1. Cognitive Expansion and Uncertainty

In this chapter I will try to show that our cognitive experience, when it expands, encounters uncertainty. To the question, Can experiential expansion be certain? my response will be negative. This brief formulation of the issue needs elucidation.

Let me first distinguish the forms of experience that are cognitive and those that are not. As we all know, certain forms of experience are purely biological or psychological, and their initial cognitive import, except in a very trivial sense, is nil. We have aesthetic, ethical, and other modes of experience that form an important part of our psychobiological or even sociobiological history but are not of much informative value. About them we may raise many interesting structural or modular questions: How they are formed? How their forms are interrelated? and the like. Even the ontological and causal bases of these forms of experience may be of much help in understanding the workings of our mind. But as such these forms of experience are not cognitive. One may say that these processes of experience take place in our body-mind complex without adding anything to our understanding of ourselves and our circumstances.

There is another way of looking at these forms of experience. When we use some such words as cognitive experience and noncognitive experience we certainly have some concepts in view, and those concepts can be shared with others. Similarly, when I say, for example, “expansion of experience leads to uncertainty,” I describe a situation that is intelligible to others, provided of course they know my language. In this way when we learn certain words denoting certain objects and concepts, we try, through those words, to convey the
same idea to others. Ways of learning words and their uses, individual or sentential, of course are in a way silently informative. When, for example, children see pictures of animals and fruit and are told of their names, they certainly learn something not known to them before. Further, when they learn to use rightly some such sentences as “This tree is green” and “The dog is an animal,” their horizon of knowledge undoubtedly expands. Through experience and successful use of language the so-called noncognitive experiences start disclosing their protocognitive implications.

To learn the uses of different types of nouns, pronouns, and abstract concepts certainly contributes to the growth of children’s knowledge. In a way this is true also of the growth of knowledge of the adult human beings. When we learn how different types of words, or parts of speech, are related, that is, syntactically connected into meaningful sentences, we start moving toward complex and abstract forms of knowledge. More abstract and complex forms of knowledge are acquired when we learn to see the relations between sentences and the states of affairs they refer to. In other words, when we learn to argue and infer and to move beyond the mere use of words or isolated sentences, our world expands further. By using language and logic, logic in language, we expand the horizon of our experience.

The distinction needs to be drawn between how language acquisition adds to our expansion of knowledge and how we consciously plan the expansion of knowledge and design our experiences accordingly. For language acquisition may be studied as a part of linguistics, computation, biology, psychology, and sociology. That study will not form a part of what we call epistemology. Certainly they are related in a complex way; epistemology is grounded in those disciplines. But the description of that relation will not be of much help to us in ascertaining what is valid knowledge, episteme.

My main interest here is to analyze how consciously structured experiences, when expanded, run into uncertainty. The implicit structures of our knowledge, when laid bare or made articulate, exhibit, broadly speaking, two patterns, inductive and deductive. But strictly speaking, these two patterns or structures, except for the purpose of analysis, are not exclusive. Induction involves deduction. Deduction involves induction. It is not surprising that some logicians are of the view that, as logic, induction and deduction are not sharply separable, only their starting points and aims differ. This difference is primarily programmatic and heuristic and not intrinsic to their nature.

In a way logic, in all its forms, deductive, inductive, and proba-
bilistic, constitutes the structuralization of our cognitive experience. At the stage of structuralization, the cognitive aspect may well be ignored. Given a set of premises, P, and set of rules of inference, R, we try to see what conclusion(s), C, can be derived. But even then the question of correctness, adequacy, or justification of how we structuralize our experience remains. If P is true and R followed, can C be false? The falsity of C may be a logical question or an epistemological one. If C turns out to be false, does it not infect P or at least a part of it? Therefore the question of justification is relevant not only in the cases of inductive logic or probability logic but also in the case of deductive logic. In brief, the question of justification may be rightly radicalized and taken to the basement or foundation of every form of logic. If logic itself is a human-made game, having no secure foundation of its own, must its limited use value (predetermined by definition and choice) be so glorified and taken so seriously?

2. Evidence, Acceptance, and Probability

Empirical realists, not committed to synthetic a priori truths, are of the view that neither in respect of empirical generalizations nor in respect of individual future events can one be absolutely certain. Available evidence can more or less confirm a universal generalization but cannot entail it. Our prediction about singular events may be more or less probable but cannot be certain. The question is, Though certainty seems to be elusive, when can (1) our acceptance of a hypothesis and (2) our belief that a particular event will take place be regarded as rational or justified?

