered? If there is a fixed relation between psychophysical activity and sensation, it can mean nothing else than that another stimulus is required, the same impression as to produce the same psychophysical activity. Now, if Weber's law is thoroughly understood, rather than referring to the relation of sensation to stimulus to the relation of sensation to stimulation of the senses, it must come to the same thing whether the outward-acting stimulus is weakened or its inner effect weakened since the weakening of the external stimulus is only possible through the weakening of the internal effect;

If, of course, Weber's law were not transferable from the external stimulus to the internal action, that is, the psychophysical activity induced by it, the difference in sensation would not remain constant, if the relative difference or ratio of the somehow measured internal effects remained constant, but changed after some a function of their absolute difference, this implication of the law could not be transferred from outside to inside; and thus one sees that the question in question is indeed of fundamental importance to our doctrine. It is the question of one of the bridges between external and internal psychophysics.

Apart from that, the question of what constitutes a dependence between the absolute and difference sensitivity is essential or not essential, of important concern, and the decision on it is very appropriate to us a step forward of clarity in the as yet obscure doctrine of irritability and excitability.

I want the law to be what the main issue in this question is, insofar as it is to be regarded as a transfer of Weber's from outside to inside, the *parallel law of Weber's* or, in short, *parallel law* . It will be pronounced like this:

If the sensitivity of two stimuli changes in the same ratio, the sensation of their difference remains the same .

Equivalent to this is the following statement:

If two stimuli are both felt to be weaker or stronger than before, their difference is as great as before for the sensation; if one had to change both stimuli in the same ratio in order to obtain the former absolute strength of sensation through both.

The question of whether the temporal alteration of the absolute sensitivity of one and the same sensory part is essentially dependent on a change in its sensitivity to difference depends on the natural connection between the question of whether a spatial difference of absolute sensitivity, ie, a difference of absolute sensitivity different parts, a variety of their sensitivity to differences essentially carry, have of themselves dependent; and herewith the question of our law transfers from the temporal to the spatial. The different parts of the retina have been shown to have different absolute and differential sensitivity to light. Do they go much parallel in this respect? The same loads are felt to be different on different parts of the body; Do you recognize with those parts of the body, which feel the same weights heavier, also given weight differences easier?

It does not seem to me that such questions have ever been made clear; even less so that they have been answered clearly and decisively, but they are fundamental questions.

It is not disputed that in the difference of absolute sensitivity over time the difference sensitivity can remain the same, but it can also be applied to spatial difference, and vice versa, so that the validity of the law in terms of time also speaks in favor of its existence in the spatial and vice versa, without but that one can avoid the task of proving it on both sides.

In general, under certain circumstances, the validity of the law can not yet be inferred from the validity of the law in other circumstances under certain circumstances, and it can, without the absolute and the difference-sensitivity being essential, ie everywhere, necessary, constitutionally in their nature, of each other But there are also circumstances which at the same time increase or diminish both, so that proving that they are not essentially interdependent is not just a matter of showing that the one always remains constant when the other other changes than that they can change as well without and against each other as with each other and in the same sense; in other words, that the parallel law exists under certain conditions, though it does not exist everywhere.

It is important not to overlook the condition of the law that the sensitivity for both stimuli really changes in the same proportion, if the constant difference in the sensation is to be expected afterwards. Set z. That, for example, a similar stimulus meets two parts of the retina of initially equal absolute sensitivity, and is accordingly at first perceived by both as equal; Now if the absolute sensitivity only of one changes in plus or minus, then, even if the parallel law is valid, a difference of sensation will immediately arise between the two, and this must grow in the same way as the difference of absolute sensitivity increases, as if it were the same Sensitivity of both the stimulus that hits one place grows, while remaining constant for the other.

All questions and relationships discussed here in relation to intense sensations find their application just as extensive. Here, too, one may ask: given differences in expansion are most easily felt when and where given expansions appear greatest; and goes therefore the just significant difference in extent with the just noticeable extent in parallel?

All that will now be dealt with in the following, as far as there is experience in it, which certainly by no means offer the desired connection and the desired completeness in order to be able to pronounce a general, simple, and nice result. However, it can generally be said that a substantial dependence between absolute and difference sensitivity is directly negated by the subsequent proof of the parallel law in the field of weight tests.

Also, the probation of Weber's law itself does not seem to be able to succeed otherwise than under the accession of the parallel law, and to say so in solidarity with its probation. For in the course of experiments, because of the constant or repeated and altered action of stimuli, irritability or absolute sensitivity must necessarily change, and it does not appear that a proof of Weber's law could succeed by a scale of different degrees of stimulus, if not at the same time Transferability of the same to the internal effect or a parallel law.

I think that this indirect argument is very binding. Meanwhile, it does not exclude the requirement of more direct probabilities, of which I will quote what I have to offer.

l) weight tests.

A 32-day one-handed series of experiments (June and July 1858) with $32 \cdot 8 \cdot 64 = 16384$ Uplifts were carried out with one and the same main weight $P = 1000$ grams and two additional weights $D = 40$ and 80 grams, which were changed from one day to another under the normal conditions given in (chapter 8), apart from the varied duration during which a weight was lifted; namely following 4 uplift times were $^1/_2$, 1, 2, applied 4 seconds and the 64 elevations with the left hand, made as much with the right one behind the other at each of these 4 uplift times at each of the 32 days of the experiment, each such figure $8 \cdot 64 = 518$ lifts included. Now each time at the 4 second lifting time a strong feeling of fatigue of the hand came in because the main weight of 1 kilogram had to be held in suspension for so long, of which I at the lower lifting times, even at two seconds (what I mean explicitly directed my attention), nothing noticed. Now this would fatigue had an influence on the difference in sensitivity, then had to this in the correct numbers and from this to (ch. 8) derived hDvalues which, when the held constant $D$to provide the measure of this sensitivity, to make it noticeable; and in general the duration of the elevation has an influence on the sensitivity of difference, for even in the case of fatigue which is not clearly felt, a longer period of uplifting must take greater advantage of the force and thus relax than a shorter one. But such influence does not emerge from my experiments. Because I got summed up for the 4 main cases following correct numbers r (for $n = 2048$ at the special numbers) and following, the difference sensitivity proportional, values 32 *hD* in total for both *D's,* and 64 *hD*in summa for left and right, in the different periods of uplifting, it being still to be noted that each of the 4 uplifting periods has formed the beginning and the end of a test day equally often [2)].

[2)] The correct numbers *r* and values 32 *hD* do not run parallel with each other in the following table, which has the reason given in (chapter 8).

$n = 2048.$

raising time		½	1	2	4
r	Left	1541	1507	1496	1546
	Rights	1561	1502	1483	1551
Total		3102	3009	2979	3097
32 *h*	Left	159509	161316	155271	183353
	Rights	192175	172139	168915	175337
64 hD		351684	333455	324186	358690

This series can also be used for the experimental proof of our accounting rule (see Chapter 8). It gave the following values of 32 hD in summa for L. and R., and for all 4 lifting times according to our invoice rule:

at $D = 0.04$ P the value 454399

- - - 0,08 - - -

913613

So in the double *D* double the *HD.*

If we consider the sums 64 *hD* below , which contain the combined result of all experiments, then if the uplifting time and the fatigue dependent thereon made a difference in the value of the difference sensitivity, the greatest difference between the lifting time ½ and 4 sec. but the values 64 *hD* are noticeably the same, and not very different for the *split times* .

