is elliptical and analytic, expressing a logical relation between two factual statements. Consequently, there will be a difference concerning the future procedure in the following respect, if further observations exhibit a value of the relative frequency deviating considerably from q. The statement in sense (i) is rejected as probably false; the statement in sense (ii), however, remains valid but becomes irrelevant for practical purposes and is replaced by a new, likewise analytic, statement referring to the increased evidence.

§ 43. Inductive and Deductive Logic

A. Can a system of inductive logic as a theory of the degree of confirmation contain *exact rules?* This is sometimes denied for the reason that the procedure of induction is not rational but intuitive. Now it must be admitted that there is no effective procedure for finding a suitable hypothesis h for the explanation of a given observational report e, nor, if a hypothesis h is proposed, for determining $c(h,e)$. However, this is no reason against the possibility of an inductive logic because in deductive logic there is likewise no effective procedure for the solution of the corresponding problems. On the other hand, there are effective procedures for testing whether an alleged proof for a logical theorem is correct, e.g., in deductive logic for a theorem of the form 'e L-implies h', and in inductive logic for a theorem of the form '$c(h,e) = r$'.

B. Inductive logic is constructed from deductive logic by the adjunction of a definition of c. Hence inductive logic presupposes deductive logic. The analogy between these two fields of logic is illustrated by examples both for purely logical statements and for those involving the application to knowledge situations. However, truth and knowledge of the evidence e, although relevant for these applications, are irrelevant for the validity of the statements in inductive logic, as for those in deductive logic.

A. *On the Possibility of Exact Rules of Induction*

The question whether an inductive logic with exact rules is at all possible is still controversial. But in one point the present opinions of most philosophers and scientists seem to agree, namely, that the inductive procedure is not, so to speak, a mechanical procedure prescribed by fixed rules. If, for instance, a report of observational results is given, and we want to find a hypothesis which is well confirmed and furnishes a good explanation for the events observed, then there is no set of fixed rules which would lead us automatically to the best hypothesis or even a good one. It is a matter of ingenuity and luck for the scientist to hit upon a suitable hypothesis; and, if he finds one, he can never be certain whether there might not be another hypothesis which would fit the observed facts still better even before any new observations are made. This point, the impossibility of an automatic inductive procedure, has been especially

emphasized, among others, by Karl Popper ([Logik] §§ 1–3 and elsewhere), who also quotes a statement by Einstein: "There is no logical way leading to these . . . laws, but only the intuition based upon a sympathetic understanding of experience" (". . . die auf Einfühlung in die Erfahrung sich stützende Intuition") (*Mein Weltbild* [1934], p. 168); compare also Einstein, *On the Method of Theoretical Physics* (Oxford, 1933), pages 11–12. The same point has sometimes been formulated by saying that it is not possible to construct an inductive machine. The latter is presumably meant as a mechanical contrivance which, when fed an observational report, would furnish a suitable hypothesis, just as a computing machine when supplied with two factors furnishes their product. I am completely in agreement that an inductive machine of *this* kind is not possible. However, I think we must be careful not to draw too far-reaching negative consequences from this fact. I do not believe that this fact excludes the possibility of a system of inductive logic with exact rules or the possibility of an inductive machine with a different, more limited, aim. It seems to me that, in this respect, the situation in inductive logic is similar to that in deductive logic. This will become clear by a comparison of the tasks of these two parts of logic.

When considering the kinds of problems dealt with in any branch of logic, deductive or inductive, one distinction is of fundamental importance. For some problems there is an effective procedure of solution, but for others there can be no such procedure. A procedure is called *effective* if it is based on rules which determine uniquely each step of the procedure and if in every case of application the procedure leads to the solution in a finite number of steps. A *procedure of decision* ('Entscheidungsverfahren') for a class of sentences is an effective procedure either, in semantics, for determining for any sentence of that class whether it is true or not (the procedure is usually applied to L-determinate sentences and hence the question is whether the sentence is L-true or L-false), or, in syntax, for determining for any sentence of that class whether it is provable in a given calculus (cf. Hilbert and Bernays [Grundlagen], Vol. II, § 3). A concept is called *effective* or *definite* if there is a procedure of decision for any given case of its application (Carnap [Syntax] § 15; [Formalization] § 29). An effective arithmetical function is also called *computable* (A. M. Turing, *Proc. London Math. Soc.*, Vol. 42 [1937]).

Now let us compare the chief kinds of problems to be solved in deductive logic and in inductive logic. Our aim is to discover whether inductive procedures are less regulated by exact rules than deductive procedures, as some philosophers believe.

In order to simplify the comparison, let us regard deductive logic, including mathematics, as the theory of L-implication, the explicatum for logical entailment (§ 20), and inductive logic as the theory of degree of confirmation, the quantitative explicatum of probability$_1$. At this stage in our discussions we do not yet know whether it is possible to find an adequate quantitative explicatum for probability$_1$. Therefore the following explanations are meant at present merely in a hypothetical sense: *if* there is an adequate explicatum c and hence a quantitative inductive logic as its theory, what is its nature in comparison with deductive logic?

In each of the two branches of logic we may distinguish three kinds of fundamental problems concerning the application of the fundamental concepts, viz., L-implication or c, respectively.

I. First Problem: To Find a Conclusion

a. Deductive logic. Given: a sentence *e* as a premise (it may be a conjunction of a set of premises); wanted: a conclusion *h* L-implied by *e* and suitable for a certain purpose. For instance, a set of axioms for geometry is given; theorems concerning certain configurations are wanted. The essential point is the fact that there is no effective procedure for the solution of problems of this kind. The work of a logician or a mathematician consists to a great extent in attempts to solve problems of this kind. Some laymen imagine a mathematician to be chiefly occupied with computation, though of a sort more complicated than computation in elementary arithmetic. In fact, however, there is a difference in principle, not only in degree of complexity, between the two kinds of activities. To find the product of 15 and 17 is a simple task; to compute the square root of 7 to five decimals is more complicated; to compute the value of a number defined by a definite integral, e.g., e or π, to five decimals is still more complicated. All these tasks of computation, however, are fundamentally of the same nature, irrespective of the degree of complexity; for all of them there is an effective procedure; and this is characteristic of computation. The mathematician, on the other hand, cannot find fruitful and interesting new theorems, say, in geometry, in algebra, in the infinitesimal calculus, by computation or by any other effective procedure. He has to find them by an activity in which rational and intuitive factors are combined. This activity is not guided by fixed rules; it requires a creative ability, which is not required in computation.

b. Inductive logic. Given: a sentence *e* as evidence; wanted: a hypothesis *h* which is highly confirmed by the evidence *e* and suitable for a certain purpose. For instance, a report concerning observations of certain phenomena on the surface of the sun is given; a hypothesis concerning the physical state of the sun is wanted which, in combination with accepted physical laws, furnishes a satisfactory explanation for the observed facts. Or, a historical report about some acts of Napoleon is given; a hypothesis concerning his character, his knowledge at the time in question, and his conscious and unconscious motives is wanted which would make his acts understandable. There is no effective procedure for solving these problems; that is the point emphasized by Einstein and Popper, as mentioned above. However, we see now that this feature is by no means characteristic of inductive thinking; it holds in just the same way for the corresponding deductive problems.