Evidence e may at best make hypothesis h probable, but it does not make it true. For acceptance AC of h one may take e as necessary but not sufficient. To raise belief b to the level of rationality R, that is, above irrationality or absurdity, ~ R, e is necessary but not sufficient or adequate. Unless some quantitative value, q, can be assigned to e, it is difficult to define (the rule of) AC of h and R of b. Any value of q in between 1 and 0.5 may be regarded as high and in between 0.5 and 0 as low.

To say this is not to clarify the issue very satisfactorily. Unless the domain of the discourse is specified it may be misleading to state that hypothesis h is justified, supported, or confirmed by the evidence e. If the elements of a domain, the telephone system of Tokyo, for example, are known to be stably related and their behavior highly predictable, to state with reference to that domain that out of 100
local telephone calls made 75 go through is to assign low probability or credibility (Cred) to the system. But knowing as we do the poor functioning of the telephone system of Calcutta, if we say the same thing in the form, \( P(h,e) = \frac{3}{5} \), informed people are likely to give high \( P \) or \( \text{Cred} \) to the system. For our experience is that out of 100 local telephone calls we make not more than 50 (on average) go through. This is because of bad maintenance or, during the rainy season, water-logging.

The parenthesized words “on average” brings to light another problematic aspect of the domain. It is possible that some lucky people of a particular area (say, exchange 75) find that out of 100 calls they make more than 70 go through. One can take the “subdomain” of the lucky Calcutta Telephone subscribers (of exchange 75) as an isolated domain. Against the known inductive (background) history of the Calcutta telephone system, one has to admit two things. First, with specific reference to the new isolated system the subscribers must not be treated as lucky. Because their short inductive background, unlike the long inductive background of the subscribers of the Calcutta telephone system as a whole, is different, the difference brought about by new and imported technology and improved functioning of the lucky system. If the lucky system is segregated from the not-so-lucky (“average”) system, then the expected outcome changes considerably. Second, there may be some very unlucky groups living in some old (exchange) areas whose experience, quantitatively speaking, is extremely disappointing. Let us suppose that out of 100 calls they make not more than 25 go through. If the systems of those unlucky groups are also treated as isolated, then the probabilistic picture changes in the opposite direction. So, in calculating the weighted average one has to be clear about the types of the systems, isolated or overlapping (including the extent of overlap), the principles of differentiating the areas, exchanges, or groups, and also the principles of aggregating them statistically.

The whole domain of the discourse may be changed if the performance of the Calcutta telephone system and that of the Tokyo telephone system are taken together, ignoring, for the time being, the different groups or subsystems within them. It is not easy to determine the \( \text{Cred} \) of the conjoined or newly unified system.

The rating of the \( \text{Cred} \) of the systems becomes more uncertain if a new variable, \( t \) or time factor, is introduced. For example, during the monsoon season and as a result of frequent heavy downpours and the resulting waterlogging in Calcutta, many of its telephones go dead every year. Naturally the performance of the Calcutta telephone system as a whole and that of its subsystems, taken either
exclusively or inclusively, goes down at a particular period of time during the year.

The problematic character of the matter may be put in an acute form in another way. Let us suppose (1) DPC is a telephone subscriber in the 44 exchange area and (2) 95 percent of the telephone lines in that area have been dead for the last six months, and, therefore, (3) almost certainly DPC’s telephone line is dead. Now let us suppose (1a) DPC is a member of Parliament (MP) and (2a) less than 5 percent of the telephones lines of those who are MPs are dead, and, therefore, (3a), almost certainly, DPC’s telephone line is not dead. Let us suppose, in addition, the premises of both the arguments are true. But even then, their conclusions turn out to be inconsistent.¹

In this way it may be shown that the more the variables are introduced in the system, the more uncertain, if not downright inconsistent, becomes the outcome of its functioning. But it may be added here that if the values of the added variables and their mutual relationships are definitely known, then the elements of uncertainty can be substantially reduced. Because then it becomes a matter of correct calculation. But even then, if the range and magnitude of the calculable values of the variables are extraordinarily large or minute, calculation or computation itself turns out to be a contributory factor to uncertainty. Besides, “the definitely knownness” condition is arbitrary, unrealistic, and in a way begs the very issue under dispute.

The relativity of the domain of the discourse may be highlighted by introducing a new domain, say, of the survival rates of the pancreatic cancer patients. Because of the known high rate of mortality among the pancreatic cancer patients if one says at some future time, $t_2$, “out of 100 patients the lives of 40 have been saved,” our reaction now (at time $t_1$) would be “it is very good; that is, the probability is pretty high.”