This result becomes more important because the duration of the uplifting has by no means been shown to influence the estimation of the weights without any influence , since the constant influences *p, q* have suffered striking changes, for the average value of them (cf. Chap. 8), in grams as follows:

raising time	left		right	
	P	*q*	*p*	*q*
½ sec .	+ 6.73	- 3,17	+ 31.49	+ 6.28
1 sec	+13.07	- 19.46	+ 43.38	+ 3.30
2 sec.	+12.38	- 16,00	+ 38.05	+ 0.36

4 sec.	- 7.95	- 3,28	+ 3.43	+ 6.04

So while at 2 seconds lifting time with the left the first-lifted vessel appeared heavier by 12.38 grams than the second-lifted one, at 4 seconds lifting time it appeared 7.95 grams lighter. When lifted with the right, the change showed the same direction, but without going to the envelope.

It is not disputed that the values of p such as q are almost the same for 1 sec. And 2 sec., But differ greatly for ½ sec. And 4 sec. But this is explained by the fact that at ½ sec. Of elevation the uplift occurs with a kind of rapid jerk, which is not quite comparable to the calm uplift in the longer uplift, while at 4 sec the strong feeling of fatigue causes incomparability.

The previous result had occasionally been obtained from a series of experiments for other purposes. In order to increase the influence of fatigue more than in the previous series, I have recently (in Jan. and Feb., 1859) deliberately directed another series under normal circumstances (apart from the period of upliftment) to this end, by adding two applied stronger weights. Unfortunately, this series has remained a fragment for reasons to be stated below; but the result of this fragment deserves praise as an enhancement of the results obtained elsewhere.

As a whole, this fragment deals only with 16 · 64 = 1024 elevations, divided into 8 days, each with 2 divisions of 64 elevations, which were made each day with the left one after the other, while I always alternate with the left and the right. Two *P's were* used = 1500 and 3000 grams, between which was changed after every two days; *D* was both necessary 0.06 *P* . For two consecutive days, the same *P* and *D were* retained; but changed from one day to another between the following two test conditions:

a) duration of each simple uplift 1 second, split time between each two double raises 5 seconds (here the usual normal times).

b) Duration of each easy lift 4 seconds; Intermediate time between each two double elevations 3 seconds.

Thus with b), the time during which the weight had to be kept in suspension, four times as long, and the time to rest between two double italics in the ratios of 3 **:** 5 is shorter than in a).

This difference also had the result that on b) on each day of the experiment a very strong feeling of fatigue in the raising hand (of course *P* = 3000 even stronger than *P* = 1500) occurred, which was not the case with a) , But for yet joined b) a pain in the spleen, to some extent on the very first days of the experiment, which was basic *,* (to limit myself daily on two departments since I which otherwise always 8 to 12 place) and the new with every b-trial days more and on the 4th became so strong [3)]that I was having a hard time completing the two experimental divisions, which kept me from continuing the series of experiments. Because it was my intention to repeat it with right-hand and zweihändigem process and at least $1\ ^1/_2$ continue month.

[3] This pain lasted for weeks, so that I got even permanent disadvantage. A mustard plaster seemed to do a good job against it.

Now, according to other experiences, I find by no means sufficient reason to establish a very sure result, and especially in groups of this number no security can be sought for; however, it will be seen below that $P = 1500$ gives an overweight of hD for a), whereas $P = 3000$ for b), and in the overall result of hD *there is* only an insignificant difference between a) and b), but a very large fatigue difference took place, which is also in a strong differences of hp expressed, we can not influence the fatigue on the differential sensitivity here *h*behold. '

I give, since it can happen here without much inconvenience, the whole specification of the numbers r from which to draw the results.

	$n = 64$				$n = 256$
	r_1	r_2	r_3	r_4	Total r
$P = 1500$. a)	52	46	58	45	201
- - - b)	52	39	48	51	190
$P = 3000$. a)	55	43	51	57	206
- - - b)	46	61	33	60	200

It can be seen, however, that here b) in the sum values r at $P = 1500$ and $P = 3000$ there is a smaller r than a); but at $P = 3000$ this depends only on the influence p, which has grown exceptionally by the fatigue , and the calculation makes hD larger for b) than a) at $P = 3000$.

In fact, if one carries out the calculation by means of the fundamental table according to the rules given (see Chapter 8), one finds the following values:

	4 h	4 hp	4 *hours*
$P = 1500$. a)	23460	+ 7726	- 2724
- - - b)	18879	+ 5023	- 2405
$P = 3000$. a)	25335	+ 1649	+ 3803
- - - b)	27071	- 18327	+ 4821
Total a)	48795	+ 9375	+ 1079
Total b)	45950	- 13304	+ 2417

Summing up, the values $2hD$ in the experiments without and with strong fatigue = 48795 **:** 45900; which deviates less than would have been possible after unbalanced contingencies. The influence p has only slightly lost at $P = 1500$ by the fatigue of positivity; At $P = 3000$, however, the envelope became severely negative due to the much greater fatigue. The influence q is too small here against D and p *in* order to add some certainty to its purpose.

In the previous two series of experiments fatigue was caused by prolonged lifting time of the weights in the experiments themselves. I have made two other long and tedious series of experiments, where it was effected by previous fatigue. The first, probably because of a subsequent circumstance, has not given decisive results; whereas the second is to be regarded as a firm confirmation of our law.

The first of these series (January to March 1856), which had other purposes than to establish the influence of fatigue, was a one-handed, with left and right especially executed. The emphasis P was lasting 1000 grams. The additional weights D were applied daily 5, 15, 20, 30, 40, 60 grams. 72 trial days, each with 640 uplifts, of which 64 come to each D with Linker and just as much with Right. Each of the 5 *D's* formed the final section of the day after the series; but since 5 in 72 does not work out, and therefore some *D's* do not form the final section as often as others, the experimental numbers are proportionately reduced, as if every D with right-hander as left-hander would have formed 8 times a final detachment of 64 uprights, and thus had engaged the whole series for 80 days.

After each day the 640 upliftments were carried out one after the other, each time on the same day a great fatigue of the arms was carried out, and now an additional division of 64 lifts was added, which is nothing but a repetition of the last section at 64 last used D was to draw a comparison between both divisions, the first without, the second after fatigue. So (assuming the above reduction) for each of the 8 *D's* 8 departments of 64 lifts with left and just as much with right in the tired state for comparison with departments staffed immediately before the fatigue. In addition, the results of the departments in the fatigued state can be compared with the results of the entirety of the departments obtained on the same test days before the fatigue.

The way in which the fatigue was carried out is closer to me in the reports of the Saxon Soc. Was 1857. pp 113 et seq., By these fatigue tests were zugleichzu exercise tests in regard to the muscle strength that I have shared here [4] . Here it suffices to say that two weights of lead, each weighing about nine-quarters pound in weight, were raised from the lowered position over the head, and lowered again, until the farther elevation of the stroke was impossible; which succeeded in the progress of the experiments through ever longer time. Each uplift lasted 1 sec, every cut 1 sec. About 1 minute after the end of the last oscillation, during which the general excitement arose and sometimes the pulse was counted, I immediately went over to the repetition of the last section.

