

## ARE THE INTENSITY DIFFERENCES OF SENSATION QUANTITATIVE ?<sup>1</sup> II.

By G. DAWES HICKS.

- § 1. *The relation of qualitative and quantitative.*
- § 2. *The physiological correlate of differences in quality and quantity of sensation.*
- § 3. *Some general points of psychological theory.*
- § 4. *Psychological criticism of Dr Myers's hypothesis.*
- § 5. *Bergson's explanation of the reason why we regard sense contents as quantities, and a criticism of that explanation.*
- § 6. *Meinong's account of intensive quantity.*
- § 7. *Meinong's interpretation of Weber's Law, and criticism thereof.*
- § 8. *Is quantitative comparison always an act of judgment?*
- § 9. *Differences of intensity may be said to be magnitudes but not quantities.*

§ 1. WITH many of the concluding observations in the preceding paper I am in accord. I agree that the question whether intensities are qualitative or quantitative is badly framed, and admits of no intelligible answer. "There is," as Mr Bradley puts it, "no such thing as quantity *merely* extensive, or as quantitative differences without quality. Because anything is qualitative, that is no reason why it should not also have quantity<sup>2</sup>." The quantitative, in other words, is but an abstract aspect of what we are actually dealing with even in that region where its importance and significance are indisputable, and quantitative explanation has always the perfection and the imperfection which attach to abstract treatment. Whatever be its nature, the material world can never find complete expression in quantitative terms. It has, and is bound to have, its own structure or collocation of parts. Though we divest the parts of every shred of qualitative distinctness, though we reduce them to what can be satisfactorily

<sup>1</sup> A contribution to the Symposium presented at the Joint Meeting of the British Psychological Society, the Aristotelian Society, and the *Mind* Association, in London, 7 June, 1913.

<sup>2</sup> *Mind*, N.S. IV. 5.

rendered in quantitative formulae, yet, however far we push this method of procedure, there will always remain at least one qualitative element that cannot be eliminated,—the particular *kind* of distribution which holds good at any given moment. Any such absolute separation as that which Münsterberg<sup>1</sup> would institute between the physical and the psychical, quality being excluded from the one no less rigidly than quantity is excluded from the other, is bound, therefore, to lead to a thoroughly false conception of the way in which the two disparates are related the one to the other. I agree, also, with the remark made in an earlier part of the paper (II, § 12), although I think it not a little damaging to the author's main thesis, that probably a sense-presentation can seldom be increased in intensity without at the same time undergoing some change in quality. Particularly in visual apprehension, the sense-experiences we have to deal with are so complex, the factors implicated are so numerous, that it is hardly conceivable there should ever be changes in intensity only with qualitative constancy.

Further, though the considerations which Dr Myers has adduced should appear inadequate to bear the weight of the hypothesis he would base upon them, few will question the value and interest of those considerations in themselves. Whatever else Münsterberg succeeded in doing in the third of his *Beiträge*, he certainly managed to make manifest the important function fulfilled by kinaesthetic factors in the development of sense-experience, and Dr Myers has accomplished a most useful piece of work in the same direction by showing how from the physiological side those factors call for recognition. He has made it abundantly evident that the prevailing tendency to interpret sensation as though it were a purely cognitive process is a mistaken tendency, and that the complexity of the total process, as involving not merely awareness of a content, but a change in the state of feeling, and consequently specific forms of movement, requires to have more justice done to it.

§ 2. I am not, however, convinced that a case has been made out, even from the physiological side, for supposing that "the ultimate difference between the quality and the intensity of sensation depends on the nature of the underlying reaction." Dr Myers, it will be noticed, passes over with very scant mention the peculiar structure of the end organs and the mode of their development. With "the mapping out of different patterns of response within the nervous system," he does, it is true, in one place (II, § 6) couple the "functional

<sup>1</sup> *Grundzüge der Psychologie*, I. 260 sqq.

differentiation of end organs and peripheral fibres" as a condition upon which the difference of sense quality may in part depend, but in the rest of his investigation the latter seem to disappear from view and exclusive prominence to be given to the former. He is in no way wishful, I take it, to minimise the significance of the elementary fact that the physical stimuli themselves are different, that, for example, ether waves and air waves are essentially dissimilar, nor to suggest that the gradual differentiation of the sensory mechanism has not been largely determined by these external dissimilarities. But what I miss in his treatment is any indication of the way in which he conceives the two factors, (*a*) the mapping out of different patterns of response within the nervous system, and (*b*) the functional differentiation of end organs and peripheral fibres, to be connected with each other. The argument certainly does seem to require that the latter be placed in a position of such decided dependence upon the former as is hardly reconcilable with the biological evidence. The broad general fact that all the organs of special sensation are originally derived from the ectoderm would seem to point in the opposite direction, and the rudimentary stages of (say) the invertebrate eye,—the gradual formation, namely, of groups of pigmented cells,—appear to be naturally explicable from the action of the physical stimulus upon the epithelium. Moreover, I note that a significant change in the presentation of the case with reference to the 'all or none' principle occurs when Dr Myers advances from muscle and nerve to sensation. In respect to the former, the rule is laid down that with a weak stimulus, only a few elements or fibres respond, whilst with a stronger stimulus, other elements or fibres are also implicated. But in respect to thermal sensations, allowance has to be made for the fact that the heat and cold spots are relatively sensitive and relatively insensitive, so that "presumably these reflexly produce relatively considerable and relatively weak reactions," and "we may suppose that the nervous impulse from a more sensitive heat spot spreads centrally and hence efferently to a greater number of nerve fibres than are reached by the stimulus of a less sensitive heat spot" (II, § 5). That is to say, when we take not any one heat (or cold) spot, but any group of such spots, there is a gradation in the intensity of the corresponding sensations, only the important factor then is not increase or decrease in the strength of the stimulus, but the relative sensitiveness or insensitiveness of the end organs concerned. In other words, there has to be recognised at the very outset, so far as sensations are concerned, the essential dependence

