separate distinct problems, to seek the limits and conditions of each, and to show how illusory is the ease with which they were thought to be solved. For such a task nothing less was needed than the fine intelligence whose premature disappearance leaves those who have known him in such deep regret.

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INTRODUCTION

Methodology and Logic

Logic is the study of proofs. The best proofs are encountered in the sciences, so that it is natural for the logician to stand near the scientist and watch his reasonings and his methods. But merely describing methods of proof is not enough: it is necessary to analyze them also. Thus the logician by this critical method of analysis proper to his discipline, can in his turn teach something to the scientist. He can make explicit to him the elements and premises used confidently by the scientist as means of proof. For the scientist may employ them while he is quite unaware of the conditions and causes of their power; he may take them as simples, wnereas the logician finds them unexpectedly complex in structure. Thus, methodology is only one half of logic. Logic may sin by insufficient attention to the proofs of science; it then works with poor and inferior material. But it can sin equally by insufficient rigour in the analysis of these proofs: that is what has happened in the logic of induction.

We perceive at first glance two types of induction. One proceeds by simple enumeration of instances. It is founded on the number of such alone, and it claims to draw conclusions that are never more than probable. The other type, on the contrary, proceeds by analyzing the conditions or circumstances. Certain of itself, it leaves everything to care, and nothing to repetition in its search for certainty. Of these two theories, the last alone seems to answer to the practice and very spirit of science. A single experiment, the scientist thinks, if improvised very carefully, can in one successful result bring all the certainty accessible; and to wish to build anything whatsoever on repetition is unworthy of intelligence.

The logician generally accepts this proud thought too lightly. He closes his mind to the possibility that things are not what they seem, that the appearance of a certainty obtained by the analysis of conditions covers a probability founded on pure and simple repetitions, that scientific induction is resolved into a complex of enumerative inductions, and that, consequently, induction by the enumeration of instances may be the one which he has to justify in order to justify science. Dominated by the impression of power that scientific induction produces, he does not wish to know anything less promising. He says with Bacon: Induction by simple enumeration is a precarious process and is exposed to the danger of contradictory instances (*Inductio per enumerationem simplicem precario concludit et periculo exponitur ab instantia contradictoria*). He then turns toward the pursuit of a theory of induction that would count the repetition of instances for nothing and which, by more noble means, would rise to certainty.

This is letting the substance go for the sake of its shadow. For the doctrine thus constructed does not render an exact account of any sort of actual induction. The conditions that it assumes are never fulfilled; and besides, they are already unreal from the standpoint of pure logic. By placing itself on the grounds of certainty, by wilfully ignoring the influence of repetition, the theory of induction terminates in complete check.

It might be considered surprising that some doubts did not appear sooner. But these doubts, arriving too late and lacking in force against a prejudice decked in the prestige of science, are often replaced by an additional error. It is felt clearly that the theory is not applicable, that it is necessary to diminish some of the power it claims to confer on induction. It is then said: certain in theory, induction is only probable in practice; and enough is thought to have been conceded. But in truth, if real inductions do not fulfil the conditions that would make them certain in theory, it follows that they are not certain at all; but by no means does it follow that they remain somewhat probable, or very probable, or extremely probable. If certainty is lacking altogether, the very possibility of probability remains to be established, and the whole theory of induction must be done over.

We propose in this work to confirm this principle and to study its consequences. We shall try to establish the logical problem of induction on its intended ground, that of probability. We shall pursue the solution of the problem, but we cannot admit having reached it. All that we can hope is to contribute to it, if only by showing how obscure the problem remains: obscure to the point that nobody has ever succeeded in proving or even stating principles capable of fully justifying induction under the conditions by which it operates.

There will be some discussion of the recent work of Mr. John Maynard Keynes, *A Treatise on Probability*. We shall attack his fundamental conception of the mechanism of induction and the proof of one or two of his essential theorems. But the value of the theorem which he has really and properly proved should only appear all the more forcibly. We regard it as the most important result yet possessed; and since we are to criticize Mr. Keynes several times, let us say here that in our opinion, no author since Mill has advanced the theory of induction as much as he.

PRELIMINARY NOTIONS

It is useful, first of all, to determine certain notions and certain principles exactly.

Certain and probable inference.—What is induction? Induction is a species of inference: but it must be stated that an inference has no need of being certain, in order to be legitimate and rigorous in its way. We first regard inference as the perception of a connection between the premises and conclusion which asserts that the conclusion is true if the premises are true. This connection is implication, and we shall say that an inference grounded in it is a *certain inference*. But there are weaker connections which are also the basis of inferences. They have not until recently received any universal name. Let us call them with Mr. Keynes *relations of probability* (*A Treatise on Probability*, London, 1921, ch. i.). The presence of one of these relations among the group A of propositions and the proposition B indicates that in the absence of any other information, if A is true, B is probable to a degree p. A is still a group of premises, B is still a conclusion, and the perception of such a relation between A and B is still an inference: let us call this second kind of inference *probable inference*.

Certain and probable premises; general definition of inference.—These terms "certain inference" and "probable inference" undoubtedly lend themselves to equivocation. But we have no better terms and it will suffice to explain them.

First of all, probable inference says that in the absence of any other information, the truth of its premises renders its conclusion probable to a degree p. It then says *less* than what is claimed by certain inference, as we have just called it; but it says it with as complete certainty. It does not conclude *in a probable manner*, but it reaches *a probable conclusion* in as certain a manner as the other reaches its conclusion.

We have so far considered only premises that are certain. But any inference which yields something, starting from premises taken as certain, still yields something starting from premises taken only as probable, and this holds for both types of inferences, certain and probable. We can even assert that starting from premises which, taken together have a probability p, a certain inference will confer on its conclusion the same probability p; and a *probable inference*, which would confer on its conclusion the probability q if its premises were certain, will confer on its conclusion the probability pq.

It can therefore be said that certain inference transfers to its conclusion the totality of the certainty or probability of its premises taken together, and that probable inference transfers to the conclusion a part of its certainty. Inference in general will be defined as an operation which transports to its conclusion either the totality or part of the certainty or probability of its premises; it is only in this wide sense that it is legitimate to postulate that induction is an inference. Notice that the conclusion of an inference extracts at most no more certainty or probability than is equal to the conjunction of its premises, consequently, no more than any one of them, and in particular, no more than *the most uncertain* among them. This very evident truth will be very useful to us.

Definition of induction.—What sort of inference is induction? It is defined in current times by the logical form of its premises and its conclusion by saying that is a passage from the individual to the universal.

At the beginning of an induction, then, *beside premises having any contents and forms whatsoever* not all of which are known, there are propositions about a certain class A of individuals or species, without assuming that the members under consideration exhaust the totality of the class A (for perfect induction does not concern us here). And in addition to this, the induction terminates in a proposition *about all the members* or else only *about any member* of this class. We shall return to this distinction in a moment.

Inductions about the relations of characters.—The extremely general term "proposition" includes mainly those propositions which refer to those secondary characters arising from the relations of characters—such as the fact, regarding a man, of having two eyes of different colours, or else of having a weight in kilogrammes equal to the number of centimetres his height exceeds a metre.

Let A be the character hypothecated by an inductive law. The character found in the conclusion often occurs in the following form: Let b and c be the classes of characters B_1, B_2, . . . B_n, . . . and C_1, C_2, . . . C_n . . .—for example, b and c may stand for temperature and density respectively, the "values" B, C being the different temperatures and different densities. Lastly, let R be a relation joining a C to each B. The character that induction attaches to the character A consists often of a certain relation R between the character of the class b and the character of the class c which accompanies the character A: thus the chemical composition of a body determines, not its density or its temperature, but only a relation R between the two, one of which remains a certain *function* of the other.

These functional laws are the objects of scientific inquiry, and are distributed elsewhere. Does not their particular form have any influence on the mechanism and logical principles of the inductions which they establish? This is a problem we would not dare to probe.*

But no such form appears up to the point where this work stops. Therefore, everything which follows should be understood indifferently as induction about characters or about the relations of characters.

Inductions about classes of individuals and inductions about classes of classes.—We must notice in the first place that every functional law assumes the appearance of a law bearing on the classes of a class, rather than on the individuals of a class. In fact, let B_1 and C_1, B_2 and C_2, . . . B_n and C_n, . . . be the characters *b* and *c* associated by the relation R. The law linking to the character S the relation R of the characters *b* and *c* links C_1 to AB_1, C_2 to AB_2, . . . C_n to AB_n, . . . It is therefore a bundle of laws fixing the characters *c* of the classes AB_1, AB_2, . . . AB_n . . . of the class A.

But it has been thought that we should, besides, reserve in the previous definition the possibility of inductions operating directly with classes of classes just like other inductions dealing with classes of individuals. Indeed, a number—the number two, for instance—is a class—the class of couples—such that any arithmetical induction by verification of a formula for various numbers has as its immediate domain, not the individuals of a class, but in truth the classes of a class.

* It is in this way that induction about the relations of characters raises directly the problem of the connection of *simplicity* with *probability*. In fact, the relations connecting the members, taken two at a time, of even two series of given characters (e.g. the curves passing through a given collection of points) are multitudinous. Any induction in this domain supposes the choice of the simplest relationships. This choice is often explained by the psychological reason that the simplest is the most convenient. But to make induction valid, the choice must be justified. Psychology does not do so; for who can assure us that the most convenient formula of what we are familiar with, will be found to be the most probable formula of what is unknown to us? This is a problem of the greatest logical difficulty. For we do not know perhaps with enough clarity what probability is, nor what the simplicity of a formula is: we can define simplicity differently from several points of view. But none of these assuredly must reduce itself to what is most flattering to the laziness of our minds.

The distinction between classes of classes and classes of individuals might very well make a difference to the logical theory of induction. But we shall not reach the point where this distinction makes itself felt, so that the substance of this study appears to us applicable indifferently to classes of particulars or to classes of classes.

Inductions from THESE to ALL and from THESE to ANY.—We must distinguish two forms that the conclusion of an induction may take with respect to *all* or *any* of the A's. Despite Stuart Mill's inference from particular to particular, these two forms have not been carefully discerned. This is because, remaining confounded in the domain of certainty, they are not separated until they appear in the domain of probability. In fact, if it is *certain* that any of the A's are B, it is certain that *all* the A's are B, and vice versa, so that these two forms *all* and *any* differ only verbally here. But when we have to do with a conclusion that is only probable, a hierarchy and even independence between *all* and *any* appear. If it is probable that *all* the A's are B, it is even more probable that *any* A is B, for then we have only one risk instead of several. On the other hand, when it is probable that *any* A is B, it may at the same time be improbable and even impossible that *all* the A's are B. This is what happens when it is certain or probable that there is, for instance, only one A out of a thousand that is not B. We must then, in the general study of induction, distinguish conclusions about *all* from conclusions about *any*, these last always being the more probable, sometimes the only probable ones.

Primary and secondary inductions.—We have said little about the premises of an induction. The conclusion referring to all the members or to any of the members of a class A, the premises should be comprised of particular propositions having as subjects members of A which are not all its members, so far as our knowledge goes. But nothing is said here about the other premises which must be taken together with these in order to obtain a valid inference. According to the form of these other premises, the force of induction arises possibly in several ways. Hence it is not necessary to assume as a principle that induction is a linear process and is analyzable in only one way.

In particular, suppose that an induction has among its premises the conclusion of another induction. We shall call it then a *secondary induction*. *Primary inductions* are those whose premises do not derive their certainty or probability from any induction.

It is possible that there are general modes of induction which, because of the premises they require, can only operate in secondary inductions. It may be that these modes are in themselves the most certain; that is to say, that they transmit to their conclusions a greater share of the certainty or probability of their premises than the modes of primary induction. But we cannot repeat too often that this intrinsic superiority is vain. For as a matter of fact, the probability conferred by an inference, of any sort, upon its conclusion is at most equal to that of the least probable of its premises. The probability supplied by any induction whatsoever cannot exceed the highest probability that a primary induction can yield. That is why primary induction should be analyzed before any other. For it is not only the logical foundation of induction, but it also marks the limit of all inductive assurance.

Mill's doctrine offers an excellent illustration of this same hierarchy of primary and secondary induction and of his misunderstanding of it. Mill thought he was presenting a very powerful method of induction. But this method is of the secondary mode because it requires as a premise the law of causality, which according to Mill is only an inductive generalization obtained by the lower and primary method of simple

enumeration. If Mill's object were not merely the description of scientific inductions, but their analysis and the discovery of the principles which underlie them, why did he not make a profounder study of simple enumeration which in the end is the basis of his whole structure? Strange to note, he himself turned away from it. Simple enumeration appeared to him as primitive, prehistoric reasoning. But when he has to build on it, he assumes not only that it yields probability, but besides, a probability which approaches certainty indefinitely in the degree to which instances are multiplied; and this considerable postulate inspires hardly any doubt or curiosity in the most noted logicians of induction. And Mill has no sooner based his doctrine on simple enumeration, than he forgets it. He then seems to consider induction by simple enumeration as lacking in force, and to put all his faith in scientific induction. The method of induction, which is the basis of his logical theory, is seen only as an historical tradition, and he thinks himself free to distrust it as Bacon had done.

This place in Mill's doctrine has often been criticized. But, strangely enough, critics have not seen the real point of Mill's error. Mill might have been reproached for not realizing the mode of the primary induction which is required as the premise of the secondary induction with which he is so much concerned because of its praiseworthy results. But instead of that, he is most often reproached for making the law of causality itself based on induction. This is the objection which has become classically attached to his theory. And thus, discussion of this particular opinion of Mill has not made visible the exact nature of the lacuna in his system; moreover it is simply a lacuna, and not a contradiction or a vicious circle.

