Enthusiastic Claims

Mapping the Code. The Human Genome Project and the Choices of Modern Science. JOEL DAVIS. Wiley, New York, 1991. x, 294 pp., illus. $19.95. Wiley Science Editions.

Mapping our Genes. The Genome Project and the Future of Medicine. LOIS WINGERSON. Dutton (Penguin), New York, 1990. xii, 338 pp., illus. $19.95.

Genome. The Story of the Most Astonishing Scientific Adventure of Our Time—The Attempt To Map All the Genes in the Human Body. JERRY E. BISHOP and MICHAEL WALDHOLZ. Simon and Schuster, New York, 1990. 352 pp., illus., + plates. $22.95.

The Human Genome Initiative may or may not revolutionize science and medicine. But it will surely prove a bonanza for journalists. The three books under review represent the first wave in what is likely to be a flood of works explaining the genome project to nonspecialist readers. If they are typical of the genre, readers will learn more about science reporting than they will about science.

What will they learn about science journalism? Above all, that it is a species of cheerleading. These books vary widely in style, emphasis, and the accuracy and clarity of their technical exposition. But they share this feature: all tell the story of the genome project as its proponents would have it told. It is a dramatic story, full of human interest, in which researchers are portrayed as heroes in the fight against disease. This flattering portrait may explain some of the dustjacket blurbs. It can't be easy to bite the hand that strokes you.

According to Joel Davis, "Mapping the human genome will be the greatest scientific and technical achievement of this century, greater even than the invention of the atomic bomb." And at $3 billion, it is cheap as well. "For that price," writes Lois Wingerson, "we can possess the original owner's manual to the human body, now written in terms we can comprehend." At least the analogy here is to cars. Elsewhere, genetic technologies are "weapons" in a "war" against disease. Who could oppose a war for health? Such efforts would in any case be fruitless. Davis tells us that "there is nothing anyone can do about it," Wingerson that "the urge . . . to learn the precise contents of human DNA . . . has become overwhelming"; and *Wall Street Journal* reporters Jerry Bishop and Michael Waldholz assert that the technology can't be stopped "any more than the technology of the automobile, the machine gun, or the atomic bomb."

Given these books' focus on the project's medical rationale (there is virtually nothing about the potential benefits for basic science or understanding of evolution), one might have hoped for at least some reflection on the claim that a crash program to sequence the human genome is the most cost-efficient way to improve health. But this claim—which has been contested both from outside and within the biomedical community—is not noted, much less assessed.

The most significant health risks for newborns in the United States result not from genetic defects but from environmental factors that produce low birth weights. We know from pilot programs that this problem can be significantly reduced through expansion of prenatal care. If the goal is improvement in health, there would seem to be more effective strategies than mapping and sequencing the entire human genome. At least this is what critics such as Troy Duster have argued. If they are wrong, we should be told why.

Arguments from within biomedicine do not fare any better. The claim that a function-based approach promises higher returns than one that is sequence-based receives no mention in these books. To put the point another way: we are told what James Watson, Walter Gilbert, Francis Collins, Charles Cantor, Renato Dulbecco, Leroy Hood, Victor McKusick, Mark Skolnick, Ray White, and other project champions think in detail, and often in their own words. We learn nothing of the views of a Bernard Davis, Luca Cavalli-Sforza, Martin Rechsteiner, or Michael Sylvanen. Moreover, the authors of all three books assume that, given knowledge of a genetic disposition to some disease, people will change their lifestyles to avoid it. Thus Bishop and Waldholz write, "Genetically susceptible individuals can be identified at birth and reared to avoid the excesses of life that would prompt early coronary disease." Why they think this scenario plausible we are not told.

Though all three are works of advocacy, the books differ in other important respects. *Mapping the Code* (sic), a book it may have taken the reviewer longer to read than the author to write, is marred by muddled descriptions, *ex cathedra* assertions on scientific issues, and numerous errors. It is by far the least satisfactory of the three.

