Chapter 7: Can Human Irrationality Be Experimentally Demonstrated?

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Introduction

The experimental study of human rationality – that is, of validity in deductive or probabilistic reasoning – has become entangled during the past decade or so in a web of paradox. On the one hand, reputable investigators tell us that "certain psychological discoveries have bleak implications for human rationality" (Nisbett & Borgida 1975), or that "for anyone who would wish to view man as a reasonable intuitive statistician, such results are discouraging" (Kahneman & Tversky 1972b) or that "people systematically violate principles of decision-making when judging probabilities, making predictions, or otherwise attempting to cope with probabilistic tasks" and they "lack the correct programs for many important judgmental tasks" (Slovic, Fischhoff & Lichtenstein 1976). On the other hand, those investigators are reminded that people could not even drive automobiles unless they could assess uncertainties fairly accurately (Edwards 1975). The ordinary person is claimed to be prone to serious and systematic error in deductive reasoning, in judging probabilities, in correcting his biases, and in many other activities. Yet, from this apparently unpromising material – indeed, from the very same students who are the typical subjects of cognitive psychologists' experiments – sufficient cadres are recruited to maintain the sophisticated institutions of modern civilisation. Earlier decades, in an era of greater optimism, may well have overestimated the natural reasoning powers of human beings. But there seems now to be a risk of underestimating them.

What is needed here is a conceptual framework within which to think coherently about problems of cognitive rationality and the relevant experimental data, and the object of the present paper is to sketch such a framework. For this purpose it is necessary first of all to examine the credentials of those normative theories by reference to which investigators may legitimately evaluate the rationality or irrationality of a native subject's inference or probability judgment. Such a normative theory, I shall argue, is itself acceptable for the purpose only so far as it accords, at crucial points, with the evidence of untutored intuition. This thesis was also argued long ago by Goodman (1954, pp. 66–67). But the argument needs to be expanded and fortified against more recent opposition. What then follows from the thesis is that ordinary human reasoning – by which I mean the reasoning of adults who have not been systematically educated in any branch of logic or probability theory – cannot be held to be faultily programmed: it sets its own standards. Of course, various kinds of mistakes are frequently made in human reasoning, both by laboratory subjects and in ordinary life. But in all such cases some malfunction of an information-processing mechanism has to be inferred, and its explanation sought. In other words, the nature of the problem constrains us to a competence-performance distinction. Our fellow humans have to be attributed a competence for reasoning validly, and this provides the

backcloth against which we can study defects in their actual performance. That is the theme to be argued in the first part of the paper.

At the same time, allegations of defects in performance need to be carefully scrutinised. Some of these allegations are correct and important. But others seem to arise from a misapplication or misconception of the relevant standards of rationality by which the experimentally revealed phenomena should be judged, even when those phenomena themselves are quite robust and incontestable. The second part of the paper will therefore suggest four categories to which a critical assessment of existing allegations of performance defects might appropriately assign them.

In sum, those who once tended to exaggerate human reasoning powers may be construed as having concentrated their attention too much on the facts of competence, while those who have more recently tended to underestimate these powers have concentrated their attention too much on the facts of performance, and in some cases have judged these facts too harshly.

I. The Argument for Rational Competence

1. Intuitions as the Basis of Normative Criteria for the Evaluation of Deductions

Investigators who wish to evaluate the validity of their subjects' deductions would turn naturally to some educationally well regarded textbook of formal logic, such as Quine (1952), Copi (1954). or Lemmon (1965). The assumption would be that all and only the rules of inference that are given or derivable in those systems of so-called natural deduction are valid principles of deducibility, so far as deducibility hinges on the interplay of the logical particles "not," "and," "or," "if," "some," and "every" (or their equivalents in French, German, or any other language). But how can an assumption of this kind be defended? I shall argue that at a crucial point it has to rely on ordinary people's intuitions of deducibility.

Note, however, that the term "intuition" here is not being used in the sense of Spinoza (1914), Bergson (1903), or Husserl (1911). It does not describe a cognitive act that is somehow superior to sensory perception. Nor, on the other hand, does it refer merely to hunches that are subsequently checkable by sensory perception or by calculation. Nor does this kind of intuition entail introspection,[1] since it may just be implicit in a spoken judgment. Its closest analogue is an intuition of grammatical well-formedness. In short, an intuition that p is here just an immediate and untutored inclination, without evidence or inference, to judge that p.

To avoid any reliance on intuition in that sense, it would be necessary to show that the assumption in question (about the nature of deducibility) is defensible within some well-recognised system of scientific procedure. Transcendental (that is, Kantian) arguments are obviously too controversial for the purpose. So either this procedure has to be empirically based and inductive, or it has to depend on some appropriate metamathematical theorem. But, as it turns out, neither of these strategies can wholly succeed: at crucial nodes an appeal to ordinary people's intuition is indispensable.

The empirical-inductive strategy offers us an account of logic (as in Stich 1975) in which it is viewed as an adjunct to science in general rather than, like geometry, an adjunct to physics in particular: Such an applied logic is understood as the combination of a formal system with appropriate interpretative rules; and it is to be tested, we are told, by assessing the explanatory and predictive power of the total theory that results from meshing it with the theories of the several sciences. In this way, it seems, what are accepted as logical truths turn out just to constitute one type of component in the total holistic system of what is accepted as scientific truth. They seem as much beholden to experiment and observation for the warranty as are any other scientific discoveries.

However, this kind of hard-line positivism comes up against some serious difficulties, which preclude it from supplying an intuition-free validation of deductive logic. First, certain regulative principles for theory construction, such as ideals of comprehensiveness, consistency, and simplicity, have in any case to be granted a priori status, so that in the defence of this status at least some principles of reasoning may be conceded an intuitive warranty. Second, much of the reasoning for which we need a logically articulate reconstruction does not take place in science at all but in law or administration, and is concerned not with what is in fact the case but with what ought to be. Third, the same logical principles have to be applied within each piece of scientific reasoning about the relative merits of two or more hypotheses, so that if ever any hypothesis has to be given up in the face of adverse experience it is always a factual, rather than a logical, one. For example, we cannot claim, as does

Reichenbach (1944), that quantum physics constitutes a restriction on the range of application of classical two-valued logic as well as of classical mechanics because it is only in accordance with shared logical principles that it would be fair to elicit and compare the differing experimental conseqences of classical mechanics and quantum theory.[2] Hence, so far as we treat the totality of acceptable scientific hypotheses as constituting a single holistic system, we also need a single set of logical principles. Fourth, logically true statements are statements that are true in all logically possible worlds, and the evidence of happenings in the actual world must thus fall far short of establishing them.

Moreover, so far as the epistemology of a particular discipline is obligated to endorse the criteria of evaluation that are generally accepted in practice by reputable investigators in the field, it is certainly the appeal to intuitions that deserves endorsement for applied logic rather than the empirical-inductive strategy. Applied logicians make no attempt at all to test out the theories empirically within the context of a project to the holistic systematisation of all knowledge. On all the issues that are much discussed – issues about modality (for example, Quine 1960), subjunctive conditionals (Lewis 1973), indirect discourse (Carnap 1947), relative identity (Griffin 1977), proper names (Kripke 1972), adverbs (Davidson 1966), and so on – an implied or explicit appeal to intuition provides some of the vital premises for the applied logician's argument.

Nor are the prospects for a metamathematical justification of applied logic any better than those for an empirical-inductive one. Any system in which rules of derivation are specified in formal terms is said to be "sound" if under some interpretation for the formalism of the system it can be proved that from true premises these rules lead only to true conclusions. So it might seem as though, by thus using a semantic definition of logical consequence to check on a syntactic one, the rationality of a set of inferential rules could be established by experts in a metamathematical proof, without any recourse to intuitions other than those involved in the perception of the proof (Dummett 1978). But, though such a strategy has an agreeably professional appeal, it does not come to grips with the whole of the underlying epistemological problem. No reason is provided for supposing that the deductive liaisons of the logical particles of natural language can be mapped onto those of the connectives and quantifiers in the normal system that is proved to be sound.

