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BOOK REVIEWS

The Cholesterol Myths: Exposing the Fallacy that Saturated Fat and Cholesterol Cause Heart Disease by Uffe Ravnskov, M.D., Ph.D. Published by the New Trends Publishing Co., Washington, D.C., 2000, xiv+297 pp. $20.00 (p). ISBN 0-9670897-0-0.

With courage and care Dr. Ravnskov exposes the lack of experimental evidence for the diet-heart theory (DHT), which claims that eating less fat and cholesterol will prevent atherosclerosis. He shows how the abstract or conclusions of a number of papers are at odds with the actual data in the papers. He demonstrates how the use of one statistical method in preference to another can give a false impression that there is an effect of drug or diet in lowering total death rates, where there is, in fact, no significant effect.

"This book was written to give you and your doctor some facts about cholesterol and coronary heart disease (CHD). They are facts that even your doctor may not know because these facts have been misunderstood; or because many scientists, health authorities and representatives of the drug companies have suppressed them altogether" (p. 5).

Surprisingly, this author has included about 2 dozen cartoons to make this very wrenching expose more palatable. The chapters are called "Myths." Here are the chapter titles, which certainly show the direction of this book:

"Myth 1: High-fat foods cause heart disease"
"Myth 2: High cholesterol causes heart disease"
"Myth 3: High-fat foods raise blood cholesterol"
"Myth 4: Cholesterol blocks arteries"
"Myth 5: Animal studies prove the diet-heart idea"
"Myth 6: Lowering your cholesterol will lengthen your life"
"Myth 7: Polyunsaturated oils are good for you"
"Myth 8: The cholesterol campaign is based on good science"
"Myth 9: All scientists support the diet-heart idea"

In a striking graph from one of the papers of John Yudkin, M.D., Ravnskov shows that the number of deaths from CHD in England and Wales between 1910 and 1956 is closely correlated with the number of new radio and television sets purchased each year. This is a perfect example of a correlation without a cause. Another line on the same graph shows that the number of grams of animal fat consumed per day changed by only ~10% during this period. There is no correlation whatsoever between fat consumption and death rates from CHD, which increased 6-fold during this time period. He notes that in the US, coronary mortality increased about 10 times between 1930 and 1960, leveled off during the 1960s, and has since decreased slowly. During the decline of heart mortality the consumption of animal fat declined also, but during the 30

years of sharply rising coronary mortality, the consumption of animal fat also decreased.

In another example: "While the death rate from coronary disease increased in most countries after World War II, it decreased in Switzerland. If this decrease had been preceded by a decline in the intake of animal fat, Switzerland would have been a model for health care in other countries. But the diet-heart proponents never mention Switzerland because during the decline in heart mortality, the Swiss intake of animal fat increased by 20%" (p. 31).

In still another example: "The Masai [of Kenya] drink 'only' half a gallon of [whole] milk each day.... Their parties are sheer orgies of meat; on such occasions four to ten pounds of meat [eaten] per person is not unusual, according to Professor [George] Mann [of Vanderbilt University in Nashville, TN, USA]. If the diet-heart idea were correct, coronary heart disease would be epidemic in Kenya. But Professor Mann found that the Masai do not die from heart disease—although they might die from laughter if they heard about the campaign against foods containing cholesterol and saturated fat. But this was not the only surprise. The cholesterol of the Masai tribesmen was not sky-high as Mann had expected; it was the lowest ever measured in the world, about 50% lower than the value of [that of] most Americans" (pp. 32–33).

Major frauds are decried: "Most supporters of the diet-heart idea think that the increased risk of CHD is present at all cholesterol levels. Those who have a cholesterol level of 200 mg/dL, for example, are worse off than those with a cholesterol level of 150 mg/dL; and those who have a cholesterol level of 250 mg/dL are at even greater risk. The pharmaceutical companies love this concept for it implies that almost everyone should be treated, even those with normal cholesterol levels.... The truth, were it known, would send pharmaceutical stocks plunging. In most studies, the increased risk is present only above a level of cholesterol that includes just a small percentage of the total population. [These are the approximately 0.5% of people with a genetic defect called familial hypercholesteremia (FH).] And women can stop worrying immediately because high cholesterol is not a risk factor for the female sex.... In fact, it seems more dangerous for women to have low cholesterol than high. Dr. Bernard Forette and a team of French researchers found that old women with very high cholesterol live the longest... <1/5 times the death rate of... women who had very low cholesterol. In their report, the French doctors warned against cholesterol lowering in elderly women. But they could as well have warned against cholesterol lowering in any woman, or, to be more precise, in anyone at all" (p. 59).

Your reviewer checked one of the citations on MRFIT (Paul et al., 1982) to find that the summary noted honestly that the treatment group had less mortality from CHD and more total mortality than the controls did. That the former was not statistically significant was in the abstract; that the latter was not statistically significant was not in the abstract, but only in the body of the paper. Another problem with both this and some other studies is that the interventions included diet, anti-hypertensive drugs and smoking cessation all at once.

The authors thought that less smoking was beneficial and that anti-hypertensive drug therapy was harmful. But the diet for the treatment group called for lower saturated fat and cholesterol intake and higher polyunsaturated fat intake; the authors did not admit the possibility that this intervention could have been harmful. In an end note Ravnskov simplified a table in this paper and showed that the entire difference in death rates of certain sub-groups was due to quitting smoking, which cut the death rate in half.

Dr. Ravnskov went on to show that higher levels of high-density-lipoprotein (HDL, "good" cholesterol) are not protective against CHD and that lower levels of low-density-lipoprotein (LDL, "bad" cholesterol) are not beneficial, although the expected associations of each with CHD are present. Triglycerides were said to be even less correlated with CHD than cholesterol is; the assay for triglycerides is worthless unless the patient has been fasting 12 hours, and the assay is only accurate to ±50%. Intimations in papers that there are "many" or "definitive" studies in reports and papers supporting the DHT were shown to be false by showing that citations often led to other reviews, each trusting the last, and ending in very few original studies, and that even those were often flawed statistically.

Dr. Ancel Keys was one of the main proponents of this myth that eating fat causes CHD. In a paper published in 1958, Keys showed a graph of the percent calories from fat in the food of various countries vs. the mean serum cholesterol levels. The data points fell on a smooth curve, showing an excellent correlation. Dr. Ravnskov added data points from a number of countries deliberately ignored by Dr. Keys. These fall nowhere near the curve. Furthermore, CHD death rates among subjects in Finland, Greece and Yugoslavia with similar serum cholesterol levels varied 5-fold depending on which area of the country the individuals lived in! Four studies in the US, one in the UK, one in Israel and one in Finland failed to show any correlation between diet and serum cholesterol levels.

"Numerous studies have shown that in people who eat a normal Western diet, the effect on blood cholesterol of eating 2 or 3 extra eggs per day over a long period of time can hardly be measured" (p. 108).

Ravnskov dealt with the smear campaign against eggs as follows: "To find out how egg consumption influenced my own blood cholesterol, I once used myself as a human guinea pig without asking the ethics committee at my university. Before and during the experiment I analyzed my [total serum] cholesterol. My usual egg consumption is one or two eggs per day, and my cholesterol value at the start of the experiment was 278 mg/dL, very close to a determination of [my] blood cholesterol made 10 years earlier." On day 0, Dr. Ravnskov ate 1 egg; on day 1, 4 eggs; on day 2, 6 eggs; and on days 3–8, 8 eggs per day! "The data from my daring experiment showed that instead of going up, my cholesterol went down a little [to 246 mg/dL]" (p. 109).

Dr. Ravnskov explains that older people have higher concentrations of cholesterol in their blood than younger people. If the serum cholesterol is graphed against the degree of atherosclerosis with all age groups lumped together,

there seems to be a direct relationship. But if only people of about the same age and sex are considered, there is only a weak relationship with a correlation coefficient of 0.29. When the subjects with FH are left out, even the weak correlation vanishes.

Some early research was ignored: "The first study designed to demonstrate a possible correlation between blood cholesterol and degree of atherosclerosis was published by the pathologist Kurt Land, and the biochemist Warren Sperry of the Department of Forensic Medicine at New York University… [in] 1936. They studied large groups of individuals who had died violent deaths. To their surprise, they found absolutely no correlation between the amount of cholesterol in the blood and the degree of atherosclerosis…. Because Land and Sperry were cautious and methodical, their study should have nipped the DHT in the bud. Or, more accurately, if those who promoted the DHT later on had read Land and Sperry's paper before beginning their research, they would probably have dropped the idea at once…. But the few who remember Land and Sperry misquote them and claim that they found a connection, or they ignore their results by arguing that cholesterol values in the dead are not identical with those in the living…. [To resolve this argument]… In the city of Agra in India, Dr. K. S. Mathur and his co-workers performed a similar study [in 1961]. Their first step was to measure blood cholesterol in 20 patients shortly before death and then a varying number of hours afterwards. They found that the cholesterol values were nearly the same if samples [were taken] before death and within 16 hours afterwards. Thus, blood samples taken very shortly after death are reliable—an important confirmation of the study done by Drs. Land and Sperry. Dr. Paterson's group in Canada confirmed this…. Next Dr. Mathur and his colleagues studied 200 people who had died in an accident, without any preceding disease. Like Drs. Land and Sperry, and like Dr. Paterson, the Indian researchers could find no connection between cholesterol values and the degree of atherosclerosis. Those with low cholesterol had just as much atherosclerosis as those whose cholesterol was high" (p. 118–120).

A report from the Framingham Study found a weak correlation coefficient, 0.36. Dr. Ravnskov found what distinguished this report from all the others he studied: only 14% of the Framingham dead were chosen for autopsy, not close to 100%, as in the other studies. The risk of preferentially selecting subjects who probably had FH hypercholesteremia was said by Ravnskov to be great. To prove that high cholesterol is the villain—and not just an innocent bystander—demands that a change in the cholesterol concentration in each individual is followed by a change in degree of atherosclerosis in the same direction. Examination of all studies on this relationship showed no correlation.

Dr. Ravnskov reviewed the evidence presented earlier—that cholesterol levels in blood, or HDL or LDL levels, or the ratio of the latter are not correlated with either atherosclerosis or heart attack rates. It follows that forcible reductions of cholesterol levels by drugs (since diet alone does not change the levels much) would not be expected to change the rate of CHD by lowering cholesterol levels. However, two things are possible with drugs. First, some

unknown mechanism unrelated to cholesterol could lengthen lifespan. Second, some side effect unrelated to cholesterol could shorten lifespan. Furthermore, the pervasive misconceptions about cholesterol have made it nearly impossible to carry out a placebo-controlled trial of new drugs because it is mistakenly considered unethical not to treat people with high cholesterol levels!

"In the 1960s, Professor Jeremy Morris of London, England, led a team of physicians and scientists in an investigation to see whether the replacement of animal fat with soybean oil could have some preventive effect on CHD. This oil is rich in polyunsaturated fatty acids, those that are considered [erroneously to be] protective against atherosclerosis and CHD. Enrolled in the trial were about 400 middle-aged men who had previously been admitted to 4 London hospitals because of a heart attack; half of these received a diet containing large amounts of soybean oil. (This is one of the few trials sponsored solely by a government, and not by a drug company or any other vested interest.).... When the researchers analyzed the results 4 years later, they could find no beneficial effects from using soybean oil. Although, in this particular trial, blood cholesterol had decreased considerably in the treatment group, 15 had died of a heart attack. In the control group, 14 had died; and the number of non-fatal heart attacks was the same in both groups" (p. 144–145). Other trials gave the same result.

These trials on patients who already had symptoms of CHD are called "secondary prevention" trials. Now Ravnskov describes some of the "primary prevention" work, that is, trials with healthy or at least symptomless patients. Much larger numbers of subjects are needed in an attempt to obtain good statistical results, and compliance is always suspect because of the severe side effects of many of the treatments or drugs used in subjects who are basically healthy, and thus may not be compliant because they lack any fear of poor health. When you recall the conclusions in Myth 2, that high cholesterol does not cause CHD, you will not be surprised at the negative findings he has described.

In 1967 the Coronary Drug Project tested nicotinic acid, clofibrate, thyroid hormone, and estrogen to lower cholesterol levels in middle-aged men who had already had at least one heart attack. After 7 years the death rates were the same as those of the controls. Worse, all 4 drugs had severe side effects. The researchers fell victim to the "surrogate endpoint." This is the use of an easily measured factor, such as total cholesterol level or blood pressure, as a surrogate or substitute for what is really important—increasing lifespan or the quality of life. In a later chapter Ravnskov calls this a "surrogate outcome."