That is why logicians who came after him and rejected Mill's theory did not guard against another form of the same insufficiency in their inductive demonstration. This consists of requiring of the primary mode of induction which is disparaged and neglected, no longer, it is true, the law of causality, but some other premise which is less universal and yet necessary for the application of the mode of induction that is admitted. In fact, it is often estimated that it is not enough, in the practice of scientific induction to start with the principle that the effect studied has a cause, but that it is also necessary to have already some idea of the nature of this cause in order to eliminate from the very start a multitude of circumstances, observed as well as unperceived, so that we can be limited to the systematic examination of a few hypotheses. This prior limiting determinable, however, can only be viewed as an analogy derived from causes already known of effects of the same sort. We thus come back by a detour to Mill's general position which requires for the premise of scientific induction the mass of previous common knowledge, itself due to some pre-scientific mode of induction. And, like Mill again, we do not wish to see that the priority thus conceded to this primary mode is a very nice case of irrevocable logic. At present, the tendency is to present primary induction as solely historical and not concerned with pure logic. As a result, the elementary methodological distinction between the primary and secondary modes of induction is not at all a current fashion. Is not this a sign that thinking on this question has not been sufficiently clear?

On the other hand, once we have recognized this simple distinction, we can no longer afford to neglect it. Every analysis and theory of the principles of induction becomes governed by the rule which makes the source and limit of the probability or certainty of induction in general depend on the probability or certainty of primary induction.

Probability and certainty.—Probability is different from certainty not only in degree, but in nature. For certainty is

absolute and probability is relative. If I have judged a proposition to be certain—certain and not only infinitely probable —no new information can ever make it doubtful again, unless my judgment of its certainty was false in the first place. On the contrary, if I have judged a proposition to have some given degree of probability, some new information may make it more or less probable than it was; but I was not mistaken in the first place because of that. The probability of any proposition whatsoever is then relative to such and such a group of evidence, or else to employ the precise language of Mr. Keynes, it is a relation of this proposition to this group.*

Common sense has never ignored the distinction; for it believes that the actual realization of an improbable prediction cannot justify it as more than an improbable event, and that chance cannot contradict rational certainty. But the logicians of induction have not always remembered this.

And has not this forgotten point been the source of the suspicion which, since Bacon, has almost invariably been attached to induction by simple enumeration? It was not satisfactory enough to say that this form of induction without analysis yields only a probability. The very reality of this modest result has been subjected to doubt. It seems that there is nothing convincing about simple enumeration and that it dissipates in absurdities. If induction by simple enumeration were valid, would it not be necessary to believe that the more one has lived, the less chances one has of dying? (Cf. Keynes, *ibid.*, ch. xxi.) And so this mode of induction is traditionally accused, not of concluding in a probable way, which would be

* *A Treatise on Probability*, ch. I. Perhaps the question is, however, a little more complex: is there actually no intrinsic probability by which a proposition recommends itself more or less to the mind without being related to any of the other opinions which happen to be given? This probability would then be as direct and immediate as certainty; there would only be a difference of degree. We do not see any reason for not admitting it, and it might be given the name of *plausibility* in order to distinguish it from the probability-relation. But the latter alone is produced by reasoning and more particularly by induction.

true, but of concluding in a very doubtful way, which would be tantamount to not concluding at all.

Now all the paradoxes that this kind of induction seems to give birth to, disappear or lose their astonishing character as soon as we remember that probability is relative. In the first place, as a matter of fact, the occurrence of a single contrary event does not prove that a prediction founded on numerous instances was not very probable, or even infinitely probable: it proves only that this prediction was not *certain*. In the second place, if a prediction is probable, according to the principle of simple enumeration, when it is brought to a situation composed uniquely of numerous favourable instances, it is not longer probable by relation to a situation which comprises additional external facts making this prediction impossible or very improbable. The two sources of paradox thus disappear; for it is no longer astonishing that a probable prediction is later invalidated, and secondly, that a prediction which is known to be erroneous is not rendered probable by arguments which would make it such in the absence of such knowledge.

The perception of this principle that probability is a relation, not a quality of propositions, takes away from probability what appeared to be its fleeting and provisional character. It makes probability a fact as rigorous as implication, for instance. The propositions that a given group of propositions renders probable to a degree p are as determinate as the propositions that this same group renders certain, and they are sometimes as difficult to discover.

But the relative character of probability, while it solidly assures its existence against the doubts suggested by the first view, introduces a profound difference between probability and certainty, and makes it more difficult to compare them. Thus, it is commonly said that probability by increasing tends to approach certainty as a limit. But that is not true, rigorously speaking. In fact, it would then be necessary for infinite probability to be certainty. This identity is accepted in current discussions. But it is not exact; for there is nothing in the increase of probability, even carried to infinity, which renders this probability less relative to given information, less alterable by some new information: a relativity which separates it infinitely from certainty. The probability that an unknown number is not 1324 is infinite and we cannot conceive of a greater probability. Nevertheless, it is enormously different from certainty. For it is relative to a state of information in which the unknown number can have one value as well as any other. If we learn that there is a probability p, however small, that the true value of the number is under 10,000, the value 1324 in the light of this fact immediately acquires the finite probability of one-tenthousandth of p; and if we are informed that the first three ciphers are 1, 3, and 2, this probability becomes relatively to the new information equal to 1/10. A probability can be infinite but that will by no means make it more absolute. In short, probability is never identical with certainty. Are they at any time equivalent? This is a difficult question, for a probability, even when infinite, allows some chance: the unknown number may be all the while 1324. This chance is undoubtedly negligible; it is very tiny or as small as possible; but it is not nothing, since the chance does exist. As to saying that it is infinitely small, that has no meaning. In fact, the expression can only be applied to a function, and it means that for any value a, there exists a value of the variable or variables making the function smaller than a. But applied to a particular value such as that of the chance subsisting under determinate conditions, the expression infinitely small is a piece of nonsense. The difference between infinite probability and certainty is then a troublesome concept. We shall, in the course of this work, have occasion to say in conformity with usage that a probability approaches or tends towards certainty: let it be quite specific that we shall thereby understand simply that it approaches the highest probability conceivable.

Limitation of this work to inductions from THESE to ALL.—Inference then is the transition to a conclusion of all or part of the certainty of the premises, and induction is an inference whose conclusion refers either to *all* or to *any* of the members of a class A. Also, certain premises refer to such members of A which (since we are not concerned with perfect induction, so called) are not totally known; we propose to investigate the logical principles of induction, that is to say, the other premises which reason declares to be necessary for all inductions.

In this investigation, we shall seek especially those principles of induction which do not logically presuppose any other induction and which we have called primary: for their principles comprehend all inductive reasoning, just as their power limits it.

We shall limit this study to inductions which conclude about *all*. It may be doubted whether they are simpler than inductions about *any*. But, because the former have alone been studied until now, they appear to be more easily analyzable. Anyway, in our opinion, the difficulties in primary induction have not been appreciated, and perhaps it would be better after all not to begin with these mistakes (cf. Keynes, *ibid.*, p. 259). It would then be necessary to stay still further away from known theories and to proceed in an entirely new spirit. But we are not going to start in that way before we have been convinced that there remains no other.

HYPOTHESIS CONCERNING THE TWO ELEMENTARY RELATIONS OF A FACT TO A LAW

Confirmation, Invalidation

CONSIDER the formula or the law: *A entails* B.* How can a particular proposition, or more briefly, a fact, affect its probability? If this fact consists of the presence of B in a case of A, it is favourable to the law "*A entails B*"; on the contrary, if it consists of the absence of B in a case of A, it is unfavourable to this law. It is conceivable that we have here the only two direct modes in which a fact can influence the probability of a law. Given the hypothesis, either a fact realizes the conclusion and lends support to the law, or else it does not realize the conclusion and refuses to support the law: such would be the ultimate effects of the inductive process. A fact which consists of anything but the presence or absence of B in a case of A cannot then act *directly* on the probability of the law *A entails B*. But, once it consists of the presence or absence of N in an instance of M, it would act on the probability of the law *M entails N* either to strengthen or to weaken it. Where a fact does have an effect, it would influence the probability of the law *A entails* B, thanks to the relation of the probabilities of two laws, in the case where such a relation, favourable or contrary, is posited by some premise. Thus, the entire influence of particular truths or facts on the probability of universal propositions or laws would operate by means of these two elementary relations which we shall call *confirmation* and *invalidation*.

This hypothesis cannot claim the force of an axiom. But

* "A *entraîne* B" is translated hereafter as "*A entails B*," for the relation between the causal character A and the effect B is indeterminate. "A involves B" might also convey the sense.—Tr.

it offers itself so naturally and introduces such great simplicity, that reason welcomes it without feeling any imposition. We have not seen it stated in any explicit manner. However, we do not think that anything ever written on induction is incompatible with it.*

We may take this principle, therefore, for our guide.

Theoretical advantage of invalidation over confirmation.—The confirmation which a favourable case lends to a law and the invalidation which a contrary instance produces do not have the same value. A favourable case increases more or less the probability of a law, whereas a contrary case annihilates it entirely. Confirmation supplies only a probability; invalidation on the contrary, creates a certainty. Confirmation is only favourable, while invalidation is fatal.

Of the two elementary operations of facts on laws, the negative effect is therefore alone certain. By that very consideration, it becomes also the more accurate and clearer operation. Indeed, confirmation through a favourable case presents two difficulties which do not exist for invalidation by a contrary case. In the first place, the very reality of this confirmation is doubted when the case which is to bring it about reproduces identically a case already used; for it is a widely accepted opinion that two verifications which are identical in all respects count only as one. In the second place, one wonders how it is possible to measure this confirmation when it does exist, and one does not know what answer to make. The corroborative action of a favourable instance therefore appears enveloped in a kind of mist, whereas the effect of a contrary instance seems to be as limpid and intelligible as it is fatal.

* One might think of induction by *concomitant variations* as not being induction by *confirmation* or *invalidation*. But that cannot be maintained. In fact, what is called thus consists of an ordinary induction which renders certain or probable the law: "A *variation* of A entails a *variation* of B," and also of a deductive transition of this law to the following: "The *elimination* of B entails the *elimination* of A," that is to say, *A entails B*, by the aid of a so-called rational principle (which no mathematician, however, would regard as serious).

That is why, by virtue of its love of lucidity and certainty, the mind inclines without deliberation to a theory of induction based uniquely on the infirmative action of experience. In fact, an induction can conclude with certainty only on the condition that it does not utilize anything but the elementary operations of invalidation. Experience, in the matter of laws, having only the prerogative of denying, can attain as much assurance as it likes only when it affirms by negation. Secondly, this negative action of facts on the probability of laws is the only one which the mind understands clearly from the very first. To base itself solely on negation, is therefore to preserve the hope of conceiving a demonstrative induction, and this also means to satisfy reason.

This propensity of the mind appears to be linked to two opinions which are, so to speak, universal. According to one, induction must be certain in principle in order to be probable in practice. According to the other, the favourable instances or verifications of a law do not corroborate it by reason of their *number*, but only by virtue of their *variety*; the latter alone can appeal to reason. For in order to have an induction certain in principle, it must rest on operations of negation. And if the variety alone of favourable cases has an effect, and not their great number, is it not because these cases themselves corroborate only by excluding mere repetition? Thus the confirmation that the instances of a law appear to lend it directly would itself be indirect and negative in essence. The outcome of induction would reduce itself to the invalidation of possible laws by contrary cases.

Such seems to be the spirit of nearly all that has been written on induction. Sometimes this principle is openly avowed, often it is tacitly assumed, but in every case it directs thinking in the subject, and there is no denying that reason favours it. Let us then postulate it expressly and see where it leads.

INDUCTION BY INVALIDATION

The mechanism of induction by invalidation: elimination.—When we ask facts only to invalidate laws, it is necessary for a certain law to be confirmed. Several possible laws must then be found related in such a manner that the rejection of some of them favours those which remain. This mechanism is called in logic "*elimination.*"

But elimination may be either *partial* or *complete.*

If at least one of a group of propositions is true, elimination is complete when all these propositions except *one* are eliminated. The one that remains is then certain, without any need of knowing what the initial probabilities of the propositions at first were, nor the manner in which the rejection of the first, of the second, of the third, . . . has increased the probability of each of the remaining ones, until finally, the rejection of the next to the last had made the last certain. This final result of complete elimination does not depend on such a calculation.

On the other hand, elimination is partial so long as there remain *several* propositions not invalidated. In order to determine the value to which the probability of one of them amounts to, we must then know the initial probabilities, and we must besides suppose that the relations of the probabilities of the propositions that are not invalidated remain what they were at the beginning. If we make this assumption, the initial probability of the invalidated propositions is distributed among the remaining ones in proportion to the initial probabilities of the latter.

In order for induction by invalidation to operate, the conditions necessary for elimination must be present. The first

of these conditions is a premise positing as true at least one of the possible laws of a certain group. If the facts furnish means of complete elimination in this group, this condition is sufficient. On the contrary, if elimination remains partial, it is indispensable, for the evaluation of the favour enjoyed by one of the remaining possible laws, to know the relation of the probabilities of these laws.

The assumption of determinism for any given character.—We must first obtain the general premise of all induction by elimination. How is it possible to form a group of laws, at least one of which is true? We can see only one way, and that is by assuming determinism. Given a character A, we postulate that *any case of A is a case of some other character X every case of which is a case of A*, or more briefly, that the character A cannot be produced without a cause. (By a cause, we mean any sufficient condition. By character we mean any property, whether it is a relation or an attribute, consisting of the existence of an antecedent or of a consequent of a specific kind. Besides, it would not be sufficient to postulate merely that there is some character X every case of which is a case of A, or that there is some cause producing A; for A might as well be produced without a cause, so that one would never be sure of finding in every cause of A some case of A. Now, that is just what is required, as we shall see.) We express this assumption by saying that the character A is *determined.*

Henceforth, every case of the character A furnishes a group of possible laws at least one of which is true. For if we designate by α the class of characters other than A belonging to the considered case, there must be at least one character of this class which *entails* A. The primary condition of induction by elimination used in favour of a law *X entails A* is therefore fulfilled if we suppose that the character A is determined.