of their intensity upon peripheral conditions. Equally so is it in respect to their quality. Dr Myers argues (*ibid.*) that just as with excessive increase in the strength of the stimulus, the extensor reflex suddenly gives place to the flexor reflex, so with like changes in the thermal stimulus, the sensation alters in quality and becomes a sensation of pain. Curiously enough, however, he passes over the question, which is surely relevant to his argument,—“whether the same end organs can give rise to pain and, with weaker strengths of stimuli, to heat, cold and touch.” If different end organs come here into play, and there seem strong reasons for thinking this to be probable, the parallelism which it is sought to establish with what happens in the case of the extensor thrust reflex so far breaks down.

§ 3. The criticism, however, which I am concerned to press is criticism of a psychological kind. At the outset, I had better perhaps refer to certain general points of psychological theory in regard to which, if I understand him rightly, I should dissent from Dr Myers. For the sake of brevity, I group them under the following five heads :—

(a) We are at one as to the necessity of distinguishing in what is called a ‘state of consciousness,’ the act or process of consciousness from the content of consciousness. But this distinction I take to be a distinction of aspects, and not a distinction of two existences. Act and content are not, I should say, what, for example, the Herbartian psychologists inclined to conceive them as being, two independent and separate entities. The distinction does not, therefore, seem to me to be accurately described as involving “two main divisions of consciousness, —the consciousness of ‘acts’ or ‘processes’ and the consciousness of ‘contents’ or ‘products.’” On the one hand, ‘acts of consciousness’ are not necessarily ‘the consciousness of acts.’ Constituting as they do the very life of the conscious subject, they are rather ways *in* or *through* which that subject is aware than objects *of* which he is aware. On the other hand, the content, although fairly enough in one sense called a product, is not necessarily a product in the sense of being wholly a construction on the part of the mind itself. And it may be, I think it is, an error to give to the content the position of an independent object, and to picture the act of consciousness as a sort of inner vision directed upon it.

(b) Although the duality of act and content be involved in the most rudimentary phases of the mental life, it does not by any means follow that even the crudest *recognition* of that duality is a primitive or

primordial fact of mind. To differentiate, for example, hearing from the sound heard implies, I should say, an experience far more complicated and the use of concepts far more abstract than we can ascribe to the animal consciousness. But this does not, I think, constitute a fundamental severance between sense and the other cognitive processes, as Dr Myers seems to suggest. I cannot discover any ground for saying that in the mature mental life there is no recognition of the distinction between the act of sensing and the content sensed, any more than I can find ground for saying that in the less mature mental life there can be no imagining or apperception before the corresponding recognition is reached.

(c) Those who lay stress upon the part played by sensations of muscular strain or tension in the so-called 'feeling of effort' do not necessarily deny that consciousness is an activity, nor even that there may not be awareness of such activity. What they do call in question is the legitimacy of identifying *that* activity with the activity of which there is awareness in the 'feeling of effort.' Certainly to me it seems doubtful whether we can ever be said to be directly aware of the activity involved in consciousness itself,—aware of it, I mean, after the manner in which we are directly aware of a colour when we actually see it or of a sound when we actually hear it. But, in any case, I should be prepared to maintain that the 'feeling of effort' evinces itself as the outcome of a long repetition of experiences, and as having acquired an appearance of simplicity which disguises from us its really complicated character.

(d) It is not precisely clear what Dr Myers means by the possibility of "a choice on the part of the organism between two or more reactions to a given stimulus" as the condition of the appearance of consciousness (II, § 1). But if he intends to suggest that conscious choice, though it may be of the crudest kind, is an essential factor of mental life, I should hesitate in following him. It cannot be supposed that the consequences of specific reactions are in any way prefigured or represented in anticipation by the primitive consciousness. And until that in some vague way comes about, I do not see what conscious choice can mean.

(e) I do not think we are entitled to lay down as a psychological presupposition that "differences in type of movement must be the cause of differentiation in the quality of sensation," on the ground that "it would be of no advantage for the organism to experience different qualities of sensation, unless those differences were serviceable

in promoting different types of response." With the appearance of conscious life, advantageousness for the organism may not be the sole principle determining the course of evolution and may become less and less so as mental evolution proceeds.