II. Second Problem: To Examine a Result

a. Deductive logic. Given: two sentences *e* and *h*; wanted: an answer to the question whether *e* L-implies *h*. For instance, on the basis of an axiom set *e* of geometry, a mathematician finds, as a conjecture, an interesting sentence *h* concerning the angles of a triangle; this constitutes a tentative solution of a problem of the first kind; now he wants to find out whether *h* is actually deducible from *e*. Here, again, there is, in general, no effective procedure; in other words, L-implication is, in general, not an effective concept. Problems of this kind are again an essential part of any work in logic and mathematics. They are closely connected with problems of the first kind; for when a mathematician has found a theorem, he wants to give an exact proof for it so as to compel the assent of others. Finding a theorem is largely a matter of extrarational factors, not guided by rules. Constructing a proof is often called a rational procedure because here fixed rules have to be taken into consideration. However, the decisive point must not be overlooked: the rules of deduction are not rules of prescription, but rules of permission and of prohibition. That is to say, the rules do not tell the logician *X* which step to take at a given point in the course of a deduction; in other words, they do not constitute an effective procedure. The rules tell *X* merely which steps are permitted and thereby they say implicitly that all other steps are prohibited; they leave it to *X* to choose one of the steps permitted. Thus, here again, it depends upon *X*'s ingenuity and luck whether he solves the problem, that is, whether he finds a series of steps permitted by the rules, such that they lead from *e* to *h*.

More specifically, the situation is this. Only in the most elementary part of logic, in propositional logic (see above, § 21) is there a general method of decision, viz., the customary method of truth-tables (see § 21B). As soon as we enter the next higher field of logic, the so-called lower functional logic as represented, for instance, by our language system $\mathfrak{L}$ (§§ 15 ff.), there cannot be a method of decision for all sentences. [This has been shown by Alonzo Church; see *Amer. Journal of Math.*, 58 (1936), 345, and *Journal of Symbolic Logic*, 1 (1936), 40.] This holds a fortiori in the higher parts of logic, including arithmetic and the higher branches of mathematics. This does not exclude the possibility of methods of decision restricted to special kinds of sentences; and indeed several such methods for certain kinds within lower functional logic have been developed and are used as helpful instruments.

b. Inductive logic. Here, the problems of the second kind occur in two different forms, because here we are concerned not only with two sentences but, in addition, with a third item, a number. (i) Given: two sentences *e* and *h;* wanted: the value of $\mathfrak{c}(h,e)$, i.e., the degree of confirmation of *h* on the evidence *e*. (ii) Given: two sentences *e* and *h* and a number *r;* wanted: an answer to the question whether $\mathfrak{c}(h,e) = r$. For instance, a physicist has found, as a conjecture, a hypothesis *h* which he believes to be a good explanation for the results *e* of certain experiments; this is his solution, intuitively found, of a problem of the first kind; now he wants to find out whether *h* is indeed highly confirmed by *e* and, more precisely, (i) what is the value of $\mathfrak{c}(h,e)$; or, if he has made the guess that this value is *r*, he wants to find out (ii) whether indeed $\mathfrak{c}(h,e) = r$. There is, in general, no effective procedure for these problems; in other words, $\mathfrak{c}$ is, in general, not a computable function. This does not exclude the existence of methods of computation for $\mathfrak{c}$ in restricted classes. We shall later, in our system of quantitative inductive logic, give such methods for the following cases: (1) for all cases where *h* and *e* are molecular sentences in any system $\mathfrak{L}$, (2) for all cases where *h* and *e* are sentences of any form, molecular or general, in any finite system $\mathfrak{L}_N$, (3) for certain cases in a system $\mathfrak{L}_\infty^\pi$ (i.e., an infinite system containing only primitive predicates of degree one, § 31). More methods of this kind could be found for other restricted classes of cases. However, no general method of computation for $\mathfrak{c}$ is possible with respect to an infinite system $\mathfrak{L}_\infty$ which contains also relations; because such a method would immediately yield a method of decision for all sentences of this system, which is known to be impossible, as stated under (a). Thus, if *e* and *h* do not belong to one of the classes for which a method of computation exists and is known, the inductive logician *X* who wants to determine the value of $\mathfrak{c}(h,e)$ cannot simply follow a way prescribed by fixed rules, but just has to try to hit upon a way to a solution by his skill and good luck. This, however, is not a peculiar feature of inductive logic but holds in just the same way for deductive logic, as we have seen.

Thus it is true that an inductive machine is impossible for finding a suitable hypothesis (first problem) and also for examining whether a given hypothesis is suitable (second problem). But, then, a deductive machine is likewise impossible if it is intended to solve the corresponding deductive problems of finding a suitable L-implied theorem or of examining whether a proposed theorem is indeed L-implied. However, for a restricted domain as described above, an inductive machine for the determination of $\mathfrak{c}(h,e)$ is possible, for example, for all cases in which *e* and *h* do not contain variables with an infinite range of values; just as a deductive machine is possible which decides whether or not *e* L-implies *h*.

III. Third Problem: To Examine a Given Proof

a. Deductive logic. Given: *e*, *h*, and an alleged proof that *e* L-implies *h;* wanted: an answer to the question whether the alleged proof is actually a proof, that is, whether it is in accordance with the rules of deductive logic. For instance, a mathematician believes to have not only a solution of the first problem, for instance, a geometrical theorem *h*, but also a solution of the second problem, a proof that the axiom set *e* L-implies the theorem *h;* he wants to make sure that his belief is right, that is, that the proof is correct. For the solution of this problem there is an effective procedure, provided the proof is given completely. We have to distinguish here two different methods which are in customary use for proving that *e* L-implies *h*. (i) The first method consists in the construction of a sequence of sentences in the object language, leading from *e* to *h* in accordance with rules of deduction. (ii) The second method consists in a proof in the metalanguage, leading to the semantical statement '*e* L-implies *h*'. Strictly speaking, an effective method for testing proofs can only be applied if a set of deductive rules has been laid down and if the proof to be tested is formulated in such a detailed form that every step in it consists in a single application of one of the rules. This condition is not often fulfilled in method (i) and almost never in method (ii). The method for testing proofs, as they are usually formulated, is not effective in the strictest sense. However, we may say that it is *practically effective* in the following sense. Suppose a mathematician shows, by either method (i) or method (ii), that the theorem *h* is deducible from the geometrical axioms *e;* and suppose he uses in his proof, as is customary in geometry, the ordinary word language without explicit rules of deduction. Then we know what we have to do in order to examine the correctness of the proof. We examine for every single step in the proof whether it is an instance of a simple deductive procedure which we know to be valid. The mathematician has made the steps in such a way that he expects us to be able to carry out this examination for every step and to come to an affirmative result. If he has not overestimated our ability to recognize instances of L-implication, we shall affirm step for step and thereby recognize the whole proof as correct. Otherwise we have to ask him to split up the step which we are unable to judge into more and simpler steps, for which we are able to decide the question of correctness. Thus, in this examination of the proof, we are not entirely left to guessing, to a trial-and-error method as in problems of the first and second kind; instead, we know practically how to proceed and we expect that, under normal conditions, we shall reach a result in a finite number of operations, viz., the examinations of the steps of the given proof. In this sense we may say that we have a practically effective method. The result may also be formulated in this way: while L-implication is not an effective concept, the concept of proof for L-implication is effective, at least practically.

