with to produce recognition and understanding. Indeed, the key concept of CS thus far is selectivity, mediated by any well-specified, computationally based strategy.

Surprisingly, the CS analysis and method may have more difficulty meeting the concerns of the Gestaltists than does behaviorism. Behaviorists frequently rely upon phenomenological descriptions of stimulus and response, as can most readily be seen when we leave the laboratory and discuss human behavior in clinical and educational settings. The definitions of stimuli and responses needed to give plausible characterizations of these settings are not given in machine-recognizable form, but require a human observer for their identification. Cognitive simulators, however, set themselves the task of constructing a completely self-contained model which can interact directly with the world. Thus, though they allow themselves the full power of all forms of computation, a healthy advance over association alone, the task they set themselves is far greater than that undertaken by behaviorism.

Too great, claims Dreyfus, a philosopher who advances a view with roots in Gestalt psychology, ordinary language philosophy, and phenomenology. His book is a partly sober, partly angry attempt to say what man can do that computers can't, and to say why. It is no simple panegyric to humankind, but attempts to meet CS on its own ground, without appeal to will, affect, or entelechy. The question raised is, Can a *digital computer* simulate the full *cognitive* abilities of man? In Dreyfus's view, selection among predefined alternatives is *all* the computer can do, but this plays a minor role in cognition. What we do in those brief moments preceding recognition and understanding is not an unconscious sorting-through of possibilities. From an indefinite welter of stimulation, the mind structures situations so that only the essential aspects are considered; the inessential are paid no heed whatsoever. Further, that to which attention is paid is not generally, if ever, a specific feature with definite meaning and significance, but is characteristically vague, as word and sentence meanings are. Perhaps most tellingly, there are no such things as immutable facts sitting about the world waiting to be processed. Facts are in the *Weltanschauung* of the beholder. If all of this is so, argues the author, we must not only give up hope of digitally simulating the cognitive processes of humans but must also dismiss the possibility of the more modest goal of devising a computationally based theory of artificial intelligence (AI) which exhibits the range and complexity of human cognition without attempting to model the same processes used by man.

Dreyfus does not resort to mysticism to explain the lawful regularities of human behavior and mental life. Thus rejecting the only characterizations of reasoning familiar to Western science—logic and magic—this view will have to many the distressing appearance of mere nay-saying. Dreyfus attempts to dispel this impression, not altogether successfully. What he offers as an explanation is not soul but body. Though realized within the realm of physics and chemistry, the Dreyfus body's parallel, analog, and "wholistic" processing cannot be mimicked by a serial, digital machine. It is more than the mind's analog-to-digital converter; it provides the means by which man sees himself in *situations* in a sense not capturable by representing potential environments as *states* of the world. The body is an integral part of our knowledge and language systems.

Though these conceptions of carnal knowledge and body English may appear foreign and vague to the programmer, the Dreyfus thesis should not be dismissed easily. He makes clear that CS and AI research faces possibly insurmountable problems, an observation which may strike some with the force of a new idea. It should come as no surprise that the book contains no knockdown arguments; the Gestalt position still awaits its definitive programmatic statement. But coming as it does as a critique of a field short on self-analysis, Dreyfus's effort could be enormously important if taken as a challenge and responded to dispassionately.

Unfortunately, it may not be, for Dreyfus has been made crotchety by the excesses of optimism and immodesty of prediction which accompanied the early promise of computer metaphors. He has responded with a lengthy polemic against CS and AI research and researchers which concludes that no progress has been or will be made toward their avowed goals. To a reader with this book as his only guide, the field will appear to be based solely on puffery and blind faith, supported at best by a philosophical analysis of the depth one might expect to read on a tee shirt. Such blatant bias can only lead to a blunting of the book's impact, particularly since the "empirical evidence," as Dreyfus somewhat grandly calls his survey, is too often called upon to shore up his case.

The hypotheses which have been explored in the 15 years of research which this book criticizes hardly cover all possibilities. Neither this research nor Dreyfus's critique has settled the issue of whether the mind's work is done by computation alone, for while we surely do not know the nature of the mind, we do not know the full power of the computational model either. In the modeler's world, the ambient conception of man may be artless; but it is still not clear to what extent it must so remain.

ROBERT K. LINDSAY
Mental Health Research Institute, University of Michigan, Ann Arbor

Memory Transfer

Chemical Transfer of Learned Information. EJNAR J. FJERDINGSTAD, Ed. North-Holland, Amsterdam, and Elsevier, New York, 1971. xxviii, 268 pp., illus.

The wish to educate oneself or one's children by taking pills or injections is both deeply embedded in folklore and consonant with our modern view of inexorable progress. In the past decade a literature has developed which claims not only that this is possible but that something like it has already been achieved. In various circles this literature has been met with exultant enthusiasm or passionate skepticism, but in most instances the response has been indifference in the face of what appears to be an implausible claim. In this book a number of investigators present their evidence that memory transfer has been accomplished. Some studies that have failed to confirm these claims are evaluated and there is much discussion of the present state of the art. This is the best summary that I have seen of this field and is a good starting point for those who may wish to examine it in some detail.

