Le Problème Logique de l'Induction. By JEAN NICOD. With a Preface by A. LALANDE. Paris: Félix Alcan: 1924. Pp. vi + 83. Price 10 francs.

THE death of Jean Nicod last year at the age of thirty removes one of the ablest of French logicians. After graduating at the Sorbonne, Nicod studied at Trinity College, Cambridge, under Mr. Bertrand Russell, and there published the paper by which he is best known —"A reduction in the number of the primitive propositions of logic" (*Proc. Camb. Phil. Soc.*, Vol. XIX.). In this, by using Dr. Sheffer's one indefinable relation of incompatibility, he shows that the five formal primitive propositions of *Principia Mathematica* may be replaced by one highly complicated proposition, thus completing what is called in the new edition of *Principia* "the most definite improvement resulting from work in mathematical logic during the past fourteen years." "Dans ce domaine [de logique et logistique], auquel se consacrent dans notre pays si peu de mathématiciens ou de philosophes," writes M. Lalande in an admirable biographical preface to this book, "il apportait les ressources d'un savoir et d'une ingéniosité qui promettaient un continuateur éminent à l'œuvre si malheureusement interrompue, en France, par la mort tragique de Louis Couturat—et que la sienne laisse de nouveau en suspens."

This slim volume is one of two theses submitted shortly before his death for a doctorate at Paris. [The second and larger one, *La Géométrie dans le Monde Sensible,* published uniformly with this, is quite as important and will doubtless receive detailed examination in MIND.] Here Nicod undertakes a critical examination of Mr. J. M. Keynes's treatment of induction in his *Treatise on Probability.* "Puisque nous devons plusieurs fois critiquer M. Keynes, disons ici qu'à notre sens, nul auteur depuis Mill n'a autant avancé la théorie logique de l'induction" (p. 11). This is perhaps a little hard on Jevons; but Mr. Keynes is right in saying that "amongst contemporary logicians there is an almost complete lack of constructive theory, and they content themselves for the most part with the easy task of criticising Mill, or with the more difficult one of following him" (*Treatise*, p. 265). Mr. Keynes, like Mill (to whom he bears many resemblances), *has* produced a constructive theory; and it is right and proper that it should be taken as the text of such a treatment as this (though a cynic might say that Nicod has chosen the easy task of criticising Mr. Keynes rather than the more difficult one of following him).

The main thesis of Nicod's book is that all induction rests ultimately upon *inductio per simplicem enumerationem*, and that to abandon this in favour of a "scientific induction" based upon analysis of the circumstances—"c'est lâcher la substance pour l'ombre". For the requisite conditions for "scientific induction" —what Mr. Keynes calls "argument by analogy"—are never fulfilled, and it is no good simply saying that this makes such inductions probable but not certain. "Mais en vérité, si les inductions réelles ne remplissent pas les conditions qui les rendraient certaines et qu'on vient de se donner dans la théorie, il s'ensuit qu'elles ne sont pas certaines, mais nullement que malgré cela elles demeurent assez probables, ou très probables, ou extrêmement probables : la certitude étant manquée, la probabilité reste toute entière à établir, et la théorie toute entière à refaire" (p. 10). This *caveat* should be commended to the notice of all writers on the philosophy of science.

Before attempting the task of setting up in order to knock down a "scientific induction," Nicod, as is proper, makes a few remarks upon the theory of probability. For him probability is the relation of the Johnson-Keynes theory. "La perception de ce principe que la probabilité est une relation, non une qualité, des propositions enlève à la probabilité ce qu'elle paraissait avoir de fuyant et de provisoire. Elle la rend un fait aussi ferme que l'implication, par exemple. Les propositions qu'un ensemble donné de propositions rend probables à un degré p sont aussi bien déterminées que les propositions que ce même ensemble rend certaines, encore qu'elles soient parfois aussi difficiles à découvrir" (p. 20). After this enthusiastic confession of faith it is a little surprising to find Nicod qualifying the doctrine in two ways. In a footnote (p. 19) he thinks that a proposition may have an intrinsic probability "aussi directe et immédiate que la certitude"; this might be called *plausibility* to distinguish it from the probability-relation, which alone is concerned in reasoning. And for a page Nicod discusses the difficult question as to whether an infinite probability (*e.g.*, that an unknown integer is not 1324) is equivalent to certainty and decides in the negative. But these are isolated points and have no effect upon his subsequent reasoning. On a third point, however, of the theory of probability Nicod seems to me to make a serious mistake which vitiates his arguments against "scientific induction". After describing the nature of probable inference from certain premisses he continues :—