The situation may be described more in detail as follows. A method of the kind (i) is usually applied in syntax with respect to a calculus *K;* here the rules constitute a definition of 'direct C-implicate (directly derivable) in *K*' (see, e.g., [Semantics] §§ 26–28). Now it is possible, although not customary, to apply an exactly analogous method in semantics, with respect to a semantical system *S*. Essentially the same rules are here formulated as definition of 'direct L-implicate in *S*'. [Instead of constructing a chain leading from the premise *e* to *h* (called a derivation in the technical sense) one may also construct a chain without a premise leading to $e \supset h$ (called a derivation with the null class of premises or a proof in the technical sense; see [Semantics] § 26, formulation B); the difference is merely a technical one, the result is the same (for languages without free variables in sentences), see T20-1b.] Even if this method is used in a symbolic language for which explicit rules of deduction have been laid down, the proofs are rarely given in a complete form. They usually proceed by larger steps, such that each step consists of several applications of the rules and hence would be divided into several steps in a complete formulation. This abbreviated formulation is, of course, convenient and even necessary in order to avoid enormous length of the proofs. In many cases, the object language used in method (i) is the ordinary word language (supplemented by some technical terms and symbols) without explicit rules of deduction; and in almost all cases this holds for the metalanguage used in method (ii). This is customary for the formulations of deductions in mathematics and in science. Likewise in this book, we use method (ii); the proofs are formulated in the word language as our metalanguage (as an example, see the proof of T19-3). Thus, in all these cases, the method of examining the proofs has only the weaker and somewhat vague practical effectiveness described above.

b. Inductive logic. Given: *e*, *h*, and *r*, and an alleged proof that $\mathfrak{c}(h,e) = r$; wanted: an answer to the question whether the alleged proof is correct. For instance, a physicist believes he has found a solution of a problem of the first kind, say, a suitable hypothesis *h* on the basis of an observational report *e*, and, moreover, a solution of the problem of the second kind for

this case, viz., what appears to him like a proof that $\mathfrak{c}(h,e) = r$; he wants to determine whether this is a correct proof. For the solution of this problem, as for the analogous problem in deductive logic, there is a procedure which is at least practically effective. However, there is this difference: of the two methods (i) and (ii) earlier described, there is an analogue here only to the second, that is, a proof in the metalanguage for the semantical sentence '$\mathfrak{c}(h,e) = r$'. No analogue to the first method is known; and it seems doubtful whether a simple and convenient method of this kind could be found. [One might perhaps think of a procedure consisting in the construction of a sequence of sentences, with a real number expression attached to each sentence expressing the $\mathfrak{c}$ of that sentence on the fixed evidence *e*. The sentence *e* itself with '1' attached to it would be the beginning of the sequence, and *h* with an expression for the number *r* attached to it would be the end. The sentences would belong to the object language, as in a proof in method (i), but the numerical expressions would still be in the metalanguage.] Thus the situation is here the same as described earlier for method (ii) in deductive logic. A proof is given, formulated in the word language, which serves as a semantical metalanguage; and we test the correctness of the proof by examining for each step whether it is valid on the basis of the tacitly presupposed standards. Thus the procedure is practically effective in just the same sense as explained earlier (although it is not effective in the strictest sense unless deductive rules are laid down for the metalanguage).

B. *The Relation between Deductive and Inductive Logic*

Deductive logic may be regarded as the theory of the L-concepts, especially L-implication. These concepts can be based on the semantical concept of range, as we have seen (§ 20). Thus deductive logic, in this sense, is seen to be a part of semantics, that part which we sometimes call L-semantics. Inductive logic, in its quantitative form, may be regarded as the theory of $\mathfrak{c}$. As we shall see later, $\mathfrak{c}$ is also based on the concept of range. The theorems of inductive logic deal not only with $\mathfrak{c}$ but also with L-implication and the other L-concepts. Thus, inductive logic is likewise a part of semantics; it presupposes deductive logic; it may be regarded as constructed out of deductive logic by the introduction of the definition for $\mathfrak{c}$. In a sense, we may say that the definition of L-implication represents the rules of deduction; in the same sense, the definition of $\mathfrak{c}$ represents the rules of induction. Except for this difference with respect to the definitions used, the procedures for constructing proofs for theorems are the same in inductive logic as in deductive logic. We have earlier spoken of proofs for theorems of the form '*e* L-implies *h*' in deductive logic (see IIIa, method (ii)), and later of proofs for theorems of the form '$\mathfrak{c}(h,e) = r$' in inductive logic (see IIIb). If we look not at the definitions used but at the forms of inference used in these two kinds of proof, we find that they are the same in both cases. Not only in proofs of theorems of deductive logic but also in those of inductive logic we apply the implicit *deductive* procedures which are customarily applied in the word language. Thus any procedure of proof in any field, also in inductive logic, is ultimately a deductive procedure. This does not mean, of course, that induction is a kind of deduction. We must clearly distinguish between theorems of inductive logic, e.g., '$\mathfrak{c}(h,e) = 3/4$', and sentences like *e* and *h* about which the theorems speak. The former belong to the metalanguage; the latter belong to the object language and hence are not a part of inductive logic but its subject matter. The previous remark concerns only the former; it means that these theorems, although belonging to inductive logic, are reached by deduction. On the other hand, the relation between *e* and *h*, as stated by the theorem mentioned, is inductive, not deductive. No deductive procedure leads from *e* to *h;* but, if we may say so, an inductive procedure, characterized by the number 3/4, connects *e* with *h*.

The far-reaching analogy which holds between inductive and deductive logic in spite of the important differences between these two fields were repeatedly emphasized in the preceding discussions. The principal common characteristic of the statements in both fields is their independence of the contingency of facts. This characteristic justifies the application of the common term 'logic' to both fields. The following representation of examples in two parallel columns will perhaps help in further clarifying the analogy.

Deductive Logic

The subsequent statements in deductive logic refer to these example sentences:

Premise e: 'All men are mortal, and Socrates is a man.'

Conclusion h: 'Socrates is mortal.'

The following is an example of an elementary statement in deductive logic:

D_1. '*e* L-implies *h* (in *E*).'
(*E* is here either the English language or a semantical language system based on English.)

Inductive Logic

The subsequent statements in inductive logic refer to these example sentences:

Evidence (or premise) *e:* 'The number of inhabitants of Chicago is three million; two million of these have black hair; *b* is an inhabitant of Chicago.'

Hypothesis (or conclusion) *h:* '*b* has black hair.'

The following is an example of an elementary statement in inductive logic:

I_1. '$\mathfrak{c}(h,e) = 2/3$ (in *E*).'

DEDUCTIVE LOGIC—*Continued*

D_2. The statement D_1 can be established by a logical analysis of the meanings of the sentences *e* and *h*, provided the definition of 'L-implication' is given.

D_3. D_1 is a complete statement. We need not add to it any reference to specific deductive rules (e.g., the mood Barbara). However, the definition of 'L-implication' is, of course, presupposed for establishing D_1.

The following is a consequence of D_2.

D_4. The question whether the premise *e* is known (well established, highly confirmed, accepted), is irrelevant for D_1. This question becomes relevant only in the *application* of D_1 (see D_6 and D_7).

D_5 follows from D_1:

D_5. 'If *e* is true, then *h* is true.'

D_6 and D_7 are consequences of D_1 concerning *applications* to possible knowledge situations. D_6 represents the theoretical application (that is, the result refers again to the knowledge situation); D_7 represents the practical application (that is, the result refers to a decision).

D_6. 'If *e* is *known* (accepted, well established) by the person *X* at the time *t*, then *h* is likewise.' [Here, 'to know' is understood in a wide sense, including not only items of *X*'s explicit knowledge, that is, those which he is able to declare explicitly, but also those which are implicitly contained in *X*'s explicit knowledge.]

D_7. 'If *e* is known by *X* at *t*, then a decision of *X* at *t* based on the assumption *h* is rationally justified.'

INDUCTIVE LOGIC—*Continued*

I_2. The statement I_1 can be established by a logical analysis of the meanings of the sentences *e* and *h*, provided the definition of 'degree of confirmation' is given.

I_3. I_1 is a complete statement. We need not add to it any reference to specific inductive rules (e.g., for I_1, a rule of the direct inductive inference). However, the definition of 'degree of confirmation' is, of course, presupposed for establishing I_1.

The following is a consequence of I_2.

I_4. The question whether the premise (evidence) *e* is known (well established, highly confirmed, accepted), is irrelevant for I_1. This question becomes relevant only in the *application* of I_1 (see I_6 and I_7).