§ 4. I return then, now, to the hypothesis propounded by Dr Myers. According to that hypothesis, if I interpret it correctly, the determining factors throughout, in regard both to the quality and the intensity of sensation, are the nervous responses and the efferent reactions which ensue upon the respective stimuli. The lower and the higher forms of sensibility, not less than the lower and the higher kinds of reflexes, are in reality governed by the 'all or none' principle. Within certain limits the type of reaction remains the same, and the grading of the latter in strength according to the strength of the stimulus (when it is so graded) means that as the strength of the stimulus increases more and more nerve and muscle fibres are called into play, the corresponding sensation meanwhile altering only in intensity. If, however, those limits be exceeded, if the increase in strength of the stimulus be excessive, the type of reaction changes, and a difference of quality makes its appearance in the corresponding sensation. Apart from the admittedly conjectural character of most of the essential steps of the argument, the objections I feel inclined to urge are these. In the first place, it is not easy to gather from Dr Myers's exposition in what relation he conceives the sensation to stand to the reaction. Which is the antecedent, and which the consequent? The whole trend of the line of thought pursued would seem to imply that when it is said that "the ultimate difference between the quality and the intensity of sensation depends on the nature of the underlying reaction," the dependence intended is a chronological dependence, and that in the order of sequence the sensation follows the reaction. But can it be maintained that this is in accordance with fact? And if it can, what becomes, on such a supposition, of the contention that "it would be of no advantage for the organism to experience different qualities of sensation, unless those differences were serviceable in promoting different types of response" (II, § 1)? If, on the other hand, the sensation precedes the reaction; if one sensation "leads to" one type of reaction, and another sensation to another type, then how can we "attribute the intensities of sensation to different degrees of the same reaction" (II, § 2), how can the circumstance that a sensation is unchanged in quality be due to the fact that the type of reaction is constant (II, § 8)? In short, how is the conception

of different qualities of sensation *promoting* different types of response reconcilable with the doctrine of "psycho-physiological parallelism," upon which Dr Myers tells us his view is based? In the second place, I fail to see in what way the hypothesis helps towards the solution of the problem which is the subject of this discussion. Suppose it be true that the correlate of differences in quality of a sensation consists in differences in type of reaction, and that the correlate of differences in intensity of a sensation consists in different degrees of the same reaction. Still, even then, the reaction forms no part of the content of the corresponding sensation, and *for the experiencing subject* no comparison of this content with the reaction is any more possible than with the external stimulus. Howsoever it may be for the scientific reflexion of the physiologist, for the experiencing subject sensual intensity most assuredly does not mean how much reaction, nor does sensual quality mean what type of reaction. There is nothing in the redness of red or the blueness of blue or in the sound of a tone heard to suggest to the experiencing subject specific types of nervous and muscular reaction, nor is there anything in the varying intensity of a colour, or in the faintness or loudness of a note, to suggest to him a greater or less number of efferent nerve and muscle fibres in a state of stimulation. And the question as to the intensity of sensations has to do, I presume, primarily with sense contents,—whether, namely, they warrant any definite assertions as to difference of amount, or whether what are taken to be differences of amount do not, in truth, indicate solely qualitative changes. I do not find that the answer to that question is materially furthered by correlating sense presentations with types and degrees of response within the nervous system rather than with kinds and amounts of external stimulation. When worked out from the psychological side, Dr Myers's theory would, I imagine, lead him, in the long run, to a conclusion resembling that of Münsterberg in the *Beiträge*, according to which muscular sensations, or sensations of strain (*Spannungsempfindungen*), mediate as a *tertium quid* between the physical and the psychical. Sensations proper, in Münsterberg's view, vary only in quality, and what is usually called a more intense sensation is, in all cases, a content of consciousness numerically and qualitatively distinct from the weaker sensation with which it is compared. Muscular sensations, on the other hand, occupy a unique position in the mental life; although sensations, they share with physical entities the characteristic of varying only in quantity and not in quality. Since

any physical stimulus necessarily evokes a change in muscular strain or tension, the equivalent of which in consciousness is a *Spannungsempfindung*, all sensations proper have as their accompaniments muscular sensations, and these afford the basis of measuring differences of intensity. Some such mode of translation into psychological terms the theory before us would appear to demand, and Dr Myers himself almost hints as much when he insists at the beginning, that "the very word intensity means a state of tension or strain" (I, § 3)—a remark, by the way, which seems to conflict with the concluding suggestion that "intensity differences are neither qualitative nor quantitative, but strictly *sui generis*." Ingenious, however, as Münsterberg's treatment of the problem undoubtedly is, it raises, I think, more perplexities than it succeeds in removing. The peculiar character ascribed to muscular sensations, as *toto genere* distinct from the character of all other sensations, is eminently unsatisfactory, and no serious attempt is made to show how it comes about that in comparing the intensities (say) of two sounds, our judgments are directed upon the sense-presentations themselves, and not upon their assumed concomitants<sup>1</sup>. Perplexities of a like kind would, I am persuaded, confront Dr Myers the moment he attempted to specify the psychological equivalents of the physiological factors which he takes to be involved in the differentiation of sensual quality and intensity. I am far from wishing to dispute the contention that kinaesthetic sensations are implicated, in some form or another, in every mode of sense experience. I think it likely enough that they are. But, after all, sensations of tension and strain have their own content, and from that content to the neural responses and muscle reactions themselves is a far cry. It seems to me that we have here a problem thrust upon us precisely the same in character as that which presented itself when the correlatives were taken to be the external stimulus and the sensation.

