Foundation of Science—The philosophy of theory and experiment
Norman Robert Campbell

15 Science is distinguished from mathematics and philosophy by its attitude towards judgements of the material world. It accepts such judgements as ultimate and fundamental.

However it does not accept all such judgements, but selects from them. How is the selection made ?

The answer is, by a refinement of the criterion by which judgements of the external world are distinguished from those of the internal world. This criterion is universal assent. The subject matter of science consists of those immediate judgements for which universal assent can be obtained.

[**C**] The universal assent should be replaced with some sort of Darwinian selection; if we deny the judgement, then we shall be penalized/ lose the gamble, etc.

16 The tendency in more recent times has been to avoid any reference to "matter" or "the material world," and to define the judgements which form the basis of science as those which are derived from "sensations" or from "routines of sensation" or some similar phrase with a psychological flavour. I have preferred the older phrase, not because I believe it is adequate, but because I believe that it is not less adequate than the later and because I desire to enter a protest against the attitude which led to its abandonment.

The abandonment of the term "matter" in the definition of the judgements which form the basis of science was due in the first instance to attacks on it made by idealist philosophers. They urged that their studies had led them to the conclusion that "matter" did not "exist," or, at least, that, if it existed , its nature was wholly different from that which seemed to be indicated by scientific studies.

17 Nobody doubts the fact of certain judgements; difference of opinion concerns only the interpretation of them, and it is only the fact that we assert when we say that they are to form the basis of our study.

19 For our present purpose we may regard 'material' and 'external' as identical.

hs The judgements which compose our conscious life can be divided into two classes, those which represent events happening within ourselves and those which represent events in the external world. $\cdots$ The distinction between these two classes arises from a difference in the extent to which the judgements are common to persons other than ourselves. In respect of the second class we find there is something common between ourselves and any other person with whom we can enter into communication. It is the community of our judgements of the second class with those of others that leads us to attribute them to some agency which is neither we nor they, but something external to all; it is the divergence of our judgements of the first class from those of others which leads us to attribute them to something inherent in our own personality.

[**C**] All these things may happen within myself; solipsism is not defetable. However, assuming this strict solipsism and the usual belief of the existence of the external world does not make any difference (perhaps by definition of solipsism). This total isomorphism makes the distinction irrelevant.

20 There is another difference between the two classes of judgements which confirms their classification as external and internal; it is a difference in the degree to which they are subject to the control of our wills.

21 Judgements do not form part of e proper sub ect matter of science until they are free) from the smallest taint of personality, unless they are wholly independent of volition and unless universal assent can be obtained for them.

23 Moreover the fact that, when judgements of the external world are concerned, we can ascertain the real opinion of any person by examining his actions rather than his speech suggests two other remarks of some interest.
I believe there are at least three groups of such judgements:
(1) Judgements of simultaneity, consecutiveness and "betweenness" in time.
(2) Judgements of coincidence and "betweenness" in space.
(3) Judgements of number, such as, The number of the group A is equal to, greater than or less than, the number of the group B.
These three groups will be termed respectively time-, space- and number judgements.
29 [**C**] However, if the number becomes too large, (3) may not be so objective.Judgements for which universal assent can be obtained.

31 The three classes of time-, space- and number-judgements probably) do not include all of those concerning which universal agreement can be obtained.

33 Physics is often regarded as more fundamental than the other sciences and there is certainly good ground for this view. Almost all other sciences base their conclusions to some degree on those of physics, and a reversal of accepted propositions of physics, if it were sufficiently extensive, would render it necessary for almost all other sciences to revise their conclusions.
[**C**] However, we should not forget that we are biological.

II The Nature of Laws

38 The discovery of the complexity of the terms involved in laws leads to the recognition as laws of propositions which are not usually called by that name, e.g., those asserting the association of the properties of a substance.

The term "concept" is defined as an idea depending for its significance on the truth of some law.

39 A law always asserts that A is uniformly associated with B, where A and B are "phenomena," knowledge of which is derived from judgements of the external world.

there will be two main questions to be answered, What are the terms between which the relation is asserted, and What exactly is the relation asserted between the terms? Neither of these questions seem to me to have received the attention which they deserve.

42 The conclusion which I am trying to enforce is that the use of certain words implies the assumption that certain laws are true, and that any statement in which those words are involved is without any meaning whatever if the laws are not true.