[4] Perhaps it is of some interest to note that the strong progress of the exercise which had asserted itself in that series of experiments in January to March 1856, when the uplift was repeated on two consecutive days in Oct. 1858, after no exercise had taken place, it had noticeably disappeared. The earlier series began with elevation numbers 104 and 128 on the first two days of the

experiment, and consequently rose to a maximum of 692; on 19 and 20 Oct. 1858, experiments 122 and 118 were obtained with the same method of employment.

Here is the statement of the values *hD* under the column *z* in the fatigued state, under *u* in the unwearied merely calculated from the divisions which preceded the fatigue operation each time, under *U* from the totality of each trial day before the fatigue; all derived from fractions with $n = 64$, distinguishing the 4 main cases 5) :

hD

D	left			right		
	U	*u*	*z*	*U*	*u*	*z*
15	2854	2447	3890	4044	2984	4822
20	4809	3349	4937	5698	4334	5801
30	7171	6570	4400	7593	7776	8233
40	8980	10485	11108	9052	13054	11693
60	13092	12352	11464	12112	14056	16470
total	36906	35203	35899	38499	42404	47019

5) Although this series was principally used to study the influence of the size of *D* , it has, despite its size, given much less regular results in this regard than other smaller series, perhaps due to the interposed fatigue itself; but the values under *z* in the right, with the exception of $D = 15$, give very good tuning values , that is, the *D's* proportional values. It has to be taken into account that for small *D's* only certain results are to be expected for very large numbers of experiments.

If we consider the sum results below as definitive results, we see that in the left the values *hD* and therefore *h are* noticeably the same with and without fatigue, whereas in the right not only in the sums, but in all individual values (with the exception of of *U* at $D = 40$, which is obviously too large) an overweight of the values under *z* over which under *u* and *U points* out. Namely, the values under *z* in the right much more weight than the left, because they are the *D's*as is normally required, whereas those on the left are quite irregular, indicating strong disturbances. In particular, on the left the values under z at $D = 30$ and $= 60$ are evidently too small, both in relation to the remaining values under *z* for the left, and for the rights corresponding to them, and if one excludes them, it shows In the case of the left *z* , too, it is predominantly *u* and *U.* It seems, therefore, to emerge from these experiments with a certain degree of certainty. that the difference sensitivity for weights is increased somewhat by a fatigue operation driven as far as possible.

In the meantime, the difference in relation to the preceding severe fatigue does not appear to be significant, nor in regard to what the left gave, what the previous series gave and the following series gives, unambiguous enough not to be able to postpone it to a subsequent circumstance ,

But before that, it will not be without interest to specify the values *p* and *q* for this series as well . They were on average for the trials at all *D's* in grams:

	P		*Q*	
	left	right	left	right
U	- 15.15	+ 7,88	- 17,50	+0.20
u	- 21,76	- 7,28	- 13.23	- 2.73
z	- 35.81	- 13.92	- 14.69	+ 0.72

According to this, the influence *p* in the transition from *U* to *z* in the left increased by 20.66, in the right by 21.80 grams, that is, by a great deal in the right, but reversely in the negative Influence of the fatigue operation on the proportions of the estimate proves, and at the same time a remarkable example of the way in which such changes occur. Already in the transition from *U* to *u* one notices a change in the same direction, in that *,* with regard to the final section, there is already a certain fatigue in relation to *U,*which refers to the means of all experiments. (I did not particularly investigate the initial values.)

As for the incidental circumstance in question, it might lie in the following:

The swinging of the weights not only fatigues the muscles, but at the same time excites the whole organism, as evidenced by a tremendously increased pulse, which was always so fast and small immediately after swinging that I was unable to count it ; but once in a while found up to 150 punches and more per minute. On the other hand, the lifting experiments with the vessels of 1 kilogr. Weight under normal circumstances for about 1 hour before the violent fatigue operation or without subsequent fatigue by no means have the same multiplying influence on the pulse. On the contrary, less than 29 experimental days of the present and a neighboring series, where I determined the pulse just before and immediately after the experiments (in the same body position and under the same posture of the arm), the pulse was 21 times more common than after the experiments and averaged 87.8 before, then 85.2. This reduction may be due to the slow steady cycle of the process, and a faster clock may have given a different result.

What supports the supposition that an excitement indicated by an increase in the number of pulses exerts an increasing influence on the sensitivity of difference is the following experience: During the previous series of experiments, my pulse was very variable by days, which is well in the very uniform way of life which I lead it could only have resulted from the fact that the daily repeated violent fatigue operation extended its influence over the entire duration of the experimental series, but in a variable manner. Unfortunately, as the consideration of the pulse did not affect me until later, in the earlier parts of the series I failed to identify and record it; but it has happened in the last 14 days. Immediately before the beginning of the daily morning observation hour he was counted, and at the end of it, before fatigue, again and taken the means. Let's put these 14 remedies together with the right numbers of the 14 observation days, which remained comparable on all days[6)], there is no exactly corresponding course, but nevertheless a clear overweight of the correct numbers as a whole for the days with a larger number of pulses. Namely, according to the size of the correct numbers *r* , I obtained the following corresponding values (for *n* = 640)

r	Pulse number [7)]	*r*	pulse rate
411	75.87	446	81.75
416	95.5	453	88
431	84.5	457	86.5
434	79.25	463	90.75
438	74.25	471	96.5
439	75.87	483	93.65
440	88.25	487	82.5
Summa 3009	578.49	3260	619.65

After that, the average for the 7 days was the lowest *r*

$r = 429.9$, pulse 81.92

and for the 7 days with the highest *r*

$r = 465.7$, pulse 88.52

the mean temperature of the first 7 days during the observation period was 15 °, 21 C., that of the last 16 ° C.

[6)] The correct numbers are added below for all 4 major cases and every 5 *D's of the* same day.
[7)] The book values of the pulse number are due to the middle education and therefore that the pulse number is partly determined from a count by a few minutes and reduced to 1 min.

It is not disputed that the number of cases is not large enough to secure the result; but a similar success in the following series reinforces the result of the previous one, as I quote below.

Since the result of the previous series was not decisive in relation to the question of our law, I made another comparative series of experiments with and without fatigue under another form (Nov. 1858). It was a 16-day, or, with the trial days off, since only one day at the other attempts were made, 32-day two-handed series of experiments. *P* = 1000, *D* = 60 grams, normal circumstances. Total number of lifts $16 \cdot 10 \cdot 64 = 10240$. Fatigue was here carried out with slower elevations of the heavy weights in a manner to be given below, so that in very severe fatigue the number of pulses increased less without comparison than in the previous series; The fatigue operation was not only applied on both arms together, as in the previous series, but also unilaterally on each arm, and compared the results. The whole setup of the experiments was this.

On each test day in the morning, after 1 minute of pulse counting, 4 4-handed departments of 64 elevations were started; in it the pulse again counted by 1 minute; then one arm alone tires; and then 2 two-handed divisions of 64 each, just like those before fatigue; then the other arm just so fatigued; then again employed two two-handed departments; then both arms tired together, and again employed two such departments. After each department, the pulse was counted again and again, but only by ½ minute each time, and the accumulation of cases was made only before the new fatigue, so that after each fatigue the two related departments are only interrupted by ½ minute pulse counting.

On the whole, so daily 4 sections of 64 were used without fatigue, 6 after fatigue. The 4 before fatigue made the beginning, the 2 after two-handed fatigue the end of each trial day; between the two, the four departments come to one-sided fatigue. In the process, both arms were changed in such a way that, if on one day the left was first fatigued, on the next day of trial it happened to the right.