There is here no analogue to D_5. From I_1 and '*e* is true' nothing can be inferred (see § 10A).

I_6 and I_7 are consequences of I_1 concerning *applications* to possible knowledge situations. I_6 represents the theoretical application, I_7, the practical application.

I_6. 'If *e* and nothing else is *known* by *X* at *t*, then *h* is confirmed by *X* at *t* to the degree 2/3.' [Here, the term 'confirmed' does not mean the logical (semantical) concept of degree of confirmation occurring in D_1 but a corresponding pragmatical concept; the latter is, however, not identical with the concept of degree of (actual) belief but means rather the degree of belief justified by the observational knowledge of *X* at *t*.] The phrase 'and nothing else' in I_6 is essential; see § 45B concerning the requirement of total evidence.

I_7. 'If *e* and nothing else is known by *X* at *t*, then a decision of *X* at *t* based on the assumption of the degree of certainty 2/3 for *h* is rationally justified (e.g., the decision to accept a bet on *h* with a betting quotient not higher than 2/3).'

It should be noticed that in inductive logic, just as in deductive logic, the reference to the knowledge of *X* does not occur in the purely logical statements (e.g., I_1) but only in the statements of application (I_6 and I_7). It is true that statements of inductive logic, like those of deductive logic, are usually applied both in everyday life and in science to a premise or evidence that is known, i.e., well established by observations. Nevertheless, it is irrelevant for the *validity* as distinguished from the practical value or applicability, of a statement of inductive logic, just as for one of deductive logic, whether the evidence is true or not and, if it is true, whether its truth is known or not.

We shall later (§ 55B) clarify the relation between deductive and inductive logic in still another way with the help of the concept of range. We shall see that a statement of deductive logic like '*e* L-implies *h*' means that the entire range of *e* is included in that of *h*, while a statement of inductive logic like '$\mathfrak{c}(h,e) = 3/4$' means that three-fourths of the range of *e* is included in that of *h*. This shows again the similarity and at the same time the difference between the two fields.

§ 44. Logical and Methodological Problems

A. With respect to deductive procedures, we distinguish between the problems of deductive logic proper, including mathematics, and those of the methodology of deduction. The latter concern the choice of suitable deductive procedures for given purposes. Analogously we distinguish between inductive logic and methodology of induction. The latter gives no exact rules but only advice how best to apply inductive procedures for given purposes. Bacon's and Mill's theories on induction belong chiefly, not to inductive logic, but to the methodology of induction. On the other hand, the beginnings of an inductive logic are found in the classical theory of probability.

B. An inductive inference does not, like a deductive inference, lead to the acquisition of a new sentence but rather to the determination of a degree of confirmation. Inductive inferences usually concern a population (of persons or things) and samples; in many cases they deal with frequencies (statistical inferences). The principal kinds of inductive inference are briefly characterized: (1) direct inference, (2) predictive inference, (3) inference by analogy, (4) inverse inference, (5) universal inference.

A. *Methodological Problems*

In order to clarify the aim of our construction of inductive logic, it seems useful to emphasize a certain distinction between two kinds of problems. The problems of the one kind constitute the field which we call inductive logic; the problems of the other kind may be called, for lack of a better term, methodological problems and, more specifically, problems of the methodology of induction. Before explaining this distinction, let us look at deductive logic, where an analogous distinction can be made which is easier to understand. Here we have first the field of deductive logic proper, including pure mathematics. To this field belong, for instance, the theorems stated in §§ 20–40 above. Then there is a second field, closely connected but not identical with deductive logic. In this second field, methods are described for practically carrying out the procedures of deductive logic and mathematics, and suggestions are made for the use of these methods in various situations and for various purposes. Here we learn, for instance, how best to look for a proof of a conjectured theorem or for a simplification of a given proof; some hints are given as to the conditions under which an indirect proof may be useful; devices are explained for proving the independence of a certain sentence from a given set of postulates, or the consistency of the set, or its completeness; other devices are given for finding convenient approximating functions for the purpose of numerical calculations (for example, T40-4 above; this theorem itself and other similar ones in § 40A belong to mathematics and hence to deductive logic; but the more or less vague general rules which tell us how to find an approximating function of this kind when we need it belong to the second field). This second field may be called methodology of deductive logic and mathematics.

Analogously, inductive logic (in its quantitative form) contains statements which attribute a certain value of $\mathfrak{c}$ to a certain case, that is, a pair of sentences *e*,*h*, or speak about relations between values of $\mathfrak{c}$ in different cases. On the other hand, the methodology of induction gives advice how best to apply the methods of inductive logic for certain purposes. We may, for instance, wish to test a given hypothesis *h;* methodology tells us which kinds of experiments will be useful for this purpose by yielding observational data e_2 which, if added to our previous knowledge e_1, will be inductively highly relevant for our hypothesis *h*, that is, such that $\mathfrak{c}(h,e_1 . e_2)$ is either considerably higher or considerably lower than $\mathfrak{c}(h,e_1)$. Sometimes, not one hypothesis but a set of competitive hypotheses is given, and we wish to come to an inductive decision among them by finding observational material which gives to one of the hypotheses a considerably higher $\mathfrak{c}$ than to the others. In another case, we may have found observational results which are not explainable by the hypotheses accepted so far and perhaps even incompatible with one of them; here, we wish to find a new hypothesis which not only is compatible with the observations but explains them as well as possible. As explained in the preceding section (problem I), there is no effective procedure leading to this aim, no more than there is in mathematics for finding a theorem suitable for a given purpose. Nevertheless, it is possible in both cases to give some useful hints in which direction and by which means to look for a result of the kind wanted; these hints are given by methodology. Inductive and deductive logic cannot give them; they are indifferent to our needs and purposes both in practical life and in theoretical work. By emphasizing the distinction between logic and methodology, we do not intend to advocate a separation of the two kinds of problems within scientific inquiry. They are usually treated in close connection, and that is very useful. There is hardly any book in mathematics—except perhaps a table of logarithms—that does not add to the mathematical theorems some indications as to how they may usefully be applied either in mathematics itself or in empirical science. Similarly, to our later theorems in inductive logic, we shall often add some remarks about their use. Some of these remarks concern the use within inductive logic, for instance, the utilization of a given theorem in proofs of later theorems; other remarks concern the use outside of inductive logic, for instance, the possibility of a practical application either of inductive logic in general or of a given theorem to knowledge situations. Remarks of both kinds belong, not to inductive logic itself, but to the methodology of induction. [Examples of methodological discussions concerning the application of inductive logic in general are our discussions of the requirements of logical independence and completeness (above, § 18B), of the requirement of total evidence (below, § 45B), and the detailed discussions of the application of inductive logic for determining practical decisions (below, §§ 49–51); examples of methodological remarks concerning the application of particular theorems to possible knowledge situations are found at many places in the subsequent chapters, e.g., in §§ 60, 61, and generally whenever in the comments on given theorems terms like 'observation', 'known', 'unknown', 'expectation', 'prediction', 'decision', 'betting', and similar ones occur.] However, the principal purpose of this book is the discussion and, if possible, solution of problems of inductive logic itself; in other words, the proof of theorems on the degree of confirmation. The discussions of problems of the methodology of induction, on the other hand, are only incidental, although for practical reasons they may be useful and sometimes even indispensable. A theoretical book on geometry need not discuss in detail, if at all, the application of geometrical theorems for the calculation of the area of a garden or the distance of the moon, because the reader can be expected to be familiar with the connection between theoretical geometry and its application to spatial relations of physical bodies. In the case of inductive logic, on the other hand, there is at the present time not yet sufficient clarity and agreement even among the writers in the field concerning the

nature of the theory and the connection between theory and practical application. Therefore today a book on inductive logic is compelled to devote a considerable part of its space to a discussion of methodological problems.

