read paper "Empiricism, Semantics, and Ontology" (1950), had maintained that the acceptance of a "linguistic framework" does not involve any ontological commitment. In support of this position, he had introduced a distinction between two kinds of questions of existence. Internal questions of existence ("Are there prime numbers above a hundred?," "Are there unicorns?") are meaningful and are settled by logical or empirical inquiry, depending upon whether the framework is formal or factual. External or "ontological" questions ("Are there numbers?," "Are there things?") are not real questions at all but, as the Vienna Circle had contended two decades before, are strictly meaningless. The selection of linguistic frameworks is a practical question, then, rather than one of theory. Thus what is "pragmatic" for Carnap is not the choice of ontologies but the choice of frameworks.

A year later Quine replied. He pointed out a number of obscurities in Carnap's internal-external dichotomy and concluded that the distinction was of no use whatsoever. Besides, he noted, it was unnecessary. Welcoming Carnap's "pragmatic" approach to the problem of choosing linguistic frameworks, Quine suggested that all that was really needed was for Carnap to cast the protective mantle of his "pragmatism" over ontological and scientific questions as well. The suggestion, needless to say, was not adopted.

Quine's case has seemed rather persuasive against the devices marshaled by Carnap in his renewed rejection of ontology. But since Quine's own pragmatic ontology is still to be presented in a comprehensive and explicit manner, the debate has remained inconclusive. It is therefore good to learn that the inquiry and the dialogue continue: in March of this year Carnap lectured at the University of Hawaii on the subject "Semantics and Abstract Entities," and at Princeton in the same month Quine read a paper which bore the working title "Existence and Quantification."

Thus far Quine has had a slight edge, perhaps, in the argument over ontology. As to the problem of logical truth, however, there is some question. Here the relevant material includes two lengthy essays, "Truth by Convention" (1935) and "Carnap on Logical Truth" (1954), a short general statement "Necessary Truth" (1963), and a brief but sharp treatment in "Mr. Strawson on Logical Theory" (1953), which is Quine's review of Peter Strawson's *Introduction to Logical Theory* (1952). The 1935 paper foreshadows the main points of controversy. It questions the sense, if any, attaching to the common assertion that mathematics and logic are "purely analytic or conventional," in contrast to the physical sciences with their supposed "non-conventional core of doctrine." It suggests that the real contrast is only that between more or less firmly accepted statements.

The issue with Carnap over logical truth is directly joined in "Two Dogmas of Empiricism" (1951). One dogma is the empiricist belief in a "fundamental cleavage between truths which are *analytic*, or grounded in meanings independently of matters of fact, and truths which are *synthetic*, or grounded in fact." The empiricist characterizes a sentence as analytic if it either is a logical truth or becomes one when synonyms replace synonyms. To this Quine objects that synonymy is as much in need of clarification as "analyticity." He then enters similar complaints about Carnap's alternative accounts of analytic sentences. Thus, according to Quine, the analytic-synthetic distinction fails, and with it the attempt to draw a sharp boundary between the (formally) analytic truths of logic and the factual truths of empirical science. The difference is one only of degree, and "turns upon our vaguely pragmatic inclination to adjust one strand of the fabric of science rather than another in accommodating some recalcitrant experience."

The fullest account of Quine's view of logical truth occurs in his extremely interesting 1954 essay on Carnap, first published complete in English in 1960. This paper is a sustained attack on what Quine calls "the linguistic doctrine of logical truth." He rests his own notion of logical truth on the concept of logical particle (such as "not," "and," "all") and on the general notion of truth. Accordingly, logical truths are true sentences that involve only logical particles essentially, the latter thought of as being given in some enumeration. He then seeks to show that such truths (whether the word "logical" is confined to elementary logic or is extended to embrace set theory) are no more "true by convention" than are the hypotheses of natural science. Both involve choice, both involve "confrontations" with experience however indirect or remote. Hence no sharp line can be drawn between them. Short of *all* truths being true by convention, none are. Thereafter, Quine reviews and finds unavailing Carnap's various attempts to characterize logical truth first syntactically and later semantically. Again rejecting the analytic-synthetic dichotomy, Quine closes with:

> The lore of our fathers is a fabric of sentences. . . . It is a pale gray lore, black with fact and white with convention. But I have found no substantial reasons for concluding that there are any quite black threads in it, or any white ones.

It is not likely, however, that the last word on analyticity and logical truth has been said. Has Quine really succeeded in showing that "analytic" truths do not differ in kind from empirical ones? Is the difference between "All black dogs are black" and "Some dogs are black"—to use Carnap's example—only one of degree? And if so, what are we to understand by "degree"?

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Without Benefit of Computer

Thought and Choice in Chess. ADRIAAN D. DE GROOT. Translated from the Dutch edition (Amsterdam, 1946). Basic Books, New York, 1966. 479 pp., illus. $10.50.

Here is a translation of a book written in the early '40's by a Dutch psychologist working within a framework created by a German psychologist, Otto Selz, whose work was published in the early '20's. Can it be relevant to living science, *anno* 1967? The answer is yes, and thereby hangs partly a minor tale in the history of psychology but mostly a tale of the viability of data when the time is ripe.