The first point I am trying to make out by highlighting the domain-relative character of probability is the inadequacy of the Bayesian assumption of equal and a priori distribution of probability values over the different elements (or states of affairs) of the domain. Second, the probability rating of the hypothesis is not only domain relative and evidence relative but the weights of the evidence themselves are also domain sensitive in an indirect but significant way. I am aware that the concepts like “evidence relative” are intuitive and, therefore, cannot be given quantitative formulation. But this is precisely one of my arguments against the simple Bayesian approach and in favor of the epistemological one. In the wake of the latter approach comes up the relevance of the concept of knower.
In this inductive context the concept of learner seems to be more appropriate. In deductive logic or procedures to explain new concept formation, it is assumed that the decision maker (person) is not handicapped by imperfection or limitation (vis-à-vis information). But to make an inductive inference one must bear in mind the limited epistemic capacity of human beings. On the basis of a small number of samples it is not at all easy for one to solve complex and massive combinatorial problems of induction. It has been rightly pointed out that even the biggest conceivable computers cannot go through, say, $2^{210}$ possibilities and that, consequently, to make a realistic and rational decision, in respect to winning war, game, lottery, and so on, to phenomena of complex kind(s), is impossible for beings like ourselves.\(^2\)

Third, my attention is directed primarily toward the epistemological problems arising out of finite powers of our memory and computational ability. Given this constraint, how do we rationally or justifiably come to expand what is available to us through experience? How do we systematize what we already have had in a presystematic manner? Rationality of inductive behavior and procedures have their descriptive as well as normative aspects. Our inferential ability is constantly being put to both cognitive and non-cognitive uses.

Unreliability, incoherence, and inconsistency associated with induction make it very difficult for one to formulate suitably the program for justification of induction. At the same time, one finds it extremely difficult and counterintuitive to deny the necessity of what we regard as nondemonstrative inference for ampliation of our information and learning new things and new relations between already known things. It has been rightly observed that "in the philosophical study of induction, no task is of greater importance than that of giving a clear characterisation of inductive procedures: only when this has been done can the problem of justification [of induction] significantly be raised."\(^3\)

3. Induction, Probability Calculation, and Rationality of Belief and Decision

The procedures known as inductive are numerous; viz., procedures pertaining to decision, acceptance, utility maximization, and learning. But, for the purpose of my study I propose to regard these different procedures as different forms of expansion. It is to be remembered that some writers like Carnap and Jeffrey are basically interested in explicating rules of inductive thinking within a static
framework, whereas others like Popper and Lakatos basically are interested in explicating the parameters of decision making in the realm of competing hypotheses. The latter’s framework is not static. And they are not willing to consider their logic as inductive.

Decision theory, as ordinarily understood, involves the concepts of utility and probability. Carnap understands probability as degree of belief. To start with, he takes it as a psychological concept in a descriptive sense, concerned with actual beliefs of actual human beings. Subsequently he addresses himself to the issue of normative decision theory by introducing some conditions of rationality. In its initial phase his conception of probability appears psychological or personalist. But, according to his own admission, he is interested mainly in a logical concept of probability.

In real life we have to make a choice among possible acts at a given time. We know the possible states of nature that are relevant to our acts. But ordinarily we do not know which of the possible states actually obtains. However, as the number of possible acts and possible states are finite, we can make a decision about a particular possible act in relation to a particular possible state of nature and also calculate the outcome of that decision. This outcome is determined by the concerned state of nature and the concerned act performed. It will also be reasonable to assume that we know the expected utility of the outcome of our act before we perform it.

Carnap’s view is undoubtedly Bayesian in inspiration but that it has important differences from the Bayesian rule of decision making should not be lost sight of. According to Bayes, we are advised, under normal conditions, to make a decision or choose an act that could maximize the probability value of the outcome. This rule may be interpreted neutrally or psychologically and rationally or normatively. Corresponding to the interpretation we arrive at different theories, descriptive decision theory, and normative decision theory.

Once the ambiguity of the concept of probability is removed and the distinction between subjective or personal probability and the statistical or objective probability is made, the difference between the descriptive theory and the normative theory becomes clearer. The concept of personal probability confers on a proposition or event a particular value representing a person’s state of belief. This concept of personal probability has its two versions: (1) expressing the actual degree of belief, and (2) expressing the rational degree of belief.

The moot point to be decided in this connection is the relative importance of the preceding two concepts of probability, the statistical and the personal, in determining the (probability) value of the
decision principle. Understandably, mathematical statisticians prefer the statistical concept of probability, which makes no reference to the psychological belief or knowledge of this or that particular person. The numerical values of statistical probability are not cognitively available to the concerned person, the decision maker. On this ground some decision theorists are opposed to the concept. They favor the personal concept of probability. Carnap seems to endorse this view. However, this is not to deny altogether the importance of the statistical probability in some empirical disciplines and particularly in the context of decision principle.