43 Unrecognised laws. An important class of words which in this manner connote laws is that which includes the names of special substances; it suggests further interesting considerations. Thus, whenever we speak of "silver" or "iron," we are implying that certain laws

are true, namely the laws asserting the association of the properties of silver or iron.

45 Concepts. The expression of laws.
Thus our first conclusion is that many of the words used in expressing scientific laws denote ideas which depend for their significance on the truth of certain other laws and would lose all meaning if those laws were not true. These words include most of the technical terms of science, but the laws on which they depend for their meaning are often not explicitly recognised as such. It will be convenient to have a name for such words and they will in future be called "concepts." A concept is a word denoting an idea which depends for its meaning or significance on the truth of some law. The conclusion at which we have arrived is that most, if not all, of the recognised laws of physics state relations between concepts, and not between simple judgements of sensation which remain significant even if no relation between them is known.

49 a division of properties into defining and non-defining is impossible; any such division would be purely artificial and would represent no distinction.

50 The recognition that laws are not entirely independent of each other, that each assumes in some measure the truth of the other , that all science is intimately cross-connected by innumerable ties, real though slight- these conclusions cannot be neglected in any inquiry into the origin of laws and the means by which they are established.

52 Definitions. If we boldly refuse to pay any attention to logical canons our difficulties vanish at once. Our words then are not instruments by means of which the process of thought is conducted, but merely convenient means of recalling to our minds thoughts which have once passed through them or of calling up in the minds of others thoughts which are passing through our own. They have nothing whatever to do with the operation by means of which we pass from one set of thoughts to another.
[**C**] This is impossible.

55 Because we state that the force on a certain body is I dyne or that extension is proportional to force, it does not follow that we can state significantly that force "is" something or other. Though laws state relations between concepts, the significance of those concepts can hardly be separated from that of the laws they are used to state.

III The Nature of Laws (continued)

We now ask what is the nature of the relation asserted by laws between the terms we have considered.
The "orthodox" view that laws assert a relation of cause and effect is considered and rejected. The origin of this is traced to a confusion between psychological relations involved in the experiment which proves a law and the material relation asserted by the law which is proved. Temporal order appears to be introduced by the conception of "processes" rather than by that of cause and effect.

But if the relation asserted by laws is not causal, what is it?

It is suggested that a proposition is to be regarded as a law if it has an importance for

science which is of a special kind.
If this conclusion is accepted, it follows that the relation asserted by a law must be some form of "uniformity," a term which includes invariability and generality.

It is suggested that the importance of a law and the decision whether or no a given proposition is to be regarded as a law is determined not only by the considerations of its formal nature, but rather by its connection with theories.

IV THE DISCOVERY AND PROOF OF LAWS

The discovery of laws is a special case of induction or the determination of a general relation from a limited number of particulars.

A familiar solution of the problem is that stated (e.g. by Mill) in the Canons of Induction.

The Canons depend on the Law of Causation. This law depends for its significance on the division of experience into facts; this division must therefore precede the use of the Canons.

How is the division to be made? To be useful for the Canons.
Similar considerations apply to the division of the facts into instances, which again must precede the use of the Canons. ... Accordingly the Canons alone cannot discover all laws; there must be some more fundamental method.

But can the Canons discover any laws ? The answer is no; but they may be useful in indicating in what direction to seek for a law more general or more precise than one previously known.

How then are laws discovered ? The question is divided into three, based on the fact that laws are always suggested before they are proved. First, how are laws suggested ? Second, what is the nature of the experiments which prove them? Third, what is the foundation of the proof?
Laws are almost always suggested by theories.
In connection with the second some slight analysis of the nature of an experiment is undertaken. The third question brings us back to Induction.

We realise that we can never prove a law to be true. On the other hand we can do what the Canons cannot do, namely prove that a suggested law is not true.
[**C**] Not this simple. How can you prove that the counterexamples are enough?

V The Explanation of Laws

Explanation consists in the substitution of more for less satisfactory ideas. Ideas may be more satisfactory either because they are more familiar or because they are simpler.

But there is another and more important kind of explanation which is effected by theories and not by other laws.

VI THEORIES

122 A theory is a connected set of propositions which are divided into teo groups. One group (hypothesis) consists of statements about some collections of ideas which are characteristic of the theory; the other group (dictionary) consists of statements of the relation between these ideas and some other ideas of a different nature. 'Dictionary' relates the ideas in the hypothesis and the ideas about something known apart from the theory.

