1. Introduction

The end of the nineteenth century is remembered as a time when psychology freed itself from philosophy and carved out an autonomous subject matter for itself. In fact, this time of emancipation was also a time of exile: while the psychologists were leaving, philosophers were slamming the door behind them. Frege is celebrated for having demonstrated the irrelevance of psychological considerations to philosophy. Some of Frege's reasons for distinguishing psychological questions from philosophical ones were sound, but one of Frege's most influential arguments, which was elaborated upon and advocated by the positivists, vastly overestimated the gap separating the two disciplines.

It is of more than historical interest to try to locate the strengths and defects in Frege's antipsychologism. Some forms of psychologism now seem to point to working hypotheses which can inform research in cognitive psychology. And conversely, if psychologism is plausible, some questions of epistemology may be seen in a new light when confronted with psychological data. Some of the obstacles that have stood in the way of this sort of co-operation have already started to fall. Psychologists are now more than ever talking about cognition in terms of information processing, an attitude which pictures thinking as an inferential process of the sort that logicians have long been investigating. Philosophers, in turn, have gradually come to see that there can be no certain foundations for empirical knowledge, and so appeal to empirical data need not subvert the proper task of epistemology.

'Psychologism' denotes a family of views, all tending to downplay
or deny distinctions between epistemology and logic on the one hand and psychology on the other. In order to begin disentangling the various strands of thought that are usually seen as psychologistic, the following distinctions will prove useful. *Epistemological psychologism* denies that there is any question of justifying a logical rule or epistemological maxim above and beyond the question of whether it is in fact followed in practice. Psychologians of this ilk may think that questions of justification are in fact unanswerable; alternatively, they may feel that such questions are answered merely by describing what sentient beings in fact do. It seems that every time a major foundationalist epistemology is taken to be undermined, both of these reactions occur. Within neoKantianism, there was a tradition which held that Kant had correctly identified a host of organizing principles which all sentient beings must use if they are to be the subjects of experience, but that any argument for the truth of these principles must fail. Jacob Fries and Leonard Nelson,¹ who revived Fries’ thought, held that Kant’s synthetic a priori truths were true, unprovable characterizations of reality and unavoidable assumptions made by all subjects of experience. The only answerable questions about the basic organizing principles of human reasoning were descriptive, not normative. In contrast, the neoKantian Friedrich Beneke² took the radical idealist line that the laws of logic are true just because they are inevitably employed by all thinking things; for him, the philosophical justification and the psychological description of the laws of thought were one problem. The same pattern of reaction occurred after Carnap’s foundationalist efforts came to be seen as unsatisfactory. Quine, in his advocacy of a naturalized epistemology, has denied that questions of justification can be answered;³ Goodman, in his formulation of the new riddle of induction, has argued that the problem of justifying induction is the problem of describing the regularities that govern our inference making.⁴ I have discussed elsewhere the difference between justifying a rule of inference and showing that it captures a regularity in how we in fact reason.⁵ And on the question of whether the failure of a foundationalist programme shows that questions of justification cannot be answered, it is worth noticing the Quine’s advice ‘Since Carnap’s foundationalism failed, why not settle for psychology’ carries weight only to the degree that Carnapian epistemology exhausts the possibilities of epistemology.

Distinct from these epistemological concerns is the view that the laws of logic and the characterization of rationality that epistemology seeks to formulate are about human mental activity. This is *metaphysical psychologism*. Frege argued that logic and arithmetic are
not about mental contents; he produced one very good argument—or at least the sketch of one—for this view. It is the subsistence argument: even if human psychology were different—even if there were no thinking creatures at all—$2 + 3$ would still equal 5. This counterfactual consideration led Frege to conclude that arithmetic statements are not about mental entities, since the statements do not depend for their truth value on any characteristic of those entities. This argument is substantially correct, I think, even though it requires some elaboration and modification. Perhaps the same reasons can be advanced for holding that some of the rules of reasoning described by epistemologists are not mental in their subject matter; principles of inductive and statistical inference have the same status as other branches of mathematics. Frege's argument for Platonism in arithmetic also favours Platonism in probability theory. How far this refutation of metaphysical psychologism extends into the subject matter of epistemology we will not try to estimate here.

The third form of psychologism, which is the subject of this paper, is the thesis that the laws of logic and the maxims of epistemology are (among other things) the laws of cognition. This is the view that the rules of correct reasoning that logicians and epistemologists try to describe have psychological reality. This version of psychologism is independent of the two psychologistic positions just sketched. It is committed to a fairly substantive conception of what adequate psychological theories of different areas of cognition are like. Although Frege's subsistence argument leaves this form of psychologism untouched, his variability argument is so sweeping in its scope that it seeks to undermine both metaphysical psychologism and the view that rules of correct reasoning have psychological reality. This argument of Frege's succeeded in locating one of the central difficulties in the way psychology was done during the time Frege was writing. The positivists expanded upon the variability argument, but by the time they espoused it, the view of psychology it presupposed had become anachronistic.

The psychologistic position that I will defend likewise assumes a view of what a psychological theory is. I will assume that an adequate account of memory, perception, problem-solving, learning, and cognition in general can be provided by an information-processing model. Providing such a model must involve selecting some logical system (i.e., some set of rules of inference characterized within a formal system) and claiming that it has psychological reality. Given this, the only remaining question for psychologism is what the relationship is between this psychologically real logical system and the true logical system.
An information-processing model in psychology must show how inputs are transformed into outputs and how the items so transformed contain information. What one takes as the inputs and outputs may vary from problem to problem; at its most general level, one might conceive of stimuli as the inputs and behaviours as the outputs. Between these two, the model will posit a number of intervening stages of processing. The objects operated upon during these stages of processing will be representations which occur within a language or a nonlinguistic representational system. Thus, in a model of this kind for visual perception, the posited mechanism might show how a hypothesis describing the information provided by two-dimensional retinal stimulations is transformed into a hypothesis (i.e., a perceptual judgement) which gives information about the three-dimensional world of physical objects. Because this mechanism transforms representations into representations (e.g., sentences into sentences), rules of inference are essential to an information-processing model.

The plausibility of psychologism depends in part on the correctness of this view of psychological theory. Psychologism would be an implausible thesis if one thought of psychology as behaviourists do, or if one held that explicitly physicalistic explanations are the only legitimate ones. I will not repeat the now familiar arguments for thinking that psychological explanations can legitimately posit beliefs and desires, nor will I repeat the reasons, which I have given elsewhere, for thinking that information-processing models have the characteristics that I have just attributed to them.