"Mais toute inférence qui donne quelque chose à partir de prémisses supposées certaines donne encore quelque chose à partir de ces mêmes prémisses supposées seulement probables, et cela soit qu'il s'agisse d'une inférence en elle-même certaine, soit d'une inference en elle-même probable. On peut même poser qu'à partir de prémisses qui ont, prises ensemble, la probabilité p, une inférence certaine confère à sa conclusion cette même probabilité p, et une inférence probable qui conférerait à la sienne la probabilité q si ses prémisses étaient certaines lui confère la probabilité pq" (p. 13).

This is true only if an important qualification be made. It follows immediately from the multiplication theorem for probabilities that $x/h = \dfrac{y/h \,.\, x/yh}{y/xh}$. Putting $p = y/h = yh/h$ and $q = x/yh$, we see that x/h (the probability of x given h) is pq if and only if $y/xh = 1$, *i.e.*, if and only if y is known to be a necessary condition for x. So when Nicod goes on to say: "On peut donc dire que l'inférence certaine transfère à sa conclusion la totalité de la certitude ou probabilité de ses prémisses prises ensemble, et que l'inférence probable lui en transfère une partie. . . . Observons que la conclusion d'une inférence n'en retire qu'une certitude ou probabilité au plus égale à celle de la conjonction de ses prémisses, par suite de l'une quelconque, et en particulier de *la plus incertaine* d'entre elles. Cette vérité fort évidente nous sera très utile" (p. 13) he is just making a bad blunder. "Cette vérité fort évidente" is simply not true in all except those uninteresting cases where the premisses are all known to be necessary to the conclusion. In the interesting cases where the premiss y supports the conclusion x but is not necessary to it, *i.e.*, where $x/yh > x/h$ and $y/xh < 1$, it is simply false. The probability of the Einstein gravitation theory on general physical grounds supports the proposition that the photographs of the 1919 eclipse expeditions were not faked by German secret service agents, but the probability of the latter is much greater than the prior probability of its premiss, and may reasonably be used as a premiss in an argument to increase the probability of the Einstein theory. So Nicod is only developing his error when he differentiates between primary inductions not using inductive conclusions as premisses and secondary inductions which do so, and says of these latter: "La probabilité fournie par une induction quelconque ne peut donc surpasser la probabilité la plus haute que l'induction primaire est susceptible de livrer. C'est pourquoi l'induction primaire doit être analysée avant toute autre. Car elle n'est pas seulement le fondement logique de l'induction; elle marque encore la limite de toute assurance inductive" (p. 17). This is not the case: the problem for the inductive logician is not to find inductive hypotheses or primary inductions that are certain or even highly probable; the problem (which is hard enough) is to find principles capable of justifying induction that have *any a priori* probability. As Mr. Keynes shows very clearly in the exposition of his Inductive Hypotheses, "it is not circular to use the inductive method to strengthen the inductive hypothesis itself, relative to some more primitive and less far-reaching assumption" (*Treatise*, p. 260). So if we have some reason to believe a little in Nicod's primary inductions, the fact that secondary inductions based upon them are true will (under certain conditions) strengthen the primary induction. Nicod's fallacy in ignoring this vitiates a great deal of his criticism of Mr. Keynes.

Nicod discusses the main question of induction under two divisions —induction by infirmation and induction by confirmation. In the former we seek to establish a law by finding instances that contradict all alternative laws: this is the "scientific induction" to which logicians from Bacon to Mr. Keynes have been devoted, but which Nicod thinks is of little importance compared with the latter —Pure Induction. So the second and longest section of his book is devoted to a discussion of induction by infirmation.