This presupposition may, moreover, be made more restricted. We can, in fact, limit the nature of the characters capable of entailing A. Thus, we can suppose that when A is a character of events it is determined in each one of its occurrences by past circumstances, or more particularly, by the immediate past and by what is immediately adjacent in space. Such is undoubtedly the limit of what we can think of postulating *a priori*, that is to say, in a primary induction. But in inductions which are relative to effects of a type already known, agreement is more common. In any case, these ulterior limitations furnished by our present knowledge are founded on analogy and hence cannot be certain.

Induction by elimination requires a deterministic assumption.—The determinism of the consequent character or effect is an indispensable premise of all induction by elimination operating in favour of a law joining two characters. This determinism constitutes the very nerve of reasoning, the lever by which the rejection of certain possibilities redounds to those which remain. Here we have a proposition which can hardly be contested.*

Range of this assumption.—There is, however, room for making three observations.

Although the majority of authors are ready to admit that induction in general rests on a deterministic principle, we have just established determinism only in connection with induction by elimination, whose mainspring is the invalidation of laws by contrary cases. It may be that the same thing is true of all

* Mr. Keynes appears to be among the authors who do not admit this deterministic postulate. In fact, Mr. Keynes thinks he can demonstrate that induction by the accumulation of instances can confer on a law a probability higher than the initial probability of determinism itself. Hence, determinism cannot be a premise of this induction. However, Mr. Keynes maintains that this induction is based on the principle of elimination. Therefore, he simply does not recognize that elimination presupposes determinism as a postulate, and cannot consequently confer a probability exceeding that of its premise.

induction. But what we have just said cannot in any way be an argument in favour of this extension of the principle of determinism.

In addition, since induction by elimination is concerned with establishing a law of the form *X entails A*, it is not universal determinism, but only the determinism of the character A. For by furnishing for each case of A a class of characters, one or the other of which entails A, the determinism of A yields all that is required for an inquiry by elimination. And were A the only determined character in the world, the establishment of *X entails A* by the elimination of the rest of a group of possible laws would not be affected.

Lastly, if the determinism of the character A is the principle by virtue of which the invalidation of the law *Y entails A* favours the law *X entails A*, where X and Y are two characters observed in a same case of A; if, in other terms, the determinism of the character A is the backbone of the establishment of a law about the production of A, that should not be understood to imply that this procedure by elimination demands the *certainty* of this determinism.

For if any reasoning whatsoever confers on its conclusion the degree r of probability or certainty by supposing as certain the premise A, this same reasoning confers on its conclusion a degree r' of probability, weaker but not null, when we suppose this same premise A to be only probable to a degree s. Any argument which is favourable to a conclusion when its premise is certain is still favourable to it, although with less strength, when this premise is only probable. This is an incontestable axiom.

In fact, if it is no longer certain, but only probable to a degree s that at least one of the propositions of a given class is true, elimination operates again as before: it collects at each step for the benefit of the subsisting alternatives the initial capital of certainty or probability, until at last this capital falls entirely to the last one.

Suppose then that it is no longer certain but only probable to a degree s that the character A cannot be produced without a cause (or without a cause of a certain sort) and let α be the class of characters which accompany A in a given individual case. The observation of one of these characters in the absence of A increases again the probability that some one of the remaining ones entails A; and the elimination of all save one makes equal to s the probability that the last, X, entails A. For we can say at this point: either the character A has been produced in the given case without a cause (or without a cause of the supposed sort), or else X causes A; now there is a probability s that the first side of this alternative is false, consequently that the second is true.

It is then inaccurate to say that the certainty of the determinism of the character whose cause is sought, is a necessary prerequisite of induction by elimination. *The probability of this determinism is sufficient.* As slight as it may be, induction by elimination has some force and renders more probable the law in favour of which it operates. The general nature of inference demands this. For it posits that the degradation of a certain premise to a probable premise lessens the force of an argument without destroying it.

However, this same nature of inference postulates as the limit of the probability that induction by elimination can confer on a law of the form X *entails* A, the probability of the determinism of A. For an argument can convey to its conclusion only a probability at most equal to that of its least certain premise.* It then remains true that induction by

* Anyway, the probability to consider is not the one that admits a cause for A in *all its occurrences*, but the one that admits a cause in *any one of its occurrences*: for what is important is the probability of the presence of a single cause in the individual occurrence of A which furnishes the list of possible causes on which elimination is operati\

elimination cannot reach certainty or approach it indefinitely unless the determinism of the character whose cause is sought is certain or infinitely probable.

Such is the meaning, which is, moreover, simply in conformity with logic, of determinism as a premise of induction by elimination.

The other conditions of induction by elimination.—Let A be a character that is determined (or that is determined by some characters of a certain kind). What more is necessary for induction by elimination to operate in favour of a law of the form *X entails A* ? It is still necessary to form the list of characters (or of the characters of that kind) accompanying A in a given individual case. Finally it is necessary that the facts eliminate all the characters of this list except the character X. The law *X entails A* is rendered certain when these three super-premises are certain; and consequently when one or the other of these premises is only probable, the law is rendered probable to the same degree as their group. Such are the conditions and the power of complete induction by elimination. We see that this sort of induction is an inference that is certain, in the sense defined at the beginning.

When the second or third condition is not fulfilled, there subsist alongside of X other characters that are not eliminated, and elimination remains partial or incomplete. By coming under the general hypothesis according to which, with each elimination of a character, the chances of the remaining characters retain their relationship, the probability conferred on the law *X entails A* by the elimination of only some of the concurrent laws *depends on the initial probabilities* of this law and of the remaining laws. So long as these probabilities are not known, we do not know which one is conferred by partial elimination upon the law *X entails A*; every conjecture about this law is a conjecture about the others. The use of partial elimination does not require the knowledge of all the characters which can be the cause of A in any given particular case. On the contrary, it requires the knowledge of the initial probabilities of the characters that are being considered. This condition ought not to be forgotten.

We now know the conditions of induction by elimination, no matter whether it is complete or partial. Let us see if the universe fulfils them.

A.—Instances Completely Known

Let us first examine instances of induction by complete elimination. We are given a character A that is determined, and the list of the characters accompanying A in a given case that are capable of being its cause. It seems that the elimination of all but one of these characters is henceforth merely a question of skill or success. But on the other hand, it may have to face an impossibility that is altogether rational in nature.

Plurality of causes.—Suppose, in fact, that among the characters present with A in the given case and capable of being its cause, we find *more than one* which invariably accompanies A. This is possible because the postulate of the determinism of A says simply that there is *at least* one of these characters entailing A. Let X and Z be two among these, *each* entailing A. It is then impossible to establish by complete elimination *any one of these two laws.* That is evident, for no one of these two can be invalidated while they are both true. But when their truth is not known, induction by elimination stops at the incomplete result that at least one of the two must be true, without being able to say which one, nor if both are true.

But is this plurality of causes in the same case of an effect a special and very rare occurrence ? Quite on the contrary, it is the general rule and is absent only in two particular cases.

The possibility of a complex cause renders complete elimination impossible.—As a matter of fact, it may be that the list of the possible causes of A in the case under consideration comprises, beside the characters L, M, N some characters compounded of several of these, such as LM, or LMN: this is what takes place every time that the information which is revealed about the determinism of A does not exclude with certainty causes that are composite or complex in nature.

Let X be the least complex character which entails A. Any other character of which X is a factor, such as LX, then also entails A. We now have the complexity of possible causes at the crux of the problem of elimination. This complexity moreover, is not exceptional, but rather normal, since it exists in all the cases where, on the list of the characters of the instance taken as a basis, the least complex and real cause is not at the same time the most complex of the possible causes.

Induction by elimination will then leave us with the possible laws *X entails A, LX entails A, MX entails A, LM . . . X entails A*, while we have not yet succeeded in demonstrating the first of these laws. It is true that the last is established, since it is presupposed by each one of the preceding ones. But it must be noticed that this last law already follows from the same premise which posits the determinism of A. For the character LM . . . X, the most complex of all, is simply the conjunction of all the possible causes of A in the considered case, and this conjunction, since A is supposed determined, cannot fail to entail A.

But when we are granted a determined character and the list of possible causes which are present in one of its occurrences, the establishment of a law of the causation of this character by the method of induction by complete elimination then meets an insurmountable theoretical obstacle in the possibility of a composite or complex cause.

Now this possibility surely exists, for many effects admit causes more complex than they are themselves. Thus, the colour of a mixture is determined by the diverse colours of its elements. We must conclude that elimination cannot be complete except for the case of the total character uniting all the causes, which has no need of the method of elimination since it entails the effect as a result of what has been postulated in the principle of determinism. And it is to be remembered that induction by complete elimination is the only type of induction that reaches conclusions with certainty.

Partial elimination: a principle directed against the complexity of causes.—It remains to be seen what probability can be gotten from an elimination which is condemned to incompleteness because of the possibility of a complex cause. Let X be the simplest character (of the list) which entails A. All the characters simpler than X or as simple as X have been eliminated; but, there remain the characters which cannot be eliminated without removing A also. These include X of course, and all the characters including X as a factor such as LX, MX, LMX, etc. The laws *X entails A*, *LX entails A*, etc., remain before us. We know that they share their total probability in the ratio of their initial probabilities.

Thereafter, we might think of appealing to an *a priori* principle of simplicity, saying that there is, for every chance p of complexity a certain degree π for a given effect to admit a cause of complexity lower or equal to p, and that this chance π, a function of p, increases in proportion as p increases and finally approaches certainty. Such a principle would be plausible, for it only puts into exact language the accepted opinion that an infinitely complex cause is infinitely less probable.

Insufficiency of such a principle.—It would give to X, the simplest cause possible, a certain advantage over LX, MX, LMX, etc. . . . However, this advantage would remain finite. Its measure would be in fact the value of π corresponding to the degree of complexity of the character X, a terminate finite value. This principle would not then permit induction by elimination to surmount the obstacle which proceeds from the possible complexity of causes except in a very imperfect manner. For it would not put induction by partial elimination, the sole remaining possibility, in a position to confer upon a given true conclusion either certainty (of course, henceforth impossible) or even a probability approaching certainty indefinitely.

Now, that is all that can be expected from induction. That induction does not yield certainty is admitted without difficulty. That practice, by limiting the facts at one's disposal, limits the probability of inductions, will be admitted still more easily. But that a purely theoretical reason, essentially indomitable, condemns induction to stop at a finite probability, is a conclusion that is only accepted as a last resort.

Another principle directed against the plurality of causes.—We may conceive another *a priori* principle of simplicity capable of bringing to induction by elimination an infinitely more efficacious aid against the obstacle of the complexity of causes—an aid that is, however, indirect. This principle would posit as improbable, no longer the *complexity* of causes but their *plurality*.

Suppose, in the first place, that this plurality is excluded. Assume not only that a character A is produced by some cause, but also that it is the effect of one identical cause, so that there exists a character X (of a specified kind or not) which is inseparable from A. Induction by elimination can then establish with certainty what this character is. It ought to be, in fact, one of those which accompany A in any particular case. But we now have the right to eliminate any character which is absent in the presence of A, as well as any character present while A is absent. In this manner we are delivered from superfluous characters such as LX by showing that if they may be sufficient to entail A, X alone is necessary. In order to establish the inseparability of A and X, it is then enough to have two cases of A having only X in common (by neglecting the characters which are not of the specified sort if there is one sort).

But to postulate in advance that a certain character A satisfies the unique condition of being both necessary and sufficient is a very bold assumption. It can be accepted by reason and yet not appear sure. Hence the following principle of probability seems to contain everything that is plausible and acceptable: *For any number n, there is a probability* **n** *that A involves a necessary condition formed by the alternative of less than n sufficient conditions, and* **n** *approaches certainty when n increases to infinity.*

It is necessary to speak in this way in order to avoid the following complication. If we said *that the number of causes of A should be less than n*, which would be the more natural way of expressing one's self, we would come up against the fact that when X is the cause of A, any character such as LX is also its cause; so that, in order to state what we mean, it would be necessary to carefully exclude from the account superfluous causes of this type. Thus the expression which appears at first to be most simple would involve in the end a much greater complexity of statement.

However, the principle is not yet quite satisfactorily stated. The preceding formula is good when we do not know anything yet about the number of the causes of the effect A. But suppose that we already know that these causes are pluralistic, that they are *at least* as many as *m*: this knowledge would result from the observation of *m* cases of A not having, taken two at a time, anything in common, i.e. they do not have anything in common that is known to be capable of causing A. Either this knowledge eliminates the operation of the above

principle, or it lets it subsist. In the first case, it is impossible to render it infinitely probable that X entails A when we already know that Y entails A, which seems quite absurd. In the second case, what follows is the quite dubious consequence that the more diverse causes of the same effect we know, the less probable is it that there are still more to be admitted. That is because the aforestated principle, if it is posited as applicable to the effects whose diversity of causes is already admitted, goes beyond what is really necessary. All that is required, is that if *m* is the minimum number of diverse causes that are known to produce an effect A, it is infinitely improbable that an infinitely greater number of causes than *m* are admissible. We may express this in precise terms as follows: *Knowing that the character A does not admit any necessary condition formed from the alternative of less than m sufficient conditions, if we designate by* **n** *the probability that A will admit a necessary condition formed from the alternative of less than n sufficient conditions, the value of* **n** *approaches unity when* **n** *increases to infinity.* Such is the principle which seems to me most easily acceptable. When we know nothing about the plurality of the causes of A, the minimum *m* takes on the value *l* and the principle reduces itself to the preceding one.*

The indefinite increase of probability by multiplying instances.—Let there be two cases of A which have nothing in common but X. They no longer suffice, as before, to show that X is both a necessary and sufficient condition of A. But we can say: either X is a sufficient condition of A or it is not. If it is not, A involves in the two cases (since these cases have nothing in common outside of A and of X) two diverse causes. Any necessary and sufficient condition of A is then formed from the alternative of at least two characters. Therefore,

* Instead of making **n** a function of *n*, we make **n** a function of *m* and *n*. For a fixed value of *m*, whatever it is, *n* approaches 1 when **n** increases to infinity; but the value of **n** for a given *n* may diminish when *m* increases.