The interval between fatigue and the beginning of the elevation of the vessels was only half a minute of the pulse payment, with a few seconds more to remove the lead weight or the lead weights and transition to the vessels. The fatigue itself was carried out as follows.

In unilateral fatigue, a lead weight of 9¼ lb. in weight was slowly raised from the lowered position to the horizontal of the shoulder height for 4 seconds after the counter and lowered again for 4 seconds. The arm was in front of me, not laterally, outstretched. This was repeated so often, until the uplift did not go away; then ½ min was paused, and again tired, until it did not go, so 5 times behind each other, each time with ½ min. I count these 5 fatigues (fractions) as a single fatigue operation. The purpose of the slowness of the uplift was to increase less the number of pulses, to add repetition, fatigue. The number of possible uplifts decreased in these 5 fatigue fractions of the same fatigue operation due to the cumulative fatigue, greatly from the first to the second, only little in the following fractions. The left man was able, especially from the beginning of the series of experiments, to carry out considerably less uplift than the right, but gradually approached it in the course of the experiment; otherwise exercise was practiced. The simultaneous fatigue of both arms occurred in much the same way as that of one arm, except that two weights of 9¼ pounds were raised simultaneously. Since the left was weaker than the right, the aim was to set the goal with both arms, always by the predominant fatigue of the left, as the immediate feeling revealed. otherwise exercise was practiced. The simultaneous fatigue of both arms occurred in much the same way as that of one arm, except that two weights of 9¼ pounds were raised simultaneously. Since the left was weaker than the right, the aim was to set the goal with both arms, always by the predominant

fatigue of the left, as the immediate feeling revealed. otherwise exercise was practiced. The simultaneous fatigue of both arms occurred in much the same way as that of one arm, except that two weights of 9¼ pounds were raised simultaneously. Since the left was weaker than the right, the aim was to set the goal with both arms, always by the predominant fatigue of the left, as the immediate feeling revealed.

It would perhaps have some interest to specify the conditions observed in these methodologically and on all days comparable to fatigue, and I also have reason below for some information in this respect. What is essential here, however, is that in each of the three daily fatigue operations the fatigue is repeated five times, with half an hour intervals, and ½ minutes after the end of each operation, the fatigue has been passed to the elevations of the vessels. Moreover, as far as alternately after days fatigue was first carried out with the left and right, I notice that fatigue of one arm did not exert any diminishing influence on the weight of the heavy weight which was obtained (about 12 minutes after) with the other arm .

With regard to the pulse conditions, the following is to be noted: It is a well-known fact that the pulse frequency is instantaneously increased by strong physical exertion; but it was strange to me, and it seems to me not without interest, that the increased pulse frequency caused by the violent exertion stretched to some extent through the trial-free days, and therefore was found on the trial days before the experiments, that this permanent increase during the experimental month grew and that it continued to sustain itself long after the end of the series of experiments, decreasing very gradually. It was this, in the course of the experiment more and more growing pulse rate a main reason for me to foresee the further continuation of these fatigue attempts, which I had intended with a certain modification. The less I believed that I could tolerate another increase of the pulse without disadvantage, when I began to feel that my head had been attacked by the violent fatigue operations; which was not too great a miracle, since the blood was strongly driven to the head at the last laborious elevations of every operation, and this belongs to the weaker parts after a previous affliction, whereas my very healthy breast felt no disadvantage. This attack of the head was manifested in a vaguely indefinable feeling, and some amplification of the eardrum I was habitually suffering had no lasting consequence after the series of experiments had been discontinued. that my head was attacked by the violent fatigue operations; which was not too great a miracle, since the blood was strongly driven to the head at the last laborious elevations of every operation, and this belongs to the weaker parts after a previous affliction, whereas my very healthy breast felt no disadvantage. This attack of the head was manifested in a vaguely indefinable feeling, and some amplification of the eardrum I was habitually suffering had no lasting consequence after the series of experiments had been discontinued. that my head was attacked by the violent fatigue operations; which was not too great a miracle, since the blood was strongly driven to the head at the last laborious elevations of every operation, and this belongs to the weaker parts after a previous affliction, whereas my very healthy breast felt no disadvantage. This attack of the head was manifested in a vaguely indefinable feeling, and some amplification of the eardrum I was habitually suffering had no lasting consequence after the series of experiments had been discontinued. whereas my very healthy breast felt no disadvantage. This attack of the head was manifested in a vaguely indefinable feeling, and some amplification of the eardrum I was habitually suffering had no lasting consequence after the series of experiments had been discontinued. whereas my very healthy breast felt no disadvantage. This attack of the head was manifested in a vaguely indefinable feeling, and some amplification of the eardrum I was habitually suffering had no lasting consequence after the series of experiments had been discontinued.

The gradual increase in pulse rate during the series of tests was now associated with a gradual increase in the correct numbers r in the 4 departments preceding fatigue surgery on the trial days, but again by no means regular; but quite clearly in a mutual dependence. After all, having been trying weights with weights for several years, this can not be made dependent on the progress of the exercise.

Here is the compilation of the number of pulses with the correct numbers for the 16 trial days of the actual test series, divided into two fractions I, II. Added are the numbers for 7 preliminary and 2 post-trial days, which are related to the pulse and fatigue 4 Experimental departments are quite similar to those of the 16-day series, whereas the subsequent fatigue was made under other forms, and mainly only for a preliminary and subsequent orientation, therefore the results obtained after fatigue following are not listed. On the first two of the seven preliminary trial days, only the pulse before fatigue is counted, but no uplift attempts are made with the vessels to test the sensitivity.

7 preliminary trial days.

date	Pulse before fatigue	r *(n* = 256)
Oct 19	87.5	no verse
20. -	85.5	no verse
Oct 21	89.25	154
23. -	91.5	155
25. -	97.5	165
27. -	102.5	152
29. -	81	153
medium	92.35	155.8

16 main trial days.

I			II		
date	Pulse before tired.	*r* *(n = 256)*	date	Pulse before tired.	*r* *(n = 256)*

Nov. 1	97	158	17th Nov.	86	172
3. -	93.75	163	19. -	100.5	199
5. -	103.5	204	21. -	89.5	178
7. -	75.5	180	23. -	94.5	191
9. -	97	169	25. -	103.5	198
11. -	87.5	165	27. -	102.5	177
13. -	91	177	29. -	94	191
15. -	95	183	Dec. 1	107	183
medium	92.53	174.9	medium	97.19	186.1

2 additional trial days .

date	Pulse before fatigue	*r* *(n = 256)*
3 Dec	98	175
5. -	100.5	170

If one takes the mean for the *7* smallest, 8 middle and 8 highest values of *r* with the corresponding pulse, then one has (with the addition of the mean test temperature)

r pulse temp.

157.1 93.1 16.6 ° C
172.9 95.5 16.7 ° -
191.1 96.7 16.2 ° -

After the 5th of December, when the last fatigue took place, I did not count the pulse again, until on the 19th of December I began a new series of experiments without fatigue operations, ie the one- and two-handed ones with *P* = 2,000 and 3,000 grams Results (see chapter 9) are given. Now, as I was examining the pulse before and after the trial of each day (and the next row), I still found an unusually high pulse rate, regardless of the fifth . to 19. Dec. no attempts at all place had seized. This frequency subsequently decreased slowly, but with an average of 8 days, continuously, acting as a means before and after the elevations of the vessels, in the means of 8 days each, as follows:

Pulse Temp.
19 Dec. to 26 Dec. 104,16 [8)]

17 °, 16C.