"La forme essentielle et nécessaire de l'induction par infirmation" is, according to Nicod, "le transfert à l'une des lois d'un groupe donné, par le rejet de tout ou partie des autres, de tout ou partie de la certitude ou probabilité de l'existence d'au moins une loi vraie dans le groupe" (p. 60). And this requires first the probability of determinism: "il faut poser qu'il est certain ou probable à un degré p que dans l'un quelconque des exemples du caractère A dont on se propose d'établir une loi de production, il y a, au sein d'une certaine classe α qui peut comprendre tous les caractères de l'exemple ou seulement certains d'entre eux, au moins un caractère qui entraîne A" (p. 60). But it requires much more than this. For even in the case where the instances are completely known so that the elimination is complete, we are up against the possibility of a plurality of causes, which it requires further principles to exclude. Nicod shows, I think quite correctly, that a principle directed against the complexity of a cause will give only a low probability to an induction of this type, but that one directed against the plurality of causes[1] together with the multiplication of non-identical instances can give a probability tending to certainty. But, of course, in no cases in nature are the instances completely known, and we hope by multiplying the instances to diminish what is common to them but not included in the law we are interested in, *i.e.*, to increase what Mr. Keynes calls "negative analogy". This, according to Mr. Keynes, is the function of the multiplication of instances in induction.[2] Nicod examines this operation with the aid of a "développement de la théorie du déterminisme" (p. 46), and he thinks that he can show that the probability cannot be made very large by this method even by the multiplication of examples to infinity. But in his attempt to do so he makes one bad mistake. He is considering the case of n instances of XA and the hypotheses that these n instances have no or one or two or . . . common antecedents of A beside X. "Mais ces différentes hypothèses peuvent être inégalement probables. Dans chacune d'elles, la probabilité de la loi X *entraîne* A est celle que lui donnerait l'hypothèse si elle était réalisée, multipliée par la probabilité de cette réalisation. La probabilité globale qui se trouve conférée à la loi est donc quelque valeur moyenne entre tous ces produits, inférieure au plus grand d'entre eux" (p. 51). This is not the case: a little probability arithmetic shows that the "probabilité globale" is the *sum* of these products and is greater than any of them. And so Nicod is wrong

[1] He suggests the following: "Sachant que le caractère A n'admet pas de condition nécessaire formée de l'alternative de moins de m conditions suffisantes, si l'on désigne par N la probabilité pour que A admette une condition nécessaire formée de l'alternative de moins de n conditions suffisantes, la valeur de N tend vers l'unité lorsque n augmente à l'infini" (pp. 35-36.)

[2] "The whole process of strengthening the argument in favour of the generalisation $g(\phi,f)$ 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 generalisation disregards" (*Treatise*, p. 227, quoted by Nicod, pp. 45-46).

in concluding that "pour que cette probabilité tende vers la certitude lorsque n croît, il faut que l'un de ces produits en fasse autant" (p. 51); and consequently that if the possibility of elimination is the function of the multiplication of instances, the probability achieved is "bien éloignée de la certitude" (p. 54).

I am dealing with this section of Nicod's book rather summarily because the arguments (which, except for the definite mistake just mentioned, are good ones) seem all to depend upon the dogma that "la probabilité conférie par un raisonnement, quel qu'il soit, à sa conclusion est au plus égale à celle de la moins probable de ses prémisses" (p. 17), a dogma which I have already shown to be absurd.

The third section of the book deals with induction by confirmation, *i.e.*, Pure Induction. Here Nicod thinks that Mr. Keynes has made an important contribution in his theorems for the justification of pure induction, the more important in that his theorem that the verification in an instance x of a law g increases its probability, *i.e.*, $g/xh > g/h$, provided that $g/h > 0$ and $x/h < 1$, "achève donc de renverser la philosophie que nous avons précédemment critiquée, et à laquelle M. Keynes lui-même demeure encore attaché" (p. 67). For this theorem establishes of pure induction "qu'elle n'a pas le déterminisme pour prémisse, que sa force ne vient pas d'une probabilité d'élimination, et que la variété au moins probable des exemples ne lui est pas nécessaire" (p. 67). Let us examine these contentions.

