BOOKS: HUMAN BEHAVIOR

The Corpse of a Wearisome Debate

Patrick Bateson

The Blank Slate
The Modern Denial of Human Nature
by Steven Pinker
Viking, New York, 2002. 527 pp. $27.95. ISBN 0-670-03151-8.

Is it really the case that, as Steven Pinker claims in *The Blank Slate*, the biological underpinning of human behavior is denied by most people? Almost daily we are told about genes for maternal behavior, promiscuity, homosexuality, language, and much else. Certainly, the simplistic idea of a straightforward pathway from gene to behavior has had its severe critics (quite properly, in my view): genes code for proteins, not behavior. However, the center of that academic debate is not about whether genes influence behavior but rather how they do so. Pinker is concerned with a very different debate between the natural and the social sciences. He argues that the social sciences are dominated by a belief that all of each individual's characteristics are generated by that person's experience. This looks like a caricature to me, one used to sustain yet another round of the tedious and increasingly irrelevant nature-nurture debate. It is all too easy to pour scorn on stupid arguments or on those people suffering from cultural lag, and Pinker should have resisted this temptation. He undoubtedly writes well and is able to express complex ideas in ways that make them intelligible to lay people. Yet too frequently he overstates his case.

Pinker bases his charge against the naïve social scientist on three strands of current scientific inquiry: cognitive psychology, behavior genetics, and evolutionary psychology. The cognitive psychologists have uncovered rules that underlie and generate highly complex behavior. No quarrel with that. But to argue that the rules are, therefore, the basis of "real" human nature is to miss a crucial point. Chess has clear rules, which can be explained to a child. Yet, the interest and the richness of the game lie in what can be generated by those rules.

Behavioral genetics has established beyond all reasonable doubt that many individual differences in behavior can be attributed to genetic differences. However, the notion that the variability in behavior can be partitioned into genetic and environmental components is utterly misleading. Doing so ignores the rich and crucial interplay between the developing individual and his or her social and physical world. The estimates of heritability, with which Pinker seems completely comfortable, depend on the population of individuals and the range of environments sampled. Worse, the effects of a particular set of genes depend critically on the environment in which they are expressed, while the effects of a particular sort of environment depend on the individual's genes. Finally, heritability estimates say nothing about the ways in which genes and environment contribute to the biological and psychological processes of development. Walking on two legs is a fundamental property of being human, and it is one of the more obvious biological differences between humans and other great apes such as chimpanzees or gorillas. Although it depends heavily on genes, it has a heritability of zero because human variability in this respect depends on the vagaries of the environment. Pinker appears to miss the irony that the dependence of high heritibilities on human diversity conflicts with conclusions from the other modern subject he draws on for his attack on the social scientists—the evidence for human universals derived from the work by evolutionary psychologists.

Like many biologists, I regard proposals about the evolution and current utility of behavior as helpful in making sense of behavior. But it does not follow that all examples of present-day behaviors that clearly benefit the individual in the modern world are products of evolution. The combination of oral linguistic ability and manual dexterity, both of which are doubtless derived from past evolutionary pressures, generated written language in several parts of the world in the last 6000 years. It is not at all likely that the different forms of written language are adaptive in the sense of having been shaped by Darwinian evolution. Moreover, proposals about past evolutionary pressures or current utility must leave open the question of how the behavior develops. Whether or not an individual's development involves some "instruction" from a normally stable feature of the environment, or whether it would be changed by altering the prevailing social and physical environment, cannot be deduced from even the most plausible evolutionary or functional argument.

Part of the problem is that Pinker is so vague in his use of the term instinct, on which much of his conception of human nature depends. Apart from its colloquial uses, the term instinct has at least nine scientific meanings: present at birth (or at a particular stage of development), not learned, developed before it can be used, unchanged once developed, shared by all members of the species (or at least of the same sex and age), organized into a distinct behavioral system (such as foraging), served by a distinct neural module, adapted during evolution, and differences among individuals that are due to their possession of different genes. One use does not necessarily imply another even though people often assume, without evidence, that it does. Behavior that has probably been shaped by Darwinian evolution and appears, ready-formed, without opportunities for learning may be changed in form and the circumstances of expression by subsequent experience. The human smile is a good example. This matters because what Pinker happily calls human nature is in reality individual nature and depends critically on the circumstances of that person's life.

Where do these shortcomings in the argument leave Pinker's thesis about human nature? In poor shape in my view. Saloon-bar assertions do not lead to the balanced discussion that should be generated on a topic as

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BROWSINGS

Sense and Nonsense. Evolutionary Perspectives on Human Behaviour. *Kevin N. Laland and Gillian R. Brown.* Oxford University Press, Oxford, 2002. 381 pp. $29.95, £16.99. ISBN 0-19-850884-0.

Franz Marc's *Monkey Frieze* (1911).

How far does our evolutionary past go toward explaining current human behavior? Laland and Brown provide a balanced consideration of this issue through discussions of the methods, strengths, and limitations of five approaches to the question: sociobiology, human behavioral ecology, evolutionary psychology, memetics, and gene-culture coevolution.

important as this one, and they do a disservice to the really powerful biological arguments that can be deployed. Furthermore, the misplaced combative style delays the honest synthesis Pinker professes he wants so much. I fear that *The Blank Slate* will become a happy hunting ground for the social scientists already predisposed to be skeptical about evolutionary thinking and that the wretched unnecessary debate over human nature is due for yet another silly round.

