phylogenetics, the reconstruction of evolutionary history. Assume that I propose that chimpanzees and humans are more closely related by descent to each other than they are to the orangutan. Numerous predictions can be derived: for example, that the primary structure of proteins (that is, their amino acid sequence) will be more similar on average between humans and chimps than between either one and the orangutan. We can then proceed to investigate the matter and, according to the outcome, accept or reject the hypothesis.

Mayr is more at home with evolutionary concepts and problems than with philosophical issues. *Toward a New Philosophy of Biology* has much to offer by way of insightful discussion of adaptation, natural selection, the origin of species, and macroevolution. There are also six chapters on Darwin. For Mayr, "The Darwinian revolution was the most fundamental of all intellectual revolutions in the history of mankind. While such revolutions as those brought about by Copernicus, Newton, Lavoisier, or Einstein affected only one particular branch of science, or the methodology of science as such, the Darwinian revolution affected every thinking man. A world view developed by anyone after 1859 was by necessity quite different from any world view formed prior to 1859" (p. 182). One can only agree.

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A Global Brain Theory

Neural Darwinism. The Theory of Neuronal Group Selection. GERALD M. EDELMAN. Basic Books, New York, 1987. xxii, 371 pp., illus. $29.95.

While not the first to use Darwinian analogies and biology's powerful population thinking to approach the problem of how to wire up a brain, Gerald Edelman came to it from having tackled the analogous self-organization problems in the immune system; in the last dozen years, he has studied the counterpart selectivity theories for the nervous system in greater depth than anyone else, and so this book has been eagerly awaited.

Neural Darwinism is a fine example of Edelman's broad, bottom-up approach to how nervous systems get themselves organized, store information, and create new behavioral patterns. It is in the tradition of Donald Hebb's 1949 *Organization of Behavior*, asking "What is the nature of categorization, generalization, and memory, and how does their interaction mediate the continually changing relationships between experience and novelty?" (p. 241). It should be read by neurobiologists, developmental biologists, the cognitive cognoscenti—including the artificial intelligentsia—and by all those hopeful technologists who are flocking to the banner of neural-like networks as an alternative way of shaping up smart machines.

But because *Neural Darwinism* is so ambitious (a "global brain theory") in its attempt to model neural ontogeny, it is an easy book to misjudge—and an even easier book to lay aside and never finish. It is marred by mind-numbing sentences such as "As a result, combinations of those particular groups whose activities are correlated with various signals arising from adaptive behavior are selected" (p. 5) and by nonbiological terminology such as "re-entrant" and "degeneracy." Edelman seldom unbends enough to use the tutorial approximations "feedback" and "different ways of doing the same thing." Incredibly, there is no glossary.

And the introduction omits the very items that could motivate readers to endure the theoretical presentation. Several decades ago, biologists began to realize that there was a lot of cell death going on in developing nervous systems, and theorists began to suggest that carving away cells might create functional patterns corresponding to long-term memory storage. Richard Dawkins in a 1971 *Nature* paper (which Edelman omits) made this explicit, though J. Z. Young's 1965 model for octopus memory is closer to the modern mainstream in selectively eliminating some synapses rather than eliminating entire cells.

Whatever the synapse turnover rate is (and no one even has estimates), there is an imbalance in the rates of making and breaking synapses during childhood. It causes us to reach adolescence with little more than half the number of cortical synapses that we had eight months after birth. What principles control the editing? Here, surely, is "neural Darwinism" in action. Since Edelman's models seem particularly relevant to the postnatal tuning-up process, it is even more surprising that this conceptually important background, from research on both humans and monkeys (for example, *Science* **232**, 232 [1986]), is omitted. Edelman treats "wiring up" as preceding "tuning up," but such data suggest overlap throughout childhood.