With regard to the part played by determinism in this theorem there should be no real disagreement between Nicod and Mr. Keynes: "The common notion, that each successive verification of a doubtful principle strengthens it, is formally proved . . . without any appeal to conceptions of law or of causality" (*Treatise*, p. 236). Determinism, of course, comes in in the establishment of the *a priori* probability of any law by the Principle of Limited Variety (though Mr. Keynes does not make this as clear in his exposition as might be desired): but that is a different matter. Nicod, however, in accordance with his strange view, thinks that since the probability of the law is increased by verification, its prior probability cannot be a premiss in the argument. This is so obviously absurd that it is amazing that Nicod did not recognise it as a *reductio ad absurdum* of his dogma.

Nicod produces two proofs that the force of pure induction does not arise out of a probability of elimination and consequently that a new instance identical with a previous one can make a law more probable, both of which propositions are denied by Mr. Keynes. For "if the new instance were identical with one of the former instances, a knowledge of the latter would enable us to predict it" (*Treatise*, p. 236). But this, according to Nicod, would only be the case if it were certain that there was some law, whereas all that is required for the theorem is that it should be in some degree probable that there is some law, and that the law in question is the law. I think that if we limit our attention to the consideration of this one theorem of Mr. Keynes's, Nicod is right. But I believe that it is impossible to find any prior probability for the law in question unless we assume not only that a thorough-going determinism is probable, but that it is certain; and that this is necessary to any reasonable theory (such as Mr. Keynes's) aiming at the justification of the proposition that any generalisation has some *a priori* probability. So the necessity for determinism comes in at a logically earlier stage; and $x/h = 1$ in the theorem if h includes a previous instance identical with x.

Nicod's second argument does not convince me at all. Even assuming determinism (*i.e.*, that XLMN . . . implies A), a second instance of XALMN . . . increases the probability of the law "X implies A" because it increases the probability of "X implies LMN. . . ." "Autrement, il serait certain que *tous* les exemples de XA sont identiques, et un seul de ces exemples suffirait à rendre X *entraîne* A certain: hypothèse qui rendrait tout nouvel exemple inutile" (p. 70). But surely all that would follow would be that XAMN . . . implies L, that XALM . . . implies N, etc.; all of which are inoffensive propositions included in the deterministic assumption.

Having decided that Mr. Keynes's theorem shows that the increasing of the probability of a law by Pure Induction does not derive its force from the possibility of eliminating irrelevant factors, Nicod goes on to consider the satisfaction of the conditions that this probability may tend to certainty as the number of instances tends to infinity. Mr. Keynes's conditions are (1) that the law has some initial probability: $g/h > \epsilon$; (2) that on the hypothesis of the falsity of the law, the probability that the law should be confirmed in n instances

$$x_1 x_2 \ldots x_n / \bar{g}h$$

tends to 0 as n tends to infinity (*Treatise*, p. 236). And Mr. Keynes suggests that if all the properties in the universe are determined by a *finite* number of generator properties, these two conditions are satisfied.

(1) About this Nicod agrees with Mr. Keynes. "Il en résulte qu'un caractère X pris au hasard possède *a priori* une chance finie d'entraîner le caractère A pris également au hasard" (because there is a finite chance that they are both determined by the same set of generator properties) (p. 75). But he qualifies Mr. Keynes's statement of his Principle of Limited Variety in an important footnote: "A parler en toute rigueur, il faudrait . . . poser, non seulement que le nombre des groupes des caractères liés [*i.e.*, determined by the same generator property or conjunction of generator properties] est quelque nombre fini x, mais encore qu'il y a une probabilité finie pour que ce nombre x soit inférieur à un nombre donné—à un milliard, par exemple. Car si tous les nombres finis ont les mêmes chances d'être ce nombre x, il est infiniment plus probable que x est supérieur qu'inférieur à un nombre assigné