§ 5. Somewhat similar obstacles beset the path of those who, like Bergson, maintain that the contents of mental states cannot rightly be

<sup>1</sup> The stress of these and allied difficulties probably occasioned the change of attitude observable in the *Grundzüge*. At all events, in the later work, Münsterberg disputes apparently the possibility of any, even an indirect, measurement of sensual intensity, and what he has to say about muscular sensations deviates markedly from his earlier mode of dealing with them. See *Grundzüge*, I, 263 *sqq.* Note, especially the remark on p. 280, "Spannung und Streben bedeutet also Kraft für die vorpsychologische Wirklichkeit und für die empirische psychophysische Persönlichkeit, im System der psychologischen Bewusstseinsinhalte bedeuten sie dagegen nur eine Erfahrung und stehen dem Probleme der messbaren Wirkungen nicht näher als die Empfindungen blau und tönend und sauer."

treated as magnitudes, that the relations of greater and less are not properly applicable to them. Bergson admits that ordinarily we do, without the slightest hesitation, pass judgments involving quantitative comparison upon the contents of our experience. He has, then, to explain how it comes about that into the field of what is purely qualitative the appearance of intensive magnitude intrudes and creates the illusion of progressive increase and decrease. The explanation is obtained by tracing back the appearance to the natural propensity of consciousness to objectify mental states, to regard them, that is to say, as extensive *quanta*. In the case of what he calls representative states, we transfer he thinks the idea of the cause which is quantitative into the effect which is purely qualitative, and the notion of intensive magnitude is only a "perception acquise"; in the case of affective states, we give the name of intensity to the larger or smaller number of sensations which we associate with the fundamental sensation, and the notion of intensive magnitude is here a "perception confuse." When, for example, we experience a pain which becomes, as we say, more and more acute, consciousness distinguishes, within the characteristic sensation which gives the tone to all the others, a larger or smaller number of sensations arising at different points of the periphery, muscular contractions, organic movements of various kinds, and the totality of these elementary psychical states expresses the new exigencies of the organism in presence of a new situation thus constituted for it. We estimate the intensity of the pain by connecting the differences of sensation with the reactions which usually accompany them, and which are more or less extended; by prefiguring, that is to say, the future bodily movements in the very midst of the sensation which is being experienced. When, again, we estimate quantitatively the loudness of a sound, we take into account not merely the change or disturbance in the vital condition of the organism, but also the fact that, by striking some object and thus expending a definite quantity of effort, or by exerting ourselves in the use of our vocal organ, we have repeatedly produced a similar sound; and the idea of this effort immediately presents itself when we transform the intensity of the sound into a magnitude<sup>1</sup>.

There are, indeed, certain portions of Bergson's analysis which seem to me to be entirely on the right lines. In dealing, for instance, with the so-called "sense of effort," he complains, not without reason, of the crudeness of the conception of "a psychic force imprisoned in the mind

<sup>1</sup> *Les données immédiates de la conscience*, Ch. I.

like the winds in the cave of Aeolus, and only waiting for an opportunity to burst forth," and of the will as watching over this force, and from time to time opening a passage for it. The considerations which he urges in favour of regarding experienced "effort" or "activity" as a content of consciousness, and not as itself identical with the activity of consciousness, although perhaps not in themselves sufficient to establish this conclusion, can, when reinforced by others which he might have used, be formed into a coherent body of evidence which it would be extremely hard to resist. To isolate the act of apprehending from the content apprehended, and to attribute to the latter a strength or intensity of its own, which may vary independently, is a procedure for which no justification is yielded, so far as I can discover, from psychological analysis. At all events, we are, I think, entitled to say that the differences of intensity which we discriminate in the content apprehended are not to be regarded as equivalent to a greater or less amount of apprehending activity. We by no means of necessity apprehend the more intense better or more accurately than we apprehend the less intense. We are more liable to overlook changes of loudness in the roar of a cannon shot than those in the buzzing of a bee. Leaving on one side the thorny issue whether, as it is misleadingly stated, the intensity of a sense content may be increased by attention, I would only insist that, in any case, such a definition of attention as "the variously related degrees of psychic energy expended upon the different aspects, elements, and objects, in the one field of consciousness"<sup>1</sup> prejudges at the start the fundamental question which it is the very business of psychological investigation to decide. For the increase of clearness and definiteness which results from attention may depend not upon a "focussing of psychical energy,"—a conception which we shall try in vain to render intelligible,—but upon the number and kinds of discriminations we are able to make in the content attended to, the distinguishable marks we are able to recognise in it,—a process which would consist largely in connecting the said content with, in relating it to, representations and ideas already possessed by the apprehending subject<sup>2</sup>. Clearness, certainly, is one thing, and intensity another, but if through attending the content becomes more intense, an explanation of that circumstance must be found that is consistent with the explanation we are enabled to give of the increase in clearness and distinctness.

<sup>1</sup> Ladd, *Psychology, Descriptive and Explanatory*, 74-5.

<sup>2</sup> Cf. my paper on "The Nature and Development of Attention," in this *Journal*, 1913, vi. 1.