At this stage we are required to draw an important line of distinction between actual decisions and rational decisions. The laws governing the actual degree of psychological belief are obviously empirical in character. To find out these laws one has to look into the actual behavior of belief under uncertain situations like betting and playing games. Sometimes to denote this psychological concept a technical term, degree of credence, or, in short, Cred, has been introduced. Different persons may entertain different degrees of credence. Others may have different kinds of credence. The betting behavior of a person shows his or her credence function. Measuring the credence function at the psychological level, it is needless to say, is extremely difficult. The same may be said of one's utility functions. It expresses the system of valuation and preference of the concerned person. It represents not only one's beliefs but also the strength with which the beliefs are entertained and likely to be acted upon. Even if utility and credence functions of one or two persons can be determined with reasonable certainty, it is not easy to expand the same to a larger group, especially when the composition of the group is heterogenous and the defining characteristics of the group are inexactely defined.4

Carnap's move from descriptive to normative decision theory, concerned with rational credence, is intended to link up descriptive decision theory with inductive logic. In different ways the works of Keynes, Ramsey, Savage, and de Finetti have clarified the concept of rational decision. But Carnap is opposed to the idea of characterizing personal probability as subjective. For subjectivity and rationality hardly go together in the context of logical probability. In this connection he rightly recalls de Finetti's observation that "probability theory is not an attempt to describe actual behaviour ... [but] ... coherent behaviour" and Keynes's explicit assertion that "probability is not subjective ... once the facts are given which determine our knowledge, what is probable or improbable in these circumstances has been fixed objectively."5
The theory of *rational* credence or utility is established not by experiments but by requirements of rationality. On the question, What exactly are rational requirements? writers are not unanimous. Though at times the (Kantian) paradigm of complete rationality has been mooted, it is not generally favored. Whose credence function, except that of an all-knowing God, could possibly be perfectly rational? The realistic rationality requirements of credence function are (1) it must be *coherent* and (2) *strictly coherent*. Taking the time-factor into account (3) the third requirement of rationality may be indicated in terms of *information* or empirical data available to the decision maker at the interval between two moments, say, $t_1$ and $t_2$ of his or her life. *Information* may be taken exclusively (that is, as information only) or inclusively (that is, together with emotional and volitional factors). The third requirement (that is, information added between the concerned time interval) must satisfy the second requirement of *strict coherence*: it must be consistent with the information already available to the decision maker.

To lay the foundation of what he calls *inductive logic* Carnap introduces some simplifying concepts. Unlike other personal probabilists like Ramsey, de Finetti, and Savage, Carnap accepts initial credence and credibility for the purpose of constructing subjective probability and rejects the concept of “adult disposition” as a basic concept. Knowing fully well that his preferred concepts are “less realistic and remoter from overt behaviour” of the adult human beings he sticks to them apparently for two reasons. First, his credibility function satisfies the stronger requirements of rationality. Second, his concepts that are in consonance with this stronger requirements of rationality represent for him “permanent dispositions” of the normative type. This approach is analogous to that of the theoretical physicist who is concerned less with observable properties of objects and more with abstract properties of the same. Carnap’s preference for “permanent dispositions” over temporary “mental inclinations” is obviously normatively motivated; that is, appropriate for the purpose of explicating utilities and acts of rational human beings. A person’s belief system can hardly be judged fairly unless mental impulses and stray actions are not ignored. Positively speaking, to ascertain the rationality of someone’s beliefs we have to concentrate on those beliefs, their contents, the evidential base of their confirmation, and how they are spread over different times of the concerned person’s life.

In addition to *credence function* and *permanent disposition* the other two concepts crucial to Carnap’s inductive logic are *measure*
function and confirmation function. The credibility function of a person is deemed to be perfectly rational and backed up by an unfailing memory. For a logical function corresponding to credence Carnap uses (inductive) measure function; and for a logical function corresponding to credibility he uses (inductive) confirmation function. Given evidence \( e \), a hypothesis \( h \) is said to be confirmed to a particular degree of belief, say, \( r \). The measure function \( (M\text{-function}) \) or confirmation function \( (C\text{-function}) \) is defined in a purely logical or set-theoretic manner. The basic actions of inductive logic for \( M\)-functions are the usual axioms of the calculus of probability. This satisfies the requirements of coherence, regularity, and so forth. The other axioms mentioned by Carnap in his inductive logic are those of invariance. Also, he speaks of the axiom of symmetry. It falls under axioms of invariance. The latter remind one of the classical principle of indifference. All axioms of inductive logic are intended to state relations among the values of \( M\)-functions or \( C\)-functions. Carnap's inductive logic, as he keeps on reminding us, is pure and not applied. Yet his insistence on using the term inductive is noteworthy. He justifies it by saying that "this theory provides the foundation for inductive reasoning (in a wide sense)."  