The version of psychologism that I will explore is a thesis of psychological reality, and these two words have excited enormous controversy in philosophy of mind in recent years. The focus of this controversy has mainly been on Chomsky’s claim that the grammar of a language is psychologically real in every fully competent speaker. Many philosophers, Goodman for example, have argued that there is no more need to think that speakers have internalized rules of grammar than there is to think that falling objects have internalized the laws of gravitation. In both cases, it is fully satisfactory to say that the objects in question obey the laws; questions of representation are otiose. As I mentioned above, information processing models are fully committed to the idea of psychological reality in that they view internalized representations as playing a causal role in determining behaviour and further cognitive activity. Although I think that the use of the notion of psychological reality can be defended, I will not attempt to do so here in a direct way; perhaps the fact that it is presupposed by a fruitful and substantive
research programme should count as some evidence that sense can be made of it.

In what follows, I will examine Frege's variability argument and the capital that was made out of it by the positivists. I will argue that Frege's criticism of psychologism of the third kind—the view that principles of rational inference are psychologically real—can be met. I then will describe some kinds of empirical evidence that may be relevant to defending this version of psychologism. The view that psychologism is an empirical, not an a priori, thesis will then be defended against a line of argument advocated by Quine. The paper concludes with a discussion of the role accorded to psychological considerations in Carnap's idea of rational reconstruction.

2. The Variability Argument and Its Heirs

Frege's antipsychologism went beyond Kant's distinction of the *questio facti* from the *questio juris*, and constitutes the beginning of the modern view that truths of logic do not describe how the human mind works. Frege's extremely influential variability argument explicitly challenged the empiricist view that the meaning of a term is the mental image that the term calls up in thought, but it implicitly had a more general application. The point was to banish psychological considerations from theory of meaning. By easy extension, the positivists used Frege's argument to proscribe psychology from epistemology in general. The argument is really quite simple: If communication is to be possible, the speakers of a language must associate the same, or nearly the same, meanings with the terms they use. But the mental images that people associate with terms vary enormously from person to person. Images vary, but meanings cannot, so meanings are not mental images. Psychology's focus on what is essentially idiosyncratic distinguishes it from the distinctively philosophical concern with what is objective and invariant across people. Frege's argument claimed that the objects of philosophical inquiry were not psychological, and he consistently took this view in his theory of meaning and philosophy of mathematics.

It is no wonder that Frege conceived of psychological phenomena as variable and erratic, given the way psychology was done at the time. Nineteenth-century German psychology viewed introspection as the appropriate method of inquiry, and proceeded in large measure by training subjects in the vernacular of the psychologist's pet theory and then bidding them to introspect. This approach was typified by
Titchener, who denied the existence of imageless thought. Although Berkeley earlier had highlighted the problems involved in thinking that general ideas are represented in thought by mental pictures, Titchener proceeded to describe the images that he associated with words like ‘but’, ‘patriotism’, and ‘triangle’. Each of these associated images was highly personal: for example, with the word ‘but’, Titchener associated ‘the back of the head of a speaker who often used this word while Titchener sat behind him on a platform’. Frege’s variability argument presupposed a view of psychology that was true to the psychology of his time.

Positivist philosophers of science used a version of the variability argument to establish their distinction between the context of discovery and the context of justification. A scientist may come to entertain a hypothesis through the most irrational and unusual flights of fancy; these quirks of creativity are the business of psychology. However, the hypotheses thus formulated can be evaluated by all scientists in the light of the evidence, and this procedure is paradigmatically public. The context of discovery is subjective and psychological; the context of justification is objective and philosophical. Hempel graphically illustrated this distinction by describing how Kekulé came to discover the structure of the benzene ring. Kekulé was dozing in front of his fireplace after a hard day’s work and hallucinated a pair of snakes which were whirling around. The snakes seized each other’s tails and formed a ring. Eureka! Once Kekulé had this flash of insight, his conjecture could be evaluated by testing it against the relevant chemical data.

If Frege’s advocacy of the variability argument is understandable in the light of introspectionist psychology, the positivists’ purveying it is less so in the light of the psychology of their time. Behaviourism, Freudianism, and Gestalt psychology were principally focused on the discovery of cross-subject invariances. For these programmes, the discovery that an ostensible psychological property of one subject fails to generalize across many or all subjects was strong evidence for thinking that the property was either of minor importance or wholly an experimental artifact.

It is a commonplace of virtually every twentieth-century school of psychology that introspective reports are to be viewed as kinds of behaviour that need to be explained. Although the schools differ vastly in how this is to be done, none takes introspective reports to automatically constitute truths which need to be accommodated by one’s theory. For cognitive psychology, it is at least possible that such reports do turn out to be vindicated by a larger context of theory; for behaviourism, such reports are viewed either as irrelevant to the
enterprise of psychology or as wholly lacking in scientific content. Yet, the variability argument relies on introspective reports; the force of the argument relies on these reports of mental imaginings being both true and fundamental to the workings of mind. Given that we now think of the positing of mental contents as a theoretical activity, it seems methodologically unsound to posit idiosyncratic thoughts and thought processes where the behaviour to be explained is invariant across subjects. If communication proceeds smoothly, then one has a prima facie reason for viewing all speakers as employing the same or similar mechanisms of speech production and understanding. If I would accept or create the scientific theory that you accept or create, were I working with the same substantive assumptions and evidence, then my psychological procedures should be viewed as the same as yours. The fact that introspective reports of mental images are idiosyncratic while linguistic behaviour is invariant should lead us to conclude that such introspective reports are either false or wholly irrelevant to the explanation of such invariances. This is merely a consequence of the maxim which advises us to posit similar causes for similar effects.

The fact that Kekulé hallucinated a pair of snakes in his quest for the structure of benzene is an idiosyncratic psychological fact. It does not show that the context of discovery is idiosyncratic. Whatever invariances there are in the thought processes that yield scientific discoveries presumably will be below the surface. Hempel’s historical example is like Frege’s focusing on mental images. One cannot generalize from the idiosyncracy of these facts to the claim that meanings are nothing psychological or that there is no logic of discovery.

Besides their use of Frege’s variability argument, the positivists gave an additional reason for denying that there is such a thing as a logic of discovery. This is the belief that the processes whereby new theories are invented are inherently creative. In maintaining that there is a logic of evaluation but no logic of discovery, the positivists were thus committed to there being a fundamental difference between these two enterprises. A formal characterization of the difference between a discovery procedure and procedures of justification will allow us to locate the source of whatever extra creativity the context of discovery possesses. We can merely adapt Chomsky’s three alternatives for linguistic theory to describe three alternatives for theories of scientific method. A discovery procedure must take sentences which characterize the evidence as inputs and yield theories and hypotheses as outputs. A decision procedure must take the evidence and a single hypothesis as inputs and then determine whether or not that
hypothesis is the best explanation, or the most acceptable hypothesis, relative to the evidence. And an evaluation procedure takes pairs of hypotheses and a body of evidence as input and says which of the two hypotheses is a better explanation of the evidence, or which hypothesis is more acceptable on the evidence. The following diagram illustrates these three alternatives.