In 1970 the Upjohn Co. (Kalamazoo, MI, USA) sponsored a trial on 2000 men and women with high cholesterol involving its then-new drug colestipol. Two years later no effect was seen in the women. The number of heart attacks in the men in the treatment group was cut in half, a remarkable result never seen before or since. But Ravnskov found the snag: The selection of the patients to be in either the treatment or control groups was done by Upjohn's scientists with the results of the participants' blood assays in hand; it was any-

thing but random. Ravnskov noticed that there were too many control patients with FH. Your reviewer notes that, in the 1996 *Physicians Desk Reference* entry for this drug, there is not a shred of evidence for longer lifespan; moreover, there were no restrictions on prescribing this drug for women.

For the World Health Organization trial, researchers assayed blood cholesterol in 30,000 healthy, middle-aged men in Edinburgh, Prague and Budapest. The 10,000 men with the highest blood cholesterol levels were selected for the trial, half to receive clofibrate, half placebo. After 5 years there were more fatal heart attacks in the clofibrate group. There were 128 total deaths in the clofibrate group and 87 in the placebo group. "Yet clofibrate is still recommended in many countries as a useful drug" (p. 151)!

The National Heart, Lung and Blood Institute embarked on a new jumbo trial called The Lipid Research Clinics Coronary Primary Prevention Trial (LRC) to test the effectiveness of cholestyramine (Bristol-Myers Squibb). To find about 4,000 test subjects, the 0.8% of 500,000 middle-aged men with the highest cholesterol levels were selected. All were given a few weeks of dietary indoctrination to solve the supposed ethical dilemma of not otherwise treating the controls. Half received cholestyramine and half placebo for 7–8 years. Of those treated, 190 (10%) had nonfatal heart attacks, vs. 212 (11.1%) of the controls. For fatal heart attacks the figures were 1.7% and 2.3%, a difference of 0.6% absolute or 12 individuals. In the summary of the paper on this trial these unimpressive results were presented as a 19% lowering (relative risk) of nonfatal heart attacks and a 30% lowering of fatal heart attacks.

This is an example of scientific fraud among efforts to support the DHT. Ravnskov notes how the reporting of differences in fatality rates by percent reduction (say, a 50% reduction in relative risk) is actually misleading when the actual death rates are quite small in both the treatment and control groups of subjects in diet or drug studies. For example, a treatment that changes the absolute survival rate over a multi-year period from 99.0% to 99.5% represents a 50% reduction in relative risk, from 1% to 0.5% absolute. This is often described in papers as a 50% reduction in death rate. However, when the difference is barely significant statistically, as was often the case, Ravnskov points out that there is no real reason to recommend adoption of the treatment, especially if there are potentially serious side effects.

Ravnskov continues: "And this was not the only way in which the LRC figures were manipulated. In order to reach their 30% figure, the LRC directors included the uncertain cases, those who may or may not have died from a heart attack, and to reach their 19% figure, they excluded the uncertain cases. If it had been the other way around the results would have been 24% rather than 30, and 15% rather than 19. In other words, they selected data that gave them the results they were seeking." Even worse, the directors abandoned the 99% confidence level with a 2-tailed t-test and settled for a 95% confidence level with a 1-tailed t-test. [In an end note Ravnskov points out that scientists have agreed that a 1-tailed t-test should be used only when it is certain that the result

will go in just one direction. It is not supposed to be used when the drug (or other intervention) may do harm rather than good.] Very revealing is the absence of the number of deaths from all causes. More men in the treatment groups died by violence or suicide (11 vs. 4). In the misleading manner used by the LRC to present results, they could have said that violent death was 175% more likely in the treatment group. In order to achieve essentially nothing, the treatment group suffered gas, heartburn, belching, bloating, abdominal pain, nausea and vomiting; contrarily, the study's report assured readers that the side effects were not serious. Some promoters then claimed that "now that it had been proven that it is worthwhile to lower cholesterol no more trials were necessary!".

Ravnskov goes on to show that trials with a seemingly positive result, even for a surrogate endpoint, are cited much more frequently than trials with a negative result. This gives a positive feedback effect, reinforcing the dogma that reducing cholesterol level is beneficial, but this sort of misdirected effort actually does not produce better health.

A study showed that patients treated with lovastatin and colestipol had their coronary arteries narrowed as shown by X-rays. The title of the paper on this study indicated the opposite: "Regression of coronary artery disease as a result of lipid lowering therapy…" Ravnskov then presents the results of a meta-analysis of 26 cholesterol-lowering trials that met his standards. Result—no benefit.

Ravnskov presents the results of a number of trials of statin drugs in which total death rates are slightly lower than those of the controls. But in an early trial (EXCEL) of lovastatin (Merck's Mevacor) on 8,000 healthy subjects with cholesterol levels between 240–300 mg/dL, the absolute death rate from all causes after just 1 year was 0.5% vs. 0.2% in the placebo group. In another trial with lovastatin on healthy subjects, 5,000 men and 1,000 women, 2.4 % in the treatment group died after 5.2 years vs. 2.3% of the controls. Regarding studies carried out on lovastatin, studies lasting 10 years, Ravnskov found no reports on total death rates. Ravnskov queried Merck & Co. directly and was told that the trial was not designed to measure the total clinical outcome!

Deaths from heart attacks were significantly lower in some trials of other statin drugs, but total deaths were 3% absolute lower at best. In the CARE trial, Ravnskov showed that a 12% reduction in heart attacks (- 1% absolute) was overbalanced by a 1,500% increase in cases of breast cancer (+4% absolute). Total deaths were not given. Once again this shows that women should not be treated with statin drugs (or at all), and the benefit for men is quite limited at best with simvastatin and pravastatin.

The incidence of breast cancer was said to be a fluke, and was not observed in the LIPID trial lasting 6 years, in which overall mortality was said to be reduced by 22%, but this was relative risk; an overall drop in mortality of 3% absolute was achieved in subjects with a broad range of initial cholesterol levels (Tonkin et al., 1998).

In middle-aged men with hypercholesteremia treated with pravastatin for 5 years, death from all causes was reduced by 22%, but this was relative risk; an

overall drop in mortality of 1% absolute was achieved (Shepherd et al., 1995).

Ravnskov tries to explain what the polyunsaturated oils are chemically. His effort is one of the few weak points in this book. The degree of saturation actually refers to whether hydrogen can be added to the oil. If so, some of the carbon-carbon bonds in the fatty acid portion of the oil molecule must have been double bonds, which may take up 2 hydrogen atoms each. Olive and canola oils are the best examples of monounsaturated oils (a sole double bond in each fatty acid portion), and safflower, cottonseed and soybean are examples of polyunsaturates (2 or more double bonds in each fatty acid portion). If hydrogen cannot be added in the presence of a catalyst, the oil (or more likely the fat) is said to be saturated, meaning that it cannot take up any more hydrogen. Palm and coconut oils are the best examples. Tallow, lard and chicken fat have some saturated and some monounsaturated fatty acids in their molecules; they are not made entirely of saturated fatty acids by any means. The risk of eating *trans* fats is presented at some length.

Ravnskov gives examples of reports of interventions with little or no statistical significance being denied time for presentation at meetings and explains that offers to write minority dissenting reports on certain trials were being denied on the grounds that the conference was supposed to produce a consensus. Statements of DHT proponents and their recommendations are quoted followed by a Ravnskov's refutation of the claimed evidence. He reiterates that even drastic lowering of cholesterol levels with drugs (diet being ineffective) is of no benefit to women and of marginal benefit to men. Ravnskov presents arguments against trying to lower cholesterol levels in children.

If Ravnskov were a lone voice among the Philistines, his credibility would be lowered. In Myth 9 he gives the names of several of the scientists who support his position. This includes Mary S. Enig, President of the Maryland Nutritionists Association, whose research concerned the hazards of trans fats and who has written *Know Your Fats*, a book on the composition and effects of fats in the diet. Michael Gurr, Professor of Biochemistry, School of Biological and Molecular Sciences, Oxford University, pointed out the insufficient correspondence in vascular pathology between animal models and man, the selection bias in epidemiologcal evidence, the lack of correlation between CHD and fat consumption, and the lack of improvement in coronary mortality after dietary and drug intervention. George Mann, Professor of Medicine and Biochemistry at Vanderbilt University (Tennessee), realized from his studies of the Masai in Kenya that animal fat could not possibly be the cause of high cholesterol and CHD, and he has been open and fearless in his criticism of the LRC directors and has called the DHT "the greatest scientific deception of this century, perhaps of any century." Michael F. Oliver, former Professor and Director of the Wynn Institute for Metabolic Research (London, UK), has warned against campaigns for cholesterol lowering in the general population; criticized those who think that the increased mortality from non-medical causes in trials, such as suicide, is due to chance; and is uneasy about the link be-

tween low cholesterol and cancer. Edward R. Pinckney, editor of a number of medical journals, published a book in 1973 called *The Cholesterol Controversy,* which summarized all the inconsistencies in the cholesterol literature; he describes the dangers of lowering one's cholesterol and devotes an entire chapter to the political drama preceding an early anti-cholesterol campaign. Raymond Reiser is a former Professor of Biochemistry at Texas A&M University. He decried the practice of referring to other reviews, each taking the last on faith, which has led to the acceptance of a phenomenon (diet-heart) that may not exist. He reviewed work on fatty acids in the diet, found flaws in most of the studies, and concluded that the type of fat in the diet does not make much difference. He analyzed the references used by the American Heart Association in its rationale for dietary recommendations and found no supportive studies but found instead some that contradicted the recommendations. Ray Rosenman is the retired Director of Cardiovascular Research in the Health Sciences Program at SRI International in Menlo Park (California) and Associate Chief of Medicine, Mount Zion Hospital and Medical Center in San Francisco, CA. In a recent review he wrote that neither diet nor the identity of serum lipids (fats or oils) can explain wide national or regional differences in rates of CHD, or the 20th century variations in rates of CHD. He also pointed out that the CHD preventive effects of diets and drugs have been exaggerated by a tendency in trial reports, reviews, and other papers to cite and inflate supportive results while suppressing discordant data. The late Russell Smith was an American experimental psychologist with a strong background in physiology, mathematics and engineering. In his 1989–91 review of the diet-heart theory he wrote: "…studies are often poorly designed and data are often inappropriately analyzed and interpreted.… Much of the literature, therefore, is nothing less than an affront to the discipline of science.…" He considered much of the work of the National Heart, Lung and Blood Institute and of the American Heart Association to be "incompetent" and "sloppy," and that their political and financial power is enormous and without equal, producing a juggernaut willing and able to suppress evidence and logic. "Equally culpable are the editors of the many journals who publish articles without regard to their quality or scientific import. It is depressing to know that billions of dollars and a highly sophisticated medical research system are being wasted chasing windmills." In an extensive 2-part review, William E. Stehbens, Department of Pathology and Molecular Medicine, Wellington School of Medicine, New Zealand, demolished the diet-heart theory and gave specific examples of misinterpretation of data, of the misleading use of relative risk rather than absolute death rates, and of the medical folly of trying to lower cholesterol levels in almost anyone (Stehbens, 2001). Now retired, Lars Werkö, previously Professor of Medicine at Sahlgren's Hospital, Gothenburg, Sweden, Scientific Director at the Astra Co. (now Astra-Zeneca), and head of the Swedish Council on Technology Assessment in Health Care, criticized the design of the Framingham Study and pointed out inaccuracies and sloppy data gathering in the MRFIT trial.

As it happens, a number of other physicians and scientists in addition to the ones in Myth 9 agree with Ravnskov's positions. Linus Pauling, in his 1986 book *How to Live Longer and Feel Better* noted that the Framingham Study showed no correlation between CHD and fat intake or with cholesterol intake.

Thomas J. Moore, a medical reporter based in Washington, D.C., wrote an article in the September 1989 issue of *The Atlantic Monthly* actually called "The Cholesterol Myth," in which he examined the literature, much as Ravnskov did. Moore's conclusions: "Lowering your cholesterol is next to impossible with diet, and often dangerous with drugs—and it won't make you live any longer." This review was also used in Moore's 1989 book *Heart Failure: A Critical Inquiry into American Medicine and the Revolution in Heart Care.*

William Campbell Douglass, Jr., M.D., in 1993 wrote a brochure called *Eat Your Cholesterol: How to Live Off the Fat of the Land and Feel Great!* (Atlanta, GA: Second Opinion Publishing). Many of the dietary studies and trials are the same ones evaluated by Ravnskov, but they are treated in a very popular tone.

John B. Allred came to nearly the same conclusions as did Dr. Ravnskov (Allred, 1993). An even more extreme view aired in the United Kingdom is that serum cholesterol level is a relatively poor predictor of CHD, that the many misclassifications may have damaging psychological effects, and that in most cases, even testing cholesterol level, as well as treating raised concentrations, is a waste of National Health Service resources (Sheldon, 1998).