An example: the dynamical theory of gases 'hypothesis' consists of formulas; the definitions of the symbols in the formulas are provided by 'dictionary.' From the propositions of the hypothesis, $PV = RT$ may be derived.

131 The reason why it was accepted is not experimental. The reason is based directly on the analogy between the pressure P and its kinetic counterpart. With a failure of the analogy all reason for accepting it would have disappeared.

132 A theory is valuable and is a theory in any sense important for science, only if it evokes ideas which are not contained in the laws which it explains.

The theory is derived largely, not from the formal constitution, but from an analogy displayed by the hypothesis.

This analogy is essential to and inseparable from the theory and is not merely an aid to its formulation. Herein lies the difference between a law and a theory, a difference which is of the first importance.

A digression is made to discuss the use, in connection with theories, of the words "molecule," "real," "cause."

136 $\boldsymbol{r}$ is needed for the theory (at least to describe). However, this is less important thanks to LLN.

That is to say, even if we could determine the variables, the deductions which could be made from the theory would be precisely the same as the deductions made from the theory with the assumed initial values.

137 "We are therefore reconciled to the impossibility of determining these variables because we know that, if we could determine them, it would not make the slightest difference to the theory. Nevertheless, it causes us, I think, some slight mental discomfort."
The author points out if the number of particles is finite, then we can actually determine $\boldsymbol{r}$s.

Then, Brownian motion (Perrin) is mentioned in this context. "He felt that his researches had made molecules real in a way that they had not been real before."
[**C**] However, the author's argument "it would not make the slightest difference" replaces the issues.

This conclusion does not lead, as might be suspected, to the result that all numerical laws are theories.

The value of theories, and especially the relative values of those of the first and second types (often called "mechanical" and "mathematical" theories), is discussed.

VII CHANCE AND PROBABILITY

Probability is a measure of our degree of knowledge concerning the happening of an event. We ask how is probability measured and why, when so measured, is it an indication of degree of knowledge ?

Objections to the frequentist definition:
(1) it gives no indication why probability and degree of knowledge are associated,
(2) if it is employed in the usual manner it leads to obvious inconsistencies. A new definition must be sought.

It is suggested that probability is a property of large collections of similar trials. ... a definition is adopted provisionally for equal probabilities.

A new definition is stated, based on the idea of degree of knowledge. Used in conjunction with certain quite arbitrary assumptions it leads. to the usual Bayes' formula.

Why probability, measured according to our definition, is an indication of degree of knowledge.

VIII The meaning of science

Two criteria for the value of scientific propositions have been recognised, universal assent and intellectual satisfaction.

The value which a scientific proposition derives from the first criterion is called its truth, that which it derives from the second its meaning. Meaning is more important in theories than in laws, but it is insisted that laws have also meaning as well as truth.

[**C**] 'Meaning' is related to satisfaction: this point of view is important.

The reasons why there has been a danger that the meaning of science will be overlooked are mainly historical. The chief changes that have taken place in the attitude towards scientific knowledge are briefly sketched, and special attention paid to the very important views of Mach. Much of those views is accepted, but it is maintained that the object of science is not "economy of thought".

IX Science and philosophy

The fact that science has both meaning and truth shows that the material world must be in some sense in harmony with our desires. An explanation of this harmony is distinctively philosophical.

232 Three conceivable explanations are quoted, none of which are actually true, but all of which will be admitted to be of the right kind.

Three possible explanations:

(i) Theological: the material world and our intellectual desires were in accord because both are under the control of a God who preferred to produce the world ion this way.

(ii) Evolutionary: men whose desires were such as the material world could satisfy; they were selected.

233 (iii) Psychological: as an illusion we see the world as such.

Analysis of the three explanations shows that they are very similar to those offered by theories: Theories explain propositions by deducing them from other propositions. (i) requires the new idea of a God, based on the analogy of human personality. (ii) introduces hypothetical ideas based on analogy with the observed development of species; the fundamental idea is the similarity of the past and the present.

235 Our evidence for believing that species have originated in a certain manner in the unobserved and unobservable past is quite different from that for believing that they are originating in this manner at the present time.

Simple logical relations are derived from analogy with corresponding variations of the causal relations.

237 (i)-(iii) are unacceptable because:

(1) The analogy is unsatisfactory, because

(a) it is not the kind of analogy which gives intellectual satisfaction

(b) the analogy is actually false;

(c) not satisfactory as an explanation.
In conjunction to (ii) (c) means this: it might be acceptable as an explanation of laws even more complicated, it is not acceptable as an explanation of something quite simple.
(2) The deductionis unsatisfactory.