X entails A is as probable as the probability that A involves a necessary and sufficient condition formed from the alternative of less than two characters, namely one character. This probability has the value **n** when $n=2$. This probability **n** has undoubtedly a minimum value in this case, but it is never less than a finite number.

But the same holds true for more than two cases. If we have a hundred cases of A, any two of which have nothing in common but X, we shall demonstrate in the same way that if *X does not entail A*, any necessary and sufficient condition of A may derive from the alternative of at least 100 characters. Consequently, *X entails A* is as probable as the falsity of this consequence. Now this probability will have the value of **n** when $n=100$. It is then clear that the indefinite accumulation of cases of A, any two of which have nothing in common with X, conferring upon the law *X entails A* a probability equal to the successive values of **n** when n increases without limit, makes this law as probable as one wishes. This result is satisfactory, for certainty then seems accessible.

It calls for certain remarks.

In the first place, the kind of induction to which it is applicable is an induction by elimination, i.e. by infirmative invalidation. The multitude of the causes of AX on which it is founded do not serve in any to confirm the law *X entails A*, by reason of favourable instances, but really serve to invalidate other laws by reason of contrary instances. And it is from this invalidation alone that the corroboration of *X entails A* is established by means of our *a priori* principle. In fact, among the necessary and sufficient conditions of A which are initially possible, the observation of n cases of A having nothing but X in common, taken two at a time, eliminates all those possible causes which are formed from the alternative of less than n characters, with the exception of those causes which contain X as one of their characters. But to say that X is

one of the characters whose alternative forms a necessary and sufficient condition of A is to say that X is a sufficient condition of A, or that X *entails A*. Such is the mechanism of this inductive inference: it really depends in the last resort only upon the infirmative action of facts on propositions of law.

It is hence remarkable that this infirmative process of elimination brings out the most striking feature of induction by simple enumeration, which is opposite to elimination in principle depending, as it seems, on the corroborative action of its instances. This feature is the rôle that the multiplication of instances plays in the establishment of the law. This numerical factor enters here without owing anything to the uncertainties of practice, since complete knowledge of the characters of each instance has been granted. Despite that, in order to make the law *X entails A* infinitely probable, an infinite number of cases of XA is required. It may appear that their direct confirmative action is being utilized. But in reality, we are utilizing only the infirmative action that they exercise on concurrent laws.

Idea of a theory of induction by repetition.—The appearance of corroboration and the reality of invalidation suggest a theory of induction by repetition. Perhaps, in fact, such induction is never anything more than what has just been described, or is always at least some analogous form of reasoning. The favourable strength of instances which verify a law would not be, as it seems, a direct confirmative influence. Their strength would itself be nothing more than the fatal virtue of those instances which invalidate laws by going counter to them. This conception, as we have just seen, is the basis of the opinion that a new instance does not fortify a law unless it satisfies the condition of being different from all former instances. It is even necessary, according to the preceding theory, where we suppose each instance completely known, that the new instance differs from each former instance in all its characters except the two which the law wishes to bind. Thus we regain through the operation of a number of varied instances that law which engenders and conditions such a variety: the conception of induction by enumeration thus qualified is satisfactory to reason. We shall come across it again in this study.

Summary.—We postulated at first a determined character A and the complete list of the characters which accompany it in one of its instances and are capable of entailing it. Unless we are sure that these characters, one or the other of which perhaps entails A, cannot entail several effects—and of that we are rarely sure—or unless A is on the contrary entailed only by the total character uniting all the others—and that is an exceptional state of things fortunately—elimination cannot be completed; so that we cannot establish by induction a law of the form *X entails A* with certainty. Incomplete elimination does not confer, moreover, on this law a definite probability except with the aid of a special principle of probability. If this principle is directed against the complexity of causes, the origin of the difficulty, the principle affords us only an inadequate basis. For it does not permit the law *X entails A*, supposed to be true, to exceed a mediocre probability. On the contrary, if this principle is directed against the plurality of causes, it furnishes a satisfactory solution by means of a detour. It permits us, in fact, to make the law *X entails A* as probable as we please on the condition that we have at our disposal as large a number as we wish of cases of XA not having anything in common but X, when taken two at a time. It thus places induction by elimination under the dependence of number, requiring, besides, variety. Such are the results of the criticism of elimination itself. These results followed from assuming that the characters of each instance of XA, at least

those which are to be considered, were completely known, and secondly, that the determinism of the character A is certain or at least infinitely probable. It is these last conditions which we must now examine.

B.—Instances Incompletely Known

The individual samples of nature are known only incompletely.—Retaining determinism, let us ask whether the characters of each instance are in fact so well known in nature, where induction operates surely in so important a way. To ask the question is to answer it. The circumstances involved in any fact of nature, whether physical or mental, are never known except partially. If we do not restrict ourselves, these circumstances, in fact, embrace the totality of the universe in time and in space, a totality which escapes us infinitely. But even if we limit ourselves to the immediate neighbourhood and past, a more profound reason makes the complete knowledge of this limited realm no less inaccessible. It is a matter of fact that the total or partial cause of a comprehensive effect is sometimes hidden. It becomes comprehensible only as the outcome of a test which consists of an experiment properly speaking, or of the application of an instrument such as a microscope, which is also really an experiment. And that is true of mental effects as well as of physical effects. A mental phenomenon may have its total or partial cause in a state which the reaction to a certain test or the answer to skilful questions alone makes manifest to the very consciousness of the subject. This is the whole problem of personality and temperament in psychology.

In the domain of the facts of nature, the result of a test may then be an important circumstance. Hence, to be sure that we have not omitted any circumstance, it would be necessary to have applied all the tests possible. In order to determine surely and completely the state of a piece of matter, it would be necessary to test its behaviour by means of all the substances which will ever be discovered and to examine it by means of all the instruments which will ever be invented. Likewise, to determine with certainty the state of a mind, even for its own consciousness, it would be necessary to apply to it all the tests that the ingenuity of psychologists will ever imagine: a task that is perfectly impossible.

Undoubtedly, it may seem to us that certain tests are sufficient to show the complete manifestation of a given physical or mental state. But we cannot be sure of this, for it remains possible that these tests let differences escape whose action has not been noticed yet, but which really have effects. Such was the electrical state of bodies before their first effects had been discovered. And above all, this more or less strong assurance and presumption of knowing everything necessary to reveal the circumstances which should be taken into consideration, are founded on experience, that is to say, on previous inductions.

In fact, how do we know that in the investigation of the cause of a certain effect, we have made an inspection of the circumstances to be taken into consideration when we have noted certain characters and certain results ? It can only be by means of an analogy with the already known causes of other effects.

Observation can inform us only indirectly that such a character has no part in the production of such an effect. So long as one is in complete ignorance about what is involved, one is just as unaware of what is not involved; and it is only by indicating the characters which should be noted that experience excludes by past selection all the others. That is particularly clear for characters that are not revealed, such as was the state of electricity before it was conceived. For if we judge it improbable that a character of this still unknown sort enters the list of the possible causes of an effect that is

being examined, it cannot be by a direct establishment of its lack of causal influence, since it has never been perceived and its very existence is not known. It is therefore really the analogy of laws already demonstrated or probable, and only this analogy, which limits the probable causes of a certain effect to a known part of the characters in the immediate spatio-temporal neighbourhood.

The force of this analogy between the unknown causes of a new effect and the known causes of similar effects is not everywhere the same. Its invariability makes induction more or less powerful according to the novelty of the effect and according to the knowledge acquired about effects of the same kind. This analogy is exact and rigorous in sciences already possessed of certainties; it is obscure and loose when its only foundation is the mass of common experience. Lastly, it is nearest failure when it deals with phenomena detached from both known science and practice, such as so-called " psychical " phenomena. It is then that we see the weakness of induction in its first steps, when it does not know yet where to turn.

Thus, induction gains autonomously a kind of momentum which increases in strength as it progresses. Its power has a " snow-ball " or cumulative effect. The limitation of " important " circumstances can finally have no more effect than a single type of character which must be taken into consideration in the production of a given effect. It suffices to discover the character of this type which is present in order to judge immediately whether it is the cause that is sought. Then that is why scientific induction says proudly: a single experiment is enough, provided that it is done by a man who knows how to direct his attention.

Conditions of primary induction.—But logic, which is sovereign, should lead back to modesty. Induction which depends on an induction for support is a secondary induction. Now no secondary induction, as certain as it may be, taken by itself, yields a result more probable than primary induction can, because no reasoning, even when certain, can render its conclusion more probable than the most doubtful of its premises. If the limitation of circumstances to be considered in the production of a certain effect is the conclusion of an induction—and we have just shown that it is—it must be because induction can be validly exercised without the principle of limited variety. And the probability it then reaches marks the definite limit of its power.

This question concerns logic and not history. In the group of laws that an empirical science contains, the problem is not that of discerning the first fruits of induction which are still devoid of analogy. The points of application of primary induction matter little. It is sufficient to have seen clearly that primary induction is at the basis of the whole induction. If care is not taken, the consideration that any law at all may be established after a few experiments by scientific induction —just as it is done in class room demonstrations and lectures —might lead one to the illusion that the same holds for all laws. But that would be to forget that any law is corroborated so easily only by being aided by the analogy of all the others already known. It would be as if one were to say that a table can stand without legs because any one of its four legs may be removed without making the table fall.

For a science of nature to be established by induction, it is then necessary that induction should know how to accommodate itself to phenomena which are partly unknown, without having any assurance that the unknown part is negligible. Pursuing the study of induction by invalidation, let us investigate how these new conditions of uncertainty alter the power that we have attributed to it.

Primary induction by elimination when applied to nature is not satisfactory.—Suppose in the first place that the complexity of the cause is excluded. Starting from one case of the effect A, it is then possible to eliminate all the characters of this case that have been observed, except the character X, since we have assumed the right to neglect characters more complex than X and containing X as an element. So long as we admitted that this complete elimination in favour of X bore on the totality of the characters present with A in the given case and capable of being its cause, the law *X entails A* was established with the same degree of probability or certainty given to the assumption of A's determinism. But here we admit that certain characters have escaped us. The complete elimination that we thought we could make has therefore affected only an incomplete list. It is really a partial elimination.

It is to be remembered that the result of a partial elimination depends on the values of certain initial probabilities. The initial probability of the eliminated characters is divided, according to the more general hypothesis, among the remaining characters in proportion to their initial probabilities. The probability that the law *X entails A* derives from the elimination of all the concurrent laws which have not escaped us, is not measured by the good will or care we have exhibited in the task of eliminating what is beyond our powers. We cannot say, as we should like, that the law is rendered as probable as is possible at the moment we have done everything in our power. No; the probability of a law depends on rules that are more firmly established than this rule of good will. The probability of the law *X entails A* depends exclusively upon the intrinsic probabilities of this law itself and upon other concurrent laws.

Of these intrinsic probabilities we have no idea. It would be necessary, in order to know them, to compare the chances that each of the observed characters has of being the cause of A, not only with those of the other characters, but again with those chances of the characters which have escaped observation, all of which are unknown, including their number. These probabilities are unknown. They are undoubtedly mediocre, because of their multitude. In any case, they are finite. Elimination can then operate to confer on the law *X entails A* only an insufficient probability and not anything near certainty.

Recourse to repetition.—We cannot be content with this result. For, once again, the power of this first indication which cannot yet depend on any analogy, limits the whole power of induction applied to nature. We should then try to improve the result. The means are evident; repeat experiments. It is commonly admitted, in fact, that in such a condition of ignorance, it is necessary to consent to depend on the number of instances.

But we are not looking simply for what is to be done. We are seeking the logical mechanism and the principle of these counsels of common sense. It is here suggested that we strengthen the obscure and mediocre probability that elimination has just reached by a procedure which is very similar to induction by simple enumeration. But we have already met the same necessity of accumulating instances of the law that we wished to establish. We recall, however, that this accumulation did not operate in a simple manner and according to appearance, but operated indirectly. It might be the same here. Under the multiplication of instances which is imposed upon us for the second time, the principle of elimination might be found again. It may thus suffice, against appearance, to render infinitely probable a law of nature. Such is the possibility which remains to be examined.

Attempt to found the influence of repetition on a principle of elimination: preliminary assumption.—This possibility requires us to posit anew an assumption on which it rested up to this point, namely, that the plurality of causes is improbable. In fact, so long as nothing is assumed in this respect, the elimination of the possible causes of A can be done only by adding to a case of A cases of *non*-A in which certain circumstances are found again and are thus eliminated. But the addition of several cases of A can eliminate nothing, so long as no weight is given to the conjecture that A proceeds in all cases from the same cause. With a view to explaining the influence of the multiplication of instances by the mechanism of elimination, it is necessary in a general way, it seems, to posit some principle directed against the plurality of causes.