Regarding the very nature of inductive logic or reasoning, there is a lot of controversy. In this respect Carnap rejects the view that inductive reasoning is an inference from some known proposition, premise, or evidence, to a new proposition or conclusion that may be a law or a singular prediction. This sort of reasoning is resorted to, according to the view in question, for acceptance or rejection of the new proposition. If we accept this view, Carnap feels we would not be able to refute Hume's position that induction has no rational grounds. To him, the paradigmatic piece of inductive reasoning concerning hypothesis \( h \) starts from some given evidence \( e \) and consists in assigning a probability value to \( h \). Its schematic form is \( c(h, e) \). The term \( c \) stands for degree of confirmation of \( h \) in the light of evidence \( e \). According to this formulation of inductive reasoning, the relation between \( c, h, \) and \( e \) is analytic. The task of the inductive logician, contrary to widespread belief, is not to infer the unknown on the basis of the known. His task is to calculate the probability value or degree of confirmation of \( h \) in terms of \( e \), whatever the values of \( h \) and \( e \) may be. Carnap's formulation of inductive logic seems to have lumped
up three different things: (1) how \( h \), given \( e \), is confirmed to the extent of \( p \); (2) how to draw the distinction between the choice procedure of \( h \)—whether a factual statement, a general hypothesis, or a particular prediction; and (3) how to decide the rationality or otherwise of a course of action \( a \), given \( e \), or a certain set of circumstances. Some writers are understandably opposed to this restrictive or analytic construal of inductive reasoning, which, they feel, ignores some very important distinctions between its different types. They are of the view that the question of acceptance, relatively ignored by Carnap, merits serious consideration.

The critics of the Carnapian system that I have in mind are Kyburg, Levi, Salmon, and Suppes. My main difficulty with Carnap centers around his analytic view of inductive reasoning. If one accepts this view, one cannot, I feel, find faults with the system of inductive logic that Carnap constructs systematically and step by step on the basis of that analytic view of inductive reasoning. Of course, by his own admission, it is not a very “powerful” system; nor has it been completed.

The main reasons for my uneasiness are briefly indicated later. In spelling out my reasons on some points I find myself in agreement or at least partial sympathy with the previously mentioned writers.

First, most of us, from the person in the street to the working scientist, are obliged to infer, explicitly or implicitly, something new on the basis of information already at our disposal. Even the radical anti-inductivists like Popper do not deny that for different needs, practical and theoretical, biological to epistemological, we try to transcend the bounds of the available information and discover new horizons of knowledge in terms of bold conjectures. Strictly speaking, Carnap himself does not deny this truism; but as a philosopher and logician the basic concept of inductive reasoning he has chosen for axiomatization is analytic, counterintuitive and departs both from common sense and science in some very important respects.

Second, if we are indifferent to rules of acceptance, it proves very difficult for us to explain how we learn from various sorts of experience and why we consult experts; for example, lawyers, medical practitioners, travel guides, in our day-to-day life. We draw upon others’ experience, expertise, and skill, not only for expanding our cognitive universe but also to make our life more joyful and relatively pain free.

Third, without these rules of acceptance and rejection the higher reaches of our theoretic life prove elusive. To cite but a few cases. (1) The particular things we experience for the first time do not
immediately appear reliable to us. Why, ordinarily, do we not rely upon strangers? We attach importance to repetition, familiarity, and confirmation or certification by others. (2) On the simple basis of direct reports of sense-experience we cannot accept or reject abstract and comprehensive views like Copernican hypothesis, the Harrod-Domar model of economic growth, EPR Paradox, or even “All emeralds are green.” Nor on the exclusive basis, a basis not endorsed by others, of my falsifying (evidential) experience a well-established theory can be discarded. (3) If we do not accept rules we will not be able to draw a cut-off line between observational statements and theoretical statements.

Finally, acceptance rules have their nonepistemic utilities. In pursuit of a moral life one needs rules for accepting certain norms of values. Why should one do one’s duty? Why should one be just? Certainly these questions and others of this sort, though in a sense nonepistemic, deserve answers based on some rules of acceptance. Unless one does one’s duty one cannot rationally expect others to do the same. We may or may not accept Kant’s Categorical Imperative but as a rule it is imperative that we have one or another such normative principle to guide us in our ethical decision making. Otherwise our civil society is bound to break down. To sustain and promote a civil society is more or less universally recognized as a normative requirement. It may be recalled here that, according to one interpretation, even the Categorical Imperative of Kant is a utilitarian principle of distribution; that is, it prescribes that all rational human beings must have equal access to primary goods like freedom.