For example, in any natural deduction system for the classical calculus of propositions the formula

$$((A \rightarrow B)\&(C \rightarrow D))$$

(or a notational variant) can constitute a premise from which

$$((A \rightarrow D)V(C \rightarrow B))$$

(or a notational variant) is derivable. And under the interpretation that Russell (1919) proposed for this calculus, a derivation could turn into an inference from

> If John's automobile is a Mini, John is poor, and if John's automobile is a Rolls, John is rich

to

> Either, if John's automobile is a Mini, John is rich, or, if John's automobile is a Rolls, John is poor

which would obviously be invalid. But what makes this invalidity obvious? The fact is that our own intuitions about the legitimate deductive liaisons of the logical particles (for example, the intuition that from the conditional "If John's automobile is a Mini, John is rich," we should be able to deduce the existence of a connection between antecedent and consequent that is independent of truth values) combine with our empirical knowledge of automobile costs to make it easy to imagine situations in which (3) is true and (4) is false. So though the propositional calculus is demonstrably sound, it resists Russell's interpretation as a logic of everyday reasoning in which conditional sentences may have a role, because it cannot capture intuitions like those on the basis of which we judge an inference from (3) to (4) to be invalid.[3] Admittedly, those intuitions might be said just to concern the *meanings* of the logical particles "if," "and," and "or," and there is nothing particularly remarkable, it might be objected, about the fact that one has to understand the meaning of an utterance to be able to appraise its validity. But the relevant point is that knowing the meanings of "if," "and," and "or" is indistinguishable from knowing, in principle, their legitimate deductive liaisons. So we cannot avoid appealing to intuitions of inferential validity in order to determine the claim of an interpreted formal system to constitute a theory of deducibility for everyday reasoning.

In other words, the problem of justification takes two rather different forms in regard to

theories of deducibility. On the one hand, there is the issue of the theory's soundness, on the other, the issue of its application. Intuitions of inferential validity supply data in relation to the latter issue, not the former. But these intuitions are nevertheless an indispensable type of evidence for any theory of deducibility in everyday reasoning. Unless we assume appropriate intuitions to be correct, we cannot take the normative theory of everyday reasoning that they support to be correct. No doubt two different people, or the same people on two different occasions, may sometimes have apparently conflicting intuitions. But such an apparent conflict always demands resolution. The people involved might come to recognise some tacit misunderstanding about the terms of the problem, so that there is no real conflict; or they might repudiate a previously robust intuition, perhaps as a result of becoming aware that an otherwise preferred solution has unacceptable implications; or they might conclude that different idiolects or conceptions of deducibility are at issue.[4]

2. Intuitions as the Basis of Normative Criteria for the Evaluation of Probability Judgments

The position in regard to normative theories of probabilistic reasoning is rather analogous. We can take the mathematical calculus of chance, as axiomatised by Kolmogorov (1950), Reichenbach (1949), or Popper (1959a), to be a formal system that is open to semantical interpretation as a theory of the constraints that probability judgments of certain kinds *ought* to place on one another. But to just what kinds of probability judgment does the theory apply? This question has been much discussed. For example, proofs or arguments are available (Ramsey 1931; de Finetti 1931) to show that where probabilities are measured by betting quotients within a suitably coherent system of wagers their system conforms to the calculus of chance. A similar conformity has been demonstrated by Reichenbach (1949) and von Mises (1957) for the conception of probability as a relative frequency; by Carnap (1950) for the conception of probability as a type of logical relation that varies in strength along a spectrum that extends from contradiction at one extreme to entailment at the other; by Popper (1959b, 1968) and Mellor (1971) for the conception of probability as a causal propensity – a causally rooted tendency – and so on.

But none of those proofs or arguments establishes which conceptions of probability are operative – and under what conditions – in the everyday reasoning of lay adults, such as are the typical subjects of experiments carried out by cognitive psychologists. That is to say, it is one thing to establish one or more probabilistic interpretations for the calculus of chance, and quite another to show that the resultant theory applies to some or all of the probability judgments that are made in everyday reasoning. In order to discover what criteria of probability are appropriate for the evaluation of lay reasoning we have to investigate what judgments of probability are intuitively acceptable to lay adults and what rational constraints these judgments are supposed to place on one another. We have to select the conception or conceptions of probability in terms of which the most coherent account of lay judgments can be given, rather than evaluate those judgments by some single independently established standard.

The importance of this selection should not be underestimated. There are at least four ways in which it can make a lot of difference.

First, where probabilities are measured by betting quotients or construed as logical relations, we have to say – properly speaking – that they are functions of propositions; where they are relative frequencies they are functions of sets; and where they are causal propensities, they are functions of properties (Cohen 1977b). Such categories of functions differ considerably in regard to their appropriateness for the evaluation of definite singular instances, as has often been pointed out (see Reichenbach 1949, pp. 376–77; Carnap 1950, pp. 226–28; Nagel 1939, pp. 60–75). They differ also in regard to their appropriateness for counterfactual inference (Cohen 1977b, pp. 306–9).

Second, where a probability is measured by a betting quotient, its statement is normally treated as an assertion about the strength of the speaker's belief in the outcome. Such a subjective fact is logically quite consistent with another speaker's having a different strength of belief in relation to the same issue. So when two people measure the probability of the same outcome subjectively by different betting quotients, they are not contradicting one another, whereas assertions of different relative frequencies, different logical relations, or different causal propensities would be logically inconsistent if they concerned the same issue.

Third, different probability functions may legitimately be assigned different values in relation to the same situation of uncertainty. Carnap, for example, demonstrated the existence of

a nondenumerably infinite number of different measures for his logical relation type of probability; the odds that are taken as appropriate to betting on a particular outcome on a given occasion need not correspond with the actual frequency of such outcomes in the relevant population; and the high frequency of B's among A's may be due to a series of coincidences, so that as a measure of causal propensity $p(B|A)$ [i.e., $p(B$ given $A)] = p(B)$ even though as a measure of relative frequency $p(B|A) > p(B)$. There is no mathematically demonstrable reason, therefore, why people should not in fact use different measures of probability for different situations or purposes, just as traders sometimes find it worthwhile to measure quantities of apples by weight and sometimes by number.

Fourth, if one or more semantic characterisations of probability are possible (as distinct from an implicit definition in terms of a set of mathematical axioms), then one might even need other formal systems than the calculus of chance to represent the syntax of some semantically defined categories of probability judgments other than the four already mentioned. The mathematics of probability may have no more reached its apogee in the work of Kolmogorov, than the mathematics of space did in that of Euclid. Nonclassical theories of probability may turn out to have an interest analogous to that of non-Euclidean geometries.

3. The Systematisation of Normative Intuitions

It has been argued so far that any normative analysis of everyday reasoning – any statement that such and such lay judgments of deducibility or probability are correct, or incorrect, as the case may be – must in the end rely for its defence on the evidence of relevant intuitions. You cannot dodge this by an appeal to textbooks of logic or statistics. Of course, on any issue that can be settled empirically we naturally treat intuitions only as hunches that either will be confirmed by favourable observation or will give way to counter-observations. And in some area, such as the grammar of natural language, the question whether ultimate data are observational or intuitive or both is currently controversial: compare Chomsky (1965) and Sampson (1975). But on indisputably normative issues – on issues about how people *may* or *ought to* think or behave as distinct from how they *do* – we cannot expect a major point at stake to be settled by observation. Here, if our aim is

to build up a comprehensive system of theory, it is prudent to check our general hypotheses against intuitions in concrete individual cases – though in order to avoid an obvious risk of bias, these must always be the intuitions of those who are not theorists themselves. For example, the practice of the courts provides much evidence for a theory of lay intuitions about probability in forensic reasoning (Cohen 1977b), but writers on this subject should not invoke their own intuitions.