Because biologists are often impatient with even lucid theoretical discussions, some will unfortunately skim until reaching the unusually attractive specific examples—which, in order to achieve their clarity, lack the richness of Edelman's more general theory. Most attractive of all is the single fold-out color plate: this computer display reminds me of the back side of a colorful tapestry, little threads running here and there, as if they were axons in a tangential section of brain; their colors denote synaptic strengths between "cortical neurons." In the first frame, thanks to the randomized initial conditions, the picture is so haphazard as to suggest that Jackson Pollock himself had finally designed a true *tabula rasa*.

As the neural-like network gains experience (the sensory surface is stimulated, one point at a time and each point connects to many "cortical neurons"), one starts to see (in the second time frame of the color plate) red patches of strongly connected cells emerging from an increasingly blue boundary area where cells are weakly interconnected. Groups emerge, the physiological boundaries becoming far sharper than the underlying smear of anatomical connections—and all without instruction. Once you comprehend it, you may feel that this one color plate is worth the price of the book.

Eventually, in this map of a model hand, each patch will correspond to a top or bottom surface of a finger, looking not unlike the detailed maps of somatosensory cortex in monkeys, the plasticity of which has been studied by Michael Merzenich and colleagues. More impressively, Edelman and co-workers Leif Finkel and John Pearson can mimic the cortical rearrangements that occur when a finger is amputated (or overstimulated), though I note a revealing exception. Real cortical maps globally rearrange themselves, including boundaries between more distant digits—but the model's map shows only a local effect on the boundary between the affected digit and its immediate neighbor. It is as if, were an enlarging California to expand north into mid-Oregon, the Oregon-Washington and Canadian borders remained fixed (rather than also distorting, as real cortical boundaries tend to do).

Not everything that involves random initial conditions and selective survival deserves to be called Darwinism. The dance evolutionary biologists call the "Darwinian two-step," randomness-then-selection continuing back and forth for many rounds to increasingly shape up nonrandom-looking results, usually cannot be seen in Edelman's examples of neural Darwinism; these repeated injections of randomness lie at the heart of what some would consider as delimiting Darwinism from simpler forms of self-organization such as clumping and zero-sum "Matthewism."

And while the group selection of the subtitle may involve both groups and selec-

tion, Edelman's examples tend not to demonstrate what usually comes to mind when the two words are used together following an invocation of Charles Darwin—who invented group selection as a striking exception (much debated by contemporary sociobiologists) to the usual rule that natural selection acts only on individuals and their progeny:

> It must not be forgotten that . . . a high standard of morality gives but a slight or no advantage to each individual man and his children over the other men of the same tribe. . . . [But a tribe whose members] were always ready to aid one another, and to sacrifice themselves for the common good, would be victorious over most other tribes; and this would be natural selection. At all times throughout the world tribes have supplanted other tribes; and as morality is one important element in their success, the standard of morality [will rise by natural selection] [*The Descent of Man*].

If one looks carefully, Edelman's more general theory usually encompasses both the Darwinian two-step and this type of group-*qua*-group selection—but both tend to be missing from the examples on which biologists will likely focus their attention.

It is very easy to pick out an unphysiological assumption or unfulfilled prediction (my grumble is about assuming shunting rather than subtractive inhibition in cerebral cortex) in a work with the breadth and depth of *Neural Darwinism*. On such a pretext, many readers will rationalize laying this admittedly difficult book aside, unfinished. Yet those who persevere may come away feeling, "The brain really could work that way," not only because Edelman's assumptions are usually close-to-physiological but because he frames the issues in ways that have been repeatedly successful, in stochastic and Darwinian contexts, in revealing emergent properties. If you are concerned with the questions Edelman addresses, this book may well be worth your time.

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The Rise of Objectivism

Sociology and Scientism. The American Quest for Objectivity, 1880–1940. ROBERT C. BANNISTER. University of North Carolina Press, Chapel Hill, 1987. x, 301 pp. $29.95.