![Diagram](image)

The formal difference between a discovery procedure and an evaluation procedure is simply that a discovery procedure is an evaluation procedure hooked onto a finite alternatives generator. That is, a discovery procedure can be viewed as having two parts. First, a finite number of competing hypotheses are generated from the evidence; then, an evaluation procedure is applied to these alternatives and orders them, thus picking out the best hypothesis (or the best hypotheses if there is a tie for first place). Hence, to claim that there is a logic of evaluation, but no logic of discovery, one must maintain that there is no logic for generating a finite set of alternatives.

If we conceive of the black boxes in the above diagram as providing algorithms for evaluation, decision, or discovery, then it is not at all clear that there is even such a thing as a 'logic' of evaluation. Is there a purely mechanical and general procedure for determining which of a pair of hypotheses is more reasonable relative to a body of evidence, or for determining which of a pair of hypotheses better explains a body of evidence? Given the holistic reasons, sophisticated considerations, and complex inferences that often enter into decisions of this kind, it is far from obvious that the evaluation procedure is purely algorithmic. Equally negative comments of course apply to the computability of a decision procedure; doubts about there being an algorithm for evaluation infect the other two alternatives because
the other two can be viewed as properly containing evaluation procedures, as mentioned above. Thus, if one holds that there is a logic of evaluation but not one of discovery, it seems rather implausible to characterize this difference in terms of the existence of algorithms.

The other alternative is to think of the logic of evaluation as consisting at least in part of a set of heuristic procedures—that is, ones which are not logically guaranteed to reach the correct answers, but are merely highly reliable techniques for doing so. A heuristic procedure is just as mechanical as an algorithm; the difference consists in whether its application carries with it a logical guarantee of success. As an example of a heuristic procedure, we can point to the work in artificial intelligence on the writing of theorem-proving programmes. Church's Theorem tells us that there are no recursive procedures for generating a correct yes or no answer to the question 'is sentence s a theorem of formal system S?' for certain formal systems. The programme sets down heuristic procedures for answering the question. These procedures are not 'informal' or just offhand pieces of advice. They are precisely stated mechanical procedures which we know cannot always lead to success. Now if a logic of evaluation is a set of heuristic procedures for evaluating hypotheses, it is not clear why there should not be a set of similar procedures to comprise a logic of discovery. Until some positive arguments are produced to show why there can be no such set of heuristic procedures, the claim that there is no logic of discovery must count as an antipsychologistic article of faith.

It is interesting that the positivist rejection of discovery procedures did not prevent them from recommending some. Reichenbach advocated the straight rule as a technique for estimating the relative frequency of a property in a population, given the proportion of items having that property in a sample. Carnap criticized the straight rule, but not because it was a rule for generating hypotheses that go beyond the evidence on which they are based. Carnap had favourite estimators of his own. Of course, given Reichenbach's and Carnap's view about the analyticity and a prioricity of probability statements, they would probably see the inferred hypothesis sanctioned by reasonable rules of estimation as deductive consequences of the evidence, the probability calculus, and suitable definitions of terms like 'best estimate'. But even granting this response, their rejection of discovery procedures is no less problematic. For why couldn't the same be said for a rule that generate 'the best explanation' or the 'best theory' from the evidence? Perhaps the crucial difference is that new empirical concepts are introduced in the
transition from evidence to theory while no novel concepts appear in the process of statistical estimation. This difference between abduction and induction is of fundamental importance, but if Reichenbach, for one, were to respond to our question in this way, it would be difficult to see how he could hold the straight rule to be the foundation of scientific inference.

The positivists denied that there is a logic of discovery because they accepted the Fregean doctrine that psychological processes are idiosyncratic. This general view is being undercut by our growing knowledge of cognitive processes. More specifically, psychological investigations into how people solve problems suggests that there may be significant cross subject uniformities in the context of discovery. The pioneering work of Bruner, Goodnow, and Austin on some simple problems of concept formation reveals that there are a small number of definite strategies that subjects tend to use as discovery procedures. Parallel uniformities may well obtain in the formation of perceptual judgments and in language acquisition. In similar stimulus situations, people tend to come up with similar perceptual judgments; and when exposed to similar sample corpuses, children tend to arrive at similar grammatical hypotheses. Within the context of creative science itself, there are a remarkable number of multiple independent discoveries. Although this sort of data is hardly conclusive, it does suggest that there may be common underlying mechanisms at work in the procedures that different people use when confronted with problems of everyday and scientific discovery.

The drift of much of recent epistemology and cognitive psychology is to view such mundane activities as perceiving, language acquisition, and common sense explaining as continuous with the more self-conscious activity of science. The substantive idea behind this working hypothesis is that the evaluation, decision, and discovery procedures to be found at work in more mundane cognitive activities do double duty in scientific method. There are not two concepts of confirmation, explanation, acceptance, and simplicity, one employed in 'ordinary' cognition, the other finding its place in the scientific method. If this thesis of 'the unity of cognitive processes' is correct, then discovering a scientific theory should have a logic if discovering a grammar or formulating a perceptual judgement does.

3. The Explanation of Error

So far, we have examined some of the ramifications of Frege's
variability argument against psychologism. This argument, and its subsequent amplification by the positivists, aimed at showing that the rules of correct reasoning studied in logic and epistemology do not have psychological reality. As we have seen, Frege’s attack on this kind of psychologism focused on the assertion that there are rules of inference which all people use in cognitive activity. Although we have tried to identify weaknesses in Frege’s criticism, a much simpler objection to the psychological reality hypothesis presents itself: granted that some rules of reasoning have psychological reality, why think that these psychological real ones are correct? After all, people make errors in their reasoning and often explicitly hold beliefs about rational methodology that are mistaken. Doesn’t the fact that people are so often mistaken in the theory and practice of reasoning show that even if some rules of inference are psychologically real, these probably are not ones that any logician or epistemologist would sanction as valid?