Normative theories are subject to the usual inductive criteria. They are better supported if they apply to a wider rather than a narrower range of significantly different kinds of intuitive inference or judgment, just as the more comprehensively explanatory theories have greater merit in natural science. But there would obviously be a point at which even the mere process of putting problems to a person in varied contexts, in order to extract his intuitions, could reasonably be taken to cross over into a procedure for changing his normative outlook instead of just recording it. Thus recent writers on ethics (for example, Rawls 1972; Daniels 1979, 1980) have distinguished between the narrow reflective equilibrium that is constituted by coherent reconstruction of a person's existing moral principles, where only an occasional intuition is repudiated (for the sake of consistency), and the wide reflective equilibrium that is obtained when a person chooses between his existing moral principles and proposed alternatives, on the basis of sociological, historical, economic, psychological, or other considerations that may weigh with him. In matters of deducibility or probability the analogue of this philosophical choice would occur in the process of education research, or philosophising whereby hitherto uncommitted students are sometimes transformed into the thoroughgoing Quineians, say, or Bayesians, or Popperians so that they come to adopt substantially different conceptions of deducibility or probability from those once operating in their untutored judgments. But the normative theories that are at issue in the present context require a narrow, not a wide, reflective equilibrium. The judgments of everyday reasoning must be evaluated in their own terms and by their own standards.

4. The Derivation of an Account of Human Competence in Deductive or Probabilistic Reasoning

If a physicist observes the position of the needle on a certain dial under chosen experimental

conditions, then the datum to be explained is the position of the needle, not the fact that someone observes it. The event observed, not the act of observing it, is what is relevant. Otherwise optics (and perhaps acoustics) would be all the science that there is. Analogously, the datum that the moralist has to take into account is the rightness or wrongness of a particular action, not the deliverance of conscience that pronounces it right or wrong; and the logician's datum is the validity or invalidity of a particular inference, not the intuition that assures us of it. So enquiry into the norms of everyday reasoning no more aims at a theory *about* intuitions than physics or chemistry aims at a theory *about* observations. Epistemology does not dominate ontology here. And fortunately it is not necessary for present purposes to determine what exactly the study of moral value, probability, or deducibility has as its proper subject matter. For example, an applied logician's proper aim may be to limn the formal consequences of linguistic definitions (Ayer 1946), the most general features of reality (Quine 1960), or the structure of ideally rational belief systems (Ellis 1979). But, whatever the ontological concern of applied logicians, they have to draw their evidential data from intuitions in concrete, individual cases; and the same is true for investigations into the norms of everyday probabilistic reasoning.

It follows that for every such normative theory, which determines how it is proper to act or reason, there is room to construct a factual theory that does take intuitions as its subject matter. This factual theory will describe or predict the intuitive judgments that formulate the data for the corresponding normative theory. It will be a psychological theory, not a logical or ethical one. It will describe a competence that normal human beings have – an ability, uniformly operative under ideal conditions and often under others, to form intuitive judgments about particular instances of right or wrong, deducibility or nondeducibility, probability or improbability. This factual theory of competence will be just as idealised as the normative theory from which it derives. And though it is a contribution to the psychology of cognition it is a by-product of the logical or philosophical analysis of norms rather than something that experimentally oriented psychologists need to devote effort to constructing. It is not only all the theory of competence that is needed in its area. It is also all that is possible, since a different competence, if it actually existed, would just generate evidence that called for a revision of the corresponding normative theory.

In other words, where you accept that a normative theory has to be based ultimately on the data of human intuition, you are committed to the acceptance of human rationality as a matter of fact in that area, in the sense that it must be correct to ascribe to normal humans beings a cognitive competence – however often faulted in performance – that corresponds point by point with the normative theory. Of course, it would be different if you believed in some other source of normative authority. If, for instance, you believe in a divinely revealed ethics, you are entitled to think that some people's competence for moral judgment may fall short of correct moral ideals: you could consistently invoke some doctrine of original sin to account for the systematic failure of untaught intuition to accord with the correct norms of moral judgment. But, if you claim no special revelation in ethics, you will have to take intuitive judgments as your basis, and then people's competence for moral judgment – as distinct, of course, from their actual performance in this – cannot be faulted. Analogously, if you claim no special revelation in matters of logic or probability, you will have to be content there too to accept the inherent rationality of your fellow adults.

To ascribe a cognitive competence, in this sense, within a given community is to characterise the content of a culturally or genetically inherited ability which, under ideal conditions, every member of the community would exercise in appropriate circumstances. It states what people can do, rather than what they will do, much as the characterisation of a linguistic competence can be taken to describe what it is that native speakers must be assumed capable of recognising about the structure of morphophonemic strings (Chomsky 1965) rather than what they do actually recognise. The fact is that conditions are rarely, if ever, ideal for the exercise of such a competence. Just as passion or self-interest may warp our moral discernment, or memory limitations may restrict the length of the sentences we utter, so too a variety of factors may interfere with the excercise of a competence for deductive or probabilistic reasoning. A local unsuitability of childhood environment may inhibit the maturation of innate ability, education (that is, education in subjects other than logic and probability theory) may fail to make the most of it, individual disabilities or normal memory limitations may set limits to what even the best environment and education can achieve, and various motivational and other factors may operate to induce malfunctions of the relevant information processing mechanisms. To suppose that all

normal adults are able to reason deductively is certainly not to suppose that they will never err in their judgments of logical validity, and still less that they will in practice execute any particular finite chain of reasoning that is called for, however complex it may be, just so long as it is licensed by intuitively evident rules of natural deduction. In practice, our rationality is "bounded" (Simon 1957, pp. 198–202). We are all able to walk, if in normal health, but it does not follow that we can all walk on a tight rope, or for a thousand miles without stopping, or that none of us ever stumbles. It is here that the issues arise that will be discussed in the second part of this paper.

In short, accounts of human competence can be read off from the appropriate normative theories, so far as they are based on the evidence of intuitions; accounts of actual performance under different conditions are to be obtained by experiment and observation; and hypotheses about the structure and operation of human information-processing mechanisms must then be tested against the facts of competence and performance that it is their task to explain. The structure of design of such a mechanism must account for the relevant competence, but its operation must be subject to various causes of malfunction that will account for the flaws found in actual performance.

One may be tempted to ask: "How do we know that any intuition of the relevant kind is veridical?" But to ask for knowledge here is to ask for what is in principle impossible, at least in the sense in which knowledge is something like justified true belief, and where there is no alternative to invoking intuition, since an intuitive judgment that p essentially lacks any external ground to justify accepting that p. The best that normative theorists can hope for in this field (and also what they need to achieve), if they do not claim any special revelation, is that the contents of all relevant intuitions – suitably sifted or qualified, if necessary – can be made to corroborate one another by being exhibited as the consequences of a consistent and relatively simple set of rules or axioms that also sanctions other intuitively acceptable, but previously unremarked, patterns of reasoning. The inductive principle of mutual corroboration here is analogous to that operative in natural science, as Bacon long ago pointed out in regard to normative theories of ethics or jurisprudence (Kocher 1957; cf. Cohen 1970).

It would be different if we were evaluating the cognitive competence of some other species, or even of human children. We should be free to find their intuitive efforts at probabilistic reasoning, for example, or their moral sensitivity, to be rather inferior by the standard of our own norms. But we cannot attribute inferior rationality to those who are themselves among the canonical arbiters of rationality. Nothing can count as an error of reasoning among our fellow adults unless even the author of the error would, under ideal conditions, agree that it is an error.

Other arguments about rationality do not concern us here: It is true that, even where animals, children, or Martians are concerned, there are limits to the extent to which we can impute irrationality. As has been well remarked (Dennett 1979, p. 11), if the ascription of a belief or desire to a mouse is to have any predictive power, the mouse must be supposed to follow the rules of logic insofar as it acts in accordance with its beliefs and desires. But this is not to suppose that the mouse has any great powers of ratiocination. Equally (Quine 1960) we have to impute a familiar logicality to others if we are to suppose that we understand when they say: different logics for my idiolect and yours are not coherently supposable. But there is always the possibility that we understand less than we think we do and that some imputations of logicality are therefore not defensible on this score. Again, evolutionary pressure in the long run eliminates any species that is not sufficiently well equipped to surmount threats to its biological needs. But evolutionary considerations are better fitted to put an explanatory gloss on the extinction of a species after this event has already occurred than to predict the precise level of rationality that is required for this or that species' continued survival within its present environment.