However, this principle does not operate here as before. When we supposed each instance perfectly known, the probability conferred upon the law by a great number of its instances, having nothing in common when taken two at a time, was the very probability posited by the principle viz. that the effect did not proceed in so many cases from diverse causes. If we excluded the plurality of causes, instead of taking it as merely improbable, two instances might suffice to demonstrate the law, and accumulation would no longer play any part. But we are now faced with imperfectly known instances: this imperfection, even excluding any plurality of causes, by itself makes necessary an infinite repetition.

Suppose, for simplicity, that the effect A admits a cause in all its occurrences. The encountering of two cases of A having nothing in common but X does not suffice *according to our knowledge* to give to the law *X entails A* a satisfactory probability. For the probability that they yield is the one obtained from assuming that their unknown parts have also nothing in common. This probability is finite, undoubtedly mediocre, and quite obscure. But the continuous accumulation of such cases of XA having nothing known in common appears capable of gradually raising the probability of the law *X entails A* and to make it as close as one wishes to certainty. Either this is necessarily true or the natural sciences ought to give up trying even to approach certainty. For in the presence of partially unknown instances—and such are all natural phenomena—the number of instances alone affords any hope of compensating the imperfection of analysis. Can its action be explained by a principle of elimination ?

Theory of the probability of elimination.—Yes, answers Mr. Keynes, and it has no other source. If a second case of XA increases the probability of the law *X entails A*, it is because it differs from the first according to our knowledge, or at least it has some chance of differing from it in our ignorance. This elimination, certain or probable, of some character from the initial concurrent case of X as a possible cause of A constitutes the whole favourable influence of a second case of XA on the probability of the law X *entails A*. Likewise, a third, a fourth, an *n*th case of XA operate only because they eliminate or have some chance of eliminating a character common to all the preceding cases. It is by this cumulative tendency of the cases of A to reduce their common part that it increases the chances of the persistent character X to be the cause of A: such would be the real source of induction by repetition.

Let us quote Mr. Keynes:

> "The whole process of strengthening the argument in favour of the generalization ϕ *entails* f* by the accumulation of further experience appears to me to consist in making the argument approximate as nearly as possible to the conditions of a perfect analogy, by steadily reducing the comprehensiveness of those resemblances ϕ_1, between the instances which our generalization disregards. Thus the advantage of additional instances, derived from experience, arises not out of their number as such, but out of their tendency to limit and reduce the comprehensiveness of the class ϕ_1. . . ." (*A Treatise on Probability*, p. 227-228.)
>
> And again: "I hold then that our object is always to increase the Negative Analogy, or, which is the same thing, to diminish the characteristics common to all the examined instances and yet not taken account of by our generalization. Our method, however, may be one which certainly achieves this object, or it may be one which possibly achieves it. The former of these, which is obviously the more satisfactory, may consist either in increasing our definite knowledge respecting instances examined already, or in finding additional instances respecting which definite knowledge is obtainable. The second of them consists in finding additional instances of the generalization, about which, however, our definite knowledge may be meagre; such further instances, if our knowledge about them were more complete, would either increase or leave unchanged the Negative Analogy; in the former case they would strengthen the argument, and in the latter case they would weaken it; and they must, therefore, be allowed some weight." (*Ibid.*, p. 234.)

* Designated in the text by $g(\phi f)$.

The theory of induction by repetition which these two passages summarize very clearly is most surely deductive. It justifies the opinion common among philosophers that several instances not possessing, according to one's sure knowledge, any difference, would not have more weight than a single instance. It encourages the idea that induction by multiplying instances is not really a valid principle, but is efficacious only to the extent that it imitates induction by analysis. It brings all induction back to elementary infirmative operations. So this doctrine has something distinctive about it which pleases the mind. The theory that Mr. Keynes proposes is hence incontestably the natural theory. But it is no less necessary to examine it.

Development of the theory of determinism.—We have shown at the beginning of this study that all induction by infirmative action in favour of the law *X entails A* requires as a premise the determinism of the character A, that is to say, the proposition that any sample of A is also an instance of at least a character entailing A.

But it is possible to have at one's disposal more definite knowledge. Let us take a certain sample of A. We have just posited that *the class α of all the characters of this sample* (*other than A*) contains at least one character which entails A. It is not inconceivable that we are in a position to say as much about a *more restricted* class. It is possible that we know that the class a of characters, which is only one part of the class α, contains even by itself at least one character which entails A. It is possible that we know this to be true of *several* partial classes a_1, a_2 . . . formed of characters of the considered sample of A. For it may be that A admits in each one of its occurrences *several* sufficient conditions. Finally, it is possible that for some of these classes—a_1 and a_2 for instance—it is certain that they contain a character which entails A, and that for others—a_3 and a_4 for instance—the same thing may be only *probable to degrees p_1 and p_2* respectively.

This is what happens in the assumption commonly made about the determinism of the characters of natural phenomena. Indeed, if we were limited to the proposition that any one of the characters of a phenomenon is entailed by some other, that would amount to scarcely anything; for the characters of a phenomenon, if they are not limited, include its relationships to all other phenomena past, present, and future. On this point people are ordinarily agreed.

It is taken as certain that any character present in a phenomenon is entailed by at least one of its other characters which involve neither the future, nor even the present, but only *the past*. Furthermore, that any such character is entailed by at least one of the characters which involve only such a date, or more exactly *such a section of the past as we wish*, however short it may be; for we believe that the state of nature in any duration, however brief it is, determines its state at all subsequent times. Again, we assume sometimes that any character of a phenomenon is entailed, if we refer to a section of the past sufficiently proximate, by at least one of the characters which involve a *region of sufficiently restricted space* around the phenomenon studied.*

We may then represent the determinism of the characters of a natural phenomenon by the familiar image of the concentric waves produced by the fall of a stone into a lake. Except that we must imagine the process backwards: the waves starting from the periphery enclose each other and run back towards the place of perturbation, where upon their arrival they reach the cause. Thus, the conditions capable of determining an event occupy at a given previous date a region becoming vaster in proportion as this date recedes further back. Running in from the outer limits of the past, so to speak, they lock themselves around the event and converge towards the very space that the event fills.

In the heart of the class α of characters of a sample of A, those characters which involve any section of the past (and which are also restricted to a finite region of space, if we admit the last assumption) form then a partial class a about which it is *certain* that it contains at least one character which entails A. There is then an infinity of these classes a culminating in the total class α.

This is not all. Consider the class of the circumstances of

* It is clear that this postulate is necessary to eliminate the influence of probable causes escaping us because of their remoteness. This is very clearly stated by M. Painlevé in the article *Mécanique* in the volume *De la Méthode dans les Sciences* (On the Method of the Sciences). We may notice that this postulate, directed against action at a distance, is on the contrary a consequence of the principle according to which any influence is transmitted from the proximate to the proximate.

the given sample of A which are contained in the present like A itself—in other terms, the circumstances which are *contemporaneous* with the effect, and no longer *temporally prior* to it. Is there not some probability that the circumstance A is also entailed by at least one character among its concomitant characters?

We shall first show that this probability cannot reach certainty as in the preceding case. The thing is evident; it may be demonstrated.

In fact, any character is just as well a conjunction of characters. Thus, all the characters of a phenomenon which are simultaneous with any character M make up all by themselves a character, and if it were *certain* that any present circumstance, hence also this integral character, is entailed by some present circumstance, it would be certain that any one at all of the present circumstances entails all the others. It would be certain that its recurrence would assure their recurrence. Now that is manifestly contrary to fact. In our universe many characters meet without entailing each other. There are then characters, at least complex ones, which are not entailed by any simultaneous character. From which fact it follows that it cannot be certain *a priori* that any character whatsoever is entailed by some simultaneous character.

It will even be admitted without difficulty that the *a priori* probability that such is the case, is very mediocre and very far from being practically equivalent to certainty, although it is not negligible completely. Let us designate it by Π. Likewise, there is a probability π for any character to entail some simultaneous character; and this probability π is neither extremely large nor negligible.

Application to induction by elimination.—In a general way, if the character A is entailed (with a certainty or probability **p**) by at least one member of a partial class a of the characters

which accompany it in any given instance, the search for a character entailing A may operate by an elimination bearing on all the characters which are members of a. But once all these characters less one are rejected, it is known with a degree **p** of certainty or probability, and only to this degree, that the character left entails A.

These results are going to be useful to us in a moment.

Can the probability of elimination be the principle of induction by repetition in its application to nature?—It is not without some hesitation that we are presenting the following arguments. They are longer and more complex than might be desired; they demand an effort of attention. But believing them correct, it is best to offer them. We must follow the argument whither it leads: ὅπῃ ἂν ὁ λόγος ὥσπερ πνεῦμα φέρῃ, ταύτῃ ἰτέον.

If the only resource of induction is infirmative; if its only mode of operation is the rejection of possible causes in favour of the remaining ones by elimination; if the favourable influence of a new instance of the law *X entails A* consists altogether of the certain or probable elimination of some new member of a class a of circumstances present with X and A in the initial instance, at least one member of which it is certain or probable (to the degree **p**) entails A, the following results ensue:

We have already shown that this view requires some principle directed against the plurality of the causes of the characters A whose production is to be rendered in some probable law. Let us grant the maximum probability by excluding any plurality of causes on the assumption that there exists at least one character (simple or complex) entailing A, and conversely, entailed by A. Assume as certain that a partial class a of the circumstances of any instance of A contains such a character. According to what has just been said, this class cannot be the class of the circumstances contemporaneous with the effect A. It must be the class of the circumstances relative to some *antecedent* duration, that will be imagined, most naturally, very close to the very appearance of A. We shall call the members of this class (for we shall consider only one of them) the *antecedents* of A in the instance in question. We shall call the characters contemporaneous with A the *concomitants* of A.

Thus, we posit it as *certain* that at least one of the antecedents of any character is inseparable from it. On the other hand, it remains *probable to the degree* Π that any character whatsoever is entailed by at least one of its concomitants, and *probable to the degree* π that it entails at least one of its concomitants. The values Π and π are neither negligible nor practically equivalent to certainty. This group of assumptions is surely the maximum of what can be thought to agree *a priori* with the determinism of natural phenomena. If we succeed in showing that under these very favourable conditions the theory under consideration is not satisfactory, we shall have really proved *a fortiori* that it is not satisfactory under any conditions.

The probability conferred upon the law *X entails A* (X being an antecedent of A in a given case) by a number *n* of its instances is, in the concept under examination, a probability of the second order. It is the probability that we shall find among these *n* instances the realization of a certain possibility of elimination of the antecedents of A, which would itself give to the law *X entails A* a certain probability. It is in this way that induction by the multiplication of instances, instead of being a true argument, would be only the shadow of one.

This idea demands more exact statement. For we do not know with certainty just to what point our *n* instances of XA force the elimination of the antecedents of A. Perhaps they keep only X; perhaps they leave another subsist, or two others, or *x* others. But these different hypotheses may be unequally

probable. In each one of them, the probability of the law *X entails A* is whatever the hypothesis would yield, if it were realized, multiplied by the probability of this realization. The round total of the probability conferred upon the law is then some average value among all these products, lower than the greatest probability among them.

In order for this probability to approach certainty when *n* increases, it is necessary for one of these products to do as much. And for that to happen, it is necessary that the multiplication to infinity of the instances of XA renders infinitely probable the realization of a possibility that elimination itself will render the law *X entails A* either certain or infinitely probable.

This possibility of elimination can be only one of the following two conditions: X *is the sole antecedent of A which is common to all the instances considered*; in this case, *X entails A* is certain. In the second case, antecedents of A other than X remain common to all these instances. But *it is infinitely probable that these other antecedents either entail A or else are entailed by* X. In both cases, *X entails A* is an infinitely probable effect.

We can even neglect here the first alternative.* For the antecedents other than those in question are characters which have escaped the means of observation employed. We know nothing of their nature, and it cannot be less probable, and more particularly, *infinitely* less probable that A is entailed by some one of them than by X.

A possibility of elimination making *X entails A* certain or infinitely probable is then a state in which X remains *alone* or else *in the presence of other antecedents of A which are with infinite probability entailed by* X.

* It is the hypothesis we met while studying the mechanism of repetition for completely known instances and for a plurality of causes which increases in improbability with an increase in their number.

Does the multiplication to infinity of the instances of XA render infinitely probable the realization of one or the other of these possible states?

It cannot render the realization of the first possibility infinitely probable. In fact, it does not diminish the initial probability for X to entail some concomitant among the antecedents of A which escape observation; and this probability **p**, without being extremely small or large, is surely not negligible. In the presence of an infinite number of cases of XA not having, so far as our knowledge goes, any common character, a finite probability *at least* equal to **p** subsists then for all those cases having in common one or more antecedents of A unknown to us. We shall designate the group of such cases Y. The probability of a state of things is at least equal to the probability of any hypothesis which implies it.

But we must carefully notice that if the existence of an unobservable concomitant entailed by X implies the presence of the same unobservable character in all the examined cases of X, the latter, on the contrary, does not imply it. For it is evidently possible for a character Y to accompany X in all the cases under consideration and yet abandon it in others. Surely, in proportion as the number of examined cases increases, this hypothesis becomes infinitely probable. But let us take care to remember that it is exactly the very principle of induction by repetition which is behind the whole process, a principle which it is exactly our office to justify in the theory we are examining.

On this theory, is it then infinitely probable that any Y concomitant with X in an infinite number of cases is entailed by X?

That is what the theory of the probability of elimination should prove. But it does not prove it; it cannot prove it, for only the falsity of the theory makes it true. Let us try to make this contradiction manifest.