Kilmer S. McCully, Ph.D., M.D., in technical papers (McCully, 2001) and in his book (McCully, 2000) wrote, "But no study anywhere has ever proven that lowering the amount of cholesterol in the diet reduces the risk of heart disease. And lowering cholesterol through drugs won't prevent arteries from hardening if homocysteine is high." McCully is the discoverer of the fact that the undesirable amino acid called homocysteine is an actual cause of atherosclerosis and CHD.

Charles T. McGee, M.D., wrote, "The cholesterol theory is a fraud on the American public… the theory will be exposed as a scam," in his book *Heart Frauds: Uncovering the Biggest Health Scam in History* (2001, p. 76).

The authors of 2 recent reports on the use of electron beam tomography (EBT) to detect calcified plaques in coronary arteries were very circumspect in interpreting their findings, especially in their abstracts, perhaps not fully aware of how completely their findings demolished the DHT. While EBT was very predictive of hard coronary events in both studies, the difference in hypercholesteremia between the patients with events and those without was not significant. There was absolutely no significance for HDL cholesterol (HDLC) levels as predictive, and the spread in the values for LDL cholesterol, total cholesterol (TC), TC/HDLC and triglycerides shows none of these to be usefully predictive despite the low p values (Hecht et al., 2001; Raggi et al., 2001).

Based on Ravnskov's meticulous analyses as well as the considerable support for his stance shown by others who have also studied the cholesterol data,

this book is recommended without reservation. Physicians and other health professionals as well as anyone threatened with cholesterol-lowering treatments would be enlightened and better able to resist worthless treatments. Health insurers might reconsider compensation for frequent (or any) clinical assays for cholesterol or triglycerides, let alone expensive treatments to lower cholesterol levels that reduce quality of life without prolonging it significantly.

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The Placebo Response: How You Can Release the Body's Inner Pharmacy for Better Health by Howard Brody with Daralyn Brody. New York: Cliff Street Books (HarperCollins), 2000. 312 pp. $25, cloth; ISBN 0-06-019493-6; HarperTrade, 2001, $14, paper; ISBN 006093297X.

"The placebo response occurs when we receive certain types of messages or signals from the environment around us. These messages work in some fashion, at some level, to *alter the meaning* of our state of health or illness" (p. xvi; italics in original).

Howard Brody has a Ph.D. in philosophy as well as a medical degree, is a practicing physician and medical educator, and has been interested from the beginning of his career in the placebo response. This book is written in casual "talking-to-you" style and brings to bear common sense as well as a wealth of practical clinical experience. I cannot recommend this book strongly enough.

The Placebo Response offers much food for thought about the powerful, mysterious placebo,[1] and anomalists will discern significant possible implications for psychic as well as for psychological phenomena. For example, the emphasis on alteration of meaning is intriguingly consonant with the psychotherapeutic approaches of Albert Ellis[2] (humanistic psychology, rational emotive therapy) and Maxie Maultsby[3] (rational behavior therapy): *intellectual* or *rational* interventions effect desired *emotional* or *affective* changes. Those approaches emphasize doing something oneself as opposed to having a psychiatrist or psychotherapist do something, and Brody also emphasizes that "previous writing about the placebo response has treated it as something done *to* you, over which you have little if any control. I'm proposing that, if we line up the scientific clues in the right order, we'll master a variety of means by which we can employ the placebo response and the inner pharmacy to benefit ourselves" (p. xix). The book then proceeds to line up plausible scientific clues and to explain what the inner pharmacy is.

Relevant to parapsychology is that the personality trait of *acquiescence*—which is by no means the same as passivity (pp. 48, 240, 246)—may be linked to placebo responsiveness (pp. 35–36). That is reminiscent of a considerable body of anecdote or folklore that psychic phenomena cannot be controlled at will, and with the sometime suggestion from the PEAR group that the mechanism at work in mind-machine interactions is one of sensing information rather than of exerting physical influence[4,5] (Figure 7 in the latter reference affords a striking illustration of this idea).

Chapter 1 of *The Placebo Response* considers what the placebo response is and how it arises. Important seem to be *expectancy* that something good will happen; *conditioning* can help, to some stimulus that has often been associated with good things happening; and *meaning*, how we interpret what is going on—*symbols* carry great significance. Thus, when mothers routinely employ Band-Aids to soothe children, those sticky pads in themselves may become able to evoke a response to the mother's loving care, even in absence of the mother (p. 8). [Perhaps the happy faces on some adhesive pads designed for children are more than a marketing gimmick!]

Brody now refines his definition of placebo response: "A change in the body (or the body-mind unit) that occurs as the result of the symbolic significance which one attributes to an event or object in the healing environment" (p. 9). The typical "sugar pill" acts because of a symbolic effect; "body-mind" indicates a refusal to "regard physical and mental treatments as being in fundamentally different categories" (p. 10); and "change" acknowledges that the placebo response can be positive or negative (in the latter case it is often

termed *nocebo*). Brody points out here that some commonly used terms for placebo—"inert treatment" and "nonspecific responses"—are misleading. Moreover, the emphasis on meaning undercuts the common misconception that deception on the part of a healer is required to induce a placebo response in the patient (p. 14).

The next chapter reviews the history of attitudes toward placebo. Once synonymous with lies or quackery, it has come to be recognized as a usable phenomenon. Denigration of "suggestible" people, and the idea that some things are "only in the mind", have been superseded: the distinction between physical and mental illness is acknowledged nowadays to be artificial. As to "suggestibility", Chapter 3 reveals that "we are all potentially placebo receptors". Deception is not needed to arouse a placebo response—if for no other reason than that some of us, when given a placebo and told that it is a placebo, choose to believe otherwise (pp. 38–40); it is, after all, common knowledge that clinical trials call for some sort of blinding and even double-blinding, so we know that what we are told we are getting may not be what we are in fact getting.[6]

The "inner pharmacy" is Brody's term for the illness-resisting, self-healing mechanisms that living systems possess: the immune system, the capacity to release endorphins, and presumably much that we do not yet know about or understand. This inner pharmacy is "hard-wired" to the mind in some fashion, and so mental activity—the controlling software—can influence it. But how all this works to produce placebo responses remains largely mysterious and there are many individual variations: "What turns on my inner pharmacy may not turn on yours; or what turns on mine in one situation may fail to do so in another situation; or what turns mine on full blast one time, may turn it on only a little on another occasion". Ulcers have responded to placebo—at a rate of 10% in some clinics and 80% in others.... (pp. 49–53).

Chapter 5 reports material evidence of the extraordinary effectiveness of placebo at least occasionally: for example, overriding or even reversing the physiological response induced by an administered medication. Ligation of mammary arteries to ease angina, and arthroscopic treatment of arthritis of the knee, turned out to be no more effective than placebo—in these cases, *pretended* surgery. In one psychiatric study, the *physicians'* expectations turned out to influence the outcome of a clinical trial.

Altogether, though, placebo response does seem to correlate with *expectancy* of cure. Chapter 6 argues that this is not, however, the whole story: *conditioning* can evoke a placebo response. Thus, childhood experience that painful injections are associated with powerfully effective medication may cause *painful* injection to induce a greater placebo response than a painless injection does (p. 75).

Conditioning may, however, work through producing expectancy, and Brody acknowledges the difficulty of designing studies to clarify that. He notes (p. 77) that laboratory studies are not necessarily valid indicators of real-life situations; this is one of the obvious similarities between the phenomenon

of placebo and of psychic happenings. *If* psychic phenomena are real, then they must involve the body-mind connection in some manner that is like what happens in the placebo response: therefore, it may not be feasible to elucidate fully the nature of spontaneous psychic phenomena by controlled laboratory investigations. Thus, "scientific" studies of consciousness-related phenomena necessitate a more encompassing approach than the traditional protocols that the natural sciences have evolved.[7,8]

In discussing conditioning and expectancy, Brody points to a largely unstudied question of considerable practical import: "Before now, when medical scientists considered the impact of how many times a medicine was taken each day, they usually had only one thing in mind: If we have to take medicines three or four times a day, we tend to forget or to give up; while if we take them only once or twice a day, we are more likely to be 'compliant.' But what if it turns out that the more often we take the medicine, compliance goes down, yet the conditioned response goes up?... For some people the fewer-times-a-day approach might be best; for others the more-times-a-day approach might be superior" (p. 81).

The importance of *meaning* and *symbols* is discussed in Chapter 7. The uncomfortable fact of nocebo is dealt with in the following chapter, one example being that of mass hysterias. Brody suggests that nocebo likely acts through the same pathways as does placebo; that suggests the optimistic view that methods for harnessing the placebo response would at the same time be methods for avoiding a nocebo effect.

Biochemical and neurological processes are reviewed in Chapter 9. Our present knowledge is rudimentary. One study indicated that pain relief by expectancy-induced placebo might work through activation of an endorphin-releasing process, whereas conditioning-induced placebo seemed to work in a different manner (p. 115). How much remains to be investigated! "The stress pathway is the oldest example in twentieth century medical science of linking biochemistry with the mind, yet there are few, if any, studies which tie it directly to the placebo response" (p. 115). How difficult the problems are is illustrated by the reports that "the best placebo responders are people with a moderate level of anxiety when they first see the physician" (p. 122): moderate by contrast to both high and low! Another illustration of the difficulty is that the once highly touted "psychoneuroimmunology has not yet produced many of the remarkable gains it first promised" (p. 125).

A fuller discussion of how difficult it is to study the placebo response is provided in Chapter 10, "The Placebo Response and its Mimics". Illnesses are typically self-limiting: how to distinguish placebo effects from natural recovery? Observational studies are subject to statistical fluctuations and their misinterpretations: chance correlations can occur (p. 134), and there are random variations and regressions to the mean. The mere fact of being in a clinical trial may induce behavioral changes conducive to better health (the Hawthorne effect, pp. 135–136), and minor problems may be noted and treated for clinic at-

tendees that would generally make them healthier and make them feel better (see also note 4 below). Where self-reporting is the only real measure, as with pain relief, the "obliging-subject" phenomenon may make it appear that there is a placebo response even where there is none (p. 136). Some "inactive" placebo pills may actually exert a physiological effect, because an appropriate placebo must feel exactly the same to the subject as the actual medication. Semantics can also get in the way: some workers apparently deny that there is a placebo effect while admitting the existence of psychosomatic effects (pp. 137–138).

In Chapter 11, the similarities and differences between conventional and alternative medicine are discussed with considerable insight. Chapters 12 through 15 provide practical and sensible suggestions for accentuating one's placebo response, and Chapter 16 talks about choosing a helping healer. The Epilogue pays respect to placebo as a part of the mystery of healing: "Just *how and why* it happens may remain a mystery. *That* it happens is as certain as anything can be in the practice of medicine" (p. 254).

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Endnotes

[1] See also reviews in *JSE* 14 (2000), pp. 485–491, of *The Powerful Placebo—From Ancient Priest to Modern Physician*, by Arthur K. Shapiro & Elaine Shapiro and of *The Placebo Effect—An Interdisciplinary Exploration*, by Anne Harrington.

[2] Albert Ellis & Robert A. Harper. (1975). *A New Guide to Rational Living.* North Hollywood, CA: Wilshire Book Co., ISBN 0-87980-042-9.

[3] Maxie C. Maultsby. (1975). *Help Yourself to Happiness Through Rational Self-Counseling.* New York: Institute for Rational-Emotive Therapy, ISBN 0-917476-07-7 and 0-917476-06-9.

[4] Robert G. Jahn & Brenda J. Dunne. (1988). *Margins of Reality: The Role of Consciousness in the Physical World.* New York: Harper Brace Jovanovich.

[5] Robert G. Jahn & Brenda J. Dunne. (2001). A modular model of mind/matter manifestations. *Journal of Scientific Exploration, 15,* 299–329.

[6] Toward the end of May 2001, the news media (e.g., *Washington Post,* May 24, p. A1) cited a Danish analysis that challenges the reality of the placebo effect: more than a hundred studies in which various treatments had been compared with placebo as well as with "no treatment" showed no difference between placebo and "no treatment" other than a minor difference as to perception of pain. I choose to disregard this Danish analysis until it is ex-

plained how one can study patients under a "no treatment" protocol without inducing a placebo response. They must know that they are in a trial, otherwise they could not be studied. How can they be prevented from hoping that what is happening to them will be of benefit? Presumably the investigators in these cases had made the common but erroneous presumption that "placebo" means "sugar pill". I suggest that the small effect noted on pain perception may show that, so far as pain management is concerned, a dummy pill adds significantly to the placebo response consequent on the symbolic significance of participating in a clinical trial. Moreover, Brody (pp. 285–286) cites a meta-analysis done in 1994 that confirmed the reality of the placebo response, contrary to the Danish study.