27. - - 3. Jan. 101.11 16 °, 81 -
4. Jan. - 11. - 98.79 15 °, 49 -
12. - - 19. - 98.78 16 °, 49 -
20. - - 27. - 89.46 18 °, 10 -

28. - - 4. Febr. 87.78 17 °, 18 -

8) Following are the numbers of each 8 days. 92.75; 109.5; 103.5; 106; 107; 113.5; 97; 104th

In this subsequental series of tests, too, a compilation of the pulse numbers for the 16 largest and 16 smallest correct numbers, taken together at comparable experimental circumstances, produced one, but only slight, advantage for the larger pulse numbers; namely in the whole (at n = 8192)

r	**mean. pulse rate**
5732	96.88
6147	98.18.

According to this, a certain connection of the increase of the numbers r with the increase of the pulse rate seems at least very probable to me .

All the following refers again only to the 16 main trial days of the series, which concerns us now first. The pulse rate before and after the first four experimental sections, which preceded fatigue, in particular, was as good as the same, namely in summa for the 16 days before 1517.5, after 1518; 94.84 before, then 94.88 before, so that the elevations of the vessels of 1 kilogram. Weight expressed no influence on the pulse.

The pulse immediately after the three fatigue operations (reduced from ½ min. To 1 min. , Divided by two test sections) was on average

	After 1st fatigue	after 2nd fatigue	after 3. fatigue
In I.	100.4	106.5	112.1
In II.	108	105.1	115.4

Thus, after comparison with the table above, the pulse after the first fatigue operation had only increased by about 8 to 10 strokes, a little more towards the following, as opposed to the tireless state, at any rate little against the tremendous increase in the previous row. Here, too, there was an overall increase from fraction I to fraction II. Finally, the mean pulse numbers, (1), (2), after the 1st and 2nd experimental sections, which followed the fatigue operations, were as follows:

	1. fatigue		2. fatigue		3. fatigue	
	(1)	(2)	(1)	(2)	(1)	(2)
I.	96.5	97.5	98.3	97.5	104	89.9

II.	100	99.9	100.9	101.5	101.8	101

So much about the pulse rates.

Since in the previous series of experiments, despite the exceptionally strong momentary increase in the pulse due to the fatigue operation, only a not very strong, not even unambiguously dependent, increase in the numbers *r* and corresponding increase in *hD* had occurred, then the relatively slower momentum was possible Increasing the number of pulses in the current row is the less expected; and thus the influence of fatigue more purely. Here is a comparison of the results in this regard before and after fatigue. All results are on 8 *hD* reduced, but derived from fatigue from the double values, and specified for the 4 divisions according to their time sequence on each day, in the data for fatigue for left and right is distinguished by First and Second, whether the hand in question was the first or twice fatigued was

8 hD **before fatigue .**

1st Dept. 28096

2nd - 35273

3rd - 32613 4th - 30930 Means 31727,4

8hD **after fatigue of the following hand.**

	left	right
First. 1st Dept.	34681	26760
- 2. -	30063	31288
Zuzweit. 1st Dept.	30888	40731
- 2. -	34602	30175
medium	32558	32239

8 *h* **after fatigue of both hands.**

I. Fraction	1st Dept.	26425
	2. -	31322
II. Fraction	1. -	30932
	2. -	30827
	medium	29877

It can be seen that the results before and after fatigue do not differ in any way that can be considered, that the parallel law is well established.

On the other hand, the values 8 *hp* , 8 *hq had* suffered most significant changes due to fatigue *(hp* as usual in the negative direction); However, the details of which I could pass over, since the sharing and discussion could not happen without circumstance.

Only the following point I believe to have to cite as unexpected. Since, generally speaking, one feels a heavier burden than usual after fatigue, it seemed likely that after one-sided fatigue this would also be unilaterally asserted, and consequently in the sections after one-sided fatigue on the left, in a positive, one-sided fatigue of the right negative sense against the departments would change without fatigue changed; and this change had to be expected most strongly on the days when the fatigue of the hand in question took place more than that of the others, that is, if a previous fatigue of the other hand had left no counteraction, the strongest in the first, the fatigue the next lying portion (half) of the section following fatigue (after ½ min. The examination of this portion, however, gives the result that in both cases, after one-sided fatigue of the right as well as the left, has changed in a positive sense, only after fatigue of the left is incomparably more than after fatigue of the right. Even after the two-sided fatigue that forms the end, hq of the first portion changes in the positive direction against the tireless state, but less than after one-sided fatigue of the left, more than after unilateral fatigue of the right. This, in my opinion, is to be interpreted that way. Fatigue had a general influence of the kind that hq grew in the positive direction; this was increased by the one-sided fatigue of the left, diminished by that of the right. On which that general influence rests is unknown;

All previous results related to temporal changes in sensitivity due to fatigue. For the question to what extent parts with greater absolute sensitivity for weights possess at the same time greater sensitivity to difference, one may consider experiments by EH Weber; by the results he according to the method of the just noticeable differences [9] , compares received at given parts with which he at the same parts by the method of equivalents [10] received; if the former method refers to the difference sensitivity, the latter to the absolute sensitivity.

[9] *Progr. Coll.* P. 96.
[10] Ibid. p. 97th

If a column of 6 special-easterers were placed on the following parts of each of the two sides of the body, the difference in weight was felt; if on one side of the body the following number of species were taken away, which hereby designate the just noticeable difference:

Volar area of the fingers 1
sole of the foot,
capit. metatars. 1
shoulder blade 2
heel 3
back of the head 4

On the other hand, they were equivalent to each other, ie, the following weights were found to be equally heavy in ounces in the following parts:

Volar surface of the fingers 4 and sole of the foot (cap metat.) 10.4

- - - 3 - shoulder blade 8
- - - 4 - heel 8,8
- - - 4,5 - back of the head 5

It can be seen that there is not the slightest correlation between the two scales. The difference just noticeable on the finger and the sole of the foot is the same, while the weights perceived as the same behave on both parts as 4 and 10.4. Conversely, the weights perceived as the same on the fingers and back of the head are almost the same, while the just noticeable difference is like 1 **:** 4.

It is undisputed that such attempts can only be considered decisive if they are made under strict comparability of circumstances; which is not to be presupposed here, since the intention was not directed to a comparison of the results of both methods, and the experiments are carried out at different times, perhaps also on different persons; However, it is hard to imagine that in a truly parallel course of absolute and differential sensitivity such discordances should be possible at all.

2) Experience in the field of light perception.

In the field of the sensation of light there is still a lack of direct attempts to what extent the parallel law is valid; but there are many facts which are related to the question of it, and are here to be discussed with reference to it, partly wondering if and how they exist with the law, and partly how they may serve to confirm it; Explanation obtained by it.

First and foremost, one may be inclined to assert a well-known fact against the parallel law, which was already touched on in the chapter on Weber's Law, but whose further discussion is postponed here. By staying in the dark for a longer time one gains the ability to see in the dark, but by staying longer in the light one loses this ability. But what does it mean to see in the dark? A light, which differs photometrically only little from the dark, but still different. In fact, this is not just an absolute impression, but a difference; because the night darkness also has its photometric value. It would seem, then, that fatigue of the eye through the light stimulus also dulls the sensitivity to differences.