Bergson's arguments relate chiefly, however, not to the processes of consciousness but to the contents apprehended thereby, and in this reference the explanation he has to offer seems to me to fail. The failure evinces itself, I think, in much the same manner whichever be the department of experience with which he is dealing. Take, for instance, his account of the way in which we come to regard a pain as increasing in intensity. He recognises that it will not do to say that the more intense pain corresponds to a greater nervous disturbance, for these disturbances are as movements unconscious, and their equivalent in consciousness has no resemblance whatsoever to motion. But, he contends, the automatic movements which tend to follow the stimulus are likely to be conscious as movements, and the differences of sensation are interpreted by us as differences of quantity because we connect them with the reactions which usually accompany them and which are more or less extended. Now, the obvious question which at once presents itself is, why should the movements that accompany the sensation be said to be unconscious, and those that follow it be said to be conscious, as movements? It becomes very soon apparent that the latter way of speaking is no more than a metaphor. By movements that are conscious as movements Bergson here means simply the sum of sensations that arise from muscular contractions, organic conditions, changes in the state of joints, tendons and skin, and so on. In other words, the apprehension of these movements as movements is just as distinct from the fact of movement itself as are all presentations from the objective events giving rise to them. The factors called in to account for the appearance of intensive magnitude ought, *ex hypothesi*, then, to be as little capable of yielding it as the sensation of pain itself. Moreover, the theory is not, I think, confirmed by the appeal which is made to experience. When I become aware that a tooth-ache from which I am suffering is becoming more acute, or that a headache is becoming more severe, I fail to detect even by the most careful introspection any reference at all to the "thousand different actions" I might take in order to avoid either of these calamities. What introspection does seem to testify is that the estimate of intensity is derived directly from the experience of the pain itself. Now, I quite admit that introspection may in this respect be deceptive, but if it is, the deception stands in need of explanation, and the difficulty of finding one along the lines that have been followed seems well-nigh insuperable. Finally, the criticism I am urging may be summed up in more general terms. True though it may be, that in mature experience our judgments as to the comparative

intensity of two sense contents are constantly aided by the knowledge we possess of the physical world, yet it is impossible to suppose that such judgments could have become possible, if ultimately such sense contents are never directly apprehensible as standing to one another in a relation of greater and less. What problematical knowledge of the cause of olfactory sensations could conceivably have originally induced us to pronounce one smell to be stronger than another, if the contents of those experiences did not themselves furnish the data for such comparison? Our scientific modes of exact measurement, our interpretation of the physical world in quantitative terms, itself presupposes psychologically the more rudimentary comparison between the contents of sense experience. Doubtless, such scientific knowledge when attained facilitates and modifies the judgments we form of the increase and decrease of sensual intensities, but in no case can the latter be wholly dependent on that knowledge.

§ 6. "The fact is," says Bergson, "that there is no point of contact between the unextended and the extended, between quality and quantity. We can interpret the one by the other, set up the one as the equivalent of the other; but sooner or later, at the beginning or at the end, we shall have to recognise the conventional character of this assimilation<sup>1</sup>." The abstract severance thus formulated is the basis upon which the refusal to recognise intensive magnitudes is rested. Extensive magnitude, so the argument runs, involves the relation of container to contained, involves, in other words, the relation of whole and part, and from the point of view of magnitude, there could be nothing in common between the extensive and intensive, save the divisibility which the relation of whole and part implies. But, since intensive qualities are indivisible, to speak of them as magnitudes is a contradiction in terms.

In his elaborate essay, *Ueber die Bedeutung des Weber'schen Gesetzes*, Meinong does not explicitly refer to Bergson's treatment of the subject, but the essay contains what is still by far the most complete and conclusive answer to the points that Bergson raises<sup>2</sup>. I note some of the main features of Meinong's argument.

The first important fact he seeks to establish is that there are

<sup>1</sup> *Les données immédiates de la conscience*, 52. I do not know how the contention in *Matière et Mémoire*, 242 sqq. et passim, that "all sensations are primarily extensive," is to be reconciled with the sharp antithesis that is drawn in the earlier work, nor how the non-quantitative character of sensations is to be sustained in the face of that contention.

<sup>2</sup> The second volume of Meinong's *Gesammelte Abhandlungen* (Leipzig: Barth), 1913, has just reached me. It contains the essay on Weber's Law which has long been out of print.

quantities which are not divisible, and that such quantities are not confined to the class of what it has been customary to call intensive quantities. Some relations are quantities, and relations are not even conceivably divisible. For example, distance, the apartness of two points in space, is undoubtedly a quantity, but it is not when rightly regarded a divisible quantity. True, it is often mistaken for such, because it is confused with length (*Strecke*); but the thought of the length between two points in space is something quite different from the separation or distinction of two points in space. Distance is a relation, whereas a length is a whole containing parts. So again similarity and dissimilarity may be quantities. We talk about a greater or less degree of similarity, a greater or less degree of dissimilarity. But neither the one nor the other of these relations is a collection of units. Quite apart, then, from such intensive qualities as a pleasure or a pain, we are bound to admit indivisible quantities.

The next thing to notice is a further distinction which has an important bearing upon that between divisible and indivisible quantities. Although in ordinary speech unlikeness or dissimilarity (*Verschiedenheit*) is often used as synonymous with difference (*Unterschied*), yet on purely empirical grounds we are able to assert that when, as a result of comparison, we affirm or deny an unlikeness, we are not judging about difference. Unlikeness is asserted not alone of quantities; mathematical difference can only hold between quantities, and moreover only between divisible quantities. Thus, the difference between two lines is itself a line, but the unlikeness between two lines, like every other unlikeness, is a relation, and in no sense a length. Again, the difference may remain the same, whilst the unlikeness is not the same. Thus between 1 and 2 there is a much greater unlikeness than between 100 and 101, though the difference is the same. And in like manner, the unlikenesses may remain constant, whilst the differences differ,—a condition of things illustrated by Weber's law.