Because Kant accepted the kind of psychologism we are exploring, realizing full well that human beings make mistakes of judgement, it is worthwhile reconstructing the considerations that led him to his psychologism. Kant reached the conclusion that rules of correct reason have psychological reality by returning again and again to the defence of a conditional: If an organism of a certain kind is to have experience, then that organism must use principle P. The kind of organism that Kant was interested in is one that is capable of self-consciousness and that learns about the world through sensory contact with it; Kant was not interested in amoebas or in Leibnizian creatures who can learn about the world through pure reflection. The principles that Kant argued for via the above conditional include the law of causality, the law of noncontradiction, and so on. For Kant, the having of experience involves the creation of a coherent unified body of perceptions of the external world of objects. Kant acknowledges that one can have a sort of mental life which fails to constitute experience: this would consist of a ‘blind play of representations’, a ‘rhapsody of perceptions’ that is ‘less than a dream’ in which ‘appearances crowd in on the soul’ (see, for example, Critique of Pure Reason, A111, A112, B196). The alternative to experience seems to be a booming buzzing confusion.

From the fact that we mortals have experiences, as opposed to a disjoint and incoherent flow of appearances, Kant was able to conclude from the above conditional that we do in fact use certain principles. This is a psychological consequence—a consequence which leads Kant to think that the laws of logic are constitutive of the laws of thought. Kant also uses this conditional as a means to argue for
the truth of the principles in question. On the face of it, there is a huge gap to be surmounted between a proposition's being something we must assume if we are to have experience and that proposition's being true. Transcendental idealism was the vehicle for closing the gap. Frege and other realists must resist this kind of argument, in that it constitutes a kind of metaphysical psychologism: it collapses the distinction between statements concerning physical reality and statements concerning our experience of that reality by interpreting the former as reports about phenomenal objects. But even accepting this kind of realism, the Kantian conditional can be used in the service of questions of justification. The Kantian conditional confers on the principles it treats the same kind of vindication that the pragmatists tried to secure for the principle of induction: If there exists an inductive method which can provide us with knowledge, then the straight rule must be capable of providing us with knowledge. This is not the strongest kind of justification one might try to secure for a principle, since it is irreducibly conditional. But it is yet quite significant, if it can be achieved. The proof of the Kantian conditional would be tremendously significant, even if purged of its idealism.

If human beings have experience and if the having of experience implies that thought proceeds in accordance with certain principles which are themselves justified, how can there be gross errors of thought? A Kantian account of the occurrence of errors, madness, and irrationality must take the following form: Even the experiences of the most deranged persons conform to the categories. Errors arise in the applications made of the categories and through the host of empirical beliefs that also figure in cognition. The categories specify only the form that all thinking takes; the contents of thought go beyond the dictates of the categories, although the contents of thought must be consistent with the categories. Kant must hold that all irrationality and error, in so far as it can be said to occur within thought and within experience, is not due to a violation of certain fundamental principles, such violation being impossible. Thus, in the Introduction to Logic, Kant writes:

But how error is possible in the formal sense of the word, that is, how a form of thought inconsistent with the understanding is possible; this is hard to comprehend; as indeed in general we cannot comprehend how any faculty can deviate from its own essential laws. We must not therefore look for the source of errors in the understanding itself and its essential laws, any more than in the limits of the understanding, in which indeed the cause of ignorance lies, but by no means that of error. Now if we had no faculty of knowledge except the understanding, we should never err. But besides the
understanding there is in us another indispensable source of knowledge. This is the sensibility, which supplies the material for thought, and besides works according to different laws from the understanding. From the sensibility, however, considered in and by itself, error cannot arise, since the senses do not judge.

Hence, the origin of all error must be sought solely in the unobserved influence of the sensibility on the understanding, or to speak more exactly, in the judgment. It is owing to this influence that in our judgments we mistake merely subjective reasons for objective, and consequently confound the mere semblance of truth with truth itself.\(^{19}\)

Unfortunately, the Kantian explanation of error just cited is not the one that Kant always gives. In the section of Book I of the *Anthropology* called ‘On Deficiencies and Diseases of the Soul with Respect to Its Cognitive Power’ (52.4), Kant talks about the sickness of *Vesania* which is the sickness of a deranged *reason*. In this ‘kind of mental derangement there is not merely lack of order and deviation from the rule for the use of reason, but also *positive unreason*; that is, another rule is present’.\(^{20}\) A person who suffers from this malady might think himself capable of doing what normal men can see to be impossible. Kant cites as examples thinking that one can square the circle, invent perpetual motion, unveil the supersensible forces of nature, and understand the mystery of the Trinity. Although one can imagine explaining these ‘deviations’ as misapplications of fundamentally correct principles of reason, Kant seems to suggest that they have their source in the employment of incorrect rules of thought. Kant goes on to remark, however, that ‘it is astonishing . . . that the forces of an unhinged mind still arrange themselves in a system and that nature strives to introduce, even into unreason, a principle that will connect them so that the power of thinking does not remain unemployed.’ Here we see the more familiar Kantian theme: it is inevitable that cognitive activity strive towards the creation of systematic unity.

The kind of explanation of error and irrationality that Kant advocates is precisely what is involved in Chomsky’s distinction between competence and performance.\(^{21}\) Instances of irrationality are consistent with psychologism in the same way that grammatical slips are consistent with viewing native speakers as having internalized the grammar of their language. In both cases, one posits a mechanism which endows people with perfect rationality or with perfect grammaticality, and then one posits in addition various devices which provide interferences with the smooth functioning of the basically ‘correct’ mechanism.\(^{22}\) Instances of ungrammatical utterance or irrational belief and behaviour are to be explained as the
impingements of interferences like lapses of memory, headaches, substantive prejudice, and so on. Psychologism does not depend on our being perfectly rational; it depends, rather, on the truth of the kind of model just sketched.

An alternative to this kind of model, with its two parts of perfect rationality plus interference devices, would be a model in which the fundamental mechanism is not perfectly rational. For example, instead of explaining the human propensity for affirming the consequent or for committing the gambler’s fallacy as arising out of an interference, one would posit a primary mechanism which itself issues in such fallacious interferences. The same alternative for linguistic theory would be to account for ‘deviant’ utterances as issuing from grammatical competence. For example, one might seek to explain the fact that an English speaker once said ‘He he went to the store’ by claiming that the grammar of English dictates that the word “he” shall be repeated in certain circumstances. The obviously more plausible alternative is to explain this repetition as a lapse in performance; the grammar of English, which characterizes competence, sanctions no such repetition.