Notwithstanding the fact that notoriously no detailed evidence is required, I here summarize a number of things about what makes it stand out among particularly striking or interesting forms.

"Buffon tells us that an officer in a jail, which seldom had light from above while food was being served, was able to see the mice after a few months, and after a few months of freedom he had to be very slow A person who has been imprisoned for 33

years could see the smallest objects at night, nothing during the day (Ruete, Ophthalmol., according to Larrey Mém. de Ohir., méd., vol. I. p.6). "

V. Reichenbach states in his writings about the so-called Od that certain persons, so-called sensitives perceive flame-like light phenomena in the perfect darkness at the poles of strong magnets, blue and blue-gray at the north poles, red, reddish-yellow and red-gray at the south poles, that they also see the tip of crystals, living human, animal and vegetable bodies, especially the fingertips, metals, sulfur, liquids, which are in chemical or crystallization acts, etc shine. Finally, the author comes to the conclusion that all the bodies of the earth at all spend light in the dark, perceptible for the sensible, some only more, the others less.

It is not the place to ask here to what extent Reichenbach's Od as a special agent has a reality; In his experience of the light perceivable in the dark by some persons, nothing seems to stand in my way; But here I mention them by name only in so far as Reichenbach, as an express condition of the perception of light, not only gives an absolute obscuration of the observation-room, but also a longer stay in it in the less sensitive, before anything can be seen. According to him, in the complete darkness, the highly sensitive often not immediately or immediately after 5 to 10 minutes catch the middle sensitive after only half to two or three hours.

I myself and older people remember quite well that used to dusk for evening lighting at the family and desks with a Talglichte. Now, after the brighter lamp-lighting has become commonplace, this is considered an eye-ruin; one can no longer see without effort.

The following has been told to me of a factory furnished so that part of the work was done by the workers at home. The earlier poorer lighting in the factory was substituted for a lighter one. It was not long before the workers demanded the formerly poorer lighting because they could not cope with the usual dim lighting they could get home.

Aubert in s. Contrib. Z. Knowledge of indirect vision [11]notes, "If you spend days in a darkened room, you estimate it as bright as a ten times brighter room was formerly valued." I myself have seen a striking example of it when, at the age of fourteen, I am suffering from measles For more than eight days in such a darkened room that the entrants were caught in it as if in the dark, after a few days it seemed very bright to me, and as boredom plagued me so much, I resorted to a rather small map of fine writing; I was able to see the colors quite well here and read the fine writing everywhere as well as usual with ordinary daylighting.I also brought books to my bed, but was never caught with them, because the newcomers did not see the book at all, even if they did had been in the room for a few minutes.I notice that my eyes were not affected by pathology. "

[11] Moleschott, sub. IV. P. 224.

Forester [12]noted regarding the application of the in chapter 11. When one looks at the weakest illumination, a small black rectangle is still recognized on a white *background (p.13): "At the beginning of the examination, everyone, unless he has previously avoided any brighter light impression for a long time, needs one If the observer only looks at a brightly illuminated surface or even at the light flame for a second, his visual acuity has already dropped by a few degrees for the next few minutes, until a second one Rest by clearing out the brighter light the energy of the retina raises again, and it is most striking how the center of the retina is particularly easily affected. "*

12) About hemeralopia p. 13. 32.

All these experiences seem to speak directly against the validity of the parallel law in the field of the sensation of light; in that the sensitivity to differences in light is weakened by blunting for the stimulus of light; for, as we have seen, the knowledge of weak light-shining or dimly-lighted objects in the dark is nothing else than a distinction of them from the dark ground; and this no longer takes place with a dulled eye.

But it is easy to see that this deviation from the parallel law takes place under quite analogous conditions, as the deviation from Weber's law at its lower limit. Just as one or both components approach Black, Weber's and Parallel Laws cease to be valid. But we do not claim the validity of the parallel law within further limits than that of Weber's.

The only question is: 1) whether there is a corresponding reason for the lower limit of the parallel law than for that of Weber's; 2) whether the deviation disappears for higher degrees of light just as in Weber's law.

Both can be answered in my opinion. As for the first, I grasp the subject from the following point of view.

The external light stimulus blunts for the effect of the external light stimulus; but the eye-black takes on relatively little darkness; on the other hand, the relative difference of the effect of an external light diminishes. In fact, the black of the eye can only deepen to a certain extent, as is the case with bright objects, but they do not go out; and even with the full black starling, where the strongest external light makes no more impression, black is still seen, and under certain circumstances colors may still be seen. It is also clear that the retina, the nerves, and other parts which cause the transmission of the stimulus to the brain, can be paralyzed or impermeable by a number of causes.

If the inner eyesight does not weaken considerably or at least to a much lesser extent than the external impression by the blunting, this must be equivalent to a relative illumination of the black of the eye with a constant light impression, and to the full black stare, which is considered the highest degree of blunting even though the strongest impression can no longer be distinguished from the black of the eye, because no one is done any more, while the inner eye-black still persists; just as well as by completely dark glasses the difference of the lights with the lights at the same time for the perception disappears.

If this explanation is to be valid, the confirmation may be demanded as follows: the same persons who, because of blunt irritability, see poorly in the dark or twilight, ie distinguish poorly, must distinguish it as well as those with non-blunt irritability in it, if the light impression The components are only strong enough at all that the brightness of the black eye, however, can be considered as vanishing. But that really is so, positive facts can be cited which are all the more proving as they have been published without relation to the above theory and without knowledge of it.

Foresters in s. Treatise on hemeralopia says (p.33): "In the evening, with bright lamp lighting, one eye for a few minutes on a piece of white paper while the other is closed and concealed, in the lighted room one does not become one even when opening the other But as soon as one goes into a very dark room, such a difference is very noticeable: in front of the strained eye, there appears to be a kind of mist which completely or partially obscures the objects which the other eye still perceives. and it is a peculiar uncertainty as to the orientation which comes over us in the dark with fields of vision that function so differently, and which vanishes immediately when one returns to a bright space. In the second series of investigations with Aubert on the spatial sense of the retina, which took place under lamplight, I have often had occasion to notice this artificial monocular hemeralopia. This dazzling condition sometimes lasted 10 minutes or longer. The gas lanterns appeared to the affected eye at some distance like cloudy, reddish oil lamps, and my surroundings were so dark that I could hardly orient myself. With alternate closing of the individual eyes, the difference in energies in both retinas was strikingly striking, but without the unrestrained eye having become more discerning in the dark. In Aubert 1 minute after the end of the effort of one eye, the artificial hemeralopia was so strong that at 24 I've often had occasion to notice this artificial monocular hemeralopia. This dazzling condition sometimes lasted 10 minutes or longer. The gas lanterns appeared to the affected eye at some distance like cloudy, reddish oil lamps, and my surroundings were so dark that I could hardly orient myself. With alternate closing of the individual eyes, the difference in energies in both retinas was strikingly striking, but without the unrestrained eye having become more discerning in the dark. In Aubert 1 minute after the end of the effort of one eye, the artificial hemeralopia was so strong that at 24 I've often had occasion to notice this artificial monocular hemeralopia. This dazzling condition sometimes lasted 10 minutes or longer. The gas lanterns appeared to the affected eye at some distance like cloudy, reddish oil lamps, and my surroundings were so dark that I could hardly orient myself. With alternate closing of the individual eyes, the difference in energies in both retinas was strikingly striking, but without the unrestrained eye having become more discerning in the dark. In Aubert 1 minute after the end of the effort of one eye, the artificial hemeralopia was so strong that at 24 The gas lanterns appeared to the affected eye at some distance like cloudy, reddish oil lamps, and my surroundings were so dark that I could hardly orient myself. With alternate closing of the individual eyes, the difference in energies in both retinas was strikingly striking, but without the unrestrained eye having become more discerning in the dark. In Aubert 1 minute after the end of the effort of one eye, the artificial hemeralopia was so strong that at 24 The gas lanterns appeared to the affected eye at some distance like cloudy, reddish oil lamps, and my surroundings were so dark that I could hardly orient myself. With alternate closing of the individual eyes, the difference in energies in both retinas was strikingly striking, but without the unrestrained eye having become more discerning in the dark. In Aubert 1 minute after the end of the effort of one eye, the artificial hemeralopia was so strong that at 24 without, however, that the unrestrained eye would have become more clear-sighted in the darkness. In Aubert 1 minute after the end of the effort of one eye, the artificial hemeralopia was so strong that at 24 without, however, that the unrestrained eye would have become more clear-sighted in the darkness. In Aubert 1 minute after the end of the effort of one eye, the artificial hemeralopia was so strong that at 24– Mill. Light source hardly the 1.32 mm. wide strokes differed, while the unaffected, as usual with this illumination, still 0.21 mm. Width perceived. With me, the blunting reached an even higher degree and lasted longer. "