What I have been arguing is that normative criteria for ordinary human reasoning rely for their substantiation on a procedure analogous to what is called "boot strapping" in artificial intelligence (see Dawes & Corrigan 1974). The intuitions of ordinary people are the basis for constructing a coherent system of rules and principles by which those same people can, if they so choose, reason much more extensively and accurately than they would otherwise do.[5] Consequently these ordinary people cannot be regarded as intrinsically irrational in regard to any such cognitive activity. An investigator who wanted to make out a serious case for deep-level human irrationality in this area might be tempted to operate with normative criteria that were the product of philosophical argument for some appropriately wide reflective

equilibrium and consequently differed from the narrow, bootstrapping, reflective equilibrium which merely reconciles intuitions. But any kind of scientific or mathematical reasoning to which such criteria directly apply has a specialised and technically regimented quality that makes it difficult or impracticable for those who have not been trained appropriately. For example, it may involve deduction within an artificial language system, or employ relatively sophisticated concepts of statistical theory. Hence the investigator's experiments would founder in characteristic indeterminacy. They would constitute an accurate test of their subjects' competence for reasoning only to the extent that these subjects were not ordinary people but specially trained experts. So the results of the test might reveal how good was the training or how effective were the procedures for selecting people to be trained; they would tell us nothing about the rationality or irrationality of untrained people. Though a person may well acquire a wide reflective equilibrium with regard to ethical issues that is inconsistent with a previously existing narrow reflective equilibrium, there is no possibility of an analogous inconsistency with regard to deducibility or probability. In the case of deducibility, narrow reflective equilibrium remains the ultimate framework of argument about the merits of other deductive systems, and in the case of probability, we are merely replacing some modes of measuring uncertainty by others.

II. Four Categories of Research into Defects of Cognitive Rationality

The past decade or so has seen the growth of a vast literature of psychological research into replicable defects of human reasoning. Often investigators are content just to argue for the existence of such defects and to suggest explanations. But sometimes they also claim justification for extensive criticisms of human rationality. Several convenient reviews of this literature are already available (for example, Slovic, Fischhoff & Lichtenstein 1977; Nisbett & Ross 1980; Einhorn & Hogarth 1981), and I do not aim to produce another here. Rather, the purpose of the second part of the paper is to establish four categories, into one or the other of which, on close assessment, any item in this literature may be seen capable of being assigned without "bleak implications for human rationality," once an account of the normative criteria for ordinary human

reasoning is agreed to entail an ascription of the corresponding competence to ordinary human adults (as argued in part I). For reasons that will emerge in the sequel, these four categories of research activity are appropriately entitled "Studies of cognitive illusions," "Tests of intelligence or education," "Misapplications of appropriate normative theory," and "Applications of inappropriate normative theory." Examples will be furnished for each category. It will be assumed in every case that the phenomena reported are replicable, and that no technical faults occur in the presentation of the data, such as miscalculations of statistical significance: if the examples furnished here are in fact faulty, others are easily found. The issues raised here do not concern the robustness of the phenomena, solely their interpretation.

The categorisation is intended to be an exhaustive one, not in the sense that every item in the literature is actually assigned to one of the four categories but that in principle it could be. Claims that human reasoning tends to be invalid in certain circumstances are either correct or incorrect. The correct claims relate either to fallacies that, on reflection, everyone would admit to be such, as in studies of cognitive illusions, or to fallacies that require some more elaborate mode of demonstration as in tests of intelligence or education; the incorrect claims result either from misapplications of appropriate normative theory or from applications of inappropriate normative theory.

1. Studies of Cognitive Illusions

In view of what has been argued above about ordinary people's competence for deductive and probabilistic reasoning, there is a prima facie presumption, in regard to any experimental data in this area, that they can be explained as a manifestation of some such competence, even though the details of the explanation may not be easy to fill in. Where no explanation of this kind is available, one possibility is that experimenters have created a cognitive illusion. They have manipulated the circumstances of a situation in such a way that subjects are induced to indulge in a form of reasoning that on a few moments' prompted reflection they would be willing to admit is invalid.

A very good example of this is the familiar four-card problem (Wason 1966). The subjects are presented with four laboratory cards showing, respectively, 'A,' 'D,' '4,' and '7,' and

know from previous experience that every card, of which these are a subset, has a letter on one side and a number on the other. They are then given this rule about the four cards in front of them: "If a card has a vowel on one side, then it has an even number on the other side." Next they are told: "Your task is to say which of the cards you need to turn over in order to find out whether the rule is true or false." The most frequent answers are "A and 4" and "only A," which are both wrong, while the right answer "A and 7" is given spontaneously by very few subjects.

Wason and his colleagues, in attempting to account for these data (see Johnson-Laird & Wason 1970), interpret the error as a bias towards seeking verification rather than falsification in testing the rule. But if that were the nature of the error one would expect "D" to show up in the answer like "4" does, since the contrapositive equivalent of the rule is, "If a card does not have an even number on one side, it does not have a vowel on the other." Perhaps it will be said that this is simply owing to a failure to grasp the equivalence of contrapositives here. But such a failure would account also for the absence of "7" from most answers, without the need to suppose that in testing a conditional rule subjects do anything other than check, in each case in which the antecedent holds true, whether the consequent does also: this is because the presence of "4" in many answers may then be put down to the prevalence of inference from an utterance that is of the form "if p then q" to an utterance that is of the form "if q then p" – a prevalence for which there is independent evidence (see II.3 in regard to the fallacy of illicit conversion). I shall assume, therefore, that the subjects' specific error here is best interpreted as a failure to apply the law of contraposition. What then causes that failure?

It would be wrong to conclude that the deductive competence of most logically untutored subjects does not embrace the law of contraposition. A subsequent experiment (Wason & Shapiro 1971) has been claimed to show that if the four cards are those related to a more concrete rule, namely, "Every time I go to Manchester, I go by train," then substantially more subjects are successful. Even better results were obtained (Johnson-Laird, Legrenzi & Sonino Legrenzi 1972) when the rule was, "If a letter is sealed, then it has a fivepenny stamp on it" and the laboratory cards were replaced by (sealed or unsealed, stamped or unstamped) envelopes.

Further experimentation (Van Duyne 1974, 1976) has also been claimed to show that degrees of realism in fact affect performance in a continuous, linear way. However, it looks as though we need to distinguish here between two different ways in which realism may be increased. One is by writing descriptive words or sentences on the cards, instead of just letters and numerals, and altering the rule accordingly: the other is by using real objects (envelopes) instead of cards. The results of Manktelow and Evans (1979) suggest that when realism is increased in the former manner the fallacy still occurs. But those results do not weaken the finding that when real objects replace cards the fallacy hardly ever occurs. It seems, therefore, that experimenters' power to generate an illusion here depends on the relative unfamiliarity and artificiality of their apparatus. In their familiar concrete concerns human beings show themselves well able to apply the law of contraposition to appropriate problems. Faced instead with a situation in which the items against which a conditional rule is to be checked are things (cards bearing letters, numerals, words, sentences, geometrical diagrams, and the like) that echo the symbolism in which the conditional rule itself is formulated, subjects' reasoning tends to be led astray in the "matching bias" to which Manktelow and Evans have traced the fallacy.

The point of describing experimental effects like Wason's as cognitive illusions is to invoke the analogy with visual illusions: it is in no way intended to derogate from their importance, nor to suggest that if the circumstances that cause the illusion occur naturally (as distinct from being the result of an experimenter's contrivance) then the illusion will not occur.[6] The discovery of any such effect in human performance generates a significant piece of evidence about the way in which the underlying information-processing mechanism operates. The findings about the four-card problem may legitimately be said (Johnson-Laird & Wason 1977) to support the view (Piaget 1972) that most people manage to apply their logical competence without ever formulating it expressly at a level of generality sufficient for it to be readily applicable to wholly unfamiliar tasks. People will distinguish form from content in their reasoning, or extrapolate accurately from one content to another, only to the extent that similarity of form is accompanied by some rough equality of vital interest. So subjects who reason fallaciously about the four-card problem need not

be supposed to lack the correct deductive "program." Indeed, none of the experimenters in the area suggests this. The subjects merely fail to recognize the similarity of their task to those familiar issues in which they have profited by using the deductive procedure of contraposition. As a result, either that procedure receives no input or its output is deleted, and the behaviour of the subjects manifests a matching bias.