Suppose X and Y are the only antecedents of A present in an infinite number of cases of A, and it is asked whether this supposition renders *X entails Y* infinitely probable.

Now, X is not an antecedent of Y, but a concomitant of Y. It is not *certain* that Y is entailed by some one of its concomitants; it is only probable to the degree Π. Hence, in the theory with which we are now concerned, the hypothesis of the elimination of all the concomitants of Y except X does not render *X entails Y* certain or infinitely probable, but only probable to the degree Π.

We might attempt to get around this conclusion through a detour by objecting that the concomitance of X and Y in an infinite number of cases makes it infinitely probable that the antecedent which entails X also entails Y. But this is to fall into a circle. For we have proved that it is not infinitely probable that all these cases will have only one antecedent in common, even if only one is observed. And it is clear that if there are several, there is a finite probability that one of them entails X and the other Y, unless it is infinitely probable that they both entail each other, in which case we find ourselves back at our start.

The whole argument may be summarized in the following way: In the theory which uses the principle of the probability of elimination to support induction by repetition, it is not infinitely probable that an antecedent followed by the effect in an infinite number of cases is the cause of it. For, in the first place, it is not infinitely probable that this antecedent does not entail some other unknown cause simultaneous with it. Secondly, it cannot be infinitely probable that if another antecedent actually accompanies the first in an infinite number of cases, it is then entailed by the first. For it is not infinitely probable *a priori* that a character is entailed by its concomitants.

Such is the chain of reasoning which seems to us to establish the untenability of both Mr. Keynes' theory and philosophical common-sense about the mechanism of induction by simple enumeration. Such induction does not really confer on the laws of nature a probability higher than the *a priori* probability that some character, simple or complex, is entailed by some concomitant character; and this probability Π is very far from certainty.

But we showed before that primary induction, which bears on all our empirical knowledge of nature, cannot yield a probability approaching certainty except by drawing on infinite repetition. It would be thus demonstrated that the idea of founding any induction on a principle of infirmative elimination leads in the end to an impasse, no matter how agreeable the idea is to the mind dominated so easily by this facile process of elimination. For to deliver to physics as a result of this principle only a mediocre probability separated from certainty by an irreducible interval, is an impasse or frustration to the physicist. It would be somewhat pessimistic to attribute such a cruel limitation to the very nature of things, and Mr. Keynes does not intend to do so.

It is interesting, if we wish to make clearer the exact scope of our results, to compare them with the position taken by M. Lachelier in his *Fondement de l'Induction* (*Foundations of Induction*).

M. Lachelier's ideas.—This writer seeks to formulate, but especially, to prove principles apparently capable of justifying induction, without delaying to determine the exact manner by which these principles apply in fact to inductions that confer a determinate probability. The analysis and verification of his principles are in our opinion fundamental to his theses.

He first presents the classical thesis according to which the essential premise of induction is the determination of phenomena by their antecedents. But—and this is his special

thesis—this first principle is not sufficient; because among the multitude of the antecedents necessary for the production of a certain effect, there may be found, we even know that there will be found, unknowable factors. When we assert that the recurrence of such unknowable antecedents should entail the recurrence of the effect, we suppose evidently, by virtue of some other principle, that all the antecedents required are in fact reunited, at least in most cases. Lachelier gives as an illustration the biological law according to which similar reproduces similar. He then observes that the intervention of imperceptible conditions is no less present in physical or chemical phenomena than in the phenomena of biology. He shows that his principle of the coherency of simultaneous characters in groups is equally necessary for all the inductions of the natural sciences. He conceives this mutual determination of concomitant circumstances as "a principle of order which guards the preservation of kinds." He perceives a teleological necessity about it which he summarizes as follows: "We can then say in a word that the possibility of induction rests on the double principle of efficient and final causes." (Lachelier: *Fondement de l'Induction*, p. 12.)

He realizes that his second principle cannot be, like the first, a principle of certainty, but that it is only a principle of probability. In fact, the coherency of concomitant characters is limited: only certain groups form "kinds" which persist. We cannot, hence, be certain that the recurrence of observable characters by the means at one's disposal assures the recurrence of the imperceptible characters which are perhaps necessary to engender the effect.

Lachelier stops there. He thinks he has solved the properly logical problem of induction by having formulated principles which evidently justify induction. For him, as for most others, the important thing is not to see what the principles of induction are—that seems too easy to them—but indeed to prove them: "They abandon things, and run to causes."* In his haste to pass to this metaphysical task, he does not perceive that the principles whose proof he pursues are not sufficient in any way to justify inductions.

In fact, it is not *certain* that the recurrence of the knowable antecedents of an effect assures the recurrence of the imperceptible antecedents necessary for the reproduction of this effect. It is, therefore, only probable; and in the second place, we can recognize in Lachelier's principle of final causes the assumption which posits the *a priori* probability Π that any character is entailed by its concomitants. Thus, all that results from the two principles of Lachelier is a mediocre probability that the recurrence of the antecedents observed in a certain case assures the recurrence of the effect observed in this same case.

This result cannot be sufficient. We should be able to improve it. It can be done as a matter of fact. How? By multiplying instances. For it is admitted that the initial probability that perceived antecedents entail the perceived effect—and this is all that Lachelier's principles tell us—is capable of being annihilated by one contrary instance, and on the other hand, is also capable of being increased by favourable instances until it approaches certainty.

Lachelier does not stop to consider this ultimate bearing of the facts, nor do his principles take them into account. According to his theory, the probability of the connection of a consequent with an antecedent depends on the probability of the antecedents among themselves, that is to say, as concomitants. His second principle furnishes a certain *a priori* probability for the connection of any two concomitants. But who can assure us that the concomitants *observed the greatest number of times* together have the greatest chances of being connected; and that the probability of their being connected can even rise above the *a priori* probability that one or the other of them is connected with some concomitant, and approach certainty? This question is neither solved, nor even stated by him. Yet it is the most important as well as the most difficult question of induction.

* Montaigne, III, ii.—In this way Lachelier devotes twelve pages to the formulation of the principles of induction and eighty to their proof.

Can the probability of elimination be the principle of induction by repetition in its application to numbers?—First of all, does induction by multiplying instances apply to numbers? Does the multiplication of the verifications of a formula or law uncertain by itself confer on the law a probability that increases towards certainty, in the domain of numbers as well as that of nature?

The possibility, in this domain, of certain demonstrations, and the exclusive value that is attached to them result in making induction by instances unnecessary in principle. It is not officially admitted in mathematics, which is content with nothing less than the certainty it has already once tasted. The very validity of induction by instances is doubted. It is thought, not only that the conclusions of induction in arithmetic state only a probability, but also that this probability is in itself precarious and unsubstantial. Illustrations may serve as suggestions and guides in discovery, but not in the establishment of theorems. If we employ them in the process of discovery, it is at our own risk and peril. No degree of certainty can be founded on experimental illustrations.

This view seems on reflection to be hardly rational. The precariousness of the probability founded on instances is not more real, in fact, in arithmetic than in nature. It proceeds in both cases from the fact that it is forgotten that probability is *relative to the information at our disposal.* Thus the discovery of a demonstration of the truth or falsity of a law about which we knew only numerous verifications cannot weaken the fact that the information which we had was sufficient then to render the law very probable, and only very probable.

But above all, if instances do not support any legitimate probability, mathematicians who still follow the guidance of instances in the investigation of theorems—and the best mathematicians have not failed to do so—are not acting rationally. Mr. Keynes expresses this point very well: "Generalizations have been suggested nearly as often, perhaps, in the logical and mathematical sciences, as in the physical, by the recognition of particular instances, even when formal proof has been forthcoming subsequently. Yet if the suggestions of analogy have no appreciable probability in the formal sciences, and should be permitted only in the material, it must be unreasonable for us to pursue them. If no finite probability exists that a formula for which we have empirical verification, is in fact universally true, Newton was acting fortunately, but not reasonably, when he hit on the Binomial Theorem by methods of empiricism. (See Keynes' reference to Jevons, *Principles of Science*, 1874, p. 231.)

"I am inclined to believe, therefore, that, if we trust the promptings of common sense, we have the same kind of ground for trusting analogy in mathematics that we have in physics, and that we ought to be able to apply any justification of the method, which suits the latter case, to the former also." (*A Treatise on Probability*, p. 243-244.)

Is not Mr. Keynes' judgment reasonable? It is inviting, and in any case authorizes us, to examine this application to numbers of his theory that the probability of elimination is the principle of induction by simple enumeration.

The insufficiency of this theory in this domain of numbers appears quite clearly. There is no need here to mention temporal distinctions in the determinism of arithmetical characters.

Is it *certain* or *infinitely probable* that any general character of the number n, formed from one or several properties—such as that of being the first, or perfect,* or square—is entailed by some other general character of this same number n?

No, that is neither certain nor infinitely probable, but only probable to a finite degree $\mathbf{p}$. For we know that the general characters of numbers form several groups and that it is not sufficient to fix one of the characters of a number for all the others to be equally fixed. Consequently, there is a finite probability not to be neglected, whose value is $1 - \mathbf{p}$ that any general character A is not entailed by any other character. If it were entailed, it would form an independent group, complex or even simple; this would be a fundamental property which does not depend on any other property.

According to the theory which we are now discussing, "The whole process of strengthening the argument in favour of the generalization ϕ *entails* f by the accumulation of further experience appears to me to consist in making the argument approximate as nearly as possible to the conditions of a perfect analogy, by steadily reducing the comprehensiveness of those resemblances between the instances which our generalization disregards." (*A Treatise on Probability*, p. 227.) And by perfect analogy, Mr. Keynes understands the union of two or several causes of XA which eliminates all the rest, that is to say, which does not have any other character in common.

Any collection of numbers presenting the two properties X and A always has, undoubtedly, other general common properties. We should then have to say, according to Mr. Keynes, that this collection, no matter how numerous it is, does not exemplify a perfect analogy, and cannot but constitute an argument *inferior* to perfect analogy which is the ideal and limit. That is the whole thesis which Mr. Keynes defends.

* A perfect number is a number equal to the sum of its factors, e.g. $6 = 3 \times 2 \times 1 = 3 + 2 + 1$. For theorems concerning such rare numbers, cf. Dickson, *History of Theory of Numbers*.—Transl.

But what probability would perfect analogy itself confer on the law *X entails A*? If we knew that two numbers *m* and *n* have no other general property in common but X and A (which is impossible), what degree of probability would result for the law *X entails A*? Would it be certainty? Would it be an infinite probability? No, it would be only the finite probability **p**. In fact, there is a probality $1-\mathbf{p}$ that the general property X depends on no other, and consequently does not depend on A. All that a perfect analogy can prove is that X is the only general property which *may* entail A. But *X entails A* does not result except to the degree in which it is probable that A is not an independent group of general properties of numbers or a fundamental general property not entailed by any other of its properties.

According to Mr. Keynes, the probability that a perfect analogy would establish, limits ideally the probability that the multiplication of instances can give. *The latter does not approach certainty, but remains lower than the finite probability* **p**: a result that is hardly satisfactory.

Conclusion of the study of induction by invalidation.—Let us summarize the preceding analysis.

We first stated the postulate that the whole influence of facts on the probability of laws resolves itself into these two primitive operations: confirmation and invalidation by means of favourable or opposing instances. We have analyzed the theoretical advantage there would be in conceiving infirmative invalidation as the only source of all inductive inference. We have noticed that philosophers and reason itself had such a propensity. We have tried to grasp the principle of this doctrine and pursue it rigorously, in order to decide whether it is tenable to the end.

In the first place we had to determine the essential and necessary form of induction by invalidation. It is the transference to one of the laws of a given group, by the rejection of all or part of the others, of the certainty or probability of the existence of at least one true law in the group. Such groups of possible laws containing certainly or probably a true law are furnished to induction by special deterministic assumptions. We must postulate that it is certain or probable to a degree **p** that in any one of the instances of the character A whose rule of generation we are trying to establish, there is, in the heart of a certain class α at least one character which entails A. The class α may comprise all the characters of the case or only certain ones selected from them. Starting then from some instance of the character A and from the class α which is in relations with it, induction seeks to make infirmative or to negate by means of contrary facts the connection of A with the greatest possible number of characters of α, thus transferring their initial chances of entailing A to those that remain. Such is *induction by elimination*. It is the only kind of induction possible on the basis of our assumptions.

We have examined the function of this type of induction in the ideal condition where individual cases, completely known to us, do not conceal from us any of their causal circumstances. But even this theory breaks down with the possibility of the complexity of causes. It cannot produce an inductive inference that is certain except by the indirect aid of some assumption directed against the plurality of causes. If we proceed to exclude the possibility of plural causes, certainty becomes accessible again. On the other hand, if we limit ourselves to the postulate that this plurality becomes more improbable as it becomes greater and greater, elimination again furnishes an infinitely probable inference, but on the condition of depending on an infinity of different favourable instances.

We next passed to nature. There no instance is known in all its circumstances for the reason mainly that a causal circumstance is not merely what one actually perceives, but also what one would perceive as a consequence of some experimental test, and we cannot make all the possibly relevant experiments in any one case, or rather we cannot be sure that we have made them. In the situation of primary induction, where we must give up expecting any assistance from empirical knowledge so long as we are aiming at universal connections, induction by elimination can obtain nothing sufficient from phenomena known incompletely to a degree also unknown.

It seems then that the doctrine which seeks in the infirmative rejection of laws by contrary instances the only source of induction should at this point be abandoned. The probability that approximates certainty, and which appears accessible to the natural sciences, must then be demanded principally from the confirmative action of favourable instances, that is to say, from induction by the multiplication of instances.