But if any of the three explanations were satisfactory as theories, would the explanations which they offer be ultimate or require yet further explanation? It is maintained as a personal opinion that they would be ultimate.

Ultimate explanations must be related to causelessness.

243 In the absence of any explanation which fulfills the necessary conditions, further discussion of the matter is impossible. But since metaphysicians pretend they have or may shortly have adequate explanations, it is worth while inquiring whether their explanations could give us what we require.

Any mention of metaphysics leads at once to the introduction of the words, reality, existence, and truth in very special senses. An inquiry is set on foot to discover what men of science mean by reality. It is suggested that the fundamental sense is derived from the form of the law which defines the concepts of material objects, and that the only things which they are prepared to maintain finally as real are material objects, hypothetical ideas corresponding through the analogy with material objects, and other persons. ....

X Fundamental Measurement

All measurable properties are capable of being placed in a natural order by means of definite physical laws which are true of them.

But the possession of order alone will not enable a property to be measured, except possibly by the use of previously established systems of measurement for other properties.

In order that a property should be measured as a fundamental magnitude, involving the measurement of no other property, it is necessary that .... **[C]** This must be the measure theory.

XI Physical Number

There is certainly a sense in which number is a property of a system and thus distinguished from both a Number and a numeral. "number", in this sense, or physical number, is a magnitude just as much as weight-though any statement beginning "number is" is really misleading.

298 we may conclude that in dividing the system into members I have already assumed that the division is such that the counting process will fulfil the necessary condition; if not, I should say that the division of the system into members had been faulty.

Our whole idea of what is an object and of what is a system made up of individual objects is intimately bound up with our experiences in enumeration; it is only those systems which can be enumerated that we can bring ourselves to regard, by any stretch of the imagination, as consisting of members which are individual objects.

XII Fraction and negative magnitudes

XIII NUMERICAL LAWS AND DERIVED MAGNITUDES

XIV UNITS AND DIMENSIONS

Some special properties of such no-dimensional magnitudes are considered and they are carefully distinguished from formal constants and other terms in numerical laws which resemble them in invariability with change of unit.

The question is raised whether, as is often stated, mass, length and time are in some way peculiarly basic magnitudes. Ultimately the conclusion is reached that they are not, and that such special features as they possess are derived partly from the importance of the dynamical equations in which they occur.

It is concluded that it is entirely misleading and incorrect to say that volume has the dimensions $(\text{length})^3$.
[**C**] This seems to be due to a wrong conceptual analysis of volume ; see Lebesgue.

V THE USES OF DIMENSIONS

The use of the theory of dimensions expounded in the preceding chapter is based on the fundamental fact that the dimensions of a derived magnitude indicate, though imperfectly, the form and physical significance of the law by which it is defined.

The chief use of the theory is in connection with the "argument from dimensions".

XVI ERRORS OF MEASUREMENT I. METHODICAL ERRORS

Our discussion of fundamental measurement in Chapter X was based on the assumption that the laws of equality and addition are strictly true. But they are not generally true. It is generally possible to find three systems A, B, C such that $A = B$ and $B = C$, but $A \neq C$. The failure of these laws is reconciled with the fundamental idea that a magnitude can be represented by a single definite numeral by the introduction of the conception of "errors of measurement." It is pointed out that these errors are of two kinds, errors of method and errors of consistency: if it is only the laws of equality and addition that fail, we are concerned with errors of method alone.
The results of the failure of these laws is examined carefully. The failure does not prevent us establishing a self-consistent standard series, though it cannot be made by the specification given in Chapter X.
The problem of establishing a system of fundamental measurement which shall be free from err ors of method is then attacked. We must alter either our definition of equality or of addition. Reasons are given why it seems more plausible to alter the former.
The new definition of equality proposed depends on the conception of a "rea l magnitude". Th e direct observation of equality involved in our previous definition is considered to show, not that the real magnitudes are equal, but only that they differ by less than some amount,