These observations related to healthy eyes. Even more instructive, however, are observations of the disease with which Forster compares the condition of the fatigued eye, in hemeralopia itself.

For the twofold case, that after prolonged lingering in the light the eye temporarily sees itself poorly in the dark and, after prolonged lingering in the dark, sees poorly in the light, appears as a permanent condition in two states of disease, hemeralopia and nyctalopia, of which the first Based on accurate observations estimable essay of foresters is present. Förster (p.32) explicitly identifies the state of the healthy retina after bright illumination with the habitual condition of the hemeralopic retina, according to the agreement of the essential features. In several, though not all, cases, even the previous prolonged stay in very bright light has been the cause of hemeralopia (p.30), and prolonged stay in the dark for 24 to 56 hours has been the most effective remedy (p.40).[13], The characteristic symptom of hemeralopia, however, is precisely that, in the twilight, the patients see no comparison worse than persons with healthy eyesight, while in bright light they see just as well. In fact, the hemeralopic distinguishes after dawn or entry into a dimly lit place; where the healthy eye is still quite able to see, nothing more, or needs a greater brightness of the objects, or at the same brightness of a larger circumference of the same, in order to distinguish them, about which forester gives test numbers. On the other hand, according to his statements, which are also based on measurements (p.20, 23): "the hemeralopic, with increasing illumination, daylight, just as sharp small objects as the healthy,

[13] This is also confirmed by Ruete after own experiences.

"Only in some cases, where the disease had lasted for a long time, or where it was of great intensity, did a weakness of the face appear during the day, which manifested itself either in the fact that the patient was very bright in recognizing small objects needed, or even by the fact that he recognized only coarser objects at all. "

The hemeralopic feature is not, as one might think, a matter of the time of day, but according to Förster's observations (p.16) the hemeralopic day looks just as bad at low degrees of illumination as at night. The hemeralopic, as well as the healthy, after entering from the light into the dark, is able gradually to adapt itself to the darkness, so that he recognizes objects which he did not recognize at first; only with the difference that he a) looks worse at first than the healthy, b) much more. (4 to 10 times) needs time to adapt, c) looks worse even after the most possible adaptation than the healthy person after adaptation; What All Förster has stated with the apparatus described on page 275.

With regard to the further, very worth reading, details of the observations on this disease, I must refer to the writing itself.

It would be very desirable if there were just as thorough observations on nyctalopia, which I know nothing about.

With regard to the spatial, it seems that the central parts of the retina, insofar as they are not slightly dull, as they are in ordinary use of the eyes, are more dulled by fatigue than the central ones, both lighter and clearer than the side. But much is still missing in a sufficient investigation of the circumstances prevailing here. A literature of the subject with some relevant observations can be found in my essay: "On Some Conditions of Binocular Vision," in the Abhandl. the Saxon Soc. Mathematics and phys. CL Bd. IV. P. 373.

3) Experiments in the area of extensive sensation.

On several parts, once on the chin and upper lip, another time on the 5 fingers, I have made comparisons according to the method of mean error and the method of equivalents, to determine, if given, as a circle distance greater on a given Skin spot appears, also the difference between two circle distances appears larger; or whether there is no significant dependency in this relationship. My attempts speak against a substantial dependence. Since, however, my observations in this respect are partly not yet fully and partly not yet fully discussed, I pass over the details for now.

XIII. Laws of Mixture Phenomena [1)].

The discussions so far concerning Weber's law, its parallel law, and the fact of the threshold, were basically concerned only with the simplest case of a very general case. It was always a question of how the sensation grows or diminishes, begins or disappears when a stimulus suffers an increase or a decrease, provided that what is growing or taken away is of equal quality as the stimulus which one increases or decreases; Thus, the stimulus of the increase or diminution does not undergo any change in its quality. But in all conceivable cases where a stimulus undergoes an increase or a diminution, a change at all, the case is that the increase or loss is of the same quality as that which which it grows or is deprived, just the simplest. It can be but z. For example, the attraction of a white light, rather than changing or weakening the intensity of all the color beams in equal proportions, can also be changed by adding colored light to the white or the white color mixture to one or another of the color beams or a mixture of color beams is not white, withdraws. And the same can be applied to other than white mixtures of colors, to mixtures of sounds or sounds, to odors, to substances which produce a sensation of taste. For the sake of brevity, let us call the phenomena dependent on such changes, as mixing phenomena, the former as homogeneous,

[1] See above p. 238 note 1).

It is easy to overlook the fact that, when the phenomena of mixing are no longer purely quantitative but also qualitative changes of the stimulus, not only quantitative, but also qualitative changes of sensation are to be expected, as empirically such as these take place, and it will be to bring them with regard to the measurability under aspects which are related to those who have found application for the quantitative changes.

In general, we now find the following:

If two simple or even already composed stimuli *A, B* , each of which is capable of producing a simple sensation of a special kind, respectively, *a, b* , for example, two colors, are presented in such a mixture or even connection of perception, that again simple impression is created the same, so agrees to this resulting impression that these resulting sensation, generally neither the impression *a* match, the *A,* even with *b,* the *B* would have produced for themselves; but it can be as a rule, as *A* or *B*in effect predominates, or the equilibrium holds the two effects, the resulting impression more closely approximates the impression *a* or *b* , or neither predominantly appears in either of the impressions before the other, as it appears, for example. B. in complementary to white complementary colors or orange flowing together yellow and red is the case. If we start by letting *A* alone, then the admixture of *B* must first reach or exceed a certain magnitude, so that the deviation from the pure *a* may become noticeable, and conversely with respect to *b,* if we set *A* to *B* ; and let's raise that, *A* and let *B*interact in such proportions that neither a nor *b* appears predominantly, then *A* or *B* must be increased only in a certain proportion, so that the resulting impression seems to approach the character of a more than that of *b* .