BOOKS: VIROLOGY

Chance Favors the Prepared Mind

Robin A. Weiss

Louis Pasteur's famous aphorism that heads this review aptly describes Baruch Blumberg's career. In 1976, Blumberg was awarded the Nobel Prize in Physiology or Medicine for his identification a decade earlier of Australia antigen and for showing that it represented infection by what we now call hepatitis B virus (HBV). His discovery has had an immense impact on human health. Screening of HBV carriers among blood donors was soon introduced. More recently, the anti-HIV drug lamivudine was found to be effective against HBV. Australia antigen itself, purified from human blood plasma, proved to be an efficacious and safe vaccine. Later, molecular cloning of the antigen's gene led the way to the world's first recombinant subunit vaccine. As immunization against HBV becomes implemented more widely, half a billion children born in Asia and Africa may be protected from hepatitis and from subsequent liver cancer as adults.

Hepatitis B
The Hunt for a Killer Virus
by Baruch S. Blumberg
Princeton University Press, Princeton, NJ, 2002. 256 pp. $27.95, £19.95. ISBN 0-691-00692-X.

In *Hepatitis B*, Blumberg relates his life in science and medicine. Born in 1925, he grew up during the Depression in Flatbush, Brooklyn. As a naval officer during World War II, he completed a physics degree, and after the war he entered medical school at Columbia. (He gives a telling description of life as an intern in New York's Bellevue Hospital.) Blumberg then took the opportunity to study for his doctorate at Oxford, to which he later returned on sabbatical and, eventually, as Master of Balliol College.

After joining the National Institutes of Health in the late 1950s, Blumberg began to explore serum proteins as markers for genetic polymorphisms, and he was drawn to field studies among remote human populations in various parts of the world. This gave him a feel for epidemiology as well as laboratory research, a broad perspective that served him so well during his subsequent long career at the Institute for Cancer Research at Fox Chase, Philadelphia. He made quite an important early contribution to the characterization of low density lipoproteins (now known to affect cardiovascular disease) but was diverted by his "big" discovery, the Australia antigen—so called because he found it in high frequency in the sera of native aborigines. Although the antigen was initially assumed to be an inherited human protein, the eureka moment came when Blumberg realized that it was actually the coat protein of the agent of serum hepatitis.

Field dress. To collect blood for his polymorphism studies, Blumberg visited an isolated Inuit village at Anaktuvuk Pass in Alaska's Brooks Range in August 1958.

Blumberg writes that he was "surprised at the apparent hostility" to his discovery of HBV. He sees this hostility as being a typical response to a Kuhnian paradigm shift. For Blumberg personally, the discovery was certainly a damascene conversion to virology. But it does not strike me as a conceptual leap in science, in the sense, say, that the "prion only" hypothesis of spongiform encephalopathies challenged our view that infectious agents must possess genomes. The epidemiology of serum hepatitis had clearly indicated the presence of an unknown infectious agent, just as AIDS, non-A non-B hepatitis, and Kaposi's sarcoma were clearly consequences of infection. As with the subsequent identification of the causative viruses of those diseases, what really rankled the leading researchers was that the hepatitis virus was found while Blumberg's group was looking at quite different things. As Blumberg writes, "We were outsiders not known to the main body of hepatitis investigators...making claims for the discovery of the virus they had sought for so many years."

Details in the book are not always accurate. Blumberg estimates that HBV infection leads to 1.5 million deaths each year, and we are told that "HBV kills as many, if not more, people than HIV." That was true 10 years ago, but UNAIDS reckons that in 2001 about 3 million people died as a result of HIV infection. Names are misspelled, and Erasmus Darwin is referred to both as Charles's uncle and, correctly, as his grandfather. A dominant Mendelian trait is called recessive, and the author writes of screening blood for hepatitis C DNA, whereas that virus has an RNA genome. Meticulous editing could have removed these minor irritations.

Outside my laboratory stands a large, obsolete electron microscope, lovingly restored by Richard Tedder. In the 1960s, it was the instrument with which two important human viral pathogens were discovered, Epstein-Barr virus and hepatitis B Dane particles. David Dane and his colleagues were the first to distinguish the 45-nm-diameter particles containing HBV genomes from the 22-nm "empty" particles that Blumberg showed could be purified from plasma to use as a vaccine. Both types of particles are included in the tiny image on the cover of *Hepatitis B*. But inside, Dane merits a single sentence; Alfred Prince, two; and other key players in the field (such as Fritz Deinhardt, Bill Robinson and Ken Murray), none at all. Despite its title, Blumberg did not intend his book to be a measured history of HBV; rather, it is a personal memoir. For a very brief but balanced story of hepatitis virus discoveries, one should read Bob Purcell's account [*Gastroenterol.* **104**, 955 (1993)].

Nonetheless, Blumberg is generous to the many colleagues who studied or collaborated with him. He displays a wry, self-deprecating humor, as befits the Brooklyn kid turned Oxford don. Writing of his attempt to interest Merck in developing the hepatitis B vaccine:

> Perhaps I was distracted by the opulence of the office and reception area; I remember thinking at the time that we could easily fit four good-size laboratory spaces into the room occupied by a single executive. The proceedings seemed to be inconclusive and somewhat depressing. When we emerged...I said to Wing [G. Willing Pepper]. "Well, I guess that was 'No.'" "To the contrary," Wing answered, "it was 'Yes.'" So much for my appreciation of corporate dealings.

Readers will find much to enjoy and absorb in Blumberg's fascinating personal story.

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