Analogously, other experimental data show, it has been argued (Wason 1960, 1968), that in an abstract task, like hypothesising about the rule that generates a given series of numbers, most people are unable to use the procedure of proving a hypothesis by eliminating alternatives to it.[7] They tend to seek confirmatory evidence for their favoured hypothesis rather than disconfirmatory evidence for alternatives to it. In addition, they often do not relinquish hypotheses that have been shown to be false. Nor is people's eliminative performance substantially better when confronted with a computer-screen simulation of a simple mechanical problem (Mynatt, Doherty & Tweney 1977). Yet it hardly needs an experiment to show that most people are quite capable of using eliminative procedures correctly when dealing with real objects – not simulated ones – in familiar everyday situations: if the soap is not in the basin, we reason, it must be in the bath; if one's caller has not come by automobile, he must have walked; and so on. So it is not that ordinary people lack competence for the kind of deductive inference that moves from "p or q" and "not-p" to "q," which is essential to all such eliminative reasoning. It seems rather that in normal investigative situations the disjunctive premise for this pattern of reasoning is supplied by previous experience, and in an artificial or unfamiliar situation we lack the relevant kind of previous experience to supply the input. To build up that experience some pursuit of confirmatory (as distinct from disconfirmatory or eliminative) strategies would not be unreasonable, as is recognised by Mynatt, Doherty, and Tweney (1978, p. 405); and retention of a falsified hypothesis would even be desirable if it explained quite a lot of the evidence and no unfalsified hypothesis were available that had as good explanatory value. So too the sharply falsificationist model of scientific progress that was originally offered by Popper (1959a) has rightly met with substantial criticism from other historians and philosophers of science (for example, Swinburne 1964; Lakatos 1970).

Again, it may seem puzzling that subjects seem unable to judge correctly in the laboratory that some event is controlled by another, or is independent of it, as the case may be, and yet the very same subjects gets along all right most of the time in their everyday life (Jenkins & Ward 1965). But the puzzle may be lessened by considering some of the ways in which their expert mental tasks are not representative of the normal conditions for such judgments: checkups are excluded, temporal variations are absent, the output considered unnaturally discrete, the response has to be a relatively hurried one, and so on.

Experimenters need to devise a great variety of experiments involving such cognitive illusions in order to test out theories about how human beings in fact perform, or fail to perform, the various acts of reasoning for which they apparently possess a competence. But one should recognise these experiments for what they are, and not conceive of the illusions that they generate as some kind of positive, though fallacious, heuristic that is employed by the subjects. Consider for example, the supposed heuristic of availability (Tversky & Kahneman 1973). A person is said to employ this heuristic whenever he estimates frequency or probability by the ease with which instances or associations are brought to mind. For example, if student subjects, when asked in the laboratory, erroneously judge English words beginning with *re* to be more frequent than words ending with *re*, they are diagnosed to have employed the heuristic of availability because the former words are more easily brought to mind than the latter. But a heuristic is a way of finding something out that one does not already have at the front of one's mind. The availability illusion consists instead in relying on data that one already has at the front of one's mind. There is a lot of evidence most people are too slow to change certain kinds of beliefs (Ross & Lepper, 1980). But no one thinks that this evidence establishes a "conservatism heuristic," rather than that it just manifests the influence of factors which make for belief inertia. Analogously, if the argument for rational competence (in part I) is accepted, the "availability" results must be interpreted to have shown, not that the subjects are estimating the frequency or probability of an x by reference to the availability of an x, but that they are doing this by reference to those x's that happen to be available. The subjects are not to be construed as operating on the evidently wild assumption that

frequency can safely be taken to equal availability. Rather, where A is the available population, they are to be construed as operating on the not so evidently wild assumption that frequency can safely be taken to equal frequency in A, which is a very different matter.

In other words, to be entitled to recognise an error in subjects' reasoning here, we have to attribute to them a conception of the frequency or probability of an x, $p(x)$, such that it is incorrect to infer $p(x) = n$, where x's are y's, from $p(x \mid y)$ [$p(x$ given $y)$] $= n$, unless the y's are a suitably representative sample of the total population. So we thereby (see part I) also attribute to them competence to avoid those incorrect inferences. It follows that their probability-estimating mechanism must be supposed to include some such procedure as: check whether available evidence constitutes a fair sample in relevant respects, and, if not, seek evidence that is of the missing kind or kinds. What happens is just that the operation of this procedure tends to be obstructed by factors like the recency or emotional salience of the existing evidential input, by the existence of competing claims for computing time, or by a preference for least effort. Cognitive illusions, in the laboratory or in real life, depend on the power of such factors to hold subjects back, under the pressure of interrogation, from obtaining an appropriate additional input to their information-processing operation, just as when a visual conjurer relies at a crucial moment on his own speed of action, and on the visual inattentiveness of those who are watching, to hold the latter back from obtaining an appropriate additional input to their visual information-processing operation.

Another procedure referred to in the literature as a "heuristic" is the method of anchoring and adjustment, whereby a natural starting point or anchor is used as a first approximation to the required judgment of frequency, probability, expected value, and so on, and is then adjusted to accommodate the implications of additional information. Tversky and Kahneman (1974) have shown the existence of a tendency for adjustments to be insufficient: subjects with high starting points end up with higher estimates than those with lower ones. This tendency has also been noted (Lichtenstein & Slovic 1973) in an experiment with people who were gambling in a Las Vegas casino. Even there, an element of conjuring was present, in that the game that was played was specially designed for the purpose. But it would obviously be implausible to suppose that any kind of cognitive illusion occurs only when the circumstances that cause it are deliberately contrived: conjuring is not the only source of visual illusions either.

A somewhat similar phenomenon has been demonstrated in relation to hindsight (Fischhoff 1975): judges with knowledge of the outcome tend to overestimate the probability that they would have declared prior to the event. This is like starting with an anchor at 100% probability and adjusting to allow, not for more information, but for less, that is, for ignorance of the actual outcome.

However, unlike in the case of the supposed heuristic of availability, there is nothing intrinsically fallacious in the procedure of anchoring and adjustment. It is a perfectly legitimate heuristic if correctly operated. What goes wrong is just that the effects of recency or salience are generally too strong to permit correct operation. Thus, Slovic et al. (1976) are right to point out that bias from anchoring, like that from availability, is congruent with the hypothesis that human reasoners resemble computers that have the right programs and just cannot execute them properly. But Slovic et al. also claim that there are certain other errors prevalent in probabilistic reasoning, concerned with sampling and prior probabilities that are not congruent with this hypothesis, and we shall see shortly that the latter claim cannot be sustained.

2. Tests of Intelligence or Education

A second category of research activity found in the literature concerns ignorance, not illusion. It demonstrates a lack of mathematical or scientific expertise.

A lack of mathematical expertise here amounts to an ignorance of principles that not everyone can be expected to acknowledge readily, still less to elicit spontaneously from their relevant competence. Possession of a competence for deductive or probabilistic reasoning entails the possession of a mechanism that must include not only certain basic procedures, corresponding to a set of axioms or primitive rules for the normative system concerned, but also a method of generating additional procedures, corresponding to the proof of theorems or derived rules in that normative system. But the actual operation of this method, beyond its simplest forms, may require skills that are relatively rare, just as a particular talent is required for the discovery of proofs in logic or mathematics wherever no mechanical decision procedure is known. In

the latter case what are needed in an outstanding degree are such capacities as those for discerning shared structure in superficially different materials, for memorising complex relationships, and the like – in other words, whatever promotes the proposal of worthwhile hypotheses in the trial-and-error search for appropriate connexions. Correspondingly, only people with those skills in an outstanding degree can be expected to generate interesting new procedures for eliciting deductive consequences or estimating probabilities. Only they will be able to supply spontaneously the input, in terms of perceived similarities and the like that will enable the method of generating additional procedures to operate fruitfully. Others will have to learn these proofs or derivations, or acquire the additional procedures, at second hand. Education must supplement innate intelligence, where intelligence is understood not as the competence that everyone has but as the level of those skills that are required to supply the novel input essential for the discovery of proofs. So experiment in this area may be able to show us the limits of ordinary people's intelligence, in the appropriate sense, or the extent to which subjects have profited from logical or mathematical education. But it cannot demonstrate an erroneous competence.