Rawls’s principles of difference may also be regarded as rules of acceptance in the context of alternative theories regarding just society. The point I am emphasizing is simple: the rules of acceptance or what Kyburg calls the rules of detachment are important not only in epistemic or theoretical affairs but also in nonepistemic, utilitarian, or practical affairs. Strictly speaking, one might even plausibly argue against the dichotomy between practical reason and theoretical reason, between epistemic utilities and nonepistemic utilities.

Some strong defenders of the Carnapian position like Bar-Hillel are of the view that rules of acceptance are irrelevant. According to their understanding, the rule of this type simply says that under such and such circumstances one can rationally accept \( h \). Carnap clearly states that “rules of acceptance as ultimate rules for inductive reasoning are inadequate . . . [because they] give in some respect too much, in another respect too little.” Clarifying the point Carnap states that if the total evidence \( e \) available to a person \( X \) is
adequate for the acceptance of $h$, it amounts to saying that for that person $h$ is true. In other words, if $e$ entails $h$, then by logic alone $X$ could take the rational decision under a given set of circumstances. But that sounds counterintuitive. For, some other practical considerations enter into the picture; for example, the concerned person’s varying nonepistemic utilities.

Carnap also tries to show how the traditional rules of acceptance are of no use to a person in making a decision under certain circumstances. If two possible courses of actions, under identical circumstances, are equiprobable or nearly equiprobable, the ordinary rules of acceptance will not be of much help to the concerned person in choosing his actual course of action. However, he concedes that “sometimes rules of acceptance may be useful.” But the position taken by Bar-Hillel is apparently uncompromising. He says: “We do need further development of inductive logic, and further investigation into scientific methodology, in particular into the comparability of scientific theories, but nobody needs rules of acceptance of any kind.” But one wonders how Bar-Hillel reconciles the desirability of further development of inductive logic and further investigation into scientific methodology, on the one hand, and uselessness of rules of acceptance of any kind, on the other. In fact, Carnap himself shows in his own way how inductive logic is of help in taking practical decision under some specific uncertain situations. Even if the highly idealized concept of total evidence is weakened by disclosure or dispensed with altogether, it is difficult to deny the relation, positive or negative, justificatory or falsificatory, between $h$ and $e$. The only logical requirement to be insisted on is that $e$ must be relevant to $h$. Bar-Hillel’s strongly negative position is clearly inconsistent with his own concession made to some of the rules of acceptance enumerated by Kyburg. He cannot deny that the estimation of probability of $h$ has to be relativized to a corpus of evidential statements. Nor does he deny the importance of the rule expressed in terms of the information content of $h$, relativized to a corpus of evidential statements satisfying some principle of acceptability and also a condition of consistent totality. Equally understandable is his agreement with Kyburg that the simplicity and the fruitfulness of $h$ is, as a matter of fact, recognized by working scientists as a rule of acceptance. It is baffling to see that having conceded this much Bar-Hillel should still underestimate the significance of these rules of acceptance.

I find his denigration of the concept of simplicity totally unwarranted. It is true that this concept has been differently interpreted, but nobody denies its importance in the context of theory choice. It would be wrong to suggest that scientists or methodologists
of science do not need any rule of acceptance. Both the person of common sense and the scientist do many things and follow many methodological rules without being aware of them. The working scientist, as we often find, is not necessarily his or her best philosopher. For example, many good scientists firmly believe that their method is inductive and that for the relevance of deduction one must turn to mathematics. The expressions like "the covering law model," "hypothetico-deductive model," and "deductive-nomological model" sound rather strange to their ears. Obviously this does not mean either that, in the structure of scientific explanation, there is no inductive component in the form of certain laws or theories, or that, from generalizations alone, without referring to some particular statements of initial conditions, one can explain or predict the explanandum.

The main difficulty with Carnap's system is it would not concede any degree of confirmation to any general statement other than zero. But this does not disturb Carnap or his followers. For, their basic concern, it seems to me, is not structure of scientific explanation and analysis of its different components but the degree of confirmation of \( h \) in the light of \( e \) and to systematize its rules in a deductive and axiomatic form. The \( p \)-value of \( r \) in the formula \( p(h, e) = r \) is an analytic outcome of \( h \) and \( e \). The term \( e \) being deductively closed total evidence. To determine the quantitative probability of \( h \) thus is a matter of calculation or computation and not conjecture. Given this view of inductive reasoning, it is not at all surprising that some writers like Bar-Hillel and even Hintikka, for different reasons, find nothing very philosophically important in the discussion on the rules of acceptance.\(^9\)