[7] Robert G. Jahn & Brenda J. Dunne. (1997). Science of the subjective. *Journal of Scientific Exploration, 11,* 201–224.

[8] Imants Baruss. (2001). The art of science: science of the future in light of alterations of consciousness. *Journal of Scientific Exploration, 15,* 57–68.

Seeing Red: Redshifts, Cosmology and Academic Science by Halton Arp. Montreal: Aperion, 1998. iv +314 pp. Softcover, $25.00. ISBN 0-9683689-0-5.

Don't read this book! For if you should decide after reading this review to disregard this advice, you will need to prepare to have your universe turned upside down. Should you then make your way through this small-print, 306-page tour de force, you will very likely come away doubting what you thought you knew of the large-scale structure of the universe. The cosmological interpretation of redshift for quasars and active galaxy nuclei has been challenged often before, although never so successfully. But one seldom sees serious suggestions that even the redshift-distance relation for ordinary galaxies may be wrong, as you will see here. And as if the implied revolution in cosmology were not enough, your view of the professionalism of scientists and academics in general, and of astronomers in particular, will be another casualty of your reading.

One can consume this book to different degrees. For example, a short summary of the evidence and implications appears on pages 239–241. If the writing proves too technical in places, even with the aid of the extensive glossary, one can get the essence of the evidence by just scanning the plates (8 pages in color), figures, and captions that appear on almost every page. For example, it's not difficult to look at the picture of the x-ray filaments in Markarian 205, featured on the book cover as well, and to grasp the deep implications of that image. For if a low-redshift Seyfert galaxy is physically connected to and interacting with two high-redshift quasars, one on either side, then redshift can be neither a distance nor a velocity indicator. And that single picture then disproves the Big Bang and most of mainstream cosmology in its present form.

Arp knows the extragalactic sky perhaps better than any other living astronomer. He builds his case against the customary interpretation of redshift

methodically. The earliest hints of problems with redshift came in 1911 with the discovery that the bright, blue stars in our own Milky Way galaxy have systematically higher redshifts than the rest of the stars by about 10 km/s. Later observations showed that the O-stars in clusters within our galaxy are redshifted with respect to the B-stars by another 10 km/s or so—something called the "K-effect" and still disputed because it has no accepted theoretical explanation. However, doubts about the validity of the data were undercut and the K-effect confirmed by more recent measures of the redshifts of supergiant stars in the two Magellanic Clouds, nearby companion galaxies of the Milky Way. These too are redshifted by about 30 km/s with respect to other stars in those small galaxies. Yet no one suspects that all supergiant stars in our galaxy or in our immediate neighbors are fleeing away symmetrically from our own location well out toward one edge of the Milky Way.

Companion galaxies in general seem to have net redshifts that exceed that of their parents. All eleven companion galaxies in the Local Group have redshifts with respect to their parent, the Andromeda galaxy in the center of the group. Likewise, all eleven companion galaxies of the neighboring M81 group have redshifts relative to M81. Yet, if these companions were orbiting their parent galaxy, roughly 50% of them ought to have been blueshifted. Although the evidence for companion redshifts is less definitive for more distant galaxy groups, it is still statistically significant. Excess redshifts over blueshifts for companion galaxies relative to their parents is apparently a verifiable feature of the local universe. And that means the redshift must have some cause other than velocity.

We begin to get some clues about what may be happening when Arp reminds us of basic facts about radio galaxies. These were discovered long ago to have giant, usually double, radio lobes to either side, presumably the result of explosions and ejections of material from the parent galaxy. Higher resolution radio telescopes have found filaments connecting these lobes to the central galaxy. And the ejected radio pairs are now known to correspond closely with x-ray pairs across the same galaxy.

That brings us to the keys to unlocking the whole puzzle—the quasars—because quasars also often correspond with x-ray sources. High and low redshift quasars are associated far more often than reasonable chance allows. These sometimes display interactions and connections and often form pairs across low-redshift objects, which unrelated objects would not do.

Working entirely from the observational data, Arp shows us that ejections from active galactic nuclei at speeds up to 10% of the speed of light lead to escape. But ejections at slower speeds may not, especially since all ejections apparently decelerate on the way out. Slower objects end up captured at about 400 kpc from their parent galaxy. But both captured and escaped quasars end up with quite small peculiar velocities. Moreover, the closest and therefore most recent ejections have the highest relative redshifts and the lowest intrinsic luminosities. This leads Arp to suggest that the redshift of matter is an inverse

function of the age of that matter. As much as one wishes to resist this conclusion, Arp shows case after case that conforms to it, many found well after this hypothesis was in print, each with odds of thousands to one against chance. Moreover, these apparently ejected quasars with redshifts ordered inversely with distance from their parent also tend to line up along the minor axis of the parent galaxy.

The generality of these startling conclusions is shown by repeated examples, such as the Arp/Hazard pair of similar triplet quasars, having discordant redshifts and a Seyfert galaxy between them. Many other good examples were discovered by mainstream astronomers. However, even when looking in depth at single examples, Arp makes a compelling case against coincidence. A survey of all bright quasars showed that these have a concentration around the Virgo cluster at the center of the Local Supercluster, despite having redshifts that should place them far out into the universe in that direction and make them unrelated to one another. The most conspicuous quasar in the sky is 3C273, one member of a pair of quasars almost exactly aligned across the brightest galaxy in the Virgo cluster center. A peculiar hydrogen cloud known to be in the Virgo cluster near the coordinates of 3C273 has a long, narrow shape pointing back toward the quasar, which itself has a jet pointing toward the hydrogen cloud. An x-ray radiation map (see Figure 5-16) also shows connections between the cluster to the quasar. Yet the quasar is supposed to be 54 times farther away than the cluster, according to its redshift. As Arp says, over 30 years ago, the field of astronomy took a gamble against odds of a million to one that this situation was an accident. The newer x-ray and hydrogen cloud evidence have confirmed that this was a bad gamble, although the field is not yet ready to accept its losses and move on.

Because redshift is not a good distance indicator, Arp points out that apparent brightness often is. Quasars near M49 appear relatively random on a sky map until just the brighter ones in a half-magnitude range are plotted. Then magically, there appears a line of quasars emerging from M49, with redshifts decreasing with distance, just as the observation-driven model predicts.

Whenever secondary distance indicators are available, they support this picture. In some cases, Faraday rotation caused by traversing a magnetized plasma can be measured for quasars. So the amount of such rotation ought to be a distance indicator. But it was then discovered that quasars with redshifts of about 2 had only 1/3 as much Faraday rotation as quasars with redshifts of about 1, when they ought to have had twice as much rotation. By contrast, this is in accord with Arp's model because the redshift $z = 2$ quasars are intrinsically fainter and therefore are generally seen only at closer distances than those with $z = 1$.

Arp concludes that quasars are initially faint, point-like objects of high redshift that transform into lower-redshift, compact objects surrounded by a fuzz as they evolve. These develop into small, high-surface-brightness galaxies with more material around them. Ultimately these mature into normal, quies-

cent galaxies.

In this new view of relationships among astrophysical objects, Seyfert galaxies and their close cousins, BL Lac objects, are short-lived evolutionary stages associated with quasar ejection from active galaxy nuclei. In effect, Seyferts are quasar factories. Strong quasar number counts are associated with a nearly complete sample of bright Seyferts, as compared with non-Seyfert control fields. Some of these associations have laughable explanations in mainstream journal articles. Quasar GC0248+430 is described as a "possibly microlensed quasar behind a tidal arm of a merging galaxy," which just happens to be a Seyfert.

Indeed, quasars look to astronomers like small portions of active (Seyfert-like) galactic nuclei. Their pairing across such nuclei, their alignment with radio emission pairings, the correspondences of x-ray maps, and the data from optical emission lines all strongly support the ejection interpretation. If nature has not already provided enough hints, an apparent magnitude vs. redshift (Hubble) diagram shows that Seyfert galaxies have too much redshift at fainter magnitudes and do not follow the same relationship as normal galaxies. Indeed, the Hubble diagram for Seyferts trends toward that for quasars, which likewise do not show a normal Hubble relationship between brightness and redshift. One wonders how many different ways nature must repeat this message about redshift not corresponding to distance before it sinks in with the astronomers.

Other astrophysical objects are in accord with this message. Water maser emissions are also seen in pairs roughly aligned with quasars. X-ray filaments or jets emerge from Seyfert galaxies and end at quasars, often in pairs of similar redshift on opposite sides of the minor axis of the galaxy between. And high-luminosity spiral galaxies have excess redshifts compared to normal spirals, as judged by the Tully-Fisher method of judging distances from rotation rates (which is independent of redshift).

One might well wonder what galaxy clusters have so say about this, since these are clearly physically associated groupings of galaxies. The supporting evidence they provide is truly extensive. Classically, whole galaxy clusters obey a Hubble diagram relation between redshift and brightness, with a dispersion of just a few tenths of a magnitude. But 14 clusters north of Cen A have a much larger dispersion, with a maximum range of 4 magnitudes. Such clusters have no relationship of the type claimed for ordinary galaxies and call into question that the classical Hubble relationship can have the meaning usually attributed to it—that redshift indicates distance—for anything. We may simply have been fooled by both luminosity and redshift being functions of mass, which would lead to an apparent Hubble relationship despite no true distance dependence.

Some of the cluster examples are certainly head-turners. Abell clusters of galaxies with higher redshifts are distributed right down the spines of both the Virgo cluster and its Southern Hemisphere twin, the Fornax cluster. A com-

plete sample over a large region of the southern sky showed that the strongest x-ray cluster concentration had the two brightest galaxies (M83 and Cen A) at its center, despite much larger redshift for the x-ray clusters. In general, x-ray clusters appear more commonly with redshifts of about 0.06 than chance allows, which in Arp's interpretation marks them as young and intrinsically redshifted.

Supporting data includes cooling flow measures, which indicate that at least 100 solar masses per year are being lost from these clusters. This implies 100 billion solar masses in a billion years. Where are they going? The obvious possibilities can all be ruled out. BL Lac objects, at redshifts intermediate between quasars and cluster galaxies, are apparently progenitors of clusters of galaxies. Normal galaxies within certain redshift ranges tend to align on the sky in strings, with the lowest redshift galaxy near the center. For example, 13 of the 14 brightest Northern Hemisphere spiral galaxies in uncrowded fields fall on well-marked lines of galaxies that have concentrations of fainter, higher redshift galaxies. And there are anomalous faint, blue, often active galaxies that fill out clusters in the redshift range between 0.2 and 0.4. These apparently evolve into the higher luminosity, lower redshift objects seen at $0.02 < z < 0.2$. Finally, below redshifts of about 0.02 we find strings of galaxies aligned through the brightest nearby spiral galaxies, presumably representing the last evolutionary stage of protogalaxies before becoming the slightly higher redshift companions of the original ejecting galaxies. We are so accustomed to thinking of this sequence as a time evolution that it takes some effort to rethink the whole picture as a mass-luminosity-redshift evolution at a nearly fixed time.

So the young-appearing objects with the highest redshifts are aligned on either side of eruptive objects, which implies the ejection of protogalaxies and the association of redshift with youth. Increasing distance from parent leads to brighter, lower redshift objects, so this is the direction of evolution with age. At redshifts of about 0.3 and distances of about 400 kpc from the parent, quasars become very bright at optical wavelengths and in x-rays and evolve into BL Lac-type objects—a short-lived stage because there are few of them. Finally, these evolve into clusters of galaxies, which are seen to appear at comparable distances to the BL Lac objects, implying that clusters may originate from the breakup of BL Lac objects.

There is more. Tight multiple-quasars-image groupings were originally dismissed as observational errors until the gravitational lens theory was invoked. Then many more examples were quickly found. G2237+0305 was essentially a high-redshift quasar in the nucleus of a low-redshift galaxy. Lensing was the only way out for cosmologists. The four quasar images were all within one arc second of the galaxy nucleus. But Hoyle computed the probability of such a lensing event as two in a million. Moreover, instead of being arcs as lensing theory predicted, the quasar images are extended back toward the central galaxy. Real arc images don't look much like the predicted arcs either, but

rather like part of an expanded shell. This alternative is in better agreement with the existence of radial arcs, jet arcs, dog-leg arcs, and ejected jets that end in transverse arcs.