The authors of *Genome* do a much better job of explaining the science. Their explication of technical developments, such as RFLP mapping, is both thorough and lucid. The discussion of current and potential problems with predictive medicine is also well done. However, their account of work in behavior genetics is disappointingly crude. We are told that personality "is about half inherited and half environmental in origin" (although heritability estimates apply to populations, not individuals). Highly contested studies are touted as breakthroughs. Cloninger and Bohman's "discovery" of alcoholism-susceptibility genes and David and Brenda Comings's controversial finding of a link between Tourette's syndrome and severe alcoholism are described enthusiastically and at length. The critiques are not mentioned.

Contrary views are discussed in Lois Wingerson's *Mapping Our Genes*, though in general Wingerson pays less heed to, and is less successful at explaining, the science. Her book consists essentially of a series of human interest stories, based on interviews with scientists and sufferers from genetic disease. She writes well, and some of the stories are truly engrossing. Their most interesting feature is the evident disparity in the aims of the scientists and of the victims of genetic disease. Wingerson does not comment on the divergence. But perhaps the text speaks for itself.

Consider the following passage from the story of Betty LeBlanc, three of whose children suffer from Friedrich's ataxia. Wingerson writes that during a visit to Tulane Medical Center

> Betty kept at [Michael] Wilensky like a hungry bird. "Can't you do something for my kids? Why can't you help them?" "Because I have no funds for research," he replied. Wilensky is used to this kind of nagging. He told her what he told everyone grappling with one of the incurable neurological diseases: There's nothing I can do for the disease, but there's a lot I can do for you. You need some motivation. Help us get funds or publicity. In the case of a rare disease, it takes extra effort to generate medical research.

Betty LeBlanc is "helped" by being motivated to help scientists get money. What she wants is help for her kids. Later we learn about a euphoric press conference in which researchers announced they had found the gene for cystic fibrosis; "It was the kind of event Betty LeBlanc dreams about." But what good would it do Betty LeBlanc? The discovery of a gene or marker does not equate (except in these books) with a cure for genetic disease. In the case of cystic fibrosis, it resulted in prenatal and carrier tests for families with a previously affected

child. Almost no one has used them. Betty LeBlanc surely would not. She is Catholic and opposes abortion.

Few biologists are likely to read these books, which are aimed at a popular audience. Perhaps they should. They may not learn much science. But they would learn something important about how their science is packaged and sold. Nonspecialist readers would be better off with *Proceed with Caution*, a superb analysis of the science and social implications of genetic testing by Neil Holtzman, professor of pediatrics at Johns Hopkins. Although it is not about the genome initiative, it covers much the same ground as the books reviewed. And it is far better value for the money.

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Between Bosons and Fermions

Fractional Statistics and Anyon Superconductivity. FRANK WILCZEK. World Scientific, Teaneck, NJ, 1990. x, 447 pp., illus. $68; paper, $28.

The fundamental particles of physics have long been divided into two classes, bosons and fermions. They differ in their statistics, which are, roughly speaking, +1 for bosons and −1 for fermions. For the last 20 years or so, there has been a current of speculation that there might exist other particles with statistics somewhere in between. Since this might be anywhere in between, such particles were called anyons. In the mid-1980s, these particles were indeed found as excitations in the fractional quantum Hall effect system. This is the first book to appear on the topic of anyons in condensed matter physics, a topic now very fashionable because of its possible relevance to high-temperature superconductivity. It consists of 100 pages of introductory material followed by 340 pages of reprinted articles.

Wilczek's way of giving us fractional statistics departs a bit from the usual style of scientific exposition. The book is organized not only into main sections but also into numbered subsections. This sort of organization is more familiar to philosophers than to physicists. One does discern the rarefied and slightly mysterious atmosphere of the *Tractatus Logico-Philosophicus* at times, beginning with the ethereal Japanese print on the dust jacket and noticeable throughout in occasional shifts in orthography (David Bohm acquires an umlaut) and terminology ("average field theory" for "mean field theory"). The articles apparently each represent a separate insight and contribute to the impression of physics as a succession of epiphanies rather than as the progressive accumulation of knowledge. You get used to it after a while and discover that this approach serves the author well most of the time. The explanations are clear and to the point; the format concentrates the mind. Sometimes things don't work out so well, since a linear development is not enforced by this style; pages 44–46 are identical, word for word, to pages 89–91.