One of the purposes in emphasizing the distinction between inductive logic proper and the methodology of induction is to make it clear that certain books, investigations, and discussions concerning induction do not belong to inductive logic although they are often attributed to it. This holds in particular for the works of Francis Bacon and John Stuart Mill; their discussions on induction, including Mill's methods of agreement, difference, etc., belong chiefly to the methodology of induction and give hardly a beginning of inductive logic. On the other hand, the beginnings of a systematic inductive logic can be found in another class of works, some of them written a long time before Mill, although in many of these works the word 'induction' does not even occur. I am referring to all those works which deal with the theory of probability$_1$; as previously explained (§ 12), most of the classical works on the theory of probability belong to this class, as do most of those modern books on probability which are not based on the frequency conception of probability. In most of these theories, probability has numerical values; hence, they are systems of quantitative inductive logic. Keynes's theory is an example of a comparative inductive logic supplemented by a very restricted part of quantitative inductive logic, since, according to his conception, probability has numerical values only in some cases of a special kind, while in general only a comparison is possible leading to the result that one hypothesis is more probable than another. Jeffreys starts with axioms on the primitive notion 'given p, q is more probable than r', hence with a comparative inductive logic; on its basis, a quantitative inductive logic is constructed by laying down conventions for the assignment of numerical values.

B. *Inductive Inferences*

What we call inductive logic is often called the theory of nondemonstrative or nondeductive inference. Since we use the term 'inductive' in the wide sense of 'nondeductive', we might call it the theory of inductive inference. We shall indeed often speak of inductive inferences because the term is customary and convenient. However, it should be noticed that the term 'inference' must here, in inductive logic, not be understood in the same sense as in deductive logic. Deductive and inductive logic are analogous in one respect: both investigate logical relations between sentences;

the first studies the relation of L-implication, the second that of degree of confirmation which may be regarded as a numerical measure for a partial L-implication, as we shall see (§ 55B). The term 'inference' in its customary use implies a transition from given sentences to new sentences or an acquisition of a new sentence on the basis of sentences already possessed. However, only deductive inference is inference in this sense. If an observer X has written down a list of sentences stating facts which he knows, then he may add to the list any other sentence which he finds to be L-implied by sentences of his list. If, on the other hand, he finds that his knowledge confirms another sentence to a certain degree, he must not simply add this other sentence. The result of his inductive examination cannot be formulated by the sentence alone; the value found for the degree of confirmation is an essential part of the result. If we want to give a schematized (and hence somewhat oversimplified) picture of X's procedure, we may imagine that he writes two lists of sentences; for the sake of simplicity we assume that the sentences of both lists are molecular. The first list contains the sentences which he knows; additions to this list are made in two ways: (*a*) basic sentences formulating the results of new observations which he makes and (*b*) sentences L-implied by those on the list. Only the additions of the kind (*a*) change the logical content of the list. Let us assume that the atomic sentences of X's language are logically independent of each other (according to the requirement of independence, § 18B). Then X need never cross out a sentence once written on the first list. The second list contains inductive results. These are formulated by sentences, each of them marked with a numerical value, its degree of confirmation with respect to the first list. These values, however, hold only for a certain time; as soon as a new observation sentence is added to the first list, the numerical values on the second list have to be revised. These values could be provided by an inductive machine, into which the observation sentences of the first list, kind (*a*), are fed. (In order to make the procedure effective and accessible to a machine, it must be restricted to a finite system.)

This picture makes it clear that an inductive inference does not, like a deductive inference, result in the acquisition of a sentence but in the determination of its degree of confirmation. It is in this sense, and only in this sense, that we shall use the term 'inductive inference' further on.

The most important kinds of inductive inference or, in other words, of general theorems concerning $\mathfrak{c}$ deal with cases where either or both of the sentences e and h give information about frequencies, for instance, in the form of an individual or statistical distribution (§ 26B) for some individuals with respect to a division. In these cases we might speak of *statistical inductive inferences*.

Following the usage of statisticians, we call the class of all those individuals to which a given statistical investigation refers the *population*. Any proper subclass of the population, defined by an enumeration of its elements, not by a common property, is called a *sample* from the population. The population need not necessarily consist of human beings; it may consist of things or events of any kind, persons, animals, births, deaths, molecules, electrons, specimens of grain, products of a factory, etc. The population is usually not the whole universe of individuals but only a part of it. For example, the universe may be the totality of physical things; one investigation may take as population the present inhabitants of Chicago, another may take the inhabitants of Boston in 1900, etc.; the fact that these and other populations are parts of the same universe of individuals makes it possible first to formulate these investigations in the same language system and also, if desired, to consider later a more comprehensive population containing the original ones as parts and studying their relations.

We shall now briefly characterize some of the most important kinds of inductive inference; they are neither exhaustive nor mutually exclusive.

1. The *direct inference*, that is, the inference from the population to a sample. (It might also be called internal inference or downward inference.) e may state the frequency of a property M in the population, and h the same in a sample of the population.

2. The *predictive inference*, that is, the inference from one sample to another sample not overlapping with the first. (It might also be called external inference.) This is the most important and fundamental inductive inference. From the general theorems concerning this kind we shall later (in Vol. II) derive the theorems concerning the subsequent kinds. The special case where the second sample consists of only one individual is called the *singular predictive inference*. We have indicated earlier (§ 41D) and we shall show in detail later (T108-1) that the results of the singular predictive inference stand in a close relation to the estimation of relative frequency.

3. The *inference by analogy*, the inference from one individual to another on the basis of their known similarity.

4. The *inverse inference*, the inference from a sample to the population. (It might also be called upward inference.) This inference is of greater importance in practical statistical work than the direct inference because we usually have statistical information only for some samples actually ob-

served and counted and not for the whole population. Methods for the inverse inference (often called 'inverse probability') have been much discussed both in the classical period and in modern statistics. One of the chief stimulations for the developments of modern statistical methods came from the controversies concerning the validity of the classical methods for the inverse inference.

5. The *universal inference*, the inference from a sample to a hypothesis of universal form. This inference has often been regarded as the most important kind of inductive inference. The term 'induction' was in the past often restricted to universal induction. Our later discussion will show that actually the predictive inference is more important not only from the point of view of practical decisions but also from that of theoretical science.

§ 45. Abstraction in Inductive Logic

A. The application of logic, which is not a task of logic itself but of methodology, has to do with states of observing, believing, knowing, and the like. On the other hand, logic itself, both deductive and inductive, deals not with these states but instead with sentences subject to exact rules. Thus logic gains exactness by abstracting from the vague features of actual situations. *B*. In the application of inductive logic still another difficulty is involved, which does not concern inductive logic itself. This difficulty consists in the fact that, if an observer wants to apply inductive logic to an expectation concerning a hypothesis h, he has to take as evidence e a complete report of all his observational knowledge. Many authors on probability$_1$ have not given sufficient attention to this *requirement of total evidence*. They often leave aside a great part of the available information as though it were irrelevant. However, cases of strict irrelevance are much more rare than is usually assumed. *C*. The simple structure of our language systems, the earlier requirement of completeness (§ 18B), and now the requirement of total evidence compel us to construct all examples of the application of inductive logic in a fictitious simplified form. This fact, however, does not prevent the approximative application of inductive logic to actual knowledge situations in our actual world, just as certain idealized concepts of physics can be practically applied. *D*. Abstractions may be very fruitful and even necessary for the progress of science, as the example of geometry shows. Some students reject all abstractions; others use them excessively and neglect certain features of reality. These extremes are harmful. We should rather combine both tendencies, that emphasizing the concrete as well as that emphasizing the abstract. As to inductive logic, we should overlook neither the fact that its ultimate purpose lies in its application in practical life nor the fact that it cannot be efficient without using abstract methods.