The last main observational area deals with the quantization of redshifts. In essence, redshifts do not take on all values with equal ease, as they must if they are caused mainly by the velocities of the observed objects. For example, redshifts near 0.061, 0.3, 0.6, 0.91, 1.41, and 1.96, etc., occur more frequently than chance permits. Smaller redshifts too occur at preferred periodic intervals, as Tifft has shown in a study confirmed in an independent sample by Guthrie and Napier. The existence of preferred values for redshifts proves that either we are at the center of a series of expanding shells, or redshift does not indicate velocity. Arp cautions that faint quasars with high redshifts do not continue to show this effect, perhaps because the form of the relationship changes at great distances from us (as faintness would suggest). Also, much of the spread that exists around these preferred redshift values is apparently due to the speed of ejection, which can be up to 0.1 c. The average redshift of a quasar pair generally falls closer to a preferred redshift value than does either individual redshift. BL Lac objects show the same quantization, but to a less pronounced degree, as befits their relationship to quasars. Figure 8-16 shows a striking set of bands and gaps for galaxy redshifts in the x-ray cluster Abell 85 that illustrates the redshift quantization effect at a glance.

Arp's strength is observational extragalactic astronomy. With theory he is less proficient, but he has enlisted the aid of Narlikar, Hoyle and others. The concept of mass increasing with age has no adjustable parameters (the characteristic age being given by the measured age of our own galaxy), yet it allows prediction of intrinsic redshifts for objects from K-effect stars to quasars, with results better than an order of magnitude. The Big Bang with many adjustable parameters cannot do as well. Redshift, then, indicates youth. And the slope of the Hubble diagram comes directly from our own galaxy's age. Since luminosity evolves with mass squared, the apparent brightness-redshift relationship is coincidental, and not an indicator of distance. I am no doubt biased here by seeing simpler theoretical explanations for Arp's observational constraints than his variable-mass theory can provide. But Arp concedes in places that theories need to evolve with discoveries, something that the Big Bang stopped doing at a fundamental level a generation ago.

Some of the most entertaining reading in this book is provided by Arp's interactions with his colleagues and with referees and journal editors. Arp spices up these exchanges with a bit of his own philosophy. Despite its pessimism, I wonder how any of us could have evolved a philosophy much more optimistic if we had been in Arp's shoes. Anonymous referees frequently use abusive language (such as "ludicrous") or unwarranted generalizations (such as "bizarre conclusions based on an extreme bias of the authors wishing to find non-cosmological redshifts"). It was not infrequent to find referees suggesting that the implications should have been used to prove the observations wrong! A Nobel

laureate and former teacher is quoted as saying "Arp did not get anything right in my course. I should have flunked him but I could not bear to have him repeat the course with me."

We see in the anecdotes frequent occurrences of "sniping," unbacked claims that something is true or false for some reason that is not presented to the author for rebuttal. One example: "Oh those claims have been completely disproved." Arp introduces a few names for some of these battle tactics himself. The "Pleiades maneuver" is one such: Measure so much background that the statistical significance of the obvious foreground (such as the Pleiades cluster) is reduced to insignificance. Reaction to the x-ray map showing the connection of the Virgo cluster and quasar 3C273 produced five arrogant and patronizing referee rejections at two journals and were viewed even by some colleagues "like a grisly auto accident along the highway."

Sadly, the mainstream is well adapted for survival. So when Arp succeeds in running the minefield and getting his results published despite the referees, an unwritten understanding is that no discussion or citations will follow, so the embarrassing result will soon be forgotten. Arp suggests that a sampling of referee reports, showing "manipulative, sly, insulting, arrogant, and above all *angry*" referees, ought to be published because it would allow people to evaluate the objectivity of the information they are being allowed to read.

Here are some brief quotes outlining what Arp has learned from these exchanges.

- "When presented with two possibilities, scientists tend to choose the wrong one."
- "The stronger the evidence, the more attitudes harden."
- "The game here is to lump all the previous observations into one 'hypothesis' and then claim there is no second, confirming observation."
- "No matter how many times something has been observed, it cannot be believed until it has been observed again."
- "If you take a highly intelligent person and give them the best possible, elite education, then you will most likely wind up with an academic who is completely impervious to reality."
- "When looking at this picture no amount of advanced academic education can substitute for good judgment; in fact it would undoubtedly be an impediment."
- "Local organizing committees give in to imperialistic pressures to keep rival research off programs."
- "It is the primary responsibility of a scientist to face, and resolve, discrepant observations."
- "Science is failing to self-correct. We must understand why in order to fix it."

The book has many more like these.

As with any work of such length and depth, a few errors turned up along with a few points that are of dubious merit. None of us can be experts in everything, and we are always pushing the limits of our knowledge and training. Worth a comment are these points:

- Arp's arguments against tired light models (p. 97) make a common invalid assumption that quantum particles must be responsible for the energy loss. But there is good reason to suspect that quantum particles are by no means fundamental.
- Arp's proposal (p. 219) that even planetary and satellite masses may be quantized uses an invalid statistical argument when bridging large ranges of mass. But he may well be right for small mass differences. Origin in twin pairs by fission usually creates masses in the approximate ratio of 5 to 4, which may partly explain Arp's planet statistics. It might also explain his magical 1.23 redshift quantization ratio if a similar fission process is responsible for the twin ejections in galaxies.
- On p. 234, Arp cites the surface brightness test, which must vary as $(1 + z)^4$ in the Big Bang. He applies that to his own model on the assumption that observations support it. However, the observed dependence goes as $(1 + z)^2$. Evolution of galaxies is said to be responsible for the difference in the Big Bang, but that argument would not apply to Arp's model.
- On p. 237, Arp incorrectly states that the cosmic microwave radiation must come from a thin shell, saying that this has not been explained. But that radiation is supposed to have flooded the universe shortly after the Big Bang and been cooling ever since. So every point in the universe is today receiving cooled radiation, and there is no shell anywhere. Arp correctly goes on to provide more probable explanations for the radiation than a fireball residue.
- Arp's use of statistics cries out for him to explain the difference between *a priori* and *a posteriori* probabilities, if only to assure us that *he* understands the difference and its importance.
- It was disappointing to see no mention of the role of giant elliptical galaxies in the evolutionary scheme of things.

Arp correctly points out that one side in this meaning-of-redshift debate must be completely and catastrophically wrong. This leads him to wonder how many other uncertain assumptions might exist in other areas affecting our daily lives about which we are innocently overconfident. That is perhaps the most sobering thought of all.

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A Different Approach to Cosmology by Fred Hoyle, Geoffrey Burbidge, and Jayant V. Narlikar. Cambridge, UK: Cambridge University Press, 2000. 400 pp. $59.95 hardcover. ISBN 0-5216622-3-0.

Beginning about fifty years ago, the community of astronomers was gripped by one of the great scientific debates of the twentieth century. The issue was "Has the Universe existed forever, or was it created in a singular event, from nothing?" Although the question is one to be answered by science, it has a theological tinge and it touches on such fundamental matters regarding who we are and what our place is in the larger scheme. Not surprisingly then, the debate between the major protagonists was often heated and appears to have been driven as much by philosophical as by scientific considerations.

For most astronomers, the issue was settled in 1965, with the discovery of the cosmic microwave background (CMB), interpreted as the relict radiation from the cooling fireball of the big bang. Here were all the ingredients of a popular drama: the titanic conflict, the crucial prediction, the dramatic confirmation. Since that time, the dominant view has been that the Universe originated in a big bang about 14 billion years ago and has been expanding ever since. Possibly about 99% of working cosmologists adhere to this position.

There is, however, a tiny handful of dissidents, mostly elderly survivors of the pre-CMB earlier era, who continue to maintain that the Universe had no such origin, that it expands and has done so forever, maintaining a quasi-steady state by the episodic creation of matter to fill up the emptiness which would otherwise develop. This book has been written by three of these dissidents; one of the authors, Fred Hoyle, died on 20 August 2001.

A Different Approach is in part a polemic. It advocates the authors' particular cosmology and argues that modern cosmology is suffused with conformity and led by zealots. One of the most striking images in the book is a marvellous photograph of a flock of waddling geese, with the caption "We have resisted the temptation to name some of the leading geese." However, the polemics leaven rather than dominate the book, and indeed the final chapter is devoted to a discussion of problems which have so far gone unexplained in any cosmology, including the quasi-steady state one advocated by the authors. Such open-mindedness is a welcome change from most articles and books the author has seen in this field.

The book is also a first-class exposition of modern cosmology and an account of its development from its roots in the early twentieth century. This historical perspective alone gives a breadth and depth which I have not seen elsewhere (except perhaps in Kragh's *Cosmology and Controversy,* which puts emphasis on the sociology of the debate). In the United States, American astronomers in Arizona and California made the first spectroscopic observations of spiral nebulae, accumulating the spectra of about 90 spirals within twenty years, by which time it was recognized that these nebulae are galaxies lying

beyond our own. It was found that the spectra of the galaxies were systematically shifted toward the red. The only known cause of this phenomenon is a velocity of recession, and the discovery that the redshifts of the nebulae increased *pro rata* with their distances from the observer led to the view that the Universe is expanding.

Meanwhile, in Europe, in 1917, Einstein and de Sitter were applying the equations of Einstein's 1915 theory of gravity (general relativity) to the Universe as a whole. At that time the Universe was considered to be static in the mean and Einstein was dismayed to find that his theory did not permit this: rather, his equations predicted that either the cosmos would collapse in on itself or be in a state of expansion. To save the situation, and to yield the desired static cosmos, he had to arbitrarily insert a repulsive term in his equations in order to balance the attractive gravitational one. This arbitrary "cosmological constant," inserted for the wrong reason, has acquired great significance in recent years with the discovery that there does indeed seem to be a repulsion driving the expansion at an accelerating speed.

The centrepiece of the historical material is, however, the great debate, which spanned fifteen years. A major centre of this cauldron of controversy was Cambridge, where the new radio telescope built by Martin Ryle was mapping the sky at radio wavelengths. It was here that "the war of the source counts" was fought out between Martin Ryle and his group on the one hand, and Fred Hoyle and his colleagues on the other.

The battleground was the number of radio sources plotted as a function of their flux at radio wavelengths. A universe whose properties are unchanging on a large enough scale yields one predicted relationship, an evolving universe yields another. If for simplicity we take all radio sources to have the same intrinsic luminosity, then in a steady-state universe, the number N of sources brighter than some flux S should, on a log-log plot, appear as a straight line of slope - 1.5. If the Universe is evolving, then by looking into deep space, one looks further back in time to an era when galaxies were more closely crowded together, and the fainter sources should be more abundant than expected. In that case the slope should be steeper, say - 2 or - 3. According to the authors, Ryle and his group at the Cavendish were extremely secretive about their research, keeping their data out of reach of the steady-staters until a public announcement was made, claiming that the slope was inconsistent with the steady-state theory. Unfortunately, the Cambridge radio group kept getting it wrong, and the announced slope declined from - 3 in 1955 to - 2.2 and then to - 1.8 in 1961. The modern result, after all the furor of these years, is that there is indeed a small excess of faint sources, which can equally be explained by the canonical hot big bang or the quasi-steady-state theory. It is now known that the Universe is inhomogeneous on scales of about 300 million light years, which is more than enough to yield the observed flux excess without requiring far-reaching cosmological postulates.

The big bang theory is founded on three pillars: (1) the Universe is seen to be

expanding, consistent with the concept of creation from a singular event perhaps 14 billion years ago; (2) the light elements helium and deuterium have the precise abundances expected if they were indeed created in a hot, dense fireball; and (3) we are immersed in an isotropic, thermal radiation of 2.73 degrees absolute, expected as relict radiation of the primeval creation event (although the specific temperature is not predicted with precision). There is a pleasing simplicity about this theory. For example, the relative abundances of the light elements are accounted for by a single free parameter, the photon-to-baryon ratio. Further, it has survived a number of tests which could have killed it off; for example, no stars or galaxies are known for sure to have ages in excess of the supposed age of the Universe.

