ment of historiographic approach that marks this volume leads to some questionable claims—for example, in the essays of Gilbert and Pauly. Gilbert interprets the cellular theories of Just and Goldschmidt in terms of their likely sociopolitical commitments—an intriguing exercise but an unconvincing one, especially in Just's case, not least because it rests on a reading of Just's cellular texts as a political metaphor that Gilbert does not support with appropriate corroborative evidence. Pauly, applying concepts of gender analysis to Woods Hole, contends that its vitality derived partly from the insulation it provided the research-oriented summer residents from an increasing contemporary perception of biology as effeminate, a branch of sciences that required "feminine" qualities of patience or delicacy and whose routine taxonomic work was being taken over by women. The evidence here advanced for the claim (and for a variant on it in the same chapter concerning the distress of biologists at their lack of martial utility during the First World War) is flimsy. Given that, the claim itself seems uneconomical, a gratuitous add-on to the plausible and, one would think, sufficient explanation otherwise argued by Pauly himself that the appeal of Woods Hole lay in its pleasant combination of family resort and fine laboratory, its advantageous marine-biological facilities, and the stimulation of capable colleagues.

However, socio-institutional treatment clearly pays off handsomely in Pauly's analysis of the Woods Hole influence in American biology, as it also does in Richard W. Burkhardt's splendid assessment of Whitman and Wallace Craig as pioneers of ethology in the United States. Despite the major significance of Whitman's and Craig's ideas on animal behavior, neither was able to establish the field, lacking suitable institutional opportunity (and, in Craig's case, a secure professional position). In a crisply incisive essay, one of the gems in the book, Diane Paul and Barbara Kimmelman deploy intellectual, economic, and institutional considerations to account for the rapid assimilation of Mendelism to plant breeding in the United States: Agricultural research installations, hoping to combat the economic consequences of overproduction by developing new varieties, were already well embarked at the turn of the century on programs of hybridization and cross-breeding; Mendelism explained the observations of experience and provided a guide for future work. Paul and Kimmelman explain in a similar fashion the ascendancy in American agriculture of hybrid corn over potentially superior pure-dominant varieties: The farmer would have been able to replant the pure-dominants; since hybrids would decline in quality after the first generation, he would have to purchase fresh hybrid seed each year. Their study indicates, as does *The American Development of Biology* on the whole, that historiographic adventurousness can yield not only adventure but angles of understanding and perspective that may be otherwise unattainable.

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A Medical Scientist

Charles Richard Drew. The Man and the Myth. CHARLES E. WYNES. University of Illinois Press, Champaign, 1988. xvi, 132 pp. + plates. $19.95. Blacks in the New World.

As a surgeon, teacher, moving spirit in the World War II blood supply program—and the central figure in some durable myths of American racism—Charles Drew has long deserved a biographer. Concisely, Charles E. Wynes puts the record straight.

Born in 1904, Drew was heir to the contradictions that beset the remarkable black community of Washington, D.C. Though segregated and discriminated against, its members enjoyed some patronage from the federal government's erratic support of Howard University and Freedmen's Hospital. Drew himself received an excellent education at Dunbar High School, that "fiefdom of advantaged blacks" (p. 11), a seedbed of doctors, lawyers, cabinet members, and at least one senator.

After a college education won by his athletic prowess, he entered medical school at McGill University in Montreal, making a late but intense discovery of the joys of the intellect. He returned to Washington with his M.D., drawn by his family and by a post at Howard where he hoped "to help his people" (p. 21). He soon won a chance for advanced training at Columbia University and ultimately received the degree of Doctor of Science in Medicine.

His subject was transfusion and the banking of blood and plasma, and his sense of timing was excellent: while he wrote his thesis, the opening of World War II guaranteed bloodletting on a scale never before imagined. Drew's presence in New York was also fortunate; in 1940, he was selected as medical supervisor of the "Blood for Britain" program, and he stayed on to help establish the American program that grew out of it. Here is a crucial point in the Drew legend, and Wynes is at some trouble to define Drew's contribution—essentially, technical leadership, the bloodmobile, and a central system of quality control. His popular reputation as the developer of blood plasma was undeserved. Nor did he resign in

"Charles Drew with the first mobile blood collection unit." [Moorland-Spingarn Research Center, Howard University; from *Charles Richard Drew: The Man and the Myth*]

protest against the racism, imposed by the U.S. armed forces, that rejected Negro donors and denied Drew himself the right to give blood.

Instead, he departed for the prosaic reason that his leave of absence from Howard was up. Back in Washington, he advanced steadily: professor of surgery at Howard, chief surgeon at Freedmen's Hospital, and examiner for the American Board of Surgery. He found in teaching—rather than science, administration, or clinical medicine—the true theme of his career. Stern, sometimes self-righteous, he changed black surgical training forever by insisting on the highest of standards and taking no excuses. Yet discrimination still dogged him; Drew was never able to join the American Medical Association, because the local chapter was segregated.