I should point out at once that studies of memory transfer should not be confused with the more general study of the effects of biological compounds on behavior. That behavior can be specifically influenced by chemicals derived from animals or plants has been conclusively shown. For example, thyroxin or ACTH derived from animal organs can influence behavior, as well as other bodily functions; and the behavioral effects of plant extracts require no comment in these times. This is not what experiments on memory transfer are all about. Rather they seek to identify specific compounds that are made in response to and encode the memory of a specific experience. That a hormone which an animal produces to raise its blood pressure can be transferred to another with identical results is not at issue here. Rather what is central is whether in the brain of an animal trained to make a discrimination like left from right a unique compound is made which can then direct identical behavior in a recipient. The crux of experiments on memory transfer is specificity. The situation is similar to that in studies of the immune response, where it is clear that a specific antibody is made in response to an antigen. That antibodies offer the appropriate analogy is shown from the putative transfer materials that have been studied in these experiments. They are in the class of "informational macromolecules"—proteins, polypeptides, and primarily RNA. The latter could presumably act catalytically to direct the synthesis of many molecules of the specific protein that mediates the specific memory.

Although the problem of specificity is recognized as crucial, most of the book is concerned with the more general question, Are extracts from brains of trained and of naive animals different in their behavioral effects? Many studies have been done to test this. James Dyal reviews the literature and has tabulated the outcome of all published studies he could find. By his method of reckoning, of all the experiments that have been reported "133 have yielded significant transfer effects, 115 have yielded null results, and 15 are equivocal." He then goes on to assert, "The conclusion is inescapable: the memory transfer effect is a real phenomenon!" The style of reasoning Dyal uses is exhibited by a number of the other contributors. They readily accept that the general phenomenon has not been confirmed in many laboratories. Indeed, Dyal's tabulation shows that positive results have been obtained in only a relatively small number of laboratories. Georges Ungar and his collaborators account for the largest number of entries, with 24 positive and 3 negative experiments. In contrast, the biggest heading on the negative side comes from a brief note in a 1966 issue of *Science* (**153**, 658), in which a number of skeptics, led by the then skeptical William Byrne, reported 18 unsuccessful attempts to demonstrate memory transfer. Dyal's legitimate argument is that negative reports are not conclusive because these studies are by their nature difficult to replicate. We do not know what exactly is being transferred or what is the optimal injection schedule or what is the proper route of administration. Therefore it is easy to discount negative results, since the experiments may not have been done right. Indeed, a number of contributors call attention to the problems individual laboratories have in replicating their own results. A good example in which early experiments showed positive results but subsequent experiments were negative is traced historically in the paper by Krech and Bennett. What does one do in a situation like this? Does one add up the positive and negative experiments in a given series in a given laboratory and then take the average? Or poll multiple laboratories and then take the average? Again and again one hears that there must be something here but that it is hard to find and hard to demonstrate. Some findings in biology that are initially difficult to replicate are of major significance. Is this one of them?

Although a number of contributors to this volume believe that extracts of brains of trained animals are different from those from controls, there is less agreement about the central question of specificity. Only a few studies have attempted to evaluate the specificity of the injected materials. Though some have been positive they have been confined to a few laboratories and the differences have been quite small. Yet even suggestive results about so important a matter should provoke so much research that the question should be settled promptly. Why hasn't this happened?

The major obstacle to extensive evaluation of memory transfer is that the underlying premise appears implausible to many students of brain function. As we currently understand it, the language of the nervous system is pathways, interneuronal networks, circuitry. One has only to look at a section of neuronal tissue under the microscope to be struck with the obvious fact that great informational potential resides in the complex intercellular relationships between the axons and dendrites of individual neuronal units. As we understand the nervous system, specific interneuronal relationships are laid down in accordance with a genetically determined program and then are facilitated or inhibited as a consequence of experience. Such interneuronal relationships could be modified by increasing the synthesis or altering the structure of a relatively small number of regulator proteins which might act as receptors for neurotransmitters or as enzymes which regulate the biosynthesis of neurotransmitters or the like. In this view, plasticity comes from modification of the existing circuitry by either facilitation or inhibition. We can therefore conceive of a nervous system which can perform specific functions, genetically determined or learned, without bothering to make a specific macromolecule for each specific bit of new experience. A homogenate of brain might contain no more specific information than a homogenate of a computer.

To postulate specific macromolecules encoding specific bits of psychological information one would have to conceive of a way in which experience would direct the creation of such molecules. Given even our understanding of regulation of genetic expression and of clonal selection and immunocyte formation, no plausible model has been presented in which there would be de novo synthesis of a new macromolecule to mediate each specific behavioral event. This is not to say that such a mechanism is not possible. Rather, casual observation of brain anatomy suggests that it is not necessary. And nature tends to avoid the unnecessary. Therefore new investigators who are interested in exploring this field are faced with the problem of engaging in difficult, controversial, and frustrating experiments which, if successful, support an implausible result. As is legitimately argued in this book, if unsuccessful, they cannot prove the null hypothesis. It is little wonder, then, that all this work is being conducted by a relatively small and faithful band who meet frequently to support each other in the face of the skepticism of the general community of neurobiologists.

As I read this book, encumbered by my bias, I kept wondering whether here in the making is a revolution in our concept of how the brain works. Certainly the revolution has not yet occurred. What is your bias?

SAMUEL H. BARONDES
Department of Psychiatry, University of California at San Diego, La Jolla