To the minor theme first. Written as history a science gives the appearance of orderly movement performed to a stately dialectical minuet. Prior to the turn of the century psychology emerged from its subordination to philosophy. It was experimental, viewed itself as the science of the contents of the mind, and held to a theory of the association of ideas. Then occurred the reactions. Behaviorism kept the mechanistic flavor but rejected the mentalism, especially the use of introspection as an experimental technique. Gestalt theory, contrariwise, rejected the mechanistic analysis. This makes a rather pat picture of German and American psychology. But

of course the diversity was in fact much greater; history simply trims it away.

Otto Selz was one who was trimmed. His works have never been translated into English. Seen through the eyes of G. Humphrey's 1950 book on *Thinking*, which was practically the only place where Americans could make contact with Selz, he appears to be simply another psychologist of the introspectionist period, worrying about problems of mind that are no longer of account. In fact, he also was reacting to associationism, and he attempted to provide a complete organization of methods whereby it was possible to see how the parts of thought get put together to effect the solution to a problem. His framework centers about solution methods, their hierarchical and sequential structure, and how each step plus the results it produces determines uniquely the next step. This is very much in line with modern attempts to lay bare the structure of information processing in human problem-solving by computer simulation.

The framework was also exactly appropriate to the study of the way humans think in playing chess. Adriaan De Groot, at that time both an international chess player and a student in psychology at the University of Amsterdam, collected a large number of protocols of humans deciding upon moves in a chess game. That is, the subject was presented with a chess position and asked to analyze and decide on a move while speaking his thoughts aloud, so that some record could be made. Then De Groot subjected this mass of material to an exhaustive analysis, in which he dealt in detail with the phases of the subject's investigation, the methods that he used, and the extent to which various features of the objective situation determined how he proceeded. This analysis, which became De Groot's doctoral thesis, is here finally available in English without essential modification (although with some additions). So Selz finally reenters the stream of active psychological research, in which his contributions can be seen in a new and better light.

But this leads to the major tale, although it can now be told in fewer words. Among its many consequences, the development of a science of information processing has led to the use of information-processing theories for human higher mental processes. Usually embedded in computer programs, these theories are the basis of attempts to model activities such as chess playing. Thus, they provide a technical matrix within which the kind of data that De Groot has obtained becomes extremely relevant. For the data would not excite an experimental psychologist grown up in the shade of behaviorism with statistical comparison as a security blanket. To be sure, De Groot tabulates a few comparisons, but these are all of relatively superficial features. Instead, the data are verbal behavior, and their analysis involves the extraction of their meaning against a background of inferred chess reality. This is an exercise that has a certain kinship with that of the archeologist. And the current ability to construct chess-playing programs (and others of more generalized capabilities) permits one to make use of the information so extracted—to assess the power and function of the methods—and to discover their fragmentary nature, when seen against the total demands of a program for actually playing chess.

The data are especially valuable because the subjects are not the college sophomores ubiquitous in American psychology, but are drawn from the upper ranks of the chess world: six grand masters, including two world champions (Euwe and Alekhine), five masters, and a number of others ranging from experts down to some of only modest skill. Thus the book has a good deal of intrinsic interest for those whose focus is chess, rather than psychology or information processing. The protocols for a majority of the sessions are included in an appendix.

What does the analysis yield? Not much in a highly precise form. There is clear evidence that search of consequences is a primary method of thinking in chess, and there is some characterization of the kinds of search strategies used. There is a non-obvious finding that it is not possible to distinguish the search behavior of grand masters from that of lesser players in superficial features of their "search trees." (They do select better moves.) This is followed by a demonstration that such players can be distinguished clearly by their ability to reproduce the chess positions after brief exposure. The data for this are a little thin but sufficiently provocative to warrant the attention given them.

There are a number of deficiencies in the study, of course. The data were recorded manually, since tape recorders were not available 25 years ago; the analysis is somewhat repetitious; and none of the current tools of analysis or concepts were available then. Still, wisely I think, De Groot refused to rework the material in any substantial way. He has added a 35-page epilogue that places the work somewhat in modern context. And he has labored hard (and successfully) to provide an adequate translation. It remains a gold mine for anyone working on human thinking and for anyone fascinated by chess. It is good to have it available in English.

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Extracting Information

The General Inquirer. A Computer Approach to Content Analysis. PHILIP J. STONE, DEXTER C. DUNPHY, MARSHALL S. SMITH, and DANIEL M. OGILVIE. M.I.T. Press, Cambridge, Mass., 1966. 671 pp., illus. Paper, $7.95.

This review contains information about the book it is reviewing and about the reviewer's opinion of the book. It may also contain information about the reviewer, about how he is attempting to affect the readers of the review, and perhaps about 20th-century America and the nature of *Science*. One way to retrieve some of this information is to read the review. Another way, to be preferred if the analysis is to be objective, reproducible, and suitable for testing some hypotheses about the psychological or sociological context in which the review was produced, is to describe the document with statistical or qualitative statements about its distribution of syntactic or semantic word classes. This procedure is one form of content analysis, more generally defined as "any research technique for making inferences by systematically and objectively identifying specified characteristics within text." The General Inquirer is a collection of computer programs which can be put together in a variety of ways to aid the content analyst by performing the lengthy data-processing involved in the analysis of large corpora.

It is important to note that the General Inquirer is not a contribution to linguistic theory, or a procedure for syntactic analysis, or for that matter a procedure for content analysis. It is, rather, a cleverly conceived collection