But in confirmative induction, itself, the doctrine we are studying is met again. That is, here again everything reduces to elimination; but it is an elimination which is only probable, and which would operate in a manner unknown to us on the unknown portion of the instances. It is only the probability of this hidden elimination which would produce the apparently direct confirmative force of collections of favourable instances. It is only by knowledge of the probability of their diversity that two cases indiscernible to us would count as more than one. Reason favours this theory which Mr. Keynes has so well expressed.

First of all, we have shown that it requires the aid of some principle directed against the plurality of causes. We have postulated that any character admits a unique cause, in order to remove this preliminary difficulty and examine in its very principle the theory of the probability of elimination.

Following this theory, all induction by repetition admits as its ideal and limit a certain induction by elimination, below which it always remains. Consequently, wherever the determination of a character by some other is *a priori* probable only to the degree **p**, the accumulation of instances cannot give to the connection of two characters anything but a probability lower than **p**; for this value is the maximum result that elimination itself can give. This we thought to be the case in the domain of numbers. On the other hand, wherever determinism distinguishes several partial classes in the heart of the total class of the characters of an instance, by serially ordering the circumstances of any instance into several sections $a_1, a_2, a_3, \ldots$ and postulating as *certain* that every character of one of these sections is entailed by some character of each preceding section; and also by assuming that it is *probable only to a finite degree* that it is entailed by some other character of its own section (and consequently that it entails some other character of its own section)—this is what Lachelier's second principle amounts to—we have tried to prove that the accumulation of instances, as far as they are pushed, cannot give to the proposition that one character entails another any but a finite probability. Now, such is the case with the phenomena of nature.

The conclusion we are led to is that neither in the domain of numbers, nor in the realm of nature, does the probability of elimination, postulated as the only source of induction by the repetition of instances, confer on this induction a force sufficient to approximate certainty. But the repetition of instances contained the only hope that remained to raise to practical certainty the mediocre probability furnished by deliberate elimination, employed on natural phenomena without any acquired knowledge to direct or certify its operation. Thus the theory which sees in invalidation either the overt or hidden principle of all induction found itself incapable of conferring on any law of nature an infinite probability. It does not allow physics to exceed, no matter how carefully and perseveringly it operates, a mediocre probability which is determined *a priori*.

We cannot absolutely reject this result, in our present great ignorance of the nature of induction, and consequently of its power and limits. On the other hand, we can only admit it if there exists no plausible theory allowing us to avoid it. Now, we have examined no more than one theory, which despite its advantages did not show itself reliable. Of the two elementary operations that the mind thinks it discerns in the relationship of facts to laws (invalidation and confirmation) only one of the two is admissible. For the mind tries to reduce the confirmation of a law by the proper instances to the invalidation of other concurrent laws. This doctrine, embraced by reason at first, willy-nilly, leads to nothing but a mediocre result, unfavourable to the natural sciences.

It is time to free ourselves from its prestige by observing that if we had examined it before following it, we would have recognized it as an extreme, and nearly a gamble.

There is then left the other road to be tried. It is possible and natural to conjecture that the corroboration of a law by its instances, viz. induction by repetition, possesses a force which comes elsewhere than from the probability of elimination. This is what we must do if we wish, to speak with Plato, to try to "save" (*Timeus*) our knowledge of nature and to open to it the approach, at least, to certainty.

But this idea brings us to a road where everything is still unknown. It means, in fact, to give up the doctrine which more or less distinctly, has dominated the thought of logicians. It means that we must turn away altogether from the direction in which the mind pursues first the explanation of the confirmative force of instances, and ends finally in the analysis of Mr. Keynes. We must seek elsewhere for an explanation and an analysis; for we cannot claim that this force is explained all alone. We remarked in the beginning: the invalidation of a law by a contrary instance is intelligible by itself. The mind does not ask any question about its foundation or measure.

It sees in invalidation a simple and decisive operation. On the other side, the corroboration of a law by a favourable instance does not have this clarity. Its value remains obscure. The principle according to which the probability produced by a number of instances increasing to infinity would indefinitely approximate certainty lacks evidence. It would require some proof. Now, it does not seem that we can obtain from classic works the slightest aid in the discovery of such a proof. For they all end with the theory of the probability of elimination; and we have seen that this condemns the possibility of certainty.

But a recent work offers us exactly what we are seeking: a justification of induction by repetition and a proof that it tends towards certainty when the number of instances increases to infinity. This work is already known: it is this same *Treatise on Probability* of Mr. Keynes, in which he supports the theory which we have presented and shown to be fatal to this very principle.

We shall present the elegant theorems of Mr. Keynes, detach them from a traditional philosophy definitely refuted by them, and finally examine the proofs given by their author. We shall see why one of them appears to us to be incorrect.

INDUCTION BY CONFIRMATION

MR. KEYNES' theory rests on the fundamental axiom concerning the probability of the conjunction of two propositions. The simplicity of this foundation is remarkable.

Probability of the conjunction of two propositions.—What is the probability that two propositions p and q are both true? The answer often given is that it is the *product* of their two probabilities. Now, it is not so in general, but only in the particular case where these two probabilities are independent of each other, that is to say, where the information that one of the propositions is true would not increase or diminish the probability of the other. On any other hypothesis it is clear that the probability of p and q together is no longer equal to the product of their separate probabilities. In fact, if p has q for its certain consequence, the probability of pq is that of p alone. And if p has q for its probable consequence, the joint probability of pq is even greater than the product. Inversely, if p has *non-q* for a certain consequence, the probability of pq is null; and if p has *non-q* for a probable consequence, the probability of pq is still less than the product.

The probability of pq is therefore not a function of the initial probabilities of p and of q, but a function of the initial probability of p and of the probability of q *if* p. Or else, for reasons of symmetry—for even if p and q refer to events, q may be known if we are given p—it is a function of the initial probability of q and of the probability of p, given q. Again by symmetry this is the same function. This function is, as before, the *product* of these two probabilities.

Designate by x/y the probability of x being concluded from

y. Let h be the initially given premises or information known. We shall then postulate

$$pq/h = p/h \times q/hp = q/h \times p/hq.$$

Such is the principle; it is at least infinitely plausible. We shall notice that it is universal and independent of any assumption or hypothesis whatsoever.

Justification of induction by repetition.—Let p be a general proposition or law, h the initial probability at the moment when it is considered to be indifferent whether no instance or on the contrary any number of instances of the law is known. Let q be the proposition that the law is going to be verified in the new instance E.

If the law p is true, q is certainly true also. We then have

$$q/hp = 1.$$

The principle

$$p/h \times q/hp = q/h \times p/hq$$

then yields the equation

$$\frac{p/h}{p/hq} = \frac{q/h}{1}$$

That is to say, the probability of the law before a verification is to the probability of the law after a verification $p/h : p/hq$ as the probability of this verification itself q/h is to certainty.

In order for the verification q to render the law more probable, we see then that it is necessary and sufficient

(*a*) *that p/h is not null*, that is to say, *that the law possesses independently of this verification some probability, no matter how weak it is;*

(*b*) *that q/h is less than unity*, that is to say, *that the verification q does not follow with certainty from what is already known.*

This theorem justifies induction by repetition.* It estab-

* Again we must not exaggerate the import of this purely theoretical proposition. For it says that the accumulation of the cases verifying the law renders more probable its verification in *all cases* (and conse-

lishes, besides, the dispensability of determinism as a premise. Its strength does not come from a probability of elimination, and even the probable variety of instances is not necessary. The result then is to overthrow the philosophy which we criticized before, and to which Mr. Keynes himself still remains attached. Let us stop a moment to study these important consequences.

Induction by repetition does not have determinism for a premise.—We have, in fact, just proved that any verification which was not certain in advance renders the law more probable only on the condition that the law already possesses some chance, however slight, of being true. Let X and A be the characters joined by the law. For the discovery of A in a given case of X, where its presence could not have been predicted with certainty from what was already known, i.e. to render *X entails A* more probable, the only assumption that must be made is, then, that *X entails A* has already some probability **p** which is not null but as small as one wishes. This assumption implies undoubtedly that the presence in any case of XA of some character entailing A has a probability of at least **p**. For it is probable to this degree that X itself is such a character. But that is *all* that it implies. It does not imply that the presence of such a character is *certain*.

Such induction by repetition requires only, in order to increase the probability of the law *X entails A*, that X should be determined with some degree of probability.

But in order to show that this kind of induction does not have determinism as a premise, we must show again that the law *X entails A* may attain by repetition a probability *higher* than the initial probability of the determinism of A.

quently, in *any case*); but not yet *in all the cases* STILL UNKNOWN, or *in any one of these cases*, a point which is necessary to justify real inductions. In fact, the proof given supposes that the cases recognized as favourable remain part of the sum of the cases. It is no longer valid if we consider only the sum of the cases still unknown.

That is easy. In fact, the initial probability **d** of the presence in any case of XA of some character entailing A should be equal to or higher than the initial probability p/h of the law *X entails A*. We have the right to postulate it as simply equal. It is sufficient to place one's self in the hypothesis where we would be sure that *X alone can entail A*. This particular hypothesis does not prevent the application of the theorem: the probability p/hq of the law *X entails A* after the new verification q is therefore higher than its probability p/h before this verification; and higher in respect of certainty than the probability q/h of the verification q in the prior state of information h. But it is certain that X alone can entail A: the initial probability **d** of the determinism of A is then precisely equal to the initial probability p/h of the law *X entails A*. The verification q then confers on this law a probability higher than the initial probability of the determinism of A. That proves, as we had proposed to show, that determinism is not a premise of induction by repetition.

The force of induction by repetition does not arise from a probability of elimination.—This results immediately from the preceding proposition. For all that perfect elimination can establish is that X alone *may* entail A. The law *X entails A* would then be found to be heir to all of the initial probability of A's determinism, but *only* of this probability, that being the maximum result attainable by elimination. Now we have supposed it attained; and we have shown that the first new piece of information enabled the probability to increase. That can no longer arise because the new instance has a chance to eliminate some concurrent character of X on the ground of its being a sufficient condition of A, since this elimination has already been completed. The logical mechanism of confirmation by instances does not therefore reduce itself to a probability of elimination.

Let us take, in particular, the case where the existence of some other character inseparably joined to A would be probable to the degree p, and where we would have two instances of XA not having any other character in common. These two instances would embody what Mr. Keynes calls a perfect analogy. Letting himself be guided by the doctrine of elimination, he adds that no new instance could any longer add anything to the probability of the connection of X and A.* But his own theorem demonstrates just the contrary; for it shows that the verification of this connection in a third instance, provided only that we could not have predicted it with certainty, would make the law more probable than it was. We can never be sure, it is true, that two instances of XA differ in all other respects, and we are surely inclined to think that if we looked carefully, we should find other similarities in them. But the fact remains that the probable elimination of these resemblances is not the sole origin of the favourable operation of additional instances, since, even if we suppose this elimination completed, new instances may yet continue to fortify the law.

A new instance identical with an acquired or known instance may render the law more probable.—Let us conceive a universe where two instances might be numerically two without differing in any of their characters. This supposition is unreal, and even absurd. However, it may serve to illustrate a thesis. Mr. Keynes himself employs it to this end when he asserts: 'If the new instances were identical with one of the former instances, a knowledge of the latter would enable us to predict it." (Ibid., p. 236.) Consequently, the second would tell us nothing, and hence, as a result of the theorem, would not increase the probability of the law. It will then be permissible for us also to have recourse to the fiction of two identical

* *A Treatise on Probability*, p. 226.

instances although we do it in order to deny precisely what Mr. Keynes affirms from these examples.

Let us, first of all, determine exactly what makes two identical instances mutually inferrible. Given that the second instance reproduces, with X, all the characters of the first instance other than A, we should be certain that it also reproduces A, even before we have ascertained it.

But it is evident that this certainty is nothing but *the very certainty itself of the determinism of A*. For what we should be certain of, is that the total character, formed by the union of all the characters which accompany A in one of its instances, entails A and cannot be reproduced without A.

Undoubtedly, such is the case in our universe and it is not merely an assumption, simply and mainly because this total character cannot in fact be reproduced. On the other hand, in the fictive universe in which both Mr. Keynes and I discourse, he to assert that two identical instances would be inferable one from the other, and I to doubt the assertion, the objection that might be raised about the identity of indiscernibles no longer is relevant, and our assumption becomes quite real.

Now, this assumption does not operate effectively on the hypothesis of the theorem concerning us at present. This hypothesis, it will be remembered, is only that *X entails A* must possess some initial probability, however slight it may be. We then remain free to suppose the case where the determinism of the character A would not be certain. It would then not be certain that an instance of X presenting all the characters other than the A of an instance of XA already known, must also present A. By virtue of this theorem, the establishment of the presence of A in this second instance would then increase the probability of *X entails A*, since this would be a new verification which could not have been predicted with certainty.

That is not all. Let us even postulate as certain that A is strictly determined. The discovery of an instance of XA identical with an instance already acquired might again increase the probability of *X entails A*, and that by virtue of Mr. Keynes' same theorem which at first seems to imply the contrary.

Symbolize by L, M, N . . . the characters *other than X and A* of these two instances. The first instance being known, it is certain that XLMN . . . entails A; the establishment of *A with XLMN* . . . in the second instance does not add anything to our knowledge. But the establishment of *LMN with X* in this same case does teach us something; namely, it makes the law *X entails LMN* . . . more probable by being an instance of it. In fact the verification of this law in the second instance does not depend with certainty on its verification in the first. Otherwise it would be certain that *all* the instances of XA were identical, and only one of these instances would be sufficient to render *X entails A* certain, a hypothesis which would make it futile to ever investigate any new instances.