In general, from which simple stimulus or composition of stimuli, and whatever the resulting impression, may be if we add more of another simple or compound stimulus, or take away some of the stimulus, the added or withdrawn becomes one must exceed certain size, so that the simple or resulting impression appears qualitatively changed against earlier.

These are relations which, in the case of the phenomena of mixing, lead us back to the notion of the threshold, which we can briefly contrast here with as the threshold of mixing of the threshold in the homogeneous phenomena previously considered as a homogeneous threshold.

In more detail, the homogeneous stimulus threshold and threshold of difference in the earlier conception are only the simplest special cases of the more general case of the mixing threshold. In fact, when a stimulus *B* adds to the stimulus or stimulus mixture *A* , and one asks at what value of *B* does the additive begin to be recognized as such or to make a difference against the mere effect of *A* , thus, among all possible magnitudes, which *A* may have here, the case can also be thought of, that *A is* zero; then we have the case of the ordinary homogeneous stimulus threshold; no less can under all possible qualities, which*A* may also be thought of as similar to *B* ; then we have the case of the ordinary homogeneous threshold of difference.

If we now add the case that the addition of a stimulus *B* to the stimulus *A* produces in the impression a corresponding to the mere *A* a change which is just palpable or even to a certain extent perceptible, one wonders whether *A* increases or decreases in a given ratio even *B* must be increased or diminished in the same ratio in order to bring about an equally noticeable change of *a* . Should it with arbitrarily different quality of *A* and *B*If this were the case, we would have here, in generalizing the fact of the threshold, a generalization of Weber's law, which is only the case of the general law, where the difference between *A* and *B is* vanishing.

This is still lacking in investigations; but I myself have made some experiments [2)], from which I conclude that, at least for small admixtures of color = *B* to white = *A,* the law is valid in similar limits but also with analogous restrictions, as Weber's.

[2)] Abhandl. the Saxon society. the science, mathemat. phys. Cl. Vol. VS 376.

It is easy to produce only palpable notes of color on white, whether by means of colored pigments, or by setting up a color glass askew against a window on a sheet of white paper. By repeating the experiment and experiment with the cloud-shades I have described, by means of these shades of color, using dark glasses as colorless as possible, I found that with the darkness of the glasses very broad, e.g. For example, up to $^{1}/_{14}$ the brightness of the day, can go down, without the color-marks, which are only noticeable with the naked eye, disappear. But it is always possible to drive the darkening of the eyes through the glasses so far that one disappears with the naked eye visible colors shadows, and from the other side I have even found previously [3)] , and the same has in recent experiments of Helmholtz to [4)] again found that the impression of each color, be it a homogeneous or a mixed one, approaches white at a high intensity.

[3)] Pogg. Ann. L. p. 465.
[4)] Pogg. Ann. LXXXVI.

The deviation from the law downwards, however, could also be only apparent, and be based on an analogous reason, as the corresponding deviation from Weber's law in the homogeneous phenomena. When I look at a shade of color on white with my naked eyes, and see such a dark glass before the eyes, that the white of the reason comes close to the black of the closed eye, then I have the color and the external light, which from outside into the Eye penetrate, weakened in the same proportions, but the black of the eye, which as colorless represents a small degree of white light, has not been weakened; so the excess color now has a smaller relation to white than before, and must therefore be less noticeable.

The reason of the upper limit of the law is unknown.

In reality, strictly speaking, we shall not always be dealing with quite homogeneous phenomena, that is, not with quite pure stimulus threshold or threshold of difference, the very simple Weberian law, but in general with the more general case of the mixture threshold, the law of mixtures; but homogeneous phenomena can be approximated; the consideration of the simplest cases, even if only approximatively producible, is for the time being the most important, and will therefore, later on, preferably remain our focus, especially since there are still few investigations on the legal relations of the mingling phenomena.

Even if one drops the simplest spectrum color into the otherwise obscured eye and asks what intensity it must have in order to be recognized, one is not dealing with a pure stimulus threshold, but with a mixture threshold, since one actually asks what intensity the spectrum color must be in order to make their character as an admixture to the represented by the black eye mixture of all color rays. The question, then, is of the same nature as if one asks in what intensity a color has to be added to the white so that the whites notice one. Except that, at first, it has to do with a very slight, at last, where one speaks of white par excellence, with a great intensity of whiteness or of the color-indifferent mixture, what the color is mixed for. In fact, at first, the black is black as well, nuanced only by a trace of color, and finally the white knows, nuanced with only a trace of color, appears when the color mixed becomes just noticeable.

It would therefore undoubtedly be easier for the ultraviolet to be felt, ie at a lower intensity, than it is when it does not appear in the eye as an admixture to the pale white.

The question as to whether and how a mixture phenomenon changes when all stimulus components which contribute to the mixed sensation increase or decrease in the same ratio is, of course, only a particular question in subordination to the general question as to how the mixed sensation is precipitated and changes when the stimulus components are in any condition and change.

To bring this question to the fore, three main cases appear to serve as clues; 1) if B becomes large enough to change the quality of the mixing impression against a just noticeably when adding to A ; 2) when B becomes large enough that the influence of A disappears, and the impression no longer differs markedly from that of pure b , and 3) when A and B are so balanced that the impression is neither closer to a nor at b finds. Between these three threshold cases, all modifications necessary by mixing A are necessaryand B , and it would then be necessary to find laws which represented these threshold values and the changes in sensation between them as a function of the mixing ratio of the stimuli; but there is nothing up to now, and if a determination of homogeneous thresholds by the experiment can only remain an approximate one, it seems that the more so is the mixture thresholds.

There is an important distinction to be made between the phenomena of mixing, according as the stimuli which give the impression of mixing, even when mixed, touch the organ of sensation, as is the case when compound colors strike the eye, mixtures of sounds or tones strike the ear as It happens in ordinary seeing or hearing, or as the stimuli affect the organ of sensation separately, and only their effects through the mediation of sensory organs themselves form the impression of mixing, as is the case when different colors separate into corresponding places of both eyes or different sounds Falling in both ears. We will briefly distinguish both as conjunctive and disjunctive mixed impressions.

In fact, experience teaches that with two eyes, two ears, by means of separately acting stimuli, corresponding mixed impressions can be obtained, as if the stimuli were already mixed in the same eye or ear, without knowledge of the anatomical and physiological mediation on which this is done based. But the nature of the disjunctive impressions depends on more complicated conditions, and may be co-determined by constraints in a more varied manner than that of the conjunctive. The conjunction of two light impressions *A, B* different in intensity or colorIt is always possible to do this in the same way on the same retina; but the disjunction of these stimuli in corresponding places can be done in infinitely different ways, for example by On the one *A* , on the other *A* + *B,* or on one *A,* on the other *B,* or on one , on the other *B* + and, in the case of disjunction, various relations of the stimuli on both retina may also give rise to impressionable neighbors, which in the case of conjunction on the same retina can not occur; and experience has taught that, due to these differences between conjunctive and disjunctive mixing impressions, differences in the resultant mixing impression may result, according to which the distribution of components to corresponding retinal fibers can by no means be generally replaced by coincidence of the same components on an identical fiber. The ear shows analogous conditions to a certain extent. In more detail I have this subject in my treatise "On some conditions of binocular vision" in the essays of the Saxon Soc. of the sciences, math.-phys. Cl. Vol.