Can we then write off *A Different Approach* as relict radiation from the hot fireball of the debate which consumed the participants almost half a century ago, as a polemic by a handful of people who have stubbornly held on to a long-outmoded hypothesis? In the opinion of most cosmologists, the answer is probably "yes." In this reviewer's opinion, it is emphatically "no!" The list of unsatisfactory features in the standard model is formidable: (a) From the big bang perspective, the homogeneity, isotropy and "flatness" of the Universe and the magnitude of the density fluctuations which will seed the growth of galaxies are all built in by postulate; they have no explanation in terms of the theory. Widely separated regions of the Universe, which have always been causally disconnected because of the speed of light, nevertheless have similar properties, including for example identical elements. If the Universe, starting from the size of a football or less, went through a period of superluminal expansion or inflation, then these problems can be solved, and the construction of such inflation-driven cosmological models is a cottage industry. In effect it is postulated that, in the first microseconds of the Creation, there was a large cosmological constant which has since gone to zero. There is, however, no known microparticle foundation for the hypothesis, it appears to make no testable predictions, and the amount of fine-tuning required to get it to work (itemized by Hoyle et al.) is horrendous. (b) Whenever a new result appears it is never as expected, but there is always a parameter to tweak to make agreement appear. This is described by the practitioners as refining the model, but it does raise questions about the predictive power of the theory. For example, recent observations of distant supernovae have revealed that the expansion of the Universe, having slowed, seems to be picking up again. Extraordinary fine tuning is required to stop galaxies from having accelerated out of sight of each other. (c) There are exotic compact objects connected by filamentary structures to some spiral galaxies. To the eye these look like ejecta; the problem is that the redshifts of these apparently connected objects are often wildly different, which is inconsistent with reasonable ejection speeds. And there is a statistically sound excess of quasars, supposedly distant objects from their redshifts, around nearby galaxies. To the authors, these are nearby ejecta of new matter, whose properties are evolving and which yield non-velocity redshifts. In terms

of mainstream cosmology, it can only be assumed that these are striking line-of-sight coincidences. (d) There are the periodicities which exist in the redshift distributions of galaxies and quasars. These appear most strongly in quasars or galaxies angularly close to each other. These periodicities have been claimed over a thirty-year period and have been statistically validated several times with fresh, expensively obtained high-precision data. They stand unrefuted in the refereed literature and are universally ignored: there seems to be no way to fit them into the standard paradigm. On the largest scales, there appears to be a significant excess of structure over theoretical expectations on scales of order of 400 million light years.

Much depends on which evidence one chooses to emphasize. If the canonical picture is given *a priori,* then anomalous and periodic redshifts are simply oddballs which will presumably disappear at some stage. If, on the other hand, emphasis is given to the "anomalies," and if the fine tunings and epicycles of the canonical view are seen as warning signs, then the canonical view acquires less credibility. But which evidence gets the emphasis, and why? The role of historical accident in the development of a paradigm is beautifully illustrated in several passages of the book. Thus, in the war of the source counts, forceful advocacy by Ryle persuaded Jan Oort, one of the foremost astronomers of the day, that the Universe was evolving rapidly and that the steady-state description was therefore false. Oort's influence in turn was a major factor in turning opinion against the steady-state cosmology. As stated above, it would later emerge that these radio source counts were completely erroneous; but the damage had been done, and the direction of opinion had been set.

What, then, of the cosmic microwave background, the supposed relict radiation from the fireball? Hoyle et al. draw attention to a curious coincidence. Helium-4, which is supposed to have been created primarily in the cosmic fireball, is also synthesized in stars, the energy from these stars yielding a radiation background. If all the helium-4 has been produced in this way rather than in a fireball, then the energy density (say in ergs per cc) of the starlight so produced matches rather precisely the energy density of the CMB. Thus, in a Universe of infinite age, a single mechanism which is *known* to occur, namely nuclear burning in stars, can account for both the helium-4 abundance and the 2.7-degree microwave background. For this to work, the starlight has to be thermalized. The authors propose that carbon and metallic whiskers could achieve this. Their required space density is low and could readily be accounted for as supernova ejecta. In fact, predictions that there ought to be a 3~K background temperature, arising from reprocessed starlight, can be found scattered through the literature as far back as the 1930s.

Some weaknesses in the quasi-steady-state model (to date) are its failure to make quantitative statements about the remarkable degree of thermalization and isotropy of the microwave background and the tiny fluctuations observed in it, and the lack of a detailed model of clustering of galaxies (galaxies are created by the ejection of new matter from other galaxies). A major failing in both

the authors' model and the standard one is the inability to account for the amazing fine-tuning required for life to be possible in a cosmic environment. It could well be that, a few centuries down the line, our current understanding of the cosmos will look downright Ptolemaic.

To sum up, this is an excellent and clear account, delivered in lively style, of modern cosmology as seen through the eyes of three protagonists in the great debate. The equations and occasional technical patches will, unfortunately, act as a barrier to many readers even although the essence of the story can be read between them. A charming feature of the book is that the authors, having been involved in the early debates, provide a unique "insiders" perspective on the rough-and-tumble of the period, bruised egos, dirty tricks and all. The villains are clearly identified!

Finally, what of the authors' claim that modern cosmology is conformist and dominated by a few "leading geese"? Certain facts are hard to refute. No doctoral student would dare to question the canonical picture, nor would a supervisor lead him into such a minefield. There is almost no chance that a proposal for telescope time to examine any aspect of (say) the discordant redshift claims would get through an allocation panel. No researcher on the make would risk her reputation by getting involved, and any paper giving support to (say) redshift periodicity would have to surmount a wall of hostility. For such reasons, at least in the short term, this book will have no significant influence on the community of cosmologists. I can thoroughly recommend it.

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Religion and Scientific Naturalism: Overcoming the Conflicts by David Ray Griffin. Albany: State University of New York, 2000. xvii +345 pp. paperback, ISBN 0-7914-4564-X, hardcover ISBN 0-7914-4563-1.

With this book, David Ray Griffin, general editor of the SUNY *Series in Constructive Postmodern Thought* (now nearly two dozen volumes) and himself author, co-author, or editor of half of the volumes in the series (as well as many other books and papers), has contributed a volume that should be required reading for all scientists, theologians, and philosophers—not only those who feel some unease with the current picture of the world as painted by either religion or modern science, but more importantly for those who do *not*. Readers—scientists and theologians alike—should come away from this book with the conviction that some radically new approach to or perspective on the problems of consciousness, free will, life, and their place in the material universe is badly needed. Even if all readers are not entirely convinced by the particular theoretical perspective that Griffin offers, most should come away also with

the conviction that his general argument is correct: that the continued polarization of scientific and religious views about controversial issues is not only unproductive but unnecessary, that both religion and science have discovered fundamental truths about the world but have reached erroneous conclusions by excluding those truths recognized by the other side, and that a more complete and accurate world view will emerge only when the facts and truths from both sides can be fit into some larger framework.

This "tertium quid" approach to controversial issues (as Edmund Gurney [1887] termed it) was the guiding principle behind the founding and development of psychical research (or parapsychology) in the late 19th century (Kelly, 2001), but it, like psychical research, seems to have been largely ignored in wider intellectual circles. It is heartening, therefore, to see that Griffin introduces the general SUNY series on postmodern thought with a statement defining his version of postmodernism as an approach that attempts to go beyond *both* the premodern (religious) world view and the modern (scientific) world view by taking the strengths from both and forging a third, postmodern world view. The central question for this book, therefore, as described in the Preface and in Chapter 1, is whether such a merging of the two views is even possible—that is, whether there is anything *essential* to science that absolutely conflicts with anything *essential* to religion. Griffin concludes that there is not, that the dualistic supernaturalism that has been assumed to be essential to the religious world can and must be replaced by naturalism, which acknowledges the lawful regularity of basic causal processes in the universe's operation, but, conversely, that the mechanistic, physicalistic, reductionistic, nihilistic, and atheistic "maximal" version of naturalism, currently assumed to be essential in the modern scientific world view, can and must be replaced by a "minimal" version of naturalism that is nondualistic (in the traditional sense of Cartesian dualism) but that nonetheless also acknowledges will and purpose (whether divine, human, or both) as basic, ongoing natural influences in the world's operation.

In Chapters 2 and 5, Griffin argues that "the rise of modern science itself was heavily conditioned by theological ideas and motives" (p. 110) and that, ironically, the materialistic, atheistic world view is deeply rooted in the supernaturalistic dualism of founders such as Galileo, Descartes, and Newton. This original theism is depicted in the familiar picture of inert, mechanical, purposeless matter; a free, active, and immortal soul; and an omnipotent God who made the interaction of the two possible. The glaring problem in this picture, however, of accommodating the evil and suffering in the world with an omnipotent God began a long process in which the concepts of God, purpose, free will, and meaning were gradually pushed off the stage, in a retreat from theism to the deism of scientists like Darwin, who believed in God as the first cause but as having no subsequent role in the operation of the world, to the ultimate nihilism of the modern scientific world view.

This world view, however, is inadequate to account for the rise of conscious

experience out of inert matter and for human freedom in a deterministic world, a freedom that all of us, scientists included, assume in action. In failing to account for such fundamental phenomena—"the reality, the efficacy, and the freedom of consciousness" (p. 167)—modern "maximal" naturalism shows itself inadequate not only for religion and ethics, but also for science. As a result, religious thinkers are increasingly challenging the adequacy of the modern scientific world view, and in Chapter 3 Griffin describes three recent attempts to accommodate both the religious and the scientific perspectives. In Griffin's estimation, however, all such attempts fail because they fail to recognize that *both* the supernaturalism assumed to be essential to religion and the materialism assumed to be essential to science are false.

In Chapter 6 Griffin argues that the core of the problem with all modern world views, dualistic as well as materialistic, is that they are rooted in the early modern scientific view of matter as inert. Griffin argues that what is needed now is a more serious consideration of pantheism, and in Chapters 4 and 6, he describes the version of pantheism that he finds most persuasive, the "panentheism" or "panexperientialism" of Alfred North Whitehead, so-called because in it all matter has some degree, not of mind or consciousness, but more fundamentally of experience and spontaneity.

In Whitehead's system, as described here by Griffin, observations and experiences that led to religious views can be accommodated with those that led to modern scientific views. First of all, Whitehead's system includes a naturalistic theism, in which natural laws are not part of an externally imposed and fixed or unyielding mechanism but are simply longstanding "habits" of the universe that are shaped and directed to varying degrees by many causal factors, the most basic and universal of which is divine influence. Divine influence, therefore, should be understood as "persuasive" and not "coercive," involving no "miracles" or "*interruption of* the normal cause-and-effect pattern" but being instead "an essential *factor* in that pattern" (p. 94). Second, Whitehead's system involves a more open, comprehensive scientific naturalism in which complex levels of perception, freedom, mind, and consciousness can be seen as having arisen, not *ex nihilo,* but out of the elementary experience and spontaneity of elementary levels of matter. Even the creation of the universe itself, in Whitehead's naturalism, did not involve an exception to the natural order, because it is seen as a process of bringing order out of chaos, not creation out of nothing (pp. 97, 312). Whitehead's God is not the "God of the gaps," as depicted by supernaturalism, but more fundamentally the universal force permeating every pore of the universe.

In two final and lengthy chapters, Griffin discusses in some detail two specific areas of scientific research that expose the modern scientific world view as having many "gaps" of its own. In Chapter 7 he discusses the controversy about parapsychology, which he considers "religion's basic science" (p. 20), even though most orthodox religious thinkers and liberal theologians, like most scientists, have ignored or rejected it. He concludes that parapsychology does in-

deed conflict with the modern scientific world view, and most especially with its assumption that there can be no causal influence at a distance, an assumption that he describes as growing historically out of modern science's foundation on classical dualism's mechanism and supernaturalism. On the other hand, he believes that parapsychological data directly support the more open naturalism advocated by Whitehead and thus provide a strong empirical basis for the development of a postmodern reconciliation of science and religion. Moreover, because the present scientific world view fails so completely to account for other important aspects of human experience, such as consciousness, free will, and aesthetic and ethical norms, "the fact that psi is not consistent with it provides no reason whatever for being suspicious of psi" (p. 219).

Finally, in Chapter 8, Griffin discusses the problem of evolutionary theory, not only because it has been so central in the conflict between science and religion, but also because in its modern, neo-Darwinist form, it illustrates many of the failings as well as strengths of modern scientific thought. Griffin first describes fourteen features of current Darwinian evolutionism, in an effort to identify which aspects are valid and essential for a naturalistic theory of life and its many forms and, conversely, which may be inadequate or invalid. He then summarizes some of the more important shortcomings of modern Darwinism. Some of the difficulties are philosophical, such as its materialism and positivism; but perhaps its more glaring weaknesses are its empirical shortcomings, two of which Griffin discusses in some detail. First, there is evidence, contrary to Darwinian theory, that some kind of Lamarckian, or need-induced, inheritance occurs, and new findings in genetics suggest that this is no longer an implausible hypothesis. As Griffin points out, the importance of such an hypothesis is three-fold in that it suggests "that there *are* factors involved other than natural selection of random variations… [that] purposes can directly bring about structural changes… [and] that all living things, rather than being passive results of forces acting on them, are self-determining organisms" (p. 276). Second, there is the problem of major gaps—the absence of transitional forms of life—in the fossil record, gaps that have become more, not less, pronounced during the 150 years of paleontological research since Darwin's day. Moreover, the issue involves not only this evidential problem, but several related conceptual problems, including how new and complex species could have developed through random variations, when the viability of a species would often have required the simultaneous and instant appearance of several interacting organs, skeletal structures, or other features; and also how life itself arose, especially how the myriad forms of complex life arose so suddenly and rapidly, at the time of "Biology's Big Bang" during the Cambrian period. In the remainder of the chapter, Griffin outlines "how the wider naturalism articulated in this book provides a more helpful framework" (p. 290) for addressing these and other problems and, more broadly, how it takes into account the strengths of the hitherto polarized views of religious creationists and neo-Darwinian scientists.