Outside of this hypothesis, too unnatural surely for any one to be long detained by it, an instance of AXLMN . . . identical with a preceding case increases then the probability of the law X *entails LMN* . . . when the law is recognized as possible. Now, we have postulated as certain that any X, if it is LMN . . ., entails A. The second instance of AXLMN . . . makes it more probable that any X is LMN . . . so long as the contrary is not rendered certain by the discovery of an X which is not LMN. . . . A second instance of AX identical with the first, would therefore make the law X *entails* A more probable *even if the determinism of A were strictly certain.*

Mr. Keynes' theorem has, therefore, a consequence precisely opposite to what he himself thinks he draws, deceived by the

traditional conception of the mechanism of induction. Far from implying that two instances known to be identical can count for no more than a single one, his theorem implies just the contrary.

Most certainly, there are no identical cases and perhaps there cannot be any. Mr. Keynes' assertion was made only to illustrate the doctrine that in a number of cases, it is *only* their variety, certain or probable, which operates. Likewise, we have just illustrated by means of the same fiction the contrary thesis that in a number of instances, it is *not only* their variety, certain or probable, which operates. And we have shown this to be a direct consequence of Mr. Keynes' own theorem.

State of the question.—In the first part of this chapter, we convinced ourselves that the corroborative influence of collections of instances of a law did not have to draw all its force from a probability of elimination in order to approach certainty with regard to our inductive knowledge of the laws of nature, including as a subsidiary the laws of number. The preceding theorem establishes the fact that this condition of being independent of elimination is actually satisfied. It makes certain that induction by simple enumeration is not subjected to the conditions of induction by elimination, and that it is capable in principle of elevating the maximum result of the latter. The question of approaching certainty through the accumulation of instances is now reopened, under the very conditions in which the doctrine of possibility of elimination made this approach impossible. But it is hardly solved satisfactorily. It is not yet proved that the multiplication of the instances of a law confers a probability susceptible of attaining and exceeding any fixed value.

Mr. Keynes thinks he has also proved this, but he does it with the aid of a special postulate.

Two necessary and sufficient conditions for the probability of a law to approach certainty by the multiplication of its instances to infinity.—*It is at first necessary that the law possess, from the very start, a probability that is not null no matter how small it may be.* This condition is recognized as the one we already know necessary for any increase in probability through instances. But in order for this probability to be carried beyond any limit by an infinite number of instances, *it is necessary, besides, that on the hypothesis that the law is false, its successive verification in an infinite number of cases is infinitely improbable; or in more precise terms, that its improbability exceeds any limit for a sufficiently large number of cases.*

In fact, suppose that the law is verified in all the instances known and that these instances are infinite in number. Either the law is true or it is really false. Its truth would render certain the fact of its verification in all these instances. If we admit, in conformity with the above conditions, that the falsity of the law rendered this same fact infinitely improbable and that, besides, this falsity is not infinitely improbable by itself, it follows that this fact, once established, renders the initial law infinitely improbable. And this condition which is sufficient is also necessary.

All that results results directly from the axioms of probability. Let $\frac{p}{h}$ and $\frac{\bar{p}}{h}$ be the respective probabilities of the truth and of the falsity of the law p in the state h of knowledge from which we start. Let V be the fact that the law is found to be verified in an infinite number of cases not included in the given information h. Then V/hp and $V/h\bar{p}$ are the respective probabilities that the law will be certainly verified an infinite number of times relative to our present knowledge (h), on the two hypotheses respectively of the truth and falsity of the law p. But V/hp is given as certain, and is hence equal to unity. We are trying to find the probability p/hV or the degree of probability conferred on the law p relative to our knowledge h and the establishment of the fact V.

We have

$$(1)\quad V/h = p/h \times V/hp + \bar{p}/h \times V/h\bar{p}$$

In fact, this means that the probability of V in our present state of knowledge h is divided into the respective probabilities of V on the two mutually exclusive and exhaustive alternatives p and $\bar{p}$, multiplied by the probabilities of these alternatives themselves. This is a fundamental proposition of the logic of probabilities.

Now, the principle postulated at the beginning of this whole development yields

$$p/h \times V/hp = pV/h = V/h \times p/hV$$

By substitution (1) becomes

$$V/h = V/h \times p/hV + \bar{p}/h \times V/h\bar{p}$$

And by transposition, this becomes

$$p/hV = 1 - \frac{\bar{p}/h \times V/h\bar{p}}{V/h}$$

or

$$= 1 - \frac{\bar{p}/h \times V/h\bar{p}}{p/h + \bar{p}/h \times V/h\bar{p}}$$

The upshot of this is that for the probability of p/hV to increase towards unity or certainty when the number of verifications constituting V increases to infinity, we see then that it is necessary and sufficient that p/h is not null and that $V/h\bar{p}$ tends to become null.

Replacing the second condition by a condition that is only sufficient.—Mr. Keynes substitutes for the condition that the probability of verifications of the falsity of p in the above discussion is not null i.e.

$$V/h\bar{p} > 0$$

a more onerous condition which implies it, but is not implied by it, and which he thinks can be satisfied with certainty. Let us show wherein the substituted condition is onerous.

The rule of the composition of two probabilities, applied repeatedly with more and more verifications, gives for the joint probability V/hp of n verifications $x_1, x_2, x_3, \ldots x_n$ relative to the state of knowledge h and to the hypothesis of the falsity of the law p the value

$$V/h\bar{p} = x_1 x_2 \ldots x_n/h\bar{p} = x_1/h\bar{p} \times x_2/h\bar{p}x_1 \times \ldots \times x_n/h\bar{p}x_1x_2 \ldots x_{n-1}$$

that is to say: the probability of n successive verifications is equal to the product of their probabilities being given the probabilities of the preceding verifications.

The factors of this product are all less than unity. For the product to approach zero as their number increases, it suffices evidently for the factors not to approach unity but to remain less than a fraction f, itself less than unity. That is, that there exists a finite quantity ε such that we have, no matter what n is,

$$x_n/h\bar{p}\, x_1 x_2 \ldots x_{n-1} < 1 - \varepsilon$$

Such is the condition Mr. Keynes tries to satisfy.

It is to be noticed that this condition is sufficient but no longer necessary. For a product of an increasing number of fractions may tend towards zero, whereas its factors tend towards unity; for instance the product

$$\frac{1}{2} \cdot \frac{2}{3} \cdot \frac{3}{4} \cdot \cdot \frac{n}{n+1}$$

whose last term $\frac{n}{n+1}$ tends towards 1, and whose value $\frac{1}{n+1}$ (by cancellation) tends no less towards zero (as n approaches infinity).

We can then establish the fact, not only that on the hypothesis ($\bar{p}$) of the falsity of the law, is its verification in an infinite

number of cases infinitely improbable, but in addition, that on this hypothesis ($\bar{p}$) not even an infinite number of positive verifications *will make it infinitely probable that the next instance will again verify the law p.*

We have just seen that the condition is no longer a necessary one. From the very first, it is doubtful whether our universe satisfies such a condition. It amounts to saying that if we *know* that a rule admits some exception, the observation of as many millions or billions of successive verifications as you please cannot reduce the chance below a fixed limit that the next instance has of just being an exception. It asserts that if we had once seen a man ten feet tall, the observation of as large a number of men as you please less than ten feet in height could not then render as probable as you please that men more than ten feet tall are as rare as you please; or else again, that if we had demonstrated that two properties of numbers are not always found together, the observation of their being in connection in as large as multitude of numbers as one wants to try, could not then make it as probable as one wants that they will be found together in the next number that will be tried. Such is the condition Mr. Keynes asks in order that the accumulation of instances in the absence of exceptions or contrary proof, may make it as probable as one wants that any man is less than ten feet tall or that two arithmetic properties are to be found together. We shall agree that it ought to be quite difficult to satisfy such a condition. However, he thinks he can fulfil the condition, as well as the fundamental condition of the existence of an initial probability that is not null in favour of the law p, by the aid of a very plausible postulate which he calls the postulate of the limitation of independent variety.

The postulate of the limitation of independent variety.—This postulate consists in assuming that the characters of the universe selected for consideration arrange themselves in a *finite* number of groups, a certain member of which entails the others. Mr. Keynes' work shows a very interesting development of the character and range of this postulate.

It satisfies the first condition.—The upshot of this postulate is that any character X taken at random possesses *a priori* a finite chance of entailing the character A, also taken at random. In fact, the character A possesses a finite chance of being a part of one or of several groups taken at random, since the number of these groups is finite. Hence, it possesses a finite chance of being a member of the group or groups of which X is a part, that is to say, A has a finite chance (not null) of being present in all the cases of X. The first condition would then be actually satisfied.*

But does it also satisify the second condition? Mr. Keynes' reasoning.—It is the second condition which produces a difficulty. Let us analyze Mr. Keynes' reasoning since he exhibits it in his Treatise in a more condensed form (p. 254).

The number of the individuals or instances in the domain of natural phenomena or numbers may be conceived as infinite—and it even ought to be so—since we are considering what the probability of a law becomes at the limit when the series of its instances is indefinitely prolonged. *The number of the characters* of these instances, and even of any one of them, may also be infinite. What is finite, however, is only the number of the *groups of characters* entailed by a certain member of the group, or in other terms, *the number of the characters sufficient to determine all the others.*

Consequently, *the number of non-identical or distinct cases*

* To speak in all rigour, it would be necessary to assume not only that the number of groups of connected characters is some finite number x, but also that there is a finite probability that x is less than a given number—than a billion, for instance. For if all the finite numbers have the same chances of being x, it is infinitely more probable that *x is higher or lower than any assigned number*, and hence not finite.

is finite. For it is in fact limited by the number of the combinations of these determinant characters.

It is on this basis that Mr. Keynes works.

If the law p is false, he says, it is false in at least one case. But the number of distinct cases, say N, is finite. On the other hand, it is natural to admit by virtue of the principle of indifference, that it is not more probable at any moment whatsoever for the new instance which is going to appear to be one rather than another of the existing cases. Hence, the case or cases invalidating the law p have, no matter at what moment, a chance of appearing equal to $\varepsilon = \frac{1}{N}$, and we have for all the values of n,

$$x_n/h\bar{p}x_1x_2 \ldots x_{n-1} < 1 - \varepsilon$$

This reasoning rests on an unacceptable hypothesis.—The nerve of the argument is evidently the finitude of the number of cases. From this finitude should in fact follow the existence of a finite lower limit of the probability that the next case, supposing it to be taken at random, is one of the exceptions to the law. Now, what is given as finite, is *not the number of the individual cases* but only *the number of the non-identical or distinct cases*, which we may call *the number of the species*, including *infimæ species.* Mr. Keynes' reasoning then takes for granted that the cases which are not distinct, no matter how large their number is, constitute no more than a single instance.

Now, this is such a strange assumption that we should hesitate to attribute it to him, if we could doubt that his reasoning requires it.

In fact, it comes down to this; the proportion of the individuals encountered in different species cannot give any indication of the frequency or rarity of these species in the group of the individuals of a genus. If we have met with only a single exception among as large a number as we please of individuals of species belonging to a genus, we cannot say *a priori* that it was more or highly probable that these species are frequent in this genus, nor that any new individual of this genus, taken at random, will be found to belong to these species. Experience should then not be able to modify our initial ignorance about the relative importance of the existing species, about the chances that there are that an unknown sample of a genus belongs to one rather than to the other of its species. Observation should not be able to teach us anything *de multis et paucis* (about what is frequent and what is rare). Such an assumption is in truth unacceptable: and yet it is indispensable to Mr. Keynes' argument.

In fact, if we do not make this assumption his reasoning falls asunder. For we have assumed that the law p, "*X entails A*," is false. That is because there is at least one combination of characters, one species, where X is found without A; and the number of species is finite. It is then quite true that the probability, by drawing a *species* of the genus X under conditions where all are equally probable, that we shall find a species without A, is at any moment whatsoever higher than a finite value ε. *But we do not by any manner or means actually draw a species, but always an individual.* And for the reasoning to remain applicable, it would be necessary that it should be equally probable at any moment, *not that any one of the individuals of the genus* X *should be drawn*, but really *an individual which is a member of any one whatsoever of the species of the genus* X. Now, it seems to make good sense to say, if we have always encountered among the individuals of the genus X members of species containing A, and if that has happened during as long a series of events as one pleases, that it is thereby very probable (if not as probable as one pleases), that in the heart of the genus X, species lacking A are rarer than species containing A (if not as rare as one pleases). Consequently, the individual of the genus X which is to appear, will be also

of a species containing A, *just because* our ignorance allows all individuals still unknown and belonging to the genus A the same chances of appearing.

The demonstration which Mr. Keynes based on the postulated finitude of the number of species rests then on the assumption, evidently contrary to the facts, that experience changes nothing of the initial ignorance which makes us regard an unknown individual of a genus as not having more chances of belonging to certain species of this genus than to others. That Mr. Keynes has let himself be misled by so ill grounded a construction, seems to follow from the effect of his general doctrine about the necessary diversity of fruitful cases. Although his assumption, while false in general, does apply to the probability of laws concerning the existence or non-existence of certain species, it is fully absurd to apply it to the frequency or rarity of existing species. We saw, when we studied his first theorem, that Mr. Keynes remained attached to a philosophy of induction incompatible with his positive theory. But with the second theorem, this philosophy has unfortunately introduced itself into his very argument and vitiated it.

Present state of the problem.—It seems to us that we have shown that if elimination is the only source of induction, as logicians and good sense itself incline to believe, no induction in favour of a law can exceed a mediocre probability. We also think we have shown that elimination is not the sole source of such inductions, and that the instances of a law have a corroborative force which is independent of elimination and of determinism. Finally, we have tried to show that nobody has been able to prove that these instances, by being multiplied to infinity, can raise the probability of the law above any limit. Such appears to us to be the present state of the logical problem of induction.