Now, all measurement rests upon the mental operation of comparison. As Mr Russell puts it, "without the immediate comparisons, which are necessary both logically and psychologically, nothing can be accomplished: we are always reduced in the last resort to the immediate judgment that our foot-rule has not greatly changed its size during measurement, and this judgment is prior to the results of physical science as to the extent to which bodies do actually change their sizes<sup>1</sup>." And Meinong takes pains to make clear that to whatever extent physical

<sup>1</sup> *Principles of Mathematics*, 178-9.

operations may be substituted for mental, yet there is no possibility of basing measurement wholly upon the former. The process of superposing for example would have no meaning, did we not know that when one thing exactly "covers" another, the result for the most accurate comparison can only be equality. To look upon measurement as a purely physical operation would be tantamount to supposing that addition and multiplication had been converted into physical operations because both can be carried out by a reckoning machine.

Measurement, however, in the strict sense of the term, is applicable only to divisible quantities, in regard to which to say that *A* is double of *B* means that it is the magnitude of two quantities together, each of which has the magnitude of *B*. Such measurement may be either immediate or mediate. The former, which Meinong insists is applicable to time as well as to space, can be replaced by the latter when, as constantly happens, it is more convenient to measure directly a substitute for the object than the object itself. If, for example, it is a question of determining the length of a line which forms one side of a square, the problem may be solved by measuring any one of the other sides, if for any reason it is easier to do so. And the possibility of indirectly measuring indivisible quantities depends upon an extension of this method of substitution. In all such substitutive measurement (*surrogative Messung*) that which is actually measured is always a divisible quantity which serves as a substitute for the indivisible quantity. For example, distance, as we have seen, is a relation, and as such indivisible. But every distance, whether spatial or temporal, is associated with a length, and every length is associated with a distance. A distance may, therefore, be measured by measuring the length with which it is correlated. Similarly, in the case of the thermometer, only the height of the mercurial column can be, in the strict sense, measured, but we can take that to be a measurement of the temperature, so soon as an empirically determined regularity has been found to subsist between the height of the mercury and the states of temperature. So again, velocity is not identical with a length and the time in which it has been traversed, but we regard the velocity as measured when we have measured the length and time, and divided the former by the latter. The legitimacy of this process of substitutive measurement depends upon the extent to which there may accrue to it the advantages which are obtained from direct measurement. Three things, Meinong finds, give value to direct measurement. In the first place, a discrete term, namely a number, is substituted for an element of a continuum, and thereby the intractability

of the latter is relegated to the unit. In the second place, this number stands in the same relation of magnitude to other numbers as the given quantity stands to the other quantities of the same continuum. And, in the third place, the absolute limits, zero and infinity, which have validity for indivisible no less than for divisible quantities, coincide for the numbers and the quantities. Now, of the cases of indirect measurement to which reference has been made, those of distances and velocities participate in all three advantages, whilst to the measurement of temperature by the thermometer there accrues only the first of them. It appears, therefore, that some forms of indirect measurement are more imperfect or more rudimentary than others.

In the discussion of what he calls "psychical measurement," Meinong assumes that by sensual intensity is to be understood intensity not of the act of sensing but of the content, which he takes to be no less than the former psychical in character<sup>1</sup>. He points out, what follows indeed at once from the prior investigation, that the distinction between psychical and physical does not coincide with the distinction between intensive and extensive. Some intensive quantities are to be met with in physical nature, whilst extensity, he thinks, is a characteristic of some psychical facts. Confining attention meanwhile to intensive psychical facts, Meinong dismisses as self-contradictory the conception, introduced by Fechner, of sensation-increments (*Empfindungszuwüchse*). Because, however, there are not, and cannot be, units of sensation<sup>2</sup>, it does not by any means follow that sensations are not measureable, any more than it follows that temperature is not measureable because there are no units of temperature. If regard be had to the changes of sensation-intensities, the problem does not present itself as in any sense a hopeless one. The thought of change rests upon the thought not of difference (*Unterschied*) but of unlikeness (*Verschiedenheit*)<sup>3</sup>, and the measurement of change carries us back to the measurement of distance. Change and distance do not, in themselves, imply increments and units. Whilst, therefore, the assertion that the change of sensation from  $S^1$  to  $S^2$  is equal to the change from  $S^3$  to  $S^4$  is a perfectly intelligible proposition, the assertion that

<sup>1</sup> I should differ from Meinong in this respect, but I am purposely avoiding that issue in the present discussion.

<sup>2</sup> Cf. Mr Bradley's article, "What do we mean by the Intensity of Psychical States?" in *Mind*, N.S. iv. 7. Mr Bradley contends that such units exist, although we are not able in fact to discriminate and fix them.

<sup>3</sup> For such a phrase as *eben merklicher*, or *gleich merklicher Unterschied*, there ought to be substituted the phrase *eben merkliche*, or *gleich merkliche Verschiedenheit*.

$S^1$  is so many times greater than  $S^2$  is not. If, then, the possibility of treating physical intensities as quantities be admitted—and in regard to some of them, at any rate, it cannot be disputed—the possibility of treating sensation-intensities in the same way as quantities must be conceded. In short, there is no theoretical difficulty in regard to the measurability of sense-contents. What difficulty there is is a practical difficulty, and arises from the circumstance that those operations which give to physical measurement its exactitude are not, as a rule, available. We are bound to have recourse to substitutive measurement, and the substitute must be a divisible quantity. If, however, there can be established on empirical grounds a definite series of correlations between changes of sensation, which are not numerically determinable, and changes of some extensive quantity, which are capable of numerical determination, then we should be just as entitled to take the magnitude of the latter as measuring the magnitude of the former, as we are entitled, for example, to measure temperature by means of the mercurial column of the thermometer. Only we must beware of taking for granted that no degrees of intensity are possible unless in fact we can measure them.