For example, it required the genius of a great mathematician (Bernoulli 1713) to discover and prove that, if you estimate the probability of a certain characteristic's incidence in a population from its frequency in a sample, then the probability of your estimate's being correct, within a specifiable interval of approximation, will vary with the size of the sample. So it is easily understandable that psychological experiment finds a tendency among ordinary people, untutored in statistical theory, to be ignorant of this principle and its applications (Tversky & Kahneman 1971). No doubt equally cogent experiments could be designed to establish the fact that those untutored in Euclidean geometry are still ignorant of the fact that the square on the longest side of a right-angled triangle is equal in area to the sum of the squares on the other two sides, since it required another outstanding mathematician, Pythagoras, to discover a proof of this fact. Again, it is said (Tversky & Kahneman 1971, p. 109) that at a meeting of mathematical psychologists and at a general session of the American Psychological Association the typical respondent attached excessive significance to inferences from relatively small samples. But what this adds to the previous finding is just a

reason for reassessing the extent or success of the education that the respondents had in fact undergone. And the same holds true in relation to those who are supposed to have some statistical training but still fail to recognise new examples of regression to the mean for what they are (Kahneman & Tversky 1973).

Not all errors of estimation that are due to ignorance arise from subjects' deficiencies in mathematical expertise. Some arise instead from subjects' deficiencies in scientific (for example, psychological) expertise. For example, there is a good deal of evidence (reviewed in Slovic et al. 1977, pp. 5–6) that people are often overconfident in their second-order estimates of the accuracy of their own primary estimates. What happens here is that they are unaware of the various ways in which the information-processing mechanism that generates the primary estimates may be affected by performance error. However, this is scarcely surprising, since the facts about those patterns of error are being discovered only gradually and only by difficult (and sometimes controversial) research. No doubt it would be salutary if all nonpsychologists were taught every such fact that has been properly established. But all that is discovered, when their ignorance of such a fact is discovered, is a gap in their education.

3. Misapplications of Appropriate Normative Theory

We have been concerned so far with genuine fallacies, to which experiments reveal that people are prone because of either illusion or ignorance. However the literature also contains several examples of more questionable claims that a common fallacy exists. These are situations in which the experimental data may be explained as a direct manifestation of the relevant competence without any need to suppose an error in performance. Such claims arise either through a misapplication of the appropriate normative theory or through an application of an inappropriate one.

One particularly instructive example of the former kind relates to the alleged prevalence of the fallacy of illicit conversion, and, in particular, of inference from a proposition of the form "if p then q" to one of the form "if q then p." Intellectuals have remarked for over two millennia (Hamblin 1970) on the tendency of their inferiors to commit this fallacy, and in recent years it too has been a topic for psychological investigation (Wason & Johnson-Laird 1972). The investigators conclude that, in situations in which

subjects are apparently prone to illicit conversion of conditionals, "this is not because the subjects possess faulty rules of inference but because they sometimes make unwarranted interpretations of conditional statements" (p. 65). The subjects are claimed to treat these conditionals as if they were statements of causal connexion which allow one to infer from effect to cause as well as from cause to effect.

But it is not clear that the subjects must in fact be supposed even to be making an unwarranted interpretation. We have to bear in mind here that the principles of a normative theory, such as one that systematises criteria for deducibility, inevitably involve abstraction and idealisation (see Part I, Section 3). So what are to be taken as the actual, concrete premises that are represented by the initial formulas in a primitive or derived rule for natural deduction, when such rules are taken to be the norms relevant to some actual sequence of human reasoning? The mere sentences uttered do not normally constitute all of the premises conveyed by the total act of communication, since we are presumptively entitled to take the latter as including also any judgments that are implied by the act of uttering those sentences in the contextual circumstances. For example, as far as human conversation is governed by rules of relevance, brevity, informativeness, and so on, as required by the purpose in hand (Grice 1975),[8] the information provided by the utterance of a solitary conditional sentence – if p then q – may be presumed, unless there are specific indications to the contrary, to be all that is required in the circumstances to satisfy the interest either of someone who wants to know what is also true if the antecedent of the conditional is true, or of someone who wants to know the conditions under which the consequent of the conditional sentence is true. In the former case ("If you interrupt him now, he'll be cross") the conditional is convertible because its utterance would normally be pointless unless "if not-p then not-q" were also true and "if not p then not-q" is formally equivalent to the converse of "if p, then q." In the other case ("If you give him a tip, he'll let you in") the conditional is convertible because its solitary utterance may be presumed to state the only condition under which the consequent is true.

Hence if we consider the total content of the message communicated, rather than just the conditional sentence that is uttered, it would not be fallacious or unwarranted for subjects to presume, unless there are specific indications to the contrary, that the converse of the condi-

tional is implicit in the message, and the convertibility of causal conditionals is just a special form of this. A psychological experimenter who wishes to exclude the legitimacy of presuming the converse in such a case must contrive suitable instructions to his subjects and teach them how to distinguish between the implications of a sentence uttered and the implications of its utterance. But how could we judge the suitability of such instructions without taking into account the extent of their success in averting inferences to the converse? In other words, a tendency to commit the fallacy of illicit conversion in everyday life is demonstrable only on the basis of an unrealistic assumption – namely, that when a normative theory is invoked for the evaluation of commonsense reasoning its criteria should be applied to nothing but the linguistic forms that are actually uttered.

Another line of research activity (see, for example, Wagenaar 1972) in which appropriate norms seem to be sometimes misapplied is in studies of judgments of randomness. Results over quite a variety of tests seem to confirm the hypothesis that subjects who are attempting to behave randomly will produce series that have too many alternations and too few repetitions. But as has been well pointed out (Lopes 1980), a series may have randomness with respect to its atomic or elementary events, while still possessing molecular units, such as groups of ten consecutive atomic events, that do not exhibit randomness. Or randomness may be achieved for a certain category of molecular events, at the cost of sacrificing randomness with respect to elementary events. Unless this distinction between different kinds of randomness is clearly presented to the subjects, they are not in a position to know what kind is being sought by the experimenters. And again it is not easy to see how the subjects' apparent failure to produce correct judgments of randomness should not be regarded as simply a measure of the aptness of their instructions.

A different way in which an appropriate normative theory may be misapplied was instantiated in the course of an attempt to show that, as compared with their treatment of predictive evidence, people are prone to "a major underestimation of the impact" of diagnostic evidence "which could have severe consequences in the intuitive assessment of legal, medical, or scientific evidence" (Tversky & Kahneman 1977, p. 186). Subjects were given two sets of questions that were regarded by the experimenters as similar in relevant structure. But in fact the

predictive set concerned conditional probabilities, as in – for one instance – "The chance of death from heart failure is 45% among males with congenital high blood pressure," while the diagnostic set concerned unconditional ones, as in "The radiologist who examined Bill's X-ray estimated the chance of a malignancy to be 45%," and this difference sufficed to account quite rationally for differences in the numerical answers to the two sets of questions (see Cohen 1979, pp. 403–5). Moreover, when the dissimilarity of structure was remedied, the alleged phenomenon of diagnostic underestimation failed to emerge. Other results that appear to evince this phenomenon have to be discounted for different reasons (Cohen 1979, pp. 401–3). But the failure to distinguish appropriately here between conditional and unconditional probabilities is a good example of how the appearance of a fallacy in subjects' reasoning may be generated by a slip in the application of the appropriate probabilistic analysis,[9] since within normative probability theory the distinction between conditional and unconditional probabilities is well established.

Even the so-called gambler's fallacy, or "fallacy of the maturity of chances," which is sometimes referred to in the literature on cognitive irrationality (Tversky & Kahneman 1974; Hogarth 1975), comes under some suspicion. More empirical work seems necessary here, but there are at least three possible approaches to the phenomenon that call into question its interpretation as a fallacy of probabilistic reasoning.