A. *Abstraction in Deductive and Inductive Logic*

Our theory of inductive logic will be applied not to the whole language of science with its great complexities, its large variety of forms of expres-

sion, and its variables of higher levels (e.g., for real numbers), but only to the simple language systems $\mathfrak{L}$ explained in the preceding chapter. This involves a certain simplification and schematization of inductive procedures in comparison with those actually used in the practice of science. Other kinds of schematization here involved are still more important; they will be discussed in this section. The first of them is inherent in any logical method; it could not be avoided even if we took the whole language of science as our object language, and it is a necessary factor even in deductive logic. It consists in the fact that the pure systems of both deductive and inductive logic refer simply to sentences (or to the propositions expressed by them) rather than to states of knowing, believing, assuming, etc., while any *application* of logic to an actual situation has to do with these states. This application is outside of pure logic itself; it belongs to the subject matter of the methodology of logic, as we have seen in the preceding section.

Let us first take an example from deductive logic. One of the simplest theorems of deductive logic says that i L-implies $i \vee j$. One kind of application of this theorem consists in the following rule, which is not a logical but a methodological rule: if X has good reasons for believing i, then the same reasons entitle him to believe $i \vee j$. This, however, is a crude formulation using 'believing' as a classificatory concept. A more adequate formulation would use it as a quantitative or at least as a comparative concept: if X at the time t has reasons for a belief in i to the degree r, then he has at the same time reasons for a belief in $i \vee j$ at least to the degree r. For instance, I look at a tree and, on the basis of what I see, I am convinced that a certain leaf is green; then I have the right to be convinced at least as strongly that this leaf is green or smooth. In this way, some rather vague and perhaps even problematic concepts enter the situation. Am I actually convinced? How am I to measure the strength of my conviction or at least to compare two convictions as to their strength? Is the color I want to express described accurately by 'green', or should I perhaps rather say 'greenish-blue'? We have here all the vaguenesses and other difficulties which arise on the way from an observation to the utterance of a corresponding observation sentence and our report about the belief in it. Within logic, however, all these difficulties do not appear. Not that they have been overcome; we just leave them outside, we 'abstract' from them. The advantage of this procedure is that in logic we deal only with clear-cut entities without vagueness. We have predicates and they are assumed to designate properties, and further we have other signs and their designata. The actual vagueness of the boundary line between green and blue is disregarded and likewise the vagueness of the other properties and all other designata. Furthermore, logic contains other semantical rules determining the meaning of the sentences on the basis of these designata (e.g., in the form of rules of ranges, as explained in § 18D). With the help of these rules, we determine whether or not the relation of L-implication holds between given sentences, and thus we reach one of the chief aims of deductive logic. (For instance, we show that i L-implies $i \vee j$ by showing that the range of i is contained in that of $i \vee j$.) All these procedures within deductive logic deal with neat, clear-cut entities according to exact rules and thus are not blurred by any vagueness. However, we must necessarily pay a price for this advantage; by the abstraction which we carry out in order to construct our system of logic, we disregard certain features; they remain outside the scope of logic. However, we must be careful in the characterization of this situation. Some philosophers say that, in consequence of the abstraction leading to logic or, in a similar way, to quantitative physics, certain features of reality (for instance, the 'genuine qualities' or 'qualia') remain forever outside our grasp. I do not agree with this view; although it sounds similar to what I said earlier, there is a fundamental difference. This may become clearer by the following analogy. Suppose a circular area is given, and we want to cover some of it with quadrangles which we draw within the circle and which do not overlap. This can be done in many different ways; but, whichever way we do it and however far we go with the (finite) procedure, we shall never succeed in covering the whole circular area. However, it is not true that—in analogy to the philosophical view mentioned—there is any point in the area which cannot be covered. On the contrary, for every point and even for every finite number of points there is a finite set of quadrangles covering all of them. The situation with abstraction is analogous. In any construction of a system of logic or, in other words, of a language system with exact rules, something is sacrificed, is not grasped, because of the abstraction or schematization involved. However, it is not true that there is anything that cannot be grasped by a language system and hence escapes logic. For any single fact in the world, a language system can be constructed which is capable of representing that fact while others are not covered. For instance, if we find ourselves unable to describe a certain subtle difference between two shades of color with simple predicates like 'green' and 'blue', we may make our net finer and finer by introducing more and more predicates like 'bluish-green', 'greenish-blue', etc., or by introducing quantitative scales (as in the color systems of W. Ostwald or A. C. Hardy); in this way, our language becomes more and more

precise with respect to colors. Perhaps this process of introducing more and more precise terms can never come to an end, so that some vagueness always remains. On the other hand, there is no difference in color shade, however slight, that remains forever inexpressible.

B. *The Requirement of Total Evidence*

Suppose that inductive logic supplies a simple result of the form '$\mathfrak{c}(h,e) = r$', where h and e are two given sentences and r is a given real number. How is this result to be applied to a given knowledge situation? This question is answered by the following rule, which is not a rule of inductive logic but of the methodology of induction:

(1) If e expresses the total knowledge of X at the time t, that is to say, his total knowledge of the results of his observations, then X is justified at this time to believe h to the degree r, and hence to bet on h with a betting quotient not higher than r.

One of the decisive points in this rule is the fact that it lays down the following stipulation:

(2) *Requirement of total evidence:* in the application of inductive logic to a given knowledge situation, the total evidence available must be taken as basis for determining the degree of confirmation.

There is no analogue to this requirement in deductive logic. If deductive logic says that e L-implies h and if X knows e, then he is entitled to assert h irrespective of any further knowledge he may possess. On the other hand, if inductive logic says that $\mathfrak{c}(h,e) = r$, then the mere fact that X knows e does not entitle him to believe h to the degree r; obviously it is required either that X know nothing beyond e or that the totality of his additional knowledge i be *irrelevant* for h with respect to e, i.e., that it can be shown in inductive logic that $\mathfrak{c}(h,e \,.\, i) = \mathfrak{c}(h,e)$. It cannot even be said that X may believe h at least to the degree r; by the addition of i, the $\mathfrak{c}$ for h may as well decrease as increase. The theoretical validity of the requirement of total evidence cannot be doubted. If a judge in determining the probability of the defendant's guilt were to disregard some relevant facts brought to his knowledge; if a businessman tried to estimate the gain to be expected from a certain deal but left out of consideration some risks he knows to be involved; or if a scientist pleading for a certain hypothesis omitted in his publication some experimental results unfavorable to the hypothesis, then everybody would regard such a procedure as wrong.