One of the chief features of interest in Meinong's analysis is the clear way in which it is shown that "psychical intensity" is not *sui generis*. Upon that assumption Bergson's argument throughout proceeds. His contention amounts, in short, to this,—that in order to be quantitative, a sensation would have to be built up, as Fechner supposed it was built up, of equal parts or increments. By bringing "psychical intensity" into line with intensity that is certainly not psychical, Meinong is enabled to free the former from an utterly incongruous conception. And when that is done, Bergson's thesis falls. It is perfectly true that the measurement of sensation-intensities is possible, if at all, only by a convention. But then that is equally true of the measurement of distances<sup>1</sup>. It is perfectly true likewise that we are dependent upon the immediate apprehension of a change as revealed by the subjective comparison. But then all measurements depend in the long run upon immediate judgments of equality, and these, as also the immediate judgments of greater and less, are still

<sup>1</sup> The convention in the case of distances is, as Mr Russell states it, the following. It is agreed that, "when the distances  $a_0a_1, a_1a_2 \dots a_{n-1}a_n$  are all equal and in the same sense, then  $a_0a_n$  is said to be  $n$  times each of the distances  $a_0a_1$  etc., *i.e.* is to be measured by a number  $n$  times as great." This is a convention because "owing to the fact that distances are indivisible, no distance is really a sum of other distances." *Principles of Mathematics*, 180.

possible where measurement, in the strict sense, cannot be carried out<sup>1</sup>. The real question is, how far the immediate comparison of sensations is reliable,—a question, no doubt, to which very varying answers will be given, but which does not affect the issue raised by Bergson.

§ 7. I confess I feel less satisfaction with Meinong's solution of the practical problem, acute and suggestive though his mode of handling it must be admitted to be. He maintains that, whilst in regard to sensation-intensities the appearance of equality can never be trusted, there can in normal cases be no question of an illusory appearance in the case of unlikeness. What to immediate apprehension appears unlike is unlike, although what is unlike only appears as unlike down to a certain limit—the threshold, namely—where the appearance of equality supervenes. The inferiority of judgments of equality as compared with those of unlikeness may lead to an apparent paradox, but it does so not only in the field of psychological but also in that of physical inquiry. And although at the disposal of the physicist there are vastly greater facilities for surmounting this defect of the faculty of comparison, it can never be completely overcome even by him. From this it follows at once that just appreciable unlikenesses need not be, as Exner, for example, assumed they must be, equal; but where there is equal sensitivity to unlikeness, there is a well-grounded presumption in favour of their equality. Upon these premisses Meinong rests the interpretation he has to offer of Weber's Law. So far as extensive sensations are concerned, it can be said, he thinks, that proportional sensations correspond to proportional stimuli and *vice versa*. With respect to intensive sensations, however, it is solely a question of equality of unlikenesses, and the law means that if  $R_1, R_2, R_3, R_4$  be four stimuli and  $S_1, S_2, S_3, S_4$  the corresponding sensations, then if the proportion  $R_1 : R_2 = R_3 : R_4$  hold of the stimuli, the corresponding pairs of sensations exhibit equal unlikeness, and the unlikeness of  $S_1$  and  $S_2$  is equal to that of  $S_3$  and  $S_4$ . Owing to his confusing difference and unlikeness, Fechner assumed that just appreciable differences were themselves sensations, and his logarithmic formula calls, therefore, for unreserved rejection.

Far more uncertainty, however, attaches, I think, to Weber's Law than Meinong seems inclined to admit. The assumption that just appreciable unlikenesses can, even with his proviso, be regarded as equal, is destitute of any sufficient grounds. We know but very

<sup>1</sup> Cf. Russell, *ibid.* 182.

little of the conditions upon which the appreciation of minimal unlikenesses depends. One factor at least is hardly amenable to control,—the state, namely, of the adaptation of the end organs to impression. In the case of vision, for example, the conditions on which such adaptation depends are so numerous that they cannot be reduced to uniformity, and certainly cannot be eliminated. Such a fact as this alone would almost drive us to the conclusion that the minimal unlikeness is of variable nature. Moreover, these difficulties would recur in determining, as Meinong desiderates, where there is equal sensitivity to unlikeness. And then, again, the number of deviations from Weber's Law increases as investigation proceeds, so that it is fast becoming doubtful whether any field for its applicability will in the end remain. So far as cutaneous sensations are concerned, Rivers and Head find that it does not hold in respect of protopathic sensibility<sup>1</sup>, and, indeed, it is hard to see how, except perhaps on Dr Myers's hypothesis, it is compatible with the 'all or none' principle. As regards taste and smell, the difficulties of obtaining experimental verification would certainly in any case be considerable, but, in spite of Miss Gamble's careful piece of investigation<sup>2</sup>, it cannot, I think, be claimed that the applicability of the law to either of these senses has been placed beyond the reach of doubt. With respect to vision, there does not appear to be such independence of intensity and quality as would be requisite for the establishment of the law, and the same is to be said of hearing. At all events, the law cannot be taken to be more than an interesting empirical generalisation, based upon experiments that have not been purified from interfering circumstances, and must, even where it would seem to hold, be an expression for an extremely complex set of conditions.