which is the error of measure ment. In order to apply this new definition to the determination of real magnitude s some further proposition about the errors must be introduced.
It is insisted th at the new definition and all the propositions connected with it are theoretical. T he theory of measurement is stated in the standard form of Chapt er VI.
It is examined how far the theory is in accordance with experiment and how far the hypothetical ideas involved in it (real magnitudes and errors) can be determined by experiment. It is concluded that no facts are known contrary to the theory, but on the other hand it is impossible, without introducing further propositions, to determine any of the hypothetical ideas and so produce direct confirmation of the theory. These additional propositions must include a “law of errors”; discus sions of possible laws are reserved for the next chapter.
Though it has been found so far impossible to determine real magnitudes uniquely and so avoid all the consequences of the failures of the laws of equality and addition, some progress has been made. Our measurements, though still “inaccurate” are not so inaccurate as they would have been if the theory had not been introduced. The very important conception of a “stepped” measuring instrument is introduced; it will concern us greatly in future discussions.
Lastly it is asked whether all fundamental magnitudes are affected by errors of method. All are except one; the one exception is number. Number 18, however, affected by error’s of consistency, an observation which enforces the essential distinction between the two kinds of error.

XVII ERRORS OF MEASUREMENT I l. ERRORS OF CONSISTENCY AND THE ADJUSTMENT OF OBSERVATIONS

The considerations of the preceding chapter were directed towards establishing a satisfactory standard series of fundamental magnitudes by means of which these magnitudes and others could be measured. We now make measurements and find that we have not rid ourselves of all errors ?? our measurements are not always consistent with each other.
On the other hand the measurements, even if inconsistent show some kind of order; if they did not, no further progress could be made, for mere ignorance and disorder is not a basis for any argument. During the first part of this chapter it will be assumed that the measurements show the order characteristic of what is termed a “complete collection”. Briefly it is a collection which permits the determination of the probabilities of the various inconsistent values.
We must now find, as in Chapter XVI, a theory which will explain errors of inconsistency and enable us to determine from inconsistent measurements “true values”. The question arises immediately, What are true values? The fail??re to answer clearly this question is responsible for many ambiguities and confusion in the theory of errors.
By a “true value” is always and without exception meant a value connected with some other true value by a law. This law is often recognised in the conventional theory under the name “equation of condition”. But all true values are inseparably connected with an equation of condition, even though it may be concealed by an habitual terminology. If there is no equation of condition there is no true value. Errors of consistency are not errors of measurement, but errors in the systems measured.
Since this conclusion may appear startling at first sight it is supported by a brief inquiry why we desire accurate measurement.
The problem to be solved is then restated. It is to find a way of combining inconsistent observations in such a manner as to produce true values which satisfy some law which may or may not be known accurately.
The rule always adopted for solving the problem is to take the arithmetic mean of the inconsistent observations. It is inquired how far direct experimental proof can be obtained for the

rule; it is concluded that if there are complete collections known the rule can be established as a definite experimental fact with all the certainty that can be attached to any law.
An exception is usually recognised to the rule when there is "systematic error". It is maintained that the presence of such error only means that the assumed equation of condition is not accurately true; it may be convenient to use the term, but for our purpose the problems of systematic error are merely those of determining whether the equation of condition is accurately true.
The problem is now formally solved for complete collections. But it is desirable to explain the solution, and for this purpose a theory of errors of consistency is required. The theory offered involved implicitly in all physical thought) states briefly that errors of consistency are magnified errors of method. The theory leads directly to a law of errors of consistency which is not, except in special circumstances, identical with the usually accepted law. Evidence is probably not available (but it might be obtained) for establishing the law, but it is urged that such evidence as there is tends in favour of the proposed law rather than Gauss'. Much of the evidence often adduced for the latter is irrelevant.
The remaining problem of the study of errors of inconsistency is to find true values when the collections of observations are not, as we have supposed so far, complete. If the collections are really incomplete the true values cannot be determined and there is no more to be said; but our theory suggests a method by which several incomplete collections may be combined in some cases to form a complete collection; that method is to distribute the errors of the incomplete collections in the manner in which they would be distributed if they v;,ere the errors of a complete collection.
But the rule thus obtained is not definite; a further criterion is required. That usually adopted is that the true values are to be chosen so that they are the most probable causes of the actual distribution of errors, assuming that the errors are distributed according to the law of a complete collection. The general formula to which this rule leads is deduced, assuming (as is always done if this rule is adopted) that Gauss' law is true.
The application of the rule is made by the Method of Least Squares. The problems solved by this Method are two: (1) the determination of the true values of magnitudes when they are related by an equation of condition of which the form and coefficients are known, (2) the determination of the coefficients of the equation of condition when the form is known. It is inquired how far the Method accords with the theory in solving these two problems. It ?? appears that it is in accord with the theory in solving the first (though some applications of the Method are illegitimate) ; but it is not at all in accordance with the theory in solving the second. Moreover the fact, often quoted, that the residuals are distributed according to Gauss' law does not seem to justify the foundation of the Method on that law.
The difficulties of the Method, as of any other method which is based on considerations of probability, are even more apparent when the "probable error" is considered. It seems that a definite meaning can be attributed to the "probable error of a single observation", but we fail to find any definite meaning which can be attributed to "the probable error of the general mean ". The discussion of the questions raised leads to a complete distrust of all methods of determining true values (except possibly from complete collections) by considerations of probability. If the collections are really incomplete and cannot be made complete by the device proposed, then it is useless to try to determine true values.
But can any less objectionable method be proposed? Certainly not for determining true values from collections essentially incomplete. If, however, the collection can be made complete by combining incomplete collections, a better method can be suggested. It is to make the arithmetic sum of the errors zero. It is urged that this method is preferable (I) because it is much simpler to apply in all cases, (2) because it is always an adequate expression of the theory on which it professes to be based, (3) because it does not require the same detailed