Let us now forget about the weakness of Carnap's concept of confirmation function. Without rules of acceptance the problems of having a general hypothesis or law usable as a premise for deriving the explanandum (ex) remain. Without initial conditions (ic's) from the general premise with a degree of confirmation, \( c(h, e) = p \), the explanandum cannot be derived: ex cannot be derived either from (1) ic's and \( c(h, e) = p \); or from (2) ic's, \( c(h, e) = p \), and \( e \); or even from (3) ic's, \( c(h, e) = p \), and \( e \) (as total evidence). For none of these forms of argument represents a valid deductive schema. Though (4) \( e \) (as total true evidence) entails ex is a valid deductive schema, it is hardly explanatory. If true premises entail a conclusion, that is deduction but not explanation. Most of our explanations are of the (deductive) form (5) ic's ě ex. But without acceptance rules truth of ic's (5) cannot be rationally claimed.

Comparable, if not more acute, difficulties are encountered in
the inductive forms of explanation. The reason is obvious; viz., the structural similarity between the two forms of explanation. In both types of explanation one of the premises must be statistically general and the explanandum must be the conclusion of a logically valid inductive argument form. First, statistical generalizations, informative as they are, cannot be taken as certain. Naturally, the question of their acceptance is bound to come up. Second, because the explanandum is also informative and the form of inference is inductive, the acceptance of the questionable conclusion has to satisfy certain rules. Even if the rules of acceptance are followed, that does not ensure the certainty of the conclusion of inductive inference.

Frequentists like Reichenbach and Salmon turn to the acceptance rules because they are convinced that the Carnapian mode of inductive reasoning with the analytic degree of confirmation as its central concept has very little to offer us either in practical life or in theoretical pursuit. They readily concede that the Reichenbachian analytic estimate statements (estimate of values of limiting frequencies), too, like the Carnapian analytic degree of confirmation, fail to satisfactorily guide us in practical affairs. Because of this limitation they introduce factual truth claims and thus admit, indirectly, the need for acceptance rules. The need for the rules is obvious for those who interpret the frequency estimate statements as synthetic. Even the defenders of this interpretation are not dogmatically committed to the certainty or finality of their hypothesis. When one’s accepted estimate statement or hypothesis is found inaccurate in the light of additionally available observation statements, that is, larger and more diverse samples, it is appropriately corrected. This process may be repeated in an ascending order, to move progressively toward accuracy and correctness. I will revert to this point later on in the context of probabilistic justification of induction.

Neither the empiricist’s criticism of his analytic concept of inductive reasoning nor the frequentist’s stricture seems to have deeply disturbed Carnap. Even in his latest publications he claims that his credence function takes care of both “factual basis” and “statistical probability”—rendering thus the notions superfluous for such practical affairs of life as rational decision making. He goes even further and expresses the hope that if induction is properly reformulated (presumably along the lines he has suggested) the common person’s and the scientist’s faith in induction can be reconciled with Hume’s criticism of the same.

The old puzzle of induction consists in the following dilemma. On the one hand, we see that inductive reasoning is used by the
scientist and the man in the street ... and we have the feeling that it is valid and indispensable. On the other hand, once Hume awakens our intellectual conscience, we recognise that here is a serious difficulty. Who is right, the man of common sense or the critical philosopher? We see here, as so often, that both are partially right. Hume’s criticism of the customary forms of induction was correct. But still the basic idea of common sense thinking is vindicated: induction, if properly reformulated, can be shown to be valid by rational criteria.\(^\text{12}\)

Carnap’s optimistic project of reformulating inductive logic, as we know, has been painstakingly followed up by different writers like Jeffrey, Hintikka, and Hesse in different ways. Jeffrey maintains that the degrees of our belief in various types of propositions are determined in complex ways by habit and experience, use of language, and modes of training. In a way confirmation theory is semantic and reflects the meanings of the expressions of conditional subjective probabilities. Jeffrey dispenses with the Carnapian notion of total evidence and, therefore, his way of determining one’s credibility function differs from Carnap’s.