§ 8. "It must never be forgotten," writes Sherrington, "that Weber's Law deals with judgments. The comparison of one sensation with a second of similar *quale*, but of dissimilar *quantum*, involves more than the mere neural process concerned with a simple sensation. From the very outset it works with ideas based on perceptions<sup>3</sup>." What is here said is certainly true so far as attention is confined to the elaborate comparisons on which Weber's Law is rested. But Sherrington's contention suggests an interesting question as to the ultimate psychological nature of the appreciation of unlikeness. The developed act of

<sup>1</sup> *Brain*, Nov. 1908, 428-9.

<sup>2</sup> *Amer. J. of Psychol.* 1898, x. "The Applicability of Weber's Law to Smell."

<sup>3</sup> Schäfer, *Textbook of Physiology*, II. 932-3.

comparison always involves a reference to the objective order of fact as distinguished from the sense contents. It cannot, however, be supposed that any such objective reference is present in the rudimentary sense-experience out of which the recognition of an objective order has gradually emerged. That rudimentary experience could only have contained, at the most, the simple foundation on which the later process of judging psychologically depends. The inference seems inevitable that originally appreciation of unlikeness is itself a component of sense-experience. And the inference certainly gains confirmation from the consideration that the unlikenesses which we discriminate, be they great or small, are as much given as are, for example, the distinct sense contents which are pronounced unlike. Thus, I think, we are enabled to see that the sense-data we are supposed to compare in the developed act of judging degrees of intensity are not, in truth, sense experiences in the strict acceptance of the term. They are abstractions from sense-experience, and the isolation we artificially produce by working on the given material serves to disguise from us the actual nature of the experience we thus manipulate. Unlikeness, that is to say, is not something added to the contents of sense-experience from some other function of the mind; it does not arise for the first time when a complex act of judging comes into play; it is no less an element or aspect of that sense-experience than the distinguishable contents themselves. Such unlikeness may be of various kinds. The apprehension of quantity comes, we may agree with Mr Bradley, later than that of quality, if, that is, quality be taken at its crudest stage. But we have no ground for supposing that the elementary discrimination of either the one or the other necessitates a function of mind different in kind from that of sense-experience itself.

§ 9. If pressed to give a definite answer to the question whether intensity differences of sensation are quantitative, the reply, I presume, would have to be framed in some such terms as these. As the mathematician conceives of quantity, the only quantities whose differences may likewise be described as quantities are divisible quantities. Consequently, the difference or unlikeness of two intensive quantities is not itself a quantity,—which amounts, in other words, to saying that these quantities are not multiples of an element or unit similar in quality to themselves. Using for the moment the word difference in its non-mathematical sense, one would assert that just as the difference between two distances is not itself a distance, so the difference between two sensations is not itself a sensation. To quote Mr Russell's well-

known dictum, "the difference between two intensive quantities, in fact, differs from each as much as the difference between two horses differs from a horse." The distinction, however, which Mr Russell, in his *Principles of Mathematics*, draws between quantities and magnitudes would, I gather, enable us to speak of intensity differences of sensation as magnitudes. Magnitudes, as he would employ the term, are more abstract than quantities. A specific magnitude is a common property of a number of equal quantities. An actual foot-rule, for example, is a quantity; its length is a magnitude. A quantity is anything which is capable of quantitative equality to something else—that is to say, which is capable of possessing the same magnitude as something else. Properly, one quantity ought not to be described as greater or less than another, for the relations of greater and less hold between their magnitudes. On the other hand, properly one magnitude ought not to be described as equal to another magnitude, for the relation that would be really meant in such a case would be the relation of sameness or identity. Thus, for example, suppose a sound  $A$  possesses the loudness  $a$  and a sound  $B$  possesses the loudness  $\beta$ .  $A$  and  $B$  are each of them quantities;  $a$  and  $\beta$  are magnitudes. If  $A$  is louder than  $B$ , then the difference  $a - \beta$ , let us call it  $\gamma$ , is not a sound possessing magnitude;  $\gamma$  simply is a magnitude. If  $A$  resembles  $B$  in loudness, then  $a$  and  $\beta$  are not *two* magnitudes,  $a$  is the same magnitude as, or is identical with,  $\beta$ . And the difference or resemblance of  $A$  and  $B$  in loudness is a magnitude, because it is greater or less than other differences or resemblances, such, for instance, as the difference or resemblance in loudness of the sounds  $C$  and  $D$ . "Quantities not susceptible of numerical measurement can," says Mr Russell, "be arranged in a scale of greater and smaller magnitudes, and this is the only strictly quantitative achievement of even numerical measurement. We can know that one magnitude is greater than another, and that a third is intermediate between them; also, since the differences of magnitudes are always magnitudes, there is always (theoretically, at least) an answer to the question whether the difference of one pair of magnitudes is greater than, less than, or the same as the difference of another pair of the same kind. And such propositions, though to the mathematician they may appear approximate, are just as precise and definite as the propositions of Arithmetic<sup>1</sup>."

<sup>1</sup> *Principles of Mathematics*, 183. Cf. 159.