quelconque, en sorte que la conséquence qu'on vient de tirer de la finitude de x ne s'ensuivrait plus" (p. 75). This is an admirable exposition of a flaw in Mr. Keynes's Principle—a flaw which has been considerably discussed in Cambridge, but which I have not seen exposed in print before. Mr. Keynes has been guilty of a fallacy similar to that of the confusion of Convergence with Uniform Convergence in the theory of Infinite Series or of

$$(x):(\exists y).\phi(x, y) \text{ with } (\exists y):(x).\phi(x, y).$$

What is required to give an initial probability to any generalisation is not that the number of the groups is finite, but that it is less than some number given in advance. And when we remember that Mr. Keynes's Principle has to have some *a priori* probability, we see that this apparently slight change makes an enormous difference.

Indeed Nicod's apologetic "à parler en toute rigueur" somewhat diminishes the value of his footnote. It is one thing to believe that it is *a priori* slightly probable that the number of generator properties is finite (Nicod says this is "fort acceptable"): it is quite another thing to believe that it is *a priori* slightly probable that this number is less than a billion, for example. For this would seem to be one of the sort of things that cannot be supposed to be known *a priori*. Nicod's neglect to follow up his emendation is of a piece with the absence of any discussion of Mr. Keynes's Principle in itself.[1] I imagine that Nicod would have defended himself from this charge by saying that he was concerned with the "logical problem of induction" and not with its metaphysics. Lachelier is severely criticised in that in his *Du Fondement de l'Induction* "la grande affaire n'est pas de voir quels sont les principes de l'induction—cela [lui] paraît trop facile—mais bien de prouver ces principes. . . . Dans sa hâte de passer à ce travail de métaphysique, il n'aperçoit point que les principes dont il poursuit la preuve ne suffisent aucunement à justifier les inductions" (p. 55). But it is an easy and inadequate task merely to find a principle that will justify induction—Laplace's Rule of Succession will do so: the difficult and important task is to find a plausible principle, and discussion of the plausibility of such a principle cannot be ignored.

(2) However, Nicod's refusal to be drawn into a "travail de métaphysique" has enabled him (unlike most of Mr. Keynes's critics) to save some of his ammunition for the requirements of condition (2). Mr. Keynes does not use this in the form stated above, but converts it into a product of probabilities:—

$$x_1x_2 \ldots x_n/\bar{g}h = x_1/\bar{g}h \,.\, x_2/x_1\bar{g}h \ldots x_n/x_{n-1} \ldots x_1\bar{g}h\,,$$

and says that the left-hand side tends to zero as n tends to infinity if each of the terms of the infinite product[2] is less than $1 - \epsilon$, where

[1] Indeed he does not state it in terms of generator properties at all: this perhaps accounts for his neglect to notice that a thorough-going determinism is involved.

[2] Or, of course, each of the terms after a certain point.

ϵ is some given number, however small. Nicod, though he casually mentions that this condition is sufficient but not necessary for the convergence to zero of