How are we to decide between these two kinds of models? One sign that the positing of perfect rationality or grammaticalness is preferable is that the ‘errors’—the lapses in rationality and the occurrence of ‘deviant’ utterances—are occasional and unsystematic. In line with our reply to Frege’s variability argument, what is psychologically idiosyncratic is not to be accounted for as a consequence of fundamental law. If logical rules sanction or proscribe inferences on the basis of their form, then to posit an invalid rule of inference in the laws of thought is to say that there is a potential infinity of invalid inferences that people would sanction. Similarly, to try to explain an ungrammatical lapse in performance by positing a grammatical rule which sanctions this quirk is to commit oneself to predicting that the same lapse will occur again and again. Of course, one might so frame the rules of grammar so that they sanction ‘deviant’ utterances which occur very seldom. For example, one might try to explain the repetition mentioned above by claiming that English grammar contains the rule ‘Repeat the initial word iff \( \phi \)’, and then fill in \( \phi \) in such a way that it is rarely true. But surely this rule is a philosophical joke; it is too \textit{ad hoc} to count as a serious alternative conjecture. All this is not to say that there is something a priori mistaken about positing irrational laws of thought. But the laws of thought are so general and fundamental—determining, after all, the totality of cognitive activity—that an irrational logic is plausibly posited only if there is fairly widespread and uniform irrationality at the level of behaviour.
But suppose that certain kinds of irrationality are uniform across subjects: How are we to know whether to attribute such lapses to the normal workings of an imperfectly rational information processing system or to performance factors like memory or substantive prejudice? Chomsky confronts an analogous problem in *Syntactic Structures* (pp. 23-5) when he considers how one is to account for speaker judgements on the acceptability of various strings. For example, there are infinitely many unacceptable strings generated by the rule:

\[(\text{if})^n \text{ snow is white (then grass is green)} : n \geq 0.\]

Chomsky argues that all of the strings generated by this rule are English sentences. The unusability of most of the generated strings is seen as a result of limitations on the computation space available to native speakers. An alternative explanation would replace the above rule by one which would generate only the 2 or 3 strings that are acceptable to native speakers: ‘Snow is white’, ‘If snow is white then grass is green’, and perhaps ‘If if snow is white then grass is green then grass is green’. This rule would differ from the one above merely in restricting the values of \(n\) in some suitable way. Chomsky argues that the more general rule, which generates an infinite number of unusable strings, is preferable because it is simpler and because all of the sentences generated by it can be given truth conditions.

The use of simplicity in this context is to be understood to have precisely the same point that simplicity considerations have in the rest of science. Simple generalizations are to be preferred to more complex ones, whether the domain at issue is celestial mechanics or cognitive psychology. However, in his more recent writings, Chomsky has emphasized an additional role for simplicity considerations. The language acquisition device that people use to learn their language is to be understood as including a simplicity metric. Those features of the grammar of natural languages that are learned and not innate are to be accounted for, at least in part, by appeal to rules of hypothesis choice that are psychologically real.

Dictates of simplicity may similarly be crucial in assessing the worth of our psychologistic hypothesis. Psychologism asserts that instances of irrationality are best explained in the way Chomsky would seek to explain speaker judgements on the acceptability of strings. It may be simpler, all else being equal, to posit a perfectly rational mechanism subject to interference and error to account for human cognition. Simple irrational rules of inference (like one sanctioning the fallacy of affirming the consequent) may predict more errors than are consistent with human behaviour. Perhaps the only irrational rules of inference whose psychological reality is
consistent with actual human performance will be extremely complex. If so, simplicity may favour the model of rational mechanism subject to interferences.

A paradigm of this kind of explanation can be found in work in visual illusions. The explanation of errors of perceptual judgements usually goes like this: Various cues represented in the information extracted by retinal fixations are interpreted in a way that is usually highly reliable. But the particular stimulus conditions considered are unusual, and reliable procedures lead the perceiver to formulate incorrect perceptual judgements. The important thing to notice about such explanations is that rules governing the formation of perceptual judgements (which dictate how the cues are to be interpreted) are regarded as fundamentally reasonable; error is attributed to the introduction of highly unusual inputs. As an example, consider the famous Ames room experiments in which adults of normal size can be made to look like dwarfs and giants. The perceiver assumes that the room being perceived is, like the rooms of his experience, a rectangular solid. This hypothesis clashes with the subject’s belief that most adults are fairly similar in size. The perceiver adjudicates this conflict of evidence by holding on to the hypothesis that the room is normal. When one of the people in the room is a person that the subject knows, however, there is a marked tendency in favour of holding on to the belief that that person is still the same size (rooms that shrink people being unheard of) and therefore seeing the room as distorted.

The explanation of apparent motion also fits into our pattern. When dots of light are flashed at suitably arranged intervals, durations, distances, and intensities, the subject will seem to see one dot of light move and join the second. This illusion is what makes movies possible; stationary images are projected in a series and the viewer furnishes the impletions. Is it plausible to view the mechanism that gives rise to this illusion as being rational? Given the ecological importance of detecting motion, it is highly adaptive for motion detection to err on the side of safety. Also, the impletions that the subject furnishes in apparent motion often seem to be formally quite similar to what scientists would regard as optimal solutions to a curve-fitting problem. Interpolating a smooth curve to join a series of data points is good inductive policy, whether this be done in the context of self-conscious theorizing or in the context of visual perception.

In contrast to psychological explanations which conform to the pattern described above, there is at least one area of psychological theorizing in which the opposite approach is prominent. This focuses
in the inferences made by children. Piaget has sought to explain certain behaviours by positing mechanisms which depart from the canons of logic that we regard as correct. In the light of this we could weaken our psychologistic hypothesis by restricting it to adults. However, I don’t think that Piaget has provided a good reason for doing this. The way Piaget seeks to explain the results of his conservation experiments will illustrate the point I wish to make. In one of these experiments, a child is shown two identical glasses A and B filled to the same level with water. The child is asked if the two glasses have the same or different amounts of water in them. The child will say that they are the same. Then, while the child watches, the water from glass A is poured into glass C, which is taller and narrower. Glass C is next to glass B, and of course the water in C has risen to a higher level than that in B. Up until the age of six, the child will tend to say that there is more water in the tall narrow glass. Piaget describes this by saying that the child does not have the concept of conservation until a certain age. For Piaget, the change from nonconserver to conserver is not to be viewed as a change in one particular belief. Rather, Piaget views a new thought schema as having taken hold; the child has entered a new developmental stage.

The hypothesis that the transition from nonconserver to conserver represents the acquisition of a new form of thought appears implausible, however, once one sees the formal affinities between the ‘principle’ of conservation of volume of liquids under pouring, the ‘principle’ of conservation of mass of solids under rolling, and the ‘principle’ of conservation of colour of teddy bears under holding. None of the above is really a principle; each constitutes a highly specific fact about what happens to an object of a certain kind when the object is operated on in a certain way. What unites all these specific beliefs is that they are all beliefs about conservation. But a sensitivity to invariances in the environment is probably a constraint on learning that is present from the very first. The child’s earlier acquisition of the concept of object constancy is formally identical to the later acquisition of the concept of conservation of liquids under pouring, and this in turn is formally identical to the still later acquisition (in a college physics class, perhaps) of the Newtonian concept of conservation of energy. Children in their perceptual beliefs, like scientists in their self-conscious theorizing, try to discover properties of phenomena that do not change. The search for constancies is not characteristic of a new developmental stage; it seems to be characteristic of all developmental stages.