This is a complex, challenging book, so full of ideas that it is not easy to read, and even less easy to summarize; I have only scratched the surface in my summary here. Griffin examines closely a variety of controversial issues and provides extensive references for readers who want to look more deeply into them. More importantly, he has also presented in detail his—and Whitehead's—proposal for a scientific naturalism that goes beyond the polarization of thought in which all of these issues remain mired. I share completely Griffin's belief that a reconciliation of religious and scientific perspectives in the context of a wider naturalism is essential for any significant advancement in either religion or science. I am less convinced—or perhaps just puzzled—by some of his specific arguments. His explanation (pp. 219–229) of paranormal phenomena in the context of Whitehead's theories was bewildering to me, perhaps because I have not read enough of Whitehead's own work to understand fully his ideas. I also found puzzling Griffin's categorical denial of the possibility of precognition (pp. 228–229). Given his castigation of theologians and scientists who reject phenomena *a priori,* based on their beliefs, world views, or systems of philosophy, one would think that he would be more cautious or restrained in rejecting the idea of precognition as "logically impossible"; and I found completely unconvincing his explanation (p. 229) of why his *a priori* rejection of precognition is different in kind from the similar rejection of psi by other scientists and philosophers. (Another reviewer in this journal of one of Griffin's books finds his dismissal of precognition equally unconvincing; see Beloff [1998]).

Similarly, like many other people before him, Griffin rejects classical dualism in part because of the interaction problem: "We realize that mind and body do interact. We realize, further, that if they were different in kind, this interaction would be impossible," apart from the influence of a *deus ex machina*. The conclusion, according to Griffin, must therefore be that mind and body are *not* different in kind and "that the mechanistic view of matter must be untrue" (p. 148). This conclusion may be perfectly valid (I am inclined to think it is), but I am unconvinced by his premise, that two unlike things cannot interact, because I do not think that Griffin (or anyone else) has found a more adequate definition of causation than Hume's basically neutral description of it as simply our observations of "constant conjunction." As Griffin himself points out, this understanding of causation provides "no good reason to stipulate *a priori* that only things with a common nature can causally interact" (p. 148), and without a more definitive understanding of the nature of causation, I do not see how we can do otherwise but take an agnostic position on what can and cannot interact.

Perhaps the most important problem, however, that I find with the Griffin/Whitehead position is that, although Griffin says that "panexperientialism…, and apparently it alone, can solve the mind-body problem" (p. 173), it is not so clear to me that it really does. As Griffin explains it, in panexperientialism "a distinction is made between experience as such, which even the low-

est-level individuals (i.e., particles of matter) are said to have, and *conscious* experience, which emerges only in very high-level individuals" (p. 167). Furthermore,

> a distinction is made between two ways in which low-level individuals can be organized: into 'compound individuals,' in which a higher-level of experience (which might be conscious) emerges, and merely 'aggregational societies,' such as rocks and telephones, in which no higher-level experience emerges. (p. 167)

In other words, in some complex aggregates of particles, a higher-level entity—a "mind"—emerges that makes the aggregate a "*self-determining* organism" (p. 155). In other complex aggregates, no such "higher-level experience" emerges from the group of particles. This distinction is not an "ontological dualism" but instead "an *organizational duality*" between compound individuals, which are "*self-determining*," and aggregational societies, which are not (pp. 176, 155). But what makes a higher level of experience emerge in one group of particles (e.g., a bird, or a human) but not in another (e.g., a rock, or [I would add] a computer)? Perhaps more problematically, what makes a higher level of experience emerge in one set of particles (e.g., a living bird) but not in the (apparently) same set at another time (the corpse of the same bird)? Panexperientialism, or any form of panpsychism in the broad sense, is in my view a theory deserving much more serious consideration, as Griffin urges, than it has so far received, in large part because it answers a problem fundamental in materialistic philosophies, namely, how (and when) life, mind, and consciousness emerge from insentient, inert matter. Unfortunately, in the Griffin/Whitehead system the problem of emergence has simply been placed on another level, that of how consciousness emerges from some sets of particles and not from others.

The problems that I have in fully understanding or accepting all aspects of Griffin's panexperientialism in no way detracts from what I believe is the importance of this book. One can argue with details or even with Griffin's whole theoretical system; one cannot, I am convinced, argue with the premise that the reconciliation of religious and scientific perspectives, data, and insights within the framework of a wider theory of naturalism is essential, and long overdue.

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Outposts of the Spirit by William M. Justice. Charlottesville, VA: Hampton Roads Publishing Company, 2000, 213 pp. $12.95. Softcover, ISBN 1-57174-157-7.

William M. Justice was a Protestant minister for most of his professional life. His interest in understanding the implications of psychical research urged him to consult with many personalities of the day, ranging from scholars such as Albert Einstein, Robert Thouless, and Robert Crookall to celebrated psychics like Edgar Cayce and Arthur Ford. Justice subsequently presented these interactions and the ideas they evoked in a book entitled *Outposts of the Spirit*. That work, published approximately twenty years ago, reflected Justice's striking spiritual interpretation of psychic phenomena, phenomena that he felt were ways to achieve "communion with God." "Outposts of the Spirit are those outpoints along the front lines to which the brave pioneers of the human race have reached in their search for the truth" (p. vii).

In the editor's introduction to this reprint, the question is posed, "Why publish a book written almost twenty years ago about a rapidly evolving field that moves forward with new developments almost every month?" (p. v). The Editor's answer is that there still persists the need for a way of looking at the field that accommodates prior beliefs, doubts, and natural skepticism. To assess the veracity of this answer as well as Justice's conclusions would place any reviewer in an uncomfortable position. Given my skeptical attitude toward the purported evidence for spirit communication and the paranormal in general, I am not persuaded by Justice's convictions that parapsychological phenomena provide evidence for God. Yet his convictions are admirably strong and well-articulated.

Therefore, I would like to offer a possibly better reason why it was helpful to reprint *Outposts of the Spirit*. A reader does not have to agree with the contents in order to appreciate this book. At the very least, this book offers a wonderful case study on how an intelligent and spiritual man was confronted by and subsequently integrated the paranormal into his personal and professional life. This is true human drama. To be sure, an increased interest in spiritual matters, a sense of well-being, and optimism frequently attend paranormal or transcendent experiences (Kennedy & Kathamani, 1995a,b). However, facing anomalous phenomena can also pose a serious challenge to a person's world views and psychological stability. Many people to this day struggle to accomplish what Justice did—and the inclusion of the new diagnostic code "Religious or Spiritual Problem" in the *DSM-IV* (American Psychiatric Association, 1994, p. 685; cf. Turner et al., 1995)—demonstrates that such struggles

are prevalent enough to warrant the attention and concern of professional clinicians. Justice's religious beliefs, in part, allowed him to successfully make sense of psychical research. In turn, psychical research reinforced his religious beliefs. I imagine many *JSE* readers will not share Justice's convictions, but we all can appreciate the personal journey that shaped those convictions. The stories that we are privileged to read are as fascinating as they are entertaining.

On the production level, this reprint includes new sections in addition to the "Editor's Introduction"—a foreword by George G. Ritchie, an epilogue by Lincoln B. Justice, and an afterword (biographical sketch) by William Yates. The epilogue, written by Justice's son, is particularly poignant. It is aptly entitled "The After-Death Experiences of William Justice." Three days after W. M. Justice's death, his son was in prayer. "Words in a small voice" came to the son's mind. The voice related in detail how his father was well, happy, and reunited with loved ones on the other side. The voice also confirmed some of the convictions held by W. M. Justice in life, convictions about the nature of psychic phenomena and how we relate to it as humans. My own father died in 1991, so I can only imagine the joy and serenity this experience provided Justice's son. Who wouldn't want the experience of knowing absolutely that our deceased loved ones are eternally happy? I admit that jealousy came over me while reading the epilogue.

Nevertheless, in my opinion, psychical research tells us more about the living than it does about the dead. I also imagine that many *JSE* readers will not agree with that assessment. The point is that the awesome power of paranormal and transcendent experiences is in their ability to inspire some of us to behave more kindly toward our fellow man, to force some of us to understand what they reveal about the human condition, and to draw some of us into a relationship with a higher power. Justice experienced this latter dimension to paranormal experiences. If he is enjoying life after death, no doubt he feels added elation that his personal journey to understand the implications of psychical research touched his son: "It is with great joy that I can witness the fact that my faith in God and in the power of the Holy Spirit has grown stronger as a result of the fearless way that my father sought after truth" (Lincoln B. Justice [epilogue], p. 196).

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Remote Viewing Secrets by Joseph McMoneagle. Hampton Roads Publishing Company, Inc., 1125 Stoney Ridge Road, Charlottesville, VA 22902 (2000), 296 pp. $14.95, ISBN 1-57174-159-3.

Remote viewing (RV), according to Mr. McMoneagle, "is the ability to produce information that is correct about a place, event, person, object or concept which is located somewhere else in time/space, and which is completely blind to the remote viewer and others taking part in the process of collecting the information."

RV has had a lengthy history throughout the world but probably has had only around twenty-five years of scientific study in this country. What makes Mr. McMoneagle's method different than most others is this use of strict scientific protocol. Mr. McMoneagle points out in his book that he does not wish to rewrite the history of remote viewing but instead offers an in-depth history written by Mr. Ingo Swann, which can be accessed at: www.biomindsuperpowers.com/Pages/RealStoryMain.html. I have read this and would recommend the site to anyone interested in this subject. It is about fifty-two short chapters, from which the reader will gain a vast knowledge into the history of RV.

Two other requirements that must be met are that all persons present during an RV should essentially be blind to the target, and there should be some form or means of validating the material after the RV has been accomplished. About sixty people in eight or nine laboratories in the United States over the past twenty-five years have established these necessary scientific protocols.

Using strict scientific protocol is probably what separates Mr. McMoneagle's RV from some other systems of RV or other methods used by some psychics, such as scrying from a crystal ball or interpreting tarot cards, clairvoyance, out-of-body experiences, etc. While these methods generally do not use a set scientific protocol but rather are quite varied and are dependant on the individual or psychic and therefore do not meet his requirements for being termed RV, Mr. McMoneagle does not discredit any methods if they work.

Many people confuse RV with "out-of-body experiences," but they are two completely different things. While a person experiencing "out-of-body experiences" feels as though he has left his physical body behind and has journeyed somewhere else, the person experiencing RV receives information by way of symbols, sounds, feelings, tastes and other both accurate and inaccurate stimuli. The advantages of RV over out-of-body experiences is that during an RV session, a "viewer" can discern feelings and other things that would be hidden

from a person during an out-of-body experience. RV operates best for producing information on things known to exist, and is not generally useful for information on mythical creatures, UFOs etc.

After completing high school, Mr. McMoneagle enlisted in the U.S. Army. After completing basic training he was recruited into the Army Security Agency and spent thirteen consecutive years overseas. After returning to this country he accepted a commission as a Warrant Officer and was assigned to Headquarters, Intelligence and Security Command. Less than one year later he became Remote Viewer #001 for the top-secret STARGATE project, which was a physic spy unit known by various names throughout its history.

As one of the original viewers, Mr. McMoneagle helped design and build an effective paranormal unit that serviced nearly all major Intelligence Agencies within the Federal Government for seventeen years. After retirement from the Army he was hired by Cognitive Science Lab, which was the laboratory responsible for the research and development of the STARGATE project. He is currently fully employed by them today, doing both research on psychic functioning's and RV.

I would highly recommend his book to anyone interested in the subject of RV.

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From Mesmer to Freud: Magnetic Sleep and the Roots of Psychological Healing by Adam Crabtree. New Haven, CT: Yale University Press, 1993, 413 pp. $55.00, ISBN 0-300-05588-9 (retrospective).

I was asked to examine this book, which had been obtained from a remainder catalog, as a possibility for a "Retrospective Review." In these reviews, appropriate older books that deserve current attention can be presented. I found *From Mesmer to Freud* in a local library, and a little research on my part revealed that the book is still available from major distributors. This is fortunate for those with an interest in hypnotic phenomena, especially "magnetic sleep."

It has been claimed that the 1784 discovery of magnetic sleep—an artificially induced trancelike state—marked the beginning of the modern era of psychological healing. Crabtree, a psychologist (Centre for Training in Psychotherapy, Toronto), presents the story of the discovery of magnetic sleep and its relation to psychotherapy and the healing arts. I cannot remember reading a more carefully crafted presentation of hypnosis. This book is a comprehensive and outstanding analysis of the personal, social and cultural dynamics that shaped the study of this topic along with the personalities that defined this

field. But the book is also much more than that.