The requirement has been recognized since the classical period of the theory of probability. Keynes ([Probab.], p. 313) refers to "Bernoulli's maxim, that in reckoning a probability, we must take into account all the information which we have". Although in the second axiom referred to by Keynes, Bernoulli speaks in somewhat weaker terms ("everything that can come to our knowledge" [Ars], p. 214), the formulation of the third axiom ("Not only those arguments must be considered which are favorable to an affair but also all those which can be advanced against it, so that after pondering both it becomes clear which ones outweigh the others", p. 215) and the examples given in connection with both axioms leave no doubt that the requirement of total evidence is meant. The requirement is expressed more clearly by C. S. Peirce: "I cannot make a valid probable inference without taking into account whatever knowledge I have . . . that bears on the question" ([Theory], p. 461). However, many writers since the classical period, although presumably acknowledging the requirement in theory, did not give sufficient attention to it in questions of practical application. Laplace himself, for instance, raised the following question: According to the reports of history, the sun has never failed to rise every twenty-four hours for five thousand years or 1,826,213 days; what is the probability of its rising again tomorrow morning? Using his rule of succession, Laplace gave the answer: $1 - 1/1,826,215$. Since we cannot assume that he was unaware of the fact that history reports besides sunrises also a number of other events, we must conclude that he either regarded all other known events as irrelevant for his problem or failed to consider the question of relevance. Many examples of a similar nature were constructed. Later writers criticized these examples. Aside from criticisms of the methods used for the solutions, for example, the rule of succession, the objection was raised that series of events of this kind are not a proper subject matter for the theory of probability because we have a causal explanation for them and therefore cannot regard them as matters of chance. I should prefer to give this objection a different form. I agree with Laplace against his critics in the view that the theory of probability or inductive logic applies to *all* kinds of events, including those which seem to follow so-called causal laws, that is, general formulas of physics, for instance, in the example of the sun, the laws of mechanics applied to the earth and the sun. On the other hand, I agree with the critical judgment of the later writers that Laplace's application of the theory in cases of this kind is not correct because our knowledge of mechanics is disregarded. I would say that the requirement of total evidence is here violated because there are many other known facts which are relevant for the probability of the sun's rising tomorrow. Among them are all those

facts which function as confirming instances for the laws of mechanics. They are relevant because the prediction of the sunrise for tomorrow is a prediction of an instance of these laws.

Modern authors on probability are in general more careful in the construction of their examples; but I think that even they are often not cautious enough in their tacit or explicit assumptions as to irrelevance. The cases of strict irrelevance are considerably more rare than is usually believed. Later, in the construction of our system of inductive logic, examples will be found where we might be inclined at the first look to assume irrelevance, while a closer investigation shows that it does not hold.

C. *The Applicability of Inductive Logic*

We have seen earlier (§ 18B) that the requirement of completeness compels us to imagine for the purpose of the application of inductive logic a simplified world, a universe which is not more complex in structure or more abundant in variety than the simple language system which we are able to manipulate in inductive logic. Now the requirement of total evidence compels us in the construction of examples of application to imagine in the simplified universe an observer X with a simplified biography. While every adult person in our actual world has observed an enormous number of events with an immense variety transcending all possibilities of complete description, let alone calculatory inductive analysis, we have to imagine an observer X whose entire wealth of experience is so limited that it can easily be formulated and taken as a basis for inductive procedures. Thus, examples of the application of inductive logic must necessarily show certain fictitious features and deviate more from situations which can actually occur than is the case in deductive logic. This fact, however, does not make inductive logic a fictitious theory without relevance for science or practical life. A man who wants to calculate the areas of islands and countries begins with studying geometrical theorems illustrated by examples of simple forms like triangles, rectangles, circles, etc., although none of the countries in which he is interested has any of these forms. He knows that by beginning with simple forms he will learn a method which can be applied also to more and more complex forms approximating more and more the areas in which he is interested. Analogously, the method of inductive logic, although first applied only to fictitious simple situations, can, if sufficiently developed, be applied to more and more complex cases which approximate more and more the situations in which we find ourselves in real life. Physics likewise uses certain simplified, idealized conceptions which would hold strictly only in a fictitious universe, for example, those of frictionless movement, an absolutely rigid lever, a perfect pendulum, a mass point, an ideal gas, etc. These concepts are found to be useful, however, because the simple laws stated for these ideal cases hold approximately whenever the ideal conditions are approximately fulfilled. Similarly, there are actual situations which may be regarded as approximately representing the ideal conditions dealt with in our inductive logic referring to the simple systems $\mathfrak{L}$.

Suppose, for instance, that spherical balls of equal size are drawn from an urn; the surface of these balls is in general white, but some are marked with a red point, others not; some (without regard to whether they have a red point or not) have a blue point, others not; and some have a yellow point, others not. A simple inspection does not reveal other differences between the balls. Then we may apply our system $\mathfrak{L}$ to the balls and their observed marks; we take as individuals the balls, or rather the events of the appearance of the single balls, abstracting from the fact that the actual balls have distinguishable parts and that the very markings by which we distinguish them are parts of the balls. And we take the three kinds of markings as primitive properties as though they were the only qualitative properties of the balls, abstracting from the fact that a careful inspection of the actual balls would reveal many more properties in which they differ. Suppose we have drawn one hundred balls and found that forty of them had the property M of bearing a red point and a blue point. Suppose that this is all the knowledge we have concerning the balls and that we are interested in the probability of the hypothesis h that the next ball (if and when it appears) will have the property M. Then we shall take as our evidence e the observation results concerning the hundred balls just described. This is again an idealization of the actual situation because in fact we have, of course, an enormous amount of knowledge concerning other things. We leave this other knowledge i aside because we regard it as plausible that it is not very relevant for h with respect to e, that is to say, that the value of $\mathfrak{c}(h,e)$, which we can calculate, does not differ much from the value of $\mathfrak{c}(h,e\,.\,i)$, which ought to be taken according to the requirement of total evidence but which would make the calculation too complicated. (Of course, we may be mistaken in the assumption of the near-irrelevance of i; that is to say, a closer investigation might show that, in order to come to a sufficient approximation, certain other parts of the available knowledge must be included in the evidence; just as a physicist who assumes that the influence of the friction in a certain case is so small that he may neglect it may find by a closer analysis that its influence is considerable and therefore must be taken into account.) If the temporal order of the hundred ball drawings is known and seems to be relevant (for instance, if the sequence of the colors in their temporal order of appearance shows a high degree of regularity), then we shall include in our evidence the description of this order according to one of the methods earlier explained (§ 15B). If the temporal order of the hundred drawings is not known (for instance, if we counted only the number of each kind without paying attention to the order) or if it is known but assumed to be not very relevant, then we shall take as evidence the conjunction of three hundred sentences, each of which says of one of the hundred balls whether or not it has one of the three primitive properties. It will even be sufficient to take as evidence a conjunction of one hundred sentences, each of which says of one of the hundred balls whether or not it is M. For certain rules of induction or definitions of degree of confirmation, it can be shown that the additional knowledge contained in the three hundred sentences is strictly irrelevant in this case.

Let us suppose that we have decided to take the latter conjunction of one hundred sentences concerning M and non-M as our evidence e. Then a system of inductive logic, although formulated for a simplified universe, may be applied to the actual knowledge situation just described. The application consists in calculating the value of the degree of confirmation $\mathfrak{c}$ for the hypothesis h and the evidence e specified and taking this value as the probability sought.

It is important to recognize clearly the nature of the difficulties which have just been explained. They do not occur in inductive logic itself but only in the application of inductive logic to actual situations of knowledge; hence they belong to the methodology of induction. Like deductive logic, inductive logic has to do only with clear-cut entities without any vagueness; it deals with sentences of a constructed language system; it ascribes to a pair of sentences h,e a real number r as the degree of confirmation according to exact rules. Here, as in deductive logic, the exactness, the freedom from vagueness, is obtained by abstraction and therefore at a sacrifice.

D. *Dangers and Usefulness of Abstraction*

Some scientists and philosophers feel a strong disinclination against all abstractions or schematizations. They demand that any methodological or even logical analysis of science should never lose sight of the actual behavior of scientists both in the laboratory and at the desk. They warn against neglecting any of the factors which a good scientist takes into consideration in inventing and testing his hypotheses; they emphasize that the complex judgment on the acceptability of a hypothesis cannot be based on just one number, the degree of confirmation. I think that this view contains a correct and important idea. Whenever we make an abstraction, we certainly ought to be fully aware of what we are doing and not to forget that we leave aside certain features of the real processes and that these features from which we abstract at the moment must not be entirely overlooked but must be given their rightful place at some point in the full investigation of science. On the other hand, if some authors exaggerate this valid requirement into a wholesale rejection of all abstractions and schematizations, an attitude which sometimes develops into a veritable abstractophobia, then they deprive science of some of its most fruitful methods.