There have been other challenges to the psychological reality hypothesis we are considering. Lévy-Bruhl, for example, has
Elliott Sober claimed that primitive peoples use a prelogical rather than a logical form of thought. And there is a line of contemporary thinking in psychology which understands schizophrenia as a disease in which usual logical modes of thinking are replaced with other forms of reasoning. Unfortunately, these cannot be explored here. Although these positions are not a priori mistaken, as I will argue in section 4, they are always subject to the following difficulty. It must be shown that the ‘unusual’ beliefs that the people in question have are not the result of perfectly rational inferences from mistaken premises, but that they issue from some basic property of the inferential machinery itself. It will usually be very hard to provide straightforward empirical backing for this kind of claim. What this shows is not that the irrational mechanism hypothesis is ‘unverifiable’, but rather that distinguishing between it and the rational mechanism hypothesis may often be difficult.

At this point, it might become tempting to think that there is no substantive difference between these two kinds of models because they are equivalent in terms of the behaviours they predict. This sort of conventionalism is no more plausible here than it is in physics. Two physical theories may be predictively equivalent but different in truth value because they give divergent accounts of the causal mechanisms responsible for observable phenomena. There is no reason to forbid psychology from making the same distinction. It is one thing for a computer to generate an output because it followed the computations dictated by its programme; it is something different for the computer to yield the output because its computational machinery was subject to some interfering force. This distinction between programme and interference is useful in psychology, even though we are not in a position to reach inside people’s heads and produce a physical object which represents the programme.

Psychologism’s claim that valid rules of inference have psychological reality is consistent with the fact that people often acquire beliefs about rational inference that are wildly mistaken. When this occurs, the advocate of psychologism need only say that the unconscious rules of information processing are one thing, explicit avowals of method are another. There may be great variability within our species as to the methodological maxims that have been advanced and put into practice. But psychologism sees an underlying invariant system of rules of inference at work, and in addition asserts that these rules are rational ones. Thus, psychologism, if true, identifies an important sense in which the scientific method is not parochial. Although in different cultures people may entertain different self-conscious canons of hypothesis choice, all human beings obey the
scientific method in their basic techniques for processing information.

4. Is Psychologism A Priori True?

The Kantian conditional seems to provide one road to psychologism, as we have seen. The conditional allows one to infer that an organism uses a particular set of organizing principles from the mere fact that the organism can have experiences. It is worth examining whether one can hope to conclude so much from so little. From the mere fact that a computer can solve a class of problems, one can conclude very little about the particular inferential routines that it follows. How can psychologism, which is a thesis about the character of the rules represented in the organism's basic information processing programme, extract such strong conclusions from so slender an empirical basis? As we have seen, Kant packed a great deal into his notion of experience; at the very least, it implies coherence and system. But even accepting this idea of what experience is, and accepting the claim that our mental life consists in the having of experience in this sense, we might ask if there still isn't much room for latitude. Why should the set of fundamental organizing principles that are logically required for coherent experience be unique?

The discovery of non-Euclidean geometries is usually cited as disproving Kant's uniqueness claim. Kant thought that Euclidean geometry is an indispensable tool that every sentient being uses to organize his experience; the discovery of alternative geometries suggests that an organism might be able to make do with some non-Euclidean alternative. One can imagine a similar conclusion being drawn from the proliferation of nonclassical logics in this century. Kant's view that classical logic is the only alternative has been undermined, and with it the idea that the Kantian conditional is valid: From the fact that an organism has experiences, nothing much follows concerning the logic that it uses.

One might expect discussion of the psychological reality of logic to parallel discussion of the psychological reality of geometry, but this doesn't seem to have happened. Several recent philosophers have attempted to show that any sentient organism must use classical logic, and the form of argument is remarkably Kantian. Quine, for example, has argued that the mere activity of translating someone's language commits one to attributing to him a belief in classical logic. He criticizes Lévy-Bruhl, not by mustering any specific empirical
data against the hypothesis of a primitive prelogical mind, but by considering general methodological maxims of translation.\textsuperscript{28}

Given the fact that some logicians have actually advocated systematically worked out logical systems which are inconsistent with classical logic, it is a remarkable hypothesis indeed that these logics are inherently unbelievable. Intuitionists say that some of the rules of classical logic are invalid. Can these avowals of disbelief simply be dismissed? Are we to read classical logic into our intuitionist colleagues, despite their protests? This is not to say that a person's asserting a logical principle inevitably means that he really believes it. Nor is it to deny the possibility, considered at the end of Section 3, that intuitionists may consciously advocate one method of thinking while their unconscious information processing devices silently obey another set of maxims. But in the case of intuitionists, it is quite clear how their nonclassical logic specifies a well-defined kind of mathematical practice. If we were observing these mathematicians at work, without hearing them expressly state their logical views, we might well conclude that the best explanation of their behaviour is that they advocate a nonstandard logic. The psychological reality hypothesis we reach here deviates from the one Quine thinks is universally correct. This suggests that there can be no a priori defence of psychologism. The mere fact that someone has experiences, or has beliefs, or speaks a language, entails very little about the specific character of the rules of inference that he follows.

There is a more modest version of this a priori defence of psychologism. It selects a simple inferential principle which is advocated by virtually all logics, and asserts that any sentient organism must think in accordance with it. Instead of claiming that every thinking being must believe the whole of classical logic, this position asks us to consider the hypothesis that every organism capable of having beliefs must use the principle ‘From $A \& B$, infer $A$’. There is some plausibility in the idea that any individual who can have beliefs at all must obey this principle of conjunction deletion. It isn’t simply \textit{rational} to accept this rule. It is part of the very notion of belief to do so. Here again, the Kantian conditional is pressed into service.

But even here, the Kantian conditional concludes too much. Consider a rule of inference which is behaviourally indistinguishable from that of conjunction deletion. For example, the rule ‘from $A \& B$, infer $A$, except in circumstance $C$’ would be a behaviourally indistinguishable alternative for an organism, if that organism never thought that it was in circumstance $C$. Using this bizarre rule would lead to the same beliefs as those indicated by conjunction deletion. If this is so, then the mere fact that an organism has beliefs doesn’t
imply that it uses conjunction deletion rather than some bizarre alternative.