Robert C. Bannister has written a fascinating book about an episode in the history of American sociology and social thought, providing both an account of one strand in the history of American intellectual life and a tale of passion and politics, of how men strive to shape their worlds out of the social resources available to them. Throughout *Sociology and Scientism*, Bannister's goal is to understand the history of objectivism in American sociology. He examines its various meanings and how they changed, emphasizing, at different times, subject matter, methods, scientist. He considers the growing centrality of statistics in research methodology and in sociology in general. And he explores the process by which a discipline whose original self-definition emphasized a concern with social reform increasingly distanced itself from it. It is these developments that Bannister calls "scientism."

Sociology and Scientism recounts two related developments: the engagement of sociology in the "major reorientation of American thought that centered in the years between 1907 and the outbreak of the First World War, when a language of 'efficiency' and 'social control' gradually eclipsed the humanitarian, moralistic rhetoric of earlier reformers" (p. 5) and the obsession with method that consumed sociologists between the two world wars. Focusing on seven men whose lives span the first two generations of American social science, Bannister constructs a richly detailed story of the intersection of these men's personal, professional, and intellectual lives.

Of the first generation, those who became prominent before the First World War, Bannister considers (i) Lester Ward (1839–1913), the acknowledged founder of American sociology, who spent most of his professional life in institutions such as the Bureau of Statistics and the Smithsonian, accepting a chair at Brown University near the end of his career only; (ii) Franklin Henry Giddings (1855–1929), whose sociological career was spent primarily at Columbia University, where he was particularly important in preaching the value of statistics to the second generation of American sociologists; (iii) William Graham Sumner (1840–1909) at Yale University, whose *Folkways* provided the theoretical justification for the move toward objectivism among the second generation; and (iv) Albion Small (1854–1926) of the University of Chicago, chair of the first department of sociology in the United States and editor of the first sociological journal, the *American Journal of Sociology*. Each of these men was of central importance in the history of American social thought and in establishing the institutions associated with it from the late 19th and into the first decades of the 20th century.

Among the second generation of sociologists, Bannister focuses on three differing proponents of scientism: (i) Luther Lee Bernard (1881–1951), the peripatetic (having taught at the universities of Florida, Missouri, Minnesota, and North Carolina, Cornell, and Tulane, among other places) and rebellious leader of the dissident sociologists whose struggle to establish his version of a "scientific" sociology is a central political drama in *Sociology and Scientism*; (ii) F. Stuart Chapin (1888–1974), a student of Giddings's at Columbia, an unabashed advocate of strict technical thinking in sociology and influential in shaping the Department of Sociology at the University of Minnesota, which he long chaired; and (iii) William Fielding Ogburn (1886–1959), also a Giddings student and the most professionally successful of the interwar objectivists, whose career was spent primarily at Columbia University (1919–1927) and the University of Chicago (1927–1951).

With a confidence that reflects his broad historical knowledge, Bannister places each man in the relevant context of historical events and institutions—the growing new universities and professionalized graduate training programs, the First World War and the political upheavals associated with it, the rise of Marxism and the Russian Revolution, the growth of giant monopolies and the Great Depression of the 1930s—as well as in relation to each other, operating as they did in a professional world much smaller than our own. The picture that emerges is a richly textured web of social and intellectual connections.

Bannister resists any single explanation for the rise of objectivism, arguing that it reflected the convergence of many factors that shaped American academic culture from the 1910s through the 1930s: "a growing sense of social fragmentation and the absence of common values and standards in the late Progressive Era . . . a rejection and secularization of nineteenth-century American Protestantism" (p. 233), the professionalization of scholarship, "the institutionalization and specialization of sociological scholarship, first within the university and later within foundation-sponsored institutes" (p. 234), political pressures, and individual personalities. But the list of explanatory factors is ultimately unsatisfying, reflecting the inadequacy of Bannister's theoretical work while, simultaneously, demonstrating his extensive scholarship.

Bannister is successful at moving beyond the constraints imposed by the dichotomies of intellectual content vs. social context that dominated earlier debates in intellectual history and the sociology of knowledge. He views ideas as the socially constructed products of intellectual actors who pursue goals within particular historical contexts. But these actors have yet to be seen as gendered—as men and women, although most-