The subtleties involved in the evolution of this field shocked and sometimes outraged me. In particular, Crabtree's work, outside of its academic value, is also a sobering case study of how the politics of scientists can operate in disservice to science. Postmodernists will especially appreciate Crabtree's historical analysis because it uncovers and highlights information and ideas that the field disregarded as it adopted the materialistic perspective of Western medicine. I have seen other informal reviews of this book that independently echo this point to the extent that some *JSE* readers might believe that I have read the minds of these other reviewers or plagiarized their writings. For this reason, I initially hesitated to prepare a lengthy review. Upon reflection, I feel justified in publishing comments very similar to previous reviews because it substantiates that I am not alone in what I think is valuable about this book. In short, some aspects of Western medicine overlap with Eastern medicine, though the labels they ascribe to concepts might differ (e.g., chi or energy). Throughout this work many topics of interest to the SSE are discussed, including possession, dissociation, multiple personality, and paranormal effects associated with "animal magnetism." The scope is impressive and relevant to many areas of current research. Why have I never heard of this book before? This is an example of a valuable work that was missed, but happily may now receive deserved attention. No doubt many good books are unavoidably overlooked.

Before reading this book, and despite the fact that one of my graduate school mentors was a hypnotherapist and student of Milton H. Erickson, I had no deep-seated interest in or extensive knowledge of hypnosis. After reading and digesting this book, I can say that each of those issues has changed to an appreciable degree. If you cannot find it in a library, I highly recommend you search it out. At a remainder catalog price, the book is a steal for anyone. At the $55.00 list price from Amazon.com, it is a good deal for serious students or scholars and scientists. Perusing the various reviews posted on the Amazon.com Web page for this work will show you that I am not alone in my sentiments. This is one book that I want for my personal library, and I might never have known about it except for the kind alert. Who knows what other overlooked treasures await to enhance our knowledge and research?

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The PK Man: A True Story of Mind Over Matter by Jeffrey Mishlove. Charlotteville, VA: Hampton Roads Publishing Co., 2000. 283 pp. $14.95, paperback, ISBN 1-5717-4183-6.

Jeffrey Mishlove is a talented parapsychologist used to handling controversial subjects, and in *The PK Man,* he tackles Ted Owens, a tough one indeed. Owens was a well-known "super-psychic," with apparent phenomenal psychokinetic (PK) abilities, especially with regard to the weather.

Mishlove's avowed purpose in this book is to present his many years' documentation of Ted Owens' spectacular powers. He started in 1976, after meeting Owens at a parapsychology conference. Mishlove kept a growing file of newspaper articles, affidavits, personal testimonials, and letters about Owens' predictions and warnings and their often dramatic fulfillment. Mishlove was often in contact with Owens, who would tell him when he was causing another storm, or what football team he was going to have win a game. Mishlove also sometimes enlisted other parapsychologists to help design another "experiment" around Ted Owens, and to help him gather affidavits and check for appropriate newspaper articles.

Mishlove's documentation is impressive, although, of course, it is not rigorous proof for the skeptical-minded. If Owens predicted a great storm and it occurred, as evidenced by newspaper articles, scientific proof is certainly incomplete. But at the very least, it brings up the old parapsychological conundrum of whether an event has been psychokinetically caused or precognitively predicted.

The PK Man is very well written using simple, clear prose. The story sometimes reads like a spy thriller; it is truly fascinating. Mishlove includes his own reactions to Owens' personality and powers, which is a useful gauge for the reader, since Mishlove is a parapsychologist with experience in psychic matters. But readers may not be satisfied with Mishlove's treatment of a fundamental issue about Ted Owens: his vindictive, vengeful, egocentric nature, which led him to cause many PK events that caused massive property damage and also cost human lives.

Ted Owens was born in Indiana in 1920 and was largely raised by his grandparents. Psychic phenomena were familiar to the family—his great-grandmother was an expert at the ouija board and could predict deaths; his grandfather taught him to dowse.

Mishlove recounts Owens' discovery of his extraordinary psychic powers. By Owens' own account, these powers began to manifest themselves when he was four; he spontaneously levitated one afternoon at his house. His early reading concentrated heavily in psychic matters. As a teenager he became a stage hypnotist and gave public demonstrations. After Navy service in World War II, Owens wrote to Dr. J. B. Rhine, head of the famous parapsychology lab at Duke University in North Carolina. Rhine invited Owens to Duke, and Owens worked there as Rhine's assistant for a time.

Owens was best known in his lifetime for his PK control of the weather.

Owens told Mishlove he really realized his weather control abilities in 1963, when he was living in Phoenix, Arizona, with his wife. It was extremely hot and Owens, remembering a conversation at Duke about weather control among shamans, decided to try to bring rain. So he pointed at the sky and focused his mental imagery on storms and rain. A little while later, a rain and lightning storm developed. Owens was delighted and began doing many weather experiments.

A key aspect of Owens' work, which Mishlove explores at length, is Owens' claim that he was the contact person for Space Intelligences (SIs, as Owens called them). He apparently was not in regular conscious contact with them until the 1950s. But then his psychic powers began slowly to be enhanced, and by the 1960s he communicated with them regularly. In 1965 he wrote in his diary, "Tonight they told me that they could give me only so much power, so much knowledge, at a time. Matter of fact, they said I'm an experiment with them—to find out just how much of the PK power a human being can absorb and stand" (p. 65).

For a long time the SIs initiated all the contacts. Only once Owens had developed (with their help) a visualization technique was he able to also contact them. Owens claimed he was the "prophet" of the SIs and that his demonstrations of massive, and often dangerous, PK were done at their behest and with their power.

Owens placed himself in the rank of the great prophets. As Mishlove states, "Owens used many metaphors to justify the outrageous ends to which he focused his mental powers. He often compared himself to Moses wreaking the ten plagues upon Egypt with the help of God" (p. 7). Mishlove points out that this metaphor is "inappropriate and vainglorious," but he doesn't engage the issue deeply.

Owens tried to help sometimes, such as in predicting and then (by his own account) causing storms that ended a drought in California in 1976, and similarly in England the same year. But when people disbelieved his predictions or ridiculed his claimed powers, he often slashed an entire city or state with vengeful weather. Owens was a football fan, and when teams he disliked were on winning streaks, or when a team to whom he had offered his psychic help scorned him, he vengefully caused that team to lose many games, sometimes even an entire season's worth of games. He predicted his revenges ahead of time to Mishlove as well as publicly, so Mishlove was able to collect newspaper articles about the fulfillment of Owens' predictions.

Mishlove notes, "…many of Owens' so-called demonstrations had an unsavory aspect to them. There is a clear sense that he went about with a chip on his shoulder—challenging the world to believe in his miracles and then lashing out when people reacted with sarcastic ridicule" (pp. 90–91). But Mishlove did not really explore Owens' unhappy background as part of the reason he used his PK as he did—Owens' father took to gambling, his mother rejected the child, and he was raised by his grandparents.

Mishlove also half-heartedly seems to agree with Owens' characterization

of himself as a prophet, without analyzing the vast difference between a prophet, whose spiritual character is humble and sophisticated, and Owens, who was vengeful and childish. Mishlove was clearly disturbed by Owens; near the end of their lengthy relationship, he wrote Owens a long letter chastising him for his unwillingness to grow spiritually. But his basic conclusion is that Owens was the way he was mostly because he received so little respect from society for his powers. This is quite a weak conclusion, especially concerning a man who misused his massive powers so stupendously.

However, Mishlove explicitly encourages readers to draw their own conclusions about Owens' behavior. He also addresses several other major issues, including Owens' ability to manifest UFOs and his allegations that the power came from beings. This last is an interesting topic, little researched. But, as Mishlove points out, these superpowers may in fact come from extraterrestrial beings, though smaller paranormal powers, more widely distributed, may not. He also provides a short end chapter on some of the new ideas in modern physics that make many of Owens' claims less outrageous than they might otherwise seem.

There is no other book like this one about a unique man and his abilities. Mishlove has done a very good job by bringing the issues before the public eye, and for this we should be thankful to him.

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The Madness of Adam and Eve: How Schizophrenia Shaped Humanity by David Horrobin. London: Bantam Press (Transworld Publishers), 2001. 275 pp. £18.99, cloth. ISBN 0593-046498. (Available through amazon.co.uk.)

"All the good and bad characteristics of humanity seem writ large in the children of schizophrenic parents" (p. 144).

Much evidence for this empirical claim is adduced in this fascinating book, which has several facets. In the first instance, it is essential reading for anyone who knows sufferers from schizophrenia, bipolar disorder, or dyslexia; the book offers intriguing hints about the genesis of the problems and some genuine hope for better treatments. In addition, this book offers a far-reaching, detailed, and plausible scenario of genetic mutations and environmental factors that led to the evolution of *Homo sapiens* from an ancestor held in common with chimpanzees. For anomalists and students of science, the book offers illustrations of serendipitous discovery (p. 64) and of the (grudging, inadequate, or dismissive) reception of genuinely novel ideas (p. 160). For medical scientists and practicing physicians, the book illustrates the wealth of knowledge and understanding attainable through careful clinical observation of apparent

correlations and happenings. For all of us there is food for thought about how to maintain scientific progress as it increasingly calls for cross-disciplinary work (see, for example, pp. 210, 224, and the epilogue) and how to expand medical knowledge under the (ethically quite proper) constraints imposed on clinical trials in most developed countries (see p. 221).

Horrobin has, among other things, taught medicine, founded biotech companies, and been advisor for 3 decades to the Schizophrenia Association of Great Britain. Evidence from a number of directions supports his grand synthesis, which is also made all the more plausible by several testable hypotheses that are likely to be tested within a decade or two. It is, furthermore, persuasive that he reports (e.g., on p. 224) some results that were not what he had anticipated. Along the way some fascinating facts are cited, for example, that a standard treatment for severe schizophrenia (in the 20th century!) was deliberate infection with malaria and that a Nobel Prize was awarded in 1927 for discovery of this treatment.

It is tempting to describe in more detail both Horrobin's intriguing syntheses and the many remarkable facts cited along the way. However, this book has some of the appeal that a mystery novel does; the best service to readers probably is to recommend the book in the strongest possible terms while preserving an element of suspense about the details. Readers should, however, be forewarned that there is a degree of repetition throughout the book, possibly inevitable as the various strands of evidence and argument are pulled together; and in places the technicalities of physiology or biochemistry may seem daunting. In that case, by all means skip the technical stuff, but don't fail to read further. Among the questions discussed or clues adduced are these:

- What is the most obvious physical or physiological difference between chimpanzees and other apes on the one hand, and humans on the other?
- What does it signify that our brains are 250% larger than those of chimps but contain only 100% more neurons?
- What clues to evolutionary scenarios are afforded by the way in which human physiology makes use of water?
- Why has the rate of cultural progress of *Homo* and ancestors changed so dramatically at least a couple of times, about 100,000 and again about 10,000 years ago? And why were these changes also associated with a dramatic rise in the diversity of cultures?
- Is there truth to the popular folklore that genius and madness are closely allied? Is it really the case that "Without the genes which in combination cause schizophrenia we would be like Neanderthals or *Homo erectus*" rather than *Homo sapiens*? (p. 207)
- Will further work substantiate initial indications that simple dietary supplements can synergize or even substitute for anti-schizophrenic drugs? Can diet really reduce the incidence of violence (by between 30 and 50%!) among prison populations? "Perhaps it is possible, by manipulat-

ing the environment, to prevent the expression of the illness even in those who have a 100% genetic risk" (p. 208).

- How does it come about that the incidence of schizophrenia is much the same in all studied populations: "from the Canadian Arctic to Patagonia, from Lapland to the Cape of Good Hope, from Siberia to the world of the Australian Aborigines" the incidence is between "0.5 and 1.5%—and usually between 0.7% and 1.0%… . No other illness shares a similar distribution." (p. 119)
- Why should parents in the United States using milk formula for their babies prefer that formulated for premature babies? (see p. 93)
- Why are schizophrenics so much less prone to arthritis? Why do they not flush under large doses of niacin? Can there be a physical diagnostic test for this psychiatric disorder?

In addition, Horrobin's theses and lines of argument have implications for some other topics as well:

- Are the dietary habits of adolescents much more important, even, than is currently believed? If dietary supplements can lessen the incidence of violence in prisons, might they also do so in schools? (see p. 170).
- Does this evolutionary scenario have implications for the probability of extraterrestrial intelligences? About the likely longevity of extraterrestrial civilizations?

If none of these questions interest you, then you may not find this book as exciting as I did.

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