The thought experiment just described is meant to show that the kind of evidence on which we base attributions of beliefs could be generated by many different internal mechanisms. We do not narrowly define the notion of 'program' as it is used to describe computers in such a fashion that there can only be one 'program' which could generate a given set of input–output relations. For just the same reason, we should not narrowly restrict the concept of belief so that only one set of logical beliefs can be the internal mechanism for generating linguistic and other kinds of behaviour. The hypothesis that conjunction deletion has psychological reality and the hypothesis that the bizarre alternative has psychological reality may both be consistent with observed behaviour. We do not choose between them on a priori grounds, but on the empirical ground that the claim that conjunction deletion has psychological reality is simpler. This consideration was discussed in Section 3.

Although I think that simplicity arguments constitute an entirely acceptable basis for discriminating between these two hypotheses, there is another consideration which leads to the same conclusion. If it could be shown that the process which leads to rules of inference becoming psychologically real would favour the use of conjunction deletion, rather than our bizarre alternative, this would be an additional and considerable source of confirmation. This consideration would not supplant the simplicity considerations just alluded to, but would bolster them. Given a black box, it is entirely reasonable to accept the simplest hypothesis concerning its underlying mechanism. But if one can show that the process which produced the black box itself obeyed certain regularities, this can provide additional evidence concerning the internal mechanism.

The place to look for this additional argument in favour of psychologism is the theory of natural selection. It has often been argued that the emergence of mental abilities, and particularly the evolution of rationality, lie outside the scope of evolutionary theory. Although I think that the reasons advanced for this claim are unconvincing, I will not discuss them here. It is worth noticing, however, that one cannot hold that an evolutionary account of the psychological reality of logic is possible and still believe that psychologism is a priori true. The reason for this is that the basic form of an evolutionary explanation pictures alternative traits enjoying different degrees of reproductive success. One cannot give an evolutionary account of the prevalence of a logically inevitable trait. So a naturalistic epistemology which places psychologism in a biological context
must reject the a priori arguments in favour of psychologism just considered.

5. Genetic Questions and Antipsychologistic Epistemology

One of the legacies of Frege's variability argument is the view, held by Frege himself, that genetic questions about how someone comes to believe a proposition are irrelevant to epistemological questions concerning the proposition's justification. The epistemological categories that Frege allowed himself, for example the category of a priori knowledge, were supposed to be untainted by psychological considerations. In just the same way, the positivists dismissed the context of discovery and viewed the proper task of epistemology as being one of rational reconstruction, where this kind of project was supposed to be distinct from psychological concerns. In both of these cases, it is worth asking whether the epistemological tools that were viewed as allowable were completely purged of psychological content. Although I believe that there are significant psychological elements in the notion of a priori truth, there are too many issues involved for a defence of this view to be presented here. The notion of rational reconstruction is a somewhat simpler matter, however, and so I will try to make manifest its latent psychological content. I then will argue that an epistemology which retreats from genetic questions in the way advocated by the positivists cannot hope to succeed in one of its fundamental tasks—the refutation of scepticism.

In *The Logical Structure of the World*, Carnap tries to show how the totality of our beliefs and concepts might have been constructed. He begins with concepts and beliefs concerning one's own psychological states, and tries to show how these might allow one to acquire concepts and beliefs concerning external physical objects; these, in turn provide the basis for our knowledge of the mental states of others, and this level in turn provides the basis for our knowledge of cultural and social objects. Carnap thought that this pattern of construction was one among several that an epistemologist might pursue. One might equally try to derive knowledge of one's own mental states from claims about external physical objects. The virtue of the procedure Carnap followed was that it obeyed a principle of epistemological primacy. We must construct physical objects statements out of autopsychological ones because the former are knowable only through knowledge of the latter.

If Carnap is right in holding that his order of construction is
ordered by the relation 'x is epistemologically more primary than y', then it follows that no human being could acquire his concepts and beliefs in any other order. This is a psychological conjecture which is similar in structure to the claims advanced by developmental psychologists like Piaget and Bruner, who assert that there are a number of stages of cognitive development and that there is just one temporal ordering of stages which is possible in development. Carnap's project differed from that of cognitive psychology, however, in that he was not interested in how people in fact construct their physical object concepts and beliefs from an autopsychological basis. Rather, Carnap wanted to describe how people might achieve this advance. Here the focus of philosophical attention is on psychological possibility, not psychological actuality.

What Carnap was trying to do is hardly alien to the spirit of psychological investigation. In computer models of thinking, one can try to investigate cognitive activity by constructing an artificial intelligence programme or by providing a machine simulation. In the former, all that is required is that the programme be input-output equivalent with the human system under study. In the latter, not only must the programme be input-output equivalent, but the intervening processes must closely approximate (or, in the limit, be the same as) those that really occur in the organism. Carnap's epistemology aimed at providing an artificial intelligence account of the basic features of cognitive development.

What makes Carnap's construction distinctively philosophical is not its ignoring what is psychologically actual. The distinctively philosophical aspect of rational reconstruction is its normative component. Carnap hoped to provide a way in which we might construct beliefs from an autopsychological basis which would show why the totality of beliefs constructed in this way counts as rational. The construction was to simultaneously be a justification. What I now want to argue is that Carnap's retreat from psychological actuality makes it impossible for him to successfully discharge this normative task.

There are many sceptical challenges; one of them consists in the assertion that we do not have any rational basis for our beliefs. An epistemologist will typically try to meet this challenge by giving a characterization of what rationality is, and then showing that our beliefs fulfil the requisite conditions. How could Carnap's results be used to show, for example, that we are rational in believing that there is an external world of physical objects which we experience? Let's suppose that this belief is related to beliefs about our own psychological states by some relation of Carnapian justification. This does not
show that any of us is justified in believing in an external world. Maybe we believe this for entirely different reasons, or for no reason at all. Carnap's justificatory strategy at most shows that if we in fact had arrived at our beliefs in the way he describes, then our beliefs would be justified. But given that his construction is simply the description of a psychological possibility, the most we can say about our own beliefs is that they may be justified. Rational reconstruction provides only for the possibility of knowledge; if, in addition, we are interested in whether we in fact do know anything, we must go beyond speculative reconstruction and see how beliefs in fact are arrived at.

If one individuates methods for constructing beliefs finely enough, there probably are many methods of rationally constructing beliefs. Think of the different rational computational procedures that there are for finding out the product of 34 and 23. An epistemology which can characterize even one such method for the formulation of empirical beliefs will have made an enormous advance. For it to be used to refute the sceptical claim that we are not rational in believing what we do, one must have some characterization of the procedures that we in fact use in the construction of our beliefs. These psychologically real procedures need not turn out to be the procedures that the epistemologist has identified, but they had better fall within the equivalence class of rational methods. To refute scepticism, one must have a formulation of one rational constructive method, a characterization of the method that is psychologically real, and an argument that the epistemologically sanctioned method and the psychologically real one bear an appropriate equivalence relation to each other.