Hintikka is conscious of the difficulties involved in making a conditionalization program truly realistic. He turns his main attention to developing a system in which two different tasks, formulation of singular inductive inference and that of inductive generalization, are first differentiated and then arranged together into a two-dimensional continuum. One parameter of the continuum shows how some interesting inductive procedures regarding singular predictions can be described. Another parameter describes the character of one’s assumption about inductive generalization. The aim of Hintikka’s approach is clear. Within his system he wants to show the compatibility of (1) determination of degree of confirmation of a particular prediction or decision in relation to a corpus of evidence and (2) determination of informative content of “inductive” generalization. This is clearly a departure from Carnap’s \(\lambda\)-continuum dealing with the case \(a\) in which generalizations are accepted only on a priori grounds and, therefore, are informative only about singular predictions. This approach favors the Popperian view that, from among the competing hypotheses addressed to a particular problem, that one should be preferred which has high empirical content, more prohibitive power, and, therefore, in a sense is more falsifiable. Hintikka’s a priori probability is Popper’s absolute logical probability (in an atomistic universe). If the universe is infinite, the a priori probabilities all are zero. To have a posteriori probabilities above zero one
has to assume some finite atomistic universe (which can well be large in size). Hintikka’s \( \alpha \) parameter is essentially Popperian in spirit. The small \( \alpha \) is indicative of high falsifiability value simultaneously satisfying the requirement of acceptability.

Strictly speaking, the burden of Hesse’s main argument in support of Carnap is to show how inductive support from evidence is available for universal generalization. A positive relevance theory of confirmation and high prior probabilities extended to the conclusions of analogical argument clearly suggest that convincing inductive support can be obtained for generalization in a finite domain. The sort of skepticism associated with the view that scientific generalizations all are of zero confirmation value is wholly unwarranted. Hesse’s rescue operation is apparently confined to practical belief and finite domain. The sharp distinction that she draws between the problems of supporting universal generalizations and those of supporting individual predictions “derived” from them seems to be unrealistic. Besides, her relative indifference to the problems attending universal generalization is hardly in accord with the practice of the working scientist. In this respect she repeats the mistake of Carnap.

Personally speaking, I feel that Hintikka’s reconstruction of Carnap’s position is relatively more comprehensive and promising. But the point in which I find Carnap’s position most puzzling is this. Although he and his followers repeatedly speak of their concern with the problem of determining rationality of belief and consequential action, they are apparently not very mindful of the problems obtained at the down-to-earth level. Certainly I do not deny that in constructing a system of inductive logic one is obliged to resort to some degree of idealization. But ultimately the question of referring the idealized concepts back to the practical issues of life can hardly be denied. This is important both for theory itself and also for testing its material adequacy. At the practical level we are bothered not only with the question of determining the degree of rationality of our beliefs but also how they undergo change in the light of new information, expected as well as unexpected. Besides, the problems of our finite memory and forming new concepts necessary to deal with new information are equally serious. Suppes has rightly drawn our attention to these aspects of a theory of human rationality. “A theory of rationality that does not take account of the specific human powers and limitations of attention, memory and conceptualisation may have interesting things to say but not about human rationality.”

By refusing to take into account the limitations of human
powers, cognitive and noncognitive, we try to avoid the problems raised by the skeptics and the relativists. This sophisticated manoeuvre of avoidance impoverishes our theory of rationality and fails to reach the problems at all levels.

5. Noninductive Rationality of Science: Popper on Basic Statements

When we use some words, such as *rationality* and *induction*, we should be clear in our mind in what precise sense we are using them. Our brief survey of the concerned views shows that belief may be taken in its very ordinary psychological sense. If I see, as I am seeing now, a paperweight on my writing desk, I must believe in its existence. Ordinarily neither I nor anybody near me with normal vision is likely to ask me why I do believe in the existence of this paperweight. This sort of object of perception does not ordinarily raise the question of rationality of the belief in its existence. To say, for example, "I see it and therefore I believe in its existence" is likely to be accepted as a good enough ground or reason for my belief. Even the argumentative form of the sentence containing *therefore* may appear quite superfluous. Many people will feel that in such situations I have good enough reason to be certain about it, the existence of the paperweight before me, and the question of offering reason in support or to establish the belief does not arise at all.

But then why do we raise the issue of "reason," "ground," or "support" in this context? The answer to the question seems to lie in remembering the obvious distinction, too obvious to remember all the time, between the *psychological* sense of certainty and the *linguistic* mode of presenting it to others. As we do not believe in any private language, even in the case of the first person, in this case myself, the distinction between the psychological sense and the linguistic one needs to be drawn. Instead of drawing the distinction between the psychological sense and the linguistic sense we may perhaps reformulate it as the distinction between psychological *experience* and its *content*. Though experience is personal, its content, either through the explicit use of language or some other semiotic behavior, may be shared by, or communicated to, others.

The question of rationality of belief comes up when we move from psychology to cognitive psychology or epistemology. That some of our perceptual judgments, beliefs born out of perception, turn out to be more or less erroneous, at times even downright false, is hardly disputed. This partly explains why even for beliefs we are pressed to produce reason. If the concerned belief is sought to be defended in