If some people believe that after a long run of heads the probability of tails on the next toss will be greater than $\frac{1}{2}$, then one possibility is that they should be interpreted as believing thereby in a spirit of distributive justice that regulates the whole cosmos with a policy that ensures ever-increasing probabilities of a trend-reversing intervention whenever identical outcomes begin to succeed one another within an otherwise chance set-up. On this construal, a gambler's metaphysical belief may be at fault, but not the rationality of his reasoning from it. However, such an interpretation needs independent evidence to support the attribution of belief in the particular case. Otherwise it is open to the charge of being culpably ad hoc, if not of merely repeating what is to be explained within the explanation.

Second, we may need to distinguish here between two rather different probabilities, either of which might be a matter for estimation. Is the gambler supposed to be estimating, in relation to the next toss of a fair coin, the

probability of a tails outcome within a space that consists of the two alternative outcomes: heads and tails? Or is the probability in mind, at the nth toss of a fair coin, that of having at least one tails outcome within any space that consists of n outcomes? Whereas the correct figure for the former would be $\frac{1}{2}$, the correct figure for the latter would get greater and greater than $\frac{1}{2}$ as n itself increases beyond 1, in accordance with Bernoulli's theorem. To ascertain clearly and unmistakably which of the two probabilities is being estimated it would be necessary to question the gambler in a way that would tend to discourage any incorrect estimate, since in order to convey the exact meaning of a particular type of probability assignment (or, indeed, of any other type of statement), one needs to state the conditions under which such a judgment is true. So we are left with a characteristic indeterminacy here. Any attempt to extract an exact answer from the gambler would transform the situation in a way that would tend to disconfirm the occurrence of fallacious reasoning, and to the extent that the situation was not so transformed, the exact nature of the situation would remain in doubt. But it remains an open question, in view of what was said earlier about ordinary people's ignorance of Bernoulli's theorem, whether ordinary gamblers may legitimately be expected to be aware of its implications.

Finally, it may be that the matter at issue needs to be regarded more as a pragmatic than as a cognitive phenomenon. In the long run a gambler could integrate the so-called fallacy into a winning strategy against any opponent who always insists on even odds but is willing to play as long as the gambler wants: the gambler has only to continue increasing the stakes sufficiently at each toss until tails actually comes up. But, of course, such a strategy could be executed only within the limits of any restriction that is imposed on the stakes either by the opponent or by the gambler's resources, just as any intellectual competence is subject to limitations in actual performance.

4. Applications of Inappropriate Normative Theory

There is a tendency for some investigators of irrationality to proceed as if all questions about appropriate norms have already been settled and the questions that remain open concern only the extent of actual conformity to these norms. It is as if existing textbooks of logic or statistics had some kind of canonical authority. But in fact

many important normative issues are still controversial. For example, it seemed at one time that at least the Frege-Russell logic of quantification had become a universally received doctrine. But its closeness of fit for the appraisal of natural-language reasoning is now under a powerful challenge (Sommers 1981) from work that exploits hitherto undiscovered ways of developing the Aristotelian tradition. Again, it seemed at one time to be generally agreed – and accepted by psychological investigators of decision making (see Slovic et al. 1977) – that the rational way to base action on estimates of chance was to follow the rule: "Rank possible courses of action according to their conditional subjective estimations of utility." But this rule has been seriously challenged in recent years because it seems not to take proper account of the difference between actions as symptoms, and actions as causes, of states of affairs that we act to promote or avert (Jeffrey 1980).

Great care has certainly to be taken also in selecting the normative criteria by which the correctness of subjects' probability judgments is assessed. In one experiment, for example, subjects were told that in a certain town blue and green cabs operate in a ratio of 85 to 15, respectively. A witness identifies a cab in a crash as green, and the court is told that in the relevant light conditions he can distinguish blue cabs from green ones in 80% of cases. The subjects were then asked: what is the probability (expressed as a percentage) that the cab involved in the accident was blue? The median estimated probability was .2, and investigators (Kahneman & Tversky 1972a) claim that this shows the prevalence of serious error, because it implies a failure to take base rates (that is, prior probabilities) into account. Kahneman and Tversky commented: "Much as we would like to, we have no reason to believe that the typical juror does not evaluate evidence in this fashion." Lyon and Slovic (1976) have confirmed the robustness of the phenomenon, which is impervious to variations in the topic, numerical details, and sequential formulation of the story told to the subjects (with the proviso that blue and green cabs were present in equal numbers during the tests on the witness). And they complain that "since the world operates according to Bayes's theorem, experience should confirm the importance of base rates" despite the apparent failure of subjects to recognize that it does so.

At best, these experiments would constitute a test of their subjects' intelligence or education, since the ordinary person might no more be expected to generate Bayes's theorem spontaneously than Bernoulli's. But in fact it is doubtful whether the subjects have made any kind of mathematical error at all. The experimenters seem to be reasoning as follows. In the long run, they say to themselves, the witness may be expected to make 68% correct identifications of a cab as blue ($^4/_5 \times 85\%$), 3% incorrect identifications of a cab as blue ($^1/_5 \times 15\%$), 12% correct identifications of a cab as green ($^4/_5 \times 15\%$), and 17% incorrect identifications as green ($^1/_5 \times 85\%$). Therefore he will altogether make 29% identifications as green, and the fraction of them that will be incorrect is $^{17}/_{29}$. Consequently, according to the way in which the experimenters seem to be reasoning, the probability that the cab involved in the accident was blue is $^{17}/_{29}$, not $^1/_5$.

But this last step is a questionable one. The ratio $^{17}/_{29}$ is the value of the conditional probability that a cab colour identification by the witness is incorrect, on the condition that it is an identification as green. Jurors, however, or people thinking of themselves as jurors, ought not to rely on that probability if they can avoid doing so, since reliance on it assumes the issue before the court to concern a long run of cab-colour identification problems – whereas in fact it concerns just one problem of this type. Jurors here are occupied, strictly speaking, just with the probability that the cab actually involved in the accident was blue, on the condition that the witness said it was green. And the latter probability is equivalent in the circumstances to the probability that a statement to the effect that the cab actually involved in the accident was green, is false, on the condition that the statement is made by the witness. If the jurors know that only 20% of the witness's statements about cab colours are false, they rightly estimate the probability at issue as $^1/_5$, without any transgression of Bayes's law. The fact that cab colours actually vary according to an $^{85}/_{15}$ ratio is strictly irrelevant to this estimate, because it neither raises nor lowers the probability of a specific cab-colour identification being correct on the condition that it is an identification by the witness. A probability that holds uniformly for each of a class of events because it is based on causal properties, such as the physiology of vision, cannot be altered by facts, such as chance distributions, that have no causal efficacy in the individual events. For example, if the green cab company suddenly increased the size of its fleet relative to that of the blue company, the accuracy of the witness's vision would not be affected, and

the credibility of his testimony would therefore remain precisely the same in any particular case of the relevant kind.

The same point can be put another way by emphasising the difference between probability functions that measure relative frequencies and probability functions that measure causal propensities (see Part I, Section 2 of this chapter). Propensity-type probabilities may be *estimated from* frequencies in appropriate samples (as with the witness's reliability), but what is actually evaluated is something different: a propensity, not a frequency. And propensity-type probabilities can be derived for individual events because they are predictable distributively. So it is natural to suppose that this is the kind of probability with which a jury is properly concerned, whereas the mere relative frequency of blue and green cabs is an accidentally accumulated characteristic of the town's cab population, considered collectively, and does not generate any causal propensity for the particular cab in the accident. Of course, if no testimony is mentioned and subjects know nothing except the relative frequency of the differently coloured cabs, then no causal propensity is at issue and the only basis for estimating the required probability is indeed the relative frequency. And this is in fact the kind of estimate that the investigators have then found to occur under experimental conditions (Lyon & Slovic 1976, p. 294).