knowledge of the law of errors, (4) because it is suggested directly by the most fundamental fact in the whole study of errors, (5) because it tells us plainly when we can and when we cannot determine true values and does not pretend to achieve more than any method can achieve.

The bearing of all the foregoing discussion upon the problem of establishing numerical laws is considered. Numerical laws state relationships between real magnitudes. This conclusion requires some unessential alterations in the statements of Chapter XIII. Rules are given based on the Method of Least Squares and the alternative method for deciding, as far as it is possible, whether a proposed numerical law is in accordance with the facts. But complete decision is not possible unless the collection of observations is complete.

Lastly we return once more to the problem from which we started, that of establishing and calibrating a standard series for fundamental measurement . It is finally concluded that by no process whatever is it possible to get entirely free from errors of method and that some uncertainty in assigning real magnitudes is perfectly unavoidable.

XVIII MATHEMATICAL PHYSICS

Up to this point an effort has been made to show that certain parts of physics, including that which consists of measurement, though apparently dependent on mathematical conceptions are, 1n fact, wholly independent of those conceptions; they are purely experimental. But mathematical conceptions undoubtedly do play an important part in physics, and it is now time to consider exactly how they enter into the science.

The considerations are recapitulated which led to the conclusion that measurement has nothing to ¡lo with mathematics. It depends only on the physical magnitude, number, not the mathematical conception, Number.

Guided by these considerations we return to the problem, left unsolved in Chapter XIII, of determining whether numerical laws involve mathematical conceptions. It is concluded that the facts expressed by numerical laws can be expressed as laws, and that these laws can be used to define derived magnitudes and for any other purposes for which any other laws are used, without any introduction of mathematical conceptions. They can even be expressed as relations between numerals without introducing such conceptions. On the other hand if we inquire how we obtain the numerals by which numerical laws are expressed, then we are bound to introduce the mathematical conception of Number.

But mathematical conceptions enter unavoidably and characteristically only when we attempt to explain numerical laws. The explanation is effected by mathematical theories, which usually involve, beside the simple conception of Number, the more peculiarly mathematical conceptions of limits, continuity, derivatives, and integrals. The supreme use of numerical laws is to test theories, and this use of them necessarily involves mathematics. It is here that mathematics enters as an essential part of physics.

Mathematical theories were discussed so fully in Chapter VI that it is only necessary to add a few remarks on questions that have been raised since by our discussion of numerical laws. The use of derivatives in the hypotheses of mathematical theories suggests the question to what laws these hypotheses can be analogous. In answering this question we are led to the conception of a physically significant derivative of a mathematical function used in stating a numerical law. The concept ion is examined carefully, and the necessary conditions for physical significance explained; they are fulfilled only for a comparatively small proportion of the derivatives actually employed in physics. Velocity is a physically significant derivative; but the dispersion coefficient is not. Derivatives have to be distinguished carefully from derived magnitudes, with which, ho
-??ever, they are closely associated. A derivative can be measured experimentally in the same way as a derived magnitude.

Mathematical derivatives are possessed only by continuous functions. Though actually only continuous functions are used in mathematical physics, it is worth while to inquire why this is so, and whether in any circumstances discontinuous functions could occur. These questions, though not important in their applications, have some intrinsic interest.