The basic ideas of the subject are well laid out. A very good job is done on certain points that are glossed over in the reprints, such as why two-dimensional space is special, what is an actual field configuration that typifies a topological equivalence class, and what happens to generally accepted theorems connecting spin and statistics when the latter is fractional. I was particularly pleased to see that certain shaky assumptions that are usually not mentioned by practitioners in the field are stated and discussed. In particular, the tricky question of the boundary conditions that must be applied before topological arguments can even begin is treated in the chapter on the Hopf invariant. Also, the usually unstated condition that particles cannot pass through one another, at the basis of the braid group classification of two-dimensional trajectories, is discussed and critiqued. The topological approach does have a certain amount of dirty linen to be washed. Wilczek does it here in public, and I, for one, am all for it.

There are a number of points in the introduction that are original and bear reading several times, as they don't appear in the reprints. The two best examples are a very pretty chapter on the question of domain walls between degenerate anyon ground states and some considerations on what the anyon picture implies for the stability of various fractional quantum Hall effect states. There is one section that I did read several times without being sure whether I was any the wiser or not—this was on the idea of a generalization of spontaneous symmetry breaking to something called spontaneous projectivization. (No space to explain that here, I'm afraid.) A very few bits didn't bear reading at all. This seemed to happen when the author went into condensed matter physics background material. The paragraph on the integral quantum Hall seemed to miss the point entirely. In general, however, the beginner should be able to read straight through the introduction with some effort and much profit and be ready for the reprints.

The reprints are a rich mine of stuff. The original paper of Aharonov and Bohm is there, as is a remarkable paper by Finkelstein and Rubinstein from 1968 that already contained many of the ingredients of the modern theories. Wilczek's papers from the early '80s are not slighted and lead directly on to a number of papers under the headings of Foundations and Anyons in Model Field Theories. By this time the reader is happy with Chern-Simons terms and is prepared to enter the modern history of the subject, which begins with the fractional quantum Hall effect in 1984. The final hundred pages or so are devoted to high-T_c superconductivity. If the reader is only interested in the last topic, three of the reprints themselves contain excellent introductory material, namely Laughlin's paper from *Science*, a paper by Chen, Wilczek, Witten, and Halperin, and a paper by Canright and Girvin. Nothing is missing.

And yet something *is* missing. The strong interest in anyon theory today is due almost entirely to its application to high-T_c superconductors. But none of the papers makes a really serious attempt to show that the theory is applicable to these materials. The reader is treated only to arguments by analogy when what he or she really wants is an argument from energy. The anyon excitations need a certain type of ground state to support their existence, and that ground state must minimize the expectation value of the microscopic Hamiltonian of certain compounds made with yttrium, barium, copper, and oxygen. I make that statement here because the point doesn't get much attention in the book. While reading, I often felt as if I was watching a foot race that had begun even though the starter's gun had not sounded. The most urgent thing for anyon theorists to do now is to face this issue. The whole point of condensed matter theory, after all, is to connect the microscopic with the macroscopic. It cannot afford to have such wide gaps. Surely there is a philosopher out there who wants to write the *Tractatus Empirico-Microscopicus*.

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Fisher's Letters

Statistical Inference and Analysis. Selected Correspondence of R. A. Fisher. J. H. BENNETT, Ed. Clarendon (Oxford University Press), New York, 1990. xviii, 380 pp., illus. $90.

The year 1990 marked R. A. Fisher's centenary, and many sessions were held by statistical societies in his honor. The present volume of correspondence on statistical topics is an especially noteworthy contribution. Its publication appears to complete the valu-