The issue here is an important one since it has many ramifications. If the investigators had been right to impugn the rationality of common-sense judgments in the above example, it would have certainly been difficult to defend the continued use of lay juries. Consider too what you yourself would decide in the following circumstances. You are suffering from a disease that, according to your manifest symptoms, is either A or B. For a variety of demographic reasons disease A happens to be nineteen times as common as B. The two diseases are equally fatal if untreated, but it is dangerous to combine the respectively appropriate treatments. Your physician orders a certain test which, through the operation of a fairly well understood causal process, always gives a unique diagnosis in such cases, and this diagnosis has been tried out on equal numbers of A- and B-patients and is known to be correct on 80% of those occasions. The tests report that you are suffering from disease B. Should you nevertheless opt for the treatment appropriate to A, on the supposition (reached by calculating as the experimenters did) that the probability of your suffering from A is $^{19}/_{23}$? Or should you opt for

the treatment appropriate to B, on the supposition (reached by calculating as the subjects did) that the probability of your suffering from B is $^4/_5$? It is the former option that would be the irrational one for you, qua patient, not the latter; and in a rather comparable experimental situation (Hammerton 1973) subjects tended in fact to judge the matter along just those lines. Indeed, on the other view, which is the one espoused in the literature, it would be a waste of time and money even to carry out the tests, since whatever *their* results, the base rates would still compel a more than $^4/_5$ probability in favour of disease A. So the literature under criticism is propagating an analysis that could increase the number of deaths from a rare disease of this kind.

Admittedly, the standard statistical method would be to take the prior frequency into account here, and this would be absolutely right if what was wanted was a probability for any patient considered not as a concrete particular person, not even as a randomly selected particular person, but simply as an instance of a long run of patients. The administrator who wants to secure a high rate of diagnostic success for his hospital at minimal cost would be right to seek to maximise just that probability, and therefore to dispense altogether with the tests. But a patient is concerned with success in his own particular case, not with stochastic success for the system. So he needs to evaluate a propensity-type probability, not a frequency-type one, and the standard statistical method would then be inappropriate. Note, however, that the causal propensity analysis does not involve any repudiation of Bayes's theorem. It is just that the prior probabilities have to be appropriate ones, and there is no information about you personally that establishes a greater predisposition in your case to disease A than to disease B. We have to suppose equal predispositions here, unless told that the probability of A is greater (or less) than that of B among people who share all your relevant characteristics, such as age, medical history, blood group, and so on. An analogous supposition has to be made about the cab colours, unless we are told that because of faulty maintenance, say, the probability of a blue cab's being involved in accidents that share all the relevant characteristics of the present one, such as poor braking, worn tires, and the like, is greater (or less) than that of a green cab's being involved. Similarly, in a criminal law court the object is to do justice in each individual case, without taking a defendant's past criminal record, if he has one, into account. But it is easy enough to imagine analogous cases in which a

shoplifter, say, would escape conviction on the basis of probabilistic testimony about identification, if the relative frequency of honest shoppers could be cited in his defence! Or consider an example very like that cited by Todhunter (1949/1865, p. 400) in connection with the danger of applying the standard statistical method – which he traces to Condorcet – indiscriminately. A witness of 99.9% reliability asserts that the number of the single ticket drawn in a lottery of 10,000 tickets was, say, 297: ought we really to reject that proposition just because of the size of the lottery?

The difference between frequency probability and propensity probability is a difference between two functions that both satisfy the formal axioms of the classical calculus of chance. The two functions differ in their semantics, that is, with regard to the nature x and y must have, and the relation they must bear to one another, when, for a particular n, it is true that $p(x|y) = n$. But both functions have the same logical syntax; that is, each satisfies a multiplicational law for conjunction, a complementational law for negation, and so on. Nevertheless (as remarked above) it should by no means be taken for granted that all valid types of probability judgment in everyday reasoning can be modeled by functions that share this syntax.

For example, it has been held (Kahneman & Tversky 1972b, 1973, 1974; Tversky & Kahneman 1974) that intuitive judgments of probability are biased towards predicting that outcomes will be similar to the evidence afforded by typical cases. It is claimed that people use a representativeness heuristic as a rough-and-ready, though often misleading, guide in their probabilistic reasoning. But the validity of this claim depends on the assumption that such a judgment about degree of representativeness has to be interpreted as a means towards drawing some conclusion about probability in a sense of that term that conforms to the classical calculus of chance. If instead we abandon that assumption, we can avoid imputing any fallacies here. We can suppose that the judgment of representativeness leads to a conclusion about probability in a sense in which an inference from representativeness to probability is always quite legitimate – albeit a sense that conforms to principles different from those derivable within the calculus of chance. In fact, these principles can be shown to be implicit in the logic of controlled experiment, which was first developed by Francis Bacon (Cohen 1979). Bacon, in the preface to his *Novum Organum*, described the central concern of his own enquiry in just the same terms as Bernoulli (1713, p. 211) described his, namely, the determination of "degrees of certainty." But Bacon's method defines a different concept of probability from Bernoulli's (Cohen 1980b). Hume (1739) called it "probability arising from analogy," and he wrote:

> Without some degree of resemblance, as well as union, 'tis impossible there can be any reasoning; but as this resemblance admits of many different degrees, the reasoning becomes proportionally more or less firm and certain. An experiment loses of its force, when transfer'd to instances, which are not exactly resembling; tho' 'tis evident it may still retain as much as may be the foundation of probability, as long as there is any resemblance remaining.

When all this is made precise and its implications are developed systematically, one can show that, in appropriate contexts, concern with representativeness is not a potentially fallacious heuristic but rather a quite reliable, albeit somewhat crude, mode of commonsense reasoning under conditions of uncertainty (Cohen 1979, 1980d). It appears otherwise only if evaluated against a type of normative theory that is inappropriate in the circumstances (though admirably appropriate in many other circumstances).

Conclusion

The upshot of all this may be summarised as follows. No doubt ordinary people often err in their reasoning, and such a mistake begins to be of scientific interest when it can be shown to instantiate some regular pattern of performance error. However, nothing in the existing literature on cognitive reasoning, or in any possible future results of human experimental enquiry could have bleak implications for human rationality, in the sense of implications that establish a faulty competence. At best, experimenters in this area may hope to discover revealing patterns of illusion. Often they will only be testing subjects' intelligence or education. At worst they risk imputing fallacies where none exist.

Notes

1   The same is true for intuitions of grammaticalness, pace Sampson (1975).
2   This issue is too complex to be treated adequately here; for a useful review; see Haack (1974).

3　I leave open here the much discussed question whether (Lewis and Langford 1959), Anderson and Belnap, 1974, or some other system provides a better fitting logic of everyday reasoning.

4　For example, the intuition that B is deducible from A whenever A-and-not-B is inconsistent (Lewis & Langford 1959) clashes with the intuition that one may not deduce every proposition from an inconsistent one (Anderson & Belnap 1974). So, though consistency is normally an overriding ideal for theory construction, one cannot treat the demand for it as the only foundation needed for a theory of deducibility: other intuitions, too, have to be taken into account.

5　Cf. how, in a maximally specific case, the systematic model of a clinician's judgmental strategies may be a better predictor than the clinician's own judgment (Goldberg 1970).

6　The analogy with perceptual illusion (such as the Muller & Lyer) was also drawn by Chapman and Chapman (1967, p. 194) in their interpretation of the partly experimental and partly real-life data about erroneous use of Draw-a-Person tests in psychiatric diagnosis. Both here and in their work on the psychodiagnostic use of Rorschach cards (1969) they traced the source of illusory correlations to a powerful bias by verbal association, since subjects with no clinical experience all tended to make the same erroneous correlations as many clinicians.

7　Apparently none of Wason's subjects objected, as would have been justified, that no finite number of questions and answers, whether falsificatory or verificatory, could prove such a hypothesis correct.

8　I take Grice to have established the mental or social reality of some such rules. In the logical context, however he does not use them, as I do, to explain the alleged prevalence of the fallacy of illicit conversion. Instead he tries to use them to explain away the apparent inappropriateness of a truth functional logic for the analysis of deductive reasoning a natural language, and in this he attempts an impossible task (see Cohen 1971; 1977a).

9　This has now been acknowledged by its authors (Kahneman & Tversky 1979).

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