His death following an automobile accident in North Carolina begot the most durable of the legends that surround him. Wynes carefully demolishes the story that Drew was denied treatment in a segregated hospital. The reality of the racism that Drew faced throughout his life was far more complicated, for his white countrymen acknowledged his genius even as they denied him justice. Wynes's conscientious little study, despite some labored writing, recovers that complex reality in the image of a scientist and teacher who died before his time.

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Problems of Connectionism

Connections and Symbols. STEVEN PINKER and JACQUES MEHLER, Eds. MIT Press, Cambridge, MA, 1988. viii, 255 pp., illus. Paper, $17.50. Reprinted from *Cognition*, vol. 28 (1988).

Associationism has returned to cognitive psychology in the sophisticated new form known as neural networks, or connectionism. The fact that a number of present practitioners do not want associationism back has resulted in the present volume of superb critical essays.

What are connectionist models? In such models, a set of input nodes is connected to a set of output nodes. Various weights of excitability or inhibition from each input node to each output node are given. When a set of input nodes is fired, a number of the output nodes have some probability of firing as a result of the connections. Corrective or feedback mechanisms can adjust the weights of various connections, so that the system can in effect learn from experience. In principle, anything can be connected to anything. The ingenuity of the individual investigator determines where and how strikingly the model can be applied.

From some early and interesting applications such as graded perception of category membership and the prediction of typewriter error patterns, investigators have recently gone on to elaborate attempts to simulate performance and learning patterns in language, the favored domain of non-associationist, higher-order structural cognition.

In particular, a recent model by David Rumelhart and Jay McClelland simulated the patterns over time of children's production of overregularized past tenses like *breaked* and *eated*, using only various collections of phonological stuff to represent the words; there are no rules or higher-order symbols like "verb" in the model. Perhaps, Rumelhart and McClelland suggest, most higher-order symbols can eventually be dispensed with in this way, along with linguistic rules. Other investigators have attempted to model sentence grammar (though by using form classes like "adjective" and noun" as the node-level labels).

These responses to the new associationism consist of three major essays. The first essay, by Fodor and Pylyshyn, points to the representational incapacity of connectionist models to capture some of the basic properties of human language. For example, human languages are *compositional*, in that the same item can be used in different places and still be the same item—the word *big* has the same relation to *dog* in *the big dog* and in *the dog is big*. This, surprisingly, connectionists cannot capture. Another missing property is the capacity to treat words or groups of words as individual units at one level but as part of a single higher unit at another. As one example, the unit "sentence" can itself contain other sentences as units, as in embedded sentences or relative clauses. Indeed, there are rules that affect a particular phrase unit, like a noun phrase, within one of these embedded sentences. Connectionist representations cannot say this sort of thing.

Fodor and Pylyshyn argue that to the degree these properties (and other properties of sentences that they discuss) are characteristic of human thoughts behind the sentences, connectionist models must be inadequate as general models of thought as well. Thus, connectionist inadequacies go beyond linguistic structure to thought structure.

In the second essay, Pinker and Prince give a thoroughgoing and sharp-etched critique of McClelland and Rumelhart's model of children's past-tensing. They too note how the notion that generalizations apply to units at different levels of analysis cannot be captured: some linguistic generalizations apply to individual sounds, some to their combinations, some to the combinations of words, and so on. They also question the actual success of the model. It does not, for example, stabilize on correct usage; it continues to produce many past-tense errors, whereas adults do not. It makes errors no one makes. Perhaps most devastatingly, Pinker and Prince point out that the model makes, and apparently must make, certain assumptions about the changing input to children that are factually incorrect. Early on, the model is given a sample of present and past forms of eight irregular verbs and two regulars. During this time, the model, like children, makes no overregularizations such as *breaked*. Then the input is changed to hundreds of verbs, in the reversed ratio 80 percent regular to 20 percent irregular, and overregularizations are now produced. But Pinker and Prince's inspection of children's transcripts shows no such radical change in regular-irregular ratios in either the environment for children or children's own vocabularies as they pass from accurate to overregularization uses. Thus children's performance changes without the corresponding dramatic changes in environmental input needed by the model.

Lachter and Bever's essay is devoted largely to showing that when the microstructural connectionist models do work (to some degree) on language problems, it is only because they are carefully designed to take into account implicitly the macrostructure the effects of which are to be aimed at. Their criticism can be analogized as follows. Suppose someone found an office staff with each person apparently doing just his or her own isolated function. One might find it amazing how this set of isolated microfunctions could have effects as though it followed a larger macroorganization. But of course this is neither a coincidence nor a miracle, for everyone has been assigned these apparently isolated microfunctions in a way precisely determined by the macrostructural aims. Lachter and Bever proceed to show this is what happens in connectionist models of linguistic behavior.

The authors in this volume are all impressed by some facet of connectionist successes and suggest some role for connectionism as an implementor of lower-order processes, directed by higher-order ones. At the same time, they express puzzlement as to why connectionist theories have gained at the expense of symbol-based, rule-based ones. They note that connectionist models naturally produce some favorable effects such as gradedness of response but argue that symbol-based ones can also do so. They