Our discussion of how Carnap's rational reconstruction allows one to meet a sceptical challenge has been formulated in terms of discovery procedures. We pictured Carnap as supplying a set of epistemologically sanctioned discovery procedures and then asked how this would help show that we are rational in believing what we do. Nothing turns on our formulation in terms of a logic of discovery; the same issues arise when we are faced with an evaluation procedure that an epistemologist claims to have justified. Beliefs chosen in accordance with this policy are rational; the question that the sceptic forces us to face is: were our beliefs chosen in accordance with this policy, or in accordance with some policy which is equivalent to that policy (in some yet to be specified sense of equivalence)? This is a question about our actual psychological make up. Only if the answer is 'yes' can the sceptic be refuted.

It is remarkable that genetic questions have been viewed as irrelevant to epistemological questions about justification. To try to show
that I rationally believe something because I might have arrived at the belief by a certain procedure is like trying to show that I have a disease because I might have been exposed to a particular infection. All one gets from possibilities are more possibilities. Causal theories of knowledge have refocused philosophical attention on the actual genesis of belief. A similar sort of reorientation is required on the theory of rationality, if that theory ever hopes to show that we are sometimes rational in the beliefs we hold.\footnote{\textsuperscript{31}}

6. Conclusion

Although logical truths are not true because we think in a certain way, they nevertheless may have psychological reality. This version of psychologism avoids the abdication of normative inquiry that is implicit in some psychologistic views; it also acknowledges a certain empirical possibility—that the principles of right reason which philosophy seeks to discover are used in the information-processing systems of thinking organisms. The in principle arguments advanced by Frege and the positivists against this form of psychologism can be met, and there is some amount of evidence from the work of psychologists to suggest that cognition may well have the characteristic that psychologism attributes to it.

These psychological considerations in favour of psychologism can be bolstered by a philosophical inquiry into the nature of rational belief. I have argued that if someone rationally believes a given proposition, then the person must have arrived at that belief by rational methods. On the naturalistic assumption that some significant number of our beliefs are rationally held, it becomes reasonable to suppose that our methods of information processing closely approximate the ones that epistemology seeks to discover.

To maintain that rational reconstruction can show why we are rational in believing what we do is to hold a fictionalist view of epistemology. This position holds that rational agents need not use epistemologically sanctioned canons of inference but need only have beliefs that would have been sanctioned by these canons. In keeping with their general antirealist attitude towards scientific theories, the positivists espoused an antirealist attitude towards epistemology itself. Rational reconstruction is the epistemology of As—If. A thoroughgoing realism will reject fictionalism everywhere: in physics, in psychology, and in philosophy. One never explains why an individual has a given property simply by saying that it would have had that property had it undergone a certain process.
If epistemologically sanctioned procedures are constitutive of the laws of thought, a large area of inquiry can be shared between philosophy and psychology: inductive logic may have much to learn from learning theory, and cognitive psychology may be able to gather insights from theories of hypothesis choice. Our knowledge in these two areas of inquiry is so rudimentary that it is premature to claim that psychologism is well-established. Yet it is a live option, and also delimits a fruitful line of inquiry.

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NOTES

1 In his aptly titled *Neue oder anthropologische Kritik der Vernunft* (Sämtliche Schriften, Scientia Verla Aalen, 1967, vol. 4, p. 407) Fries writes 'I do not prove that all substance is permanent; rather I point to the fact that the principle of the permanence of substance lies on every finite mind.' I owe this translation from the German to Alexander Mourelatos's useful article 'Jakob Fries', in P. Edwards (ed.), *The Encyclopedia of Philosophy*, Macmillan, 1967, vol. 3, pp. 253–5. In his 'The Impossibility of the "Theory of Knowledge"' (Socratic Method and Critical Philosophy, Yale University Press, 1949), Nelson argues that no criterion for the correctness of a cognition can be justified noncircularly.


5 *Simplicity*, Oxford University Press, 1975, ch. 5.


9 The idiosyncratic, and ultimately incommunicable, character of psychological phenomena is a motif that appears again and again in Frege's work. See, for example, *The Foundations of Arithmetic*, Blackwells, 1968, p. xvi.


16 Donald Campbell, 'Evolutionary Epistemology', in P. Schilpp (ed.), *The Philosophy of Karl Popper*, Open Court, 1974, p. 460.

17 See, for example, Richard Gregory's account of visual perception as a process of hypothesis formation in his *The Intelligent Eye*, Weidenfeld and Nicolson, 1970. In *Simplicity*, op. cit., I argue that the concept of simplicity used in scientific hypothesis choice can also be used to explain language acquisition and the formation of perceptual judgments.


22 In *The Laws of Thought* (Dover, 1958, p. 409), George Boole points to the same kind of explanation: '... the phenomena of incorrect reasoning or error, wheresoever presented, are due to the interference of other laws with those laws of which right reasoning is the product.'

23 See, for example, Noam Chomsky and Morris Halle, 'Some Controversial Questions in Phonological Theory', *Journal of Linguistics*, 1(2), 1965, pp. 101 and Chomsky's *Aspects of the Theory of Syntax*, op. cit., pp. 30—7. Chomsky and Halle think that a language acquisition device should be posited which does not accept grammatical hypotheses that linguists would regard as *ad hoc*. Chomsky and Halle's proposals about how this is to be done are discussed in chapter 3 of my *Simplicity*, op. cit.

24 See Paul Kolers' *Aspects of Motion Perception*, Pergamon Press, 1972 for a discussion of this rich and complex body of phenomenal. The view of apparent motion as a perceptual curve-fitting problem is sketched in more detail in chapter 4 of *Simplicity*, op. cit.


27 In his *Interpretation of Schizophrenia*, Brunner, 1955, Silvano Arieti takes the view that schizophrenics use a 'paleologic' which departs from usual canons of valid inference. Loren Chapman and Jean Chapman, on the other hand, argue that the reasoning of schizophrenics differs in degree but not in kind from that of normal people. The Chapmans try to explain errors in terms of subject matter effects, rather than in terms of the application of different rules of inference. See their *Disordered Thought in Schizophrenia*, Appleton-Century Crofts, 1973.


30 Rudolf Carnap, op. cit., p. 89.

31 This argument applies to ethical theories which characterize the moral features of an action in terms of what would have been done in some counterfactual situation. The pre-eminent example of this kind of account is the social contract theory, which receives its most recent articulation in John Rawls' *A Theory of Justice*, Harvard University Press, 1971.