The history of science is full of examples for the usefulness and immense fertility of abstractions. One of the most outstanding examples is geometry. It was created by an act of abstraction: attention was directed toward the spatial properties and relations of bodies, while all other properties, color, substance, weight, etc., were disregarded. Then another bold step was taken, leading away from the world of concrete things with their directly observable properties to a schema consisting of constructs: geometry was transformed into a theory of certain spatial configurations whose properties are completely and exactly determined. This geometry no longer deals with wooden or iron balls but with spheres, perfect spheres of which the balls are only more or less rough approximations. It deals with infinite straight lines, of which at best some finite segments are approximately represented by certain threads and edges of bodies. Both these steps of abstraction were taken in ancient times; we will not discuss here some later steps which went even much farther in the same direction by transforming geometry into a theory of certain sets of real numbers (Descartes), into a formal axiom system (Hilbert), and finally into a special branch of the logic of relations (Russell). The important point for our discussion appears already in the effect of the first two steps of abstraction. Today it is clear that the magnificent development of geometry through its history of more than two thousand years would have been impossible without those abstractions and that the development of physics would have been impossible without that of geometry. Thus the end result is that, not only from the point of view of the mathematician but also from that of the physicist, the abstractions in geometry are immensely useful and even practically indispensable. Although the aim remains the investigation not of the abstract configurations but of the observable spatial properties of concrete things, nevertheless it turns out that abstract geometry supplies the most efficient method for this investigation, much more efficient than any method dealing directly with observable spatial properties. Numerous other methods of abstraction or schematization have proved fruitful in physics. This shows that, if we want to obtain knowledge of the things and events of our environment as a help for our decisions in practical life, then the roundabout way which leads first away from these things to an abstract schema may in the long run be better than the direct way which stays close to the things and their observable properties.

The situation in logic is analogous. Both in deductive and in inductive logic we deal with abstract schemata, with sentences which belong to constructed language systems and are manipulated according to exact rules. This is admittedly a step away from the actual situations of observing, believing, etc., in which we find ourselves in practical life. The choice of this procedure is not based on the assumption that the actual situations are unimportant and that the exact schemata are all that matters. On the contrary, the final aim of the whole enterprise of logic as of any other cognitive endeavor is to supply methods for guiding our decisions in practical situations. (This does, of course, not mean that this final aim is also the motive in every activity in logic or science.) But here, as in physics, the roundabout way through an abstract schema is the best way also for the practical aim. Some philosophers who shy away from all abstractions have suggested that in the logical analysis of science we should not make abstractions but deal with the actual procedures, observations, statements, etc., made by scientists; we should give up the concept of truth as defined in pure semantics with respect to a constructed language system and use instead the pragmatical concept 'accepted (or verified or highly confirmed) by *X* at the time *t*'; likewise, instead of the semantical concept of L-truth (see § 20), we should use a related pragmatical concept defined in about this way: '*i* is a sentence of such a kind that, for any sentence *j*, the utterance of the conjunction $i \,.\, j$ by *X* to *Y* has the same effect on *Y* as the utterance of *j* alone'. A theory of pragmatical concepts would certainly be of interest, and a further development of such a theory from the present modest beginnings is highly desirable. However, I think the repudiation of pure radical semantics and L-semantics, and thereby both of pure deductive and of inductive logic, in favor of a merely pragmatical analysis of the language of science would lead to a method of very poor efficiency, analogous to a geometry restricted to observable spatial properties. Inductive logic deals with schemata; but it is developed not for the sake of these schemata, but finally for the purpose of giving help to the man who wants to know how certain he can be that his crop will not be destroyed by a drought, to the insurance company which wants to calculate a premium rate for life insurance that is not too high but still profitable, to the engineer who wants to find the degree of certainty that the bridge he constructs will be able to carry a certain load, to the physicist who wants to find out which of a set of competing theories is best supported by the experimental results known to him. The decisive point is that just for these practical applications the method which uses abstract schemata is the most efficient one.

One of the factors contributing to the origin of the controversy about abstractions is a psychological one; it is the difference between two constitutional types. Persons of the one type (extroverts) are attentive to and have a liking for nature with all its complexities and its inexhaustible richness of qualities; consequently, they dislike to see any of these qualities overlooked or neglected in a description or a scientific theory. Persons of the other type (introverts) like the neatness and exactness of formal structures more than the richness of qualities; consequently, they are inclined to replace in their thinking the full picture of reality by a simplified schema. In the field of science and of theoretical investigation in general, both types do valuable work; their functions complement each other, and both are indispensable. Students of the first type are the best observers; they call our attention to subtle and easily overlooked features of reality. They alone, however, would not be able to reach generalizations of a high level, because abstractions are needed for this purpose. Therefore, a science developed by them alone would be rich in details but weak in power of explanation and prediction. (This is a warning to those who are afraid of abstractions, especially in inductive logic.) Students of the second type are the best originators and users of abstract methods which, when sufficiently developed, may be applied as powerful instruments for the purpose of description, explanation, and prediction. Their chief weakness is the ever present temptation to overschematize and oversimplify and hence to overlook important factors in the actual situation; the result may be a theory which is wonderful to look at in its exactness, symmetry, and formal elegance, and yet woefully inadequate for the task of application for which it is intended. (This is a warning directed at the author of this book by his critical super-ego.)

It seems to me that the contrast between the two types, as long as its expression is a controversy between thesis and antithesis, the danger of abstractions versus their usefulness, is futile. It may become fruitful if expressed as a difference in emphasis rather than in assertion; either type

emphasizes one side of the whole method of research and works as a safeguard against its neglect. History and personal experiences show us that either type is tempted to underestimate the value of the work of the other type. However, it is clear that science can progress only by the cooperation of both types, by the combination of both directions in the working method.

The foregoing distinction of two types is a customary but obviously oversimplified description of the situation. Instead of speaking of two types, one directed toward the concrete, the other toward the abstract, it would be more correct to apply a continuous scale of comparison: a person X tends less toward the concrete and more toward the abstract than another person Y. (In other words, a comparative concept is here more adequate than the two classificatory concepts; see § 4.)

§ 46. Is a Quantitative Inductive Logic Impossible?

Some students regard a quantitative degree of confirmation and hence a quantitative inductive logic as impossible because there are very many different factors determining the choice of the "best" hypothesis, and some of them cannot be numerically evaluated. However, the task of inductive logic is not to represent all these factors, but only the logical ones; the methodological (practical, technological) and other nonlogical factors lie outside its scope. Some authors, among them Kries, believe (1) that even the logical factors, for example, the extension, precision, and variety of the confirming material, are in principle inaccessible to numerical evaluation; and (2) that it is impossible to define a quantitative degree of confirmation dependent upon these factors. The first of these assertions is easily refuted.

The different attitude of the two psychological types discussed above manifested itself clearly each time in the development of modern science when attempts were made to introduce quantitative concepts, measurement, and mathematical methods into a new field, for instance, psychology, social sciences, and biology. Those who made these attempts were convinced from the beginning that the application of mathematical methods was possible though perhaps difficult. Even if they had to admit that the initial steps taken were far from perfect, they were not discouraged; they did not believe that these defects were necessary, due to an inherent nonquantitative character of the field in question. They expected that the method could and would be improved and that, when further developed, it would yield many new results unobtainable by the traditional methods alone. The opponents, on the other hand, believed either that it was impossible in principle to apply quantitative concepts to the special field ("How should it be possible to measure an intensive magnitude like a degree of intelligence, the intensity of an emotion, the