$$x_1x_2 \ . \ . \ . \ x_n/\bar{g}h,$$

discusses it in Mr. Keynes's converted form, *i.e.*, that on the assumption of the falsity of the law, there is always a probability greater than ϵ that the next instance will disobey the law. Mr. Keynes seems to have thought that this followed so obviously from his Principle of Limited Variety as to require no serious exposition: his "raisonnement . . . sous une forme assez condensée" (p. 76) consists of only one line (*Treatise*, p. 254). Nicod gives the deduction as follows: (*a*) If the law is false, it must be false in at least one instance. (*b*) But the number of non-identical instances is some finite number N, and (*c*) there is no reason to suppose that any one of these will be the next instance rather than any other. (*d*) So the probability that the next instance will contradict the law is greater than $\frac{1}{N}$. It is proposition (*c*) in this argument that Nicod very rightly disputes. For it assumes that the number of numerically distinct instances arising out of each group of generator properties is equal, and that the fact that we continue not meeting we one of these instances does not diminish the probability that shall meet it in the next example, an assumption which I agree with Nicod in thinking "véritablement inacceptable" (p. 77). [Here, by the way, Nicod is venturing into metaphysics.] But I see a possible method of escape for Mr. Keynes in the fact, casually mentioned by Nicod, that the condition discussed is more than is required. The *necessary* and sufficient condition that an infinite product $(1 - a_1)(1 - a_2) \ . \ . \ . \ (1 - a_n) \ . \ . \ .$[1] should tend to zero is that the infinite series $a_1 + a_2 + \ . \ . \ . + a_n + \ . \ . \ .$ should diverge. And this can take place even if a_n tends to zero: the series $\sum\frac{1}{n}$ is divergent, for example. So the condition is satisfied if the probability that the next instance will not satisfy the law (on the hypothesis of the falsity of the law and its satisfaction in the previous $n - 1$ instances) does not tend to zero more rapidly than $\frac{1}{n}$, which is not unplausible. But it is no part of my task in criticising Nicod to embark on the more difficult one of improving on Mr. Keynes.

Nicod concludes his brochure with a statement of the "état actuel du problème". "Il nous semble avoir montré que si l'élimination est le seul ressort de l'induction, comme les auteurs et le bon sens lui-même inclinent à le croire, aucune induction en faveur d'une loi ne peut dépasser une probabilité médiocre. Il nous semble également avoir montré que l'élimination n'est pas le seul ressort de telles inductions, et que les exemples d'une loi ont une force corroborante qui n'en est pas tirée et qui ne suppose point le déterminisme. Il nous semble, enfin, avoir montré que l'on n'a pas

[1] All the a's being positive numbers less than 1.

su prouver encore que ces exemples, en se multipliant à l'infini, peuvent élever la probabilité de la loi au-dessus de toute limite. Tel nous paraît être l'état actuel du problème logique de l'induction" (pp. 78-79). I think that Nicod, misled by his dogma about the probability of a conclusion being always less than that of a premiss, has not proved the first of these contentions. As to the second, Nicod has pointed out many places where multiplication of instances in itself seems to be of importance: about some of these places he seems to me to be mistaken. With regard to the place of determinism and the question as to whether inductive probabilities can ever tend to certainty, I think that his discussion of the logical problems is incomplete without a metaphysical discussion which he denies himself. Nevertheless Nicod has boldly faced most of the important questions at issue: this little book, written without that verbosity which is so characteristic of treatments of induction, is certainly the most important work on the subject since Mr. Keynes's *Treatise on Probability.*

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The Growth of the Mind. BY PROF. K. KOFFKA. Trans. by R. M. OGDEN. Kegan Paul, Trench, Trubner & Co. 15s.

PROFESSOR KOFFKA'S book was written for school-teachers, and it is unfortunate that it should be so. There is, of necessity, a great deal of simple familiar material side by side with an unelaborate exposition of a new and interesting advance in psychological theory. Since, apart from a word or two in Köhler's "Mentality of Apes" and a few scattered articles, it is the only work embodying the principles of the 'Gestalt' school in this language, it is a pity that it does not go into greater detail in its exposition, though, as it is, its importance can hardly be exaggerated.

The principle feature of the Gestalt teaching is that mental life and the behaviour of organisms display organic unities. The static unity of equilibrium which is the resultant of several forces having certain relations to one another, and the dynamic unity of process which a swinging pendulum goes through in order to attain rest, are paralleled in perception on the one hand, and in instinct, reflex action and learning on the other.

An impasse had been reached. The ordinary mechanical account of pathway connexions between the periphery, the central nervous system, and the motor nerves, along which impulses ran like electric currents, had been found wanting when an attempt was made to give an explanation of instinct and learning, and many people threw up the scientific sponge and took refuge in psycho-vitalism. The Gestalt school provide an alternative to the mechanistic explanation, though it must be pointed out that the word 'mechanistic' is used here to denote a particular mechanism; the new school provides a different one.

In the case of reflex activity a complexity is involved on the old