ERROR AND WORSE IN THE SCIENTIFIC LITERATURE

RICHARD D. LUMSDEN*

Received 27 January 1992; Revised 1 April 1982

Abstract

When is anything clearly and conclusively demonstrated in science? Most scientists, whether evolutionists or creationists, appreciate that ideas are always being tested, findings evaluated, and that few theories are ever proven, at least to the extent that they become scientific laws. Of late, however, there have been issues of veracity that have gone beyond the traditional academic uncertainties. Some of the current stumbling blocks to science as a search for truth are reviewed. Not all are unique to the purely secular scientific establishment.

Introduction

Uncritical deferral to the putative "facts" of natural science over against biblical literality suggests naivete about the "real world" of the practice, interpretation, and publication of secular scientific investigation. However, a hypercritical view of science may engender recourse to unsound metaphysical principles.

While a critical evaluation of the scientific literature (secular and creationist) is always in order, when attempting to sift fact from fiction, creationists *must* be as rigorous in guarding themselves against commiting biased selectivity, where data would be fitted to their models, as the evolutionists *should* be. The hypothesis should not become a self-fulfilling prophesy. Promulgating error through teaching (which includes publication), irrespective of its source or the teacher's (author's) purpose, is biblically condemned.

Dr. George Matzko's (1991) note addressing the veracity of the science literature we read—therefore to what extent one should take it at face value—is timely and provocative. While the cited account of the stresses and strains besetting "big time research" (Lepkowski, 1991) suffers from some hyperbole, the pressures generated in the secular academic scientific establishment and their effect on research quality are real enough. Critiques such as Matzko's (1991) and Lepkowski's (1991) are always in order. For that matter, so are those of Van Till et al. (1988, 1990) and Ross (1989), which would take strict creation science to task. Debate is the way of science. However, one must be careful not to build straw men in the process.

The Ivory Tower

Prior to my present academic affiliation, I served as a grant-funded, grant-dependent and therefore grant-seeking professor and administrator at a major secular university for more than 20 years. At such institutions, research productivity, measured chiefly by the number of papers published per unit time, and, to a degree, the dollar amounts of extramural funds acquired to support it, has become the major criterion, *de facto* if not *"de jure,"* for tenure and promotion in the science faculty. This is not to say that the universities eschew quality for quantity. The two are not mutually exclusive. Reviewed publication and grant support are generally taken as evidence of peer recognition of quality scholarship, and justifiably so in most instances.

However, since doing research today takes the fiscal resources of Solomon (if not his wisdom), the institution's administrators encourage principal investigators to pursue grant support aggressively. They take what they can get, accept what it takes to get it, and grantsmanship becomes a valued commodity in its own right. Then, with success, administrative "encouragement" devolves to expectation. Graduate programs, in particular, have come to rely heavily on extramural funding. Moreover, grants, besides supporting the research effort often include coverage of a percentage of the principal investigator's academic salary and an overhead allowance (the rate for which can be 50% or more of direct costs) to the institution. Academic departments may find themselves justified (or otherwise) on the basis of their grant support.

This emphasis on research stems, first of all, from the traditional role of the *university,* compared to a school or college, as a generator of knowledge as well as a disseminator of it. The term "research university" is a redundancy. Moreover, in recent times, universities have come to a realization of the commercial value of their "intellectual properties," especially in the technology sector. Developing and marketing these properties have become a major academic enterprise, given the ever rising cost basis for higher education as a whole. Patents may count as much as publications.

Professor Page Smith (quoted in Lepowski, 1991, pp. 40-42, and Matzko, 1991, p. 111) finds it unconscionable that ". . . professors are *forced* to do research to make a living, in order to avoid being humiliated—and terminated" (my emphasis). This statement misconstrues the issues. For a *salaried* university professor, tangible scholarship is neither an avocation nor an elective activity. Research has always been part of the job description (at the *university)* and the qualifications the professor submitted for appointment consideration in the first place. After all, the Ph.D. is a research degree, not simply a teaching certificate. The expectation that the employee do his job is hardly outrageous. On the other hand, it is the university's obligation to provide the facilities and wherewithal to do the research, as well as the teaching, it expects its faculty to conduct. The "distortion" to which Page subsequently alludes is the *de facto* requirement that the professors capitalize, as well as conduct, the university's research mission.

Where an individual's research is concerned, intramural budgets are seldom adequate to support more than "pilot work" or to get a beginning investigator off the ground. Without extramural funds, the fledgling Assistant Professor does not stay airborne very long. In today's academic marketplace, would-be scientist scholars must first be entrepreneurs of a sort, and since the most fundable areas of research are the

*Richard D. Lumsden, Ph.D., 2620 Fern St., New Orleans, LA 70125.

most competitive, there is an element of "natural selection," if not survival of the fittest. This situation is not altogether conducive to the pursuit of scientific knowledge for its own sake or inevitably encouraging of its best purposes. Preoccupied with the means, the ends may be less emphasized, or worse, the means may become ends in themselves.

Scientific competition, especially when there is a financial factor involved, can be less than ennobling, as witnessed by some recent instances of alleged plagiarism, formal litigation (Crewdson, 1992; Marshall, 1992), and even criminal prosecution (Holden, 1992).

However, it is my observation that instances of outright fraud—e.g., the deplorable Baltimore, Imanishi-Kari et al. affair (alleging data fabrication) that Matzko (1991) cites (detailed by Hall, 1991)–are remarkable for their rarity. Of course, this celebrated case, precipitated by the article of Weaver et al. (1986), challenged by Margot O'Toole (Zurer, 1991) and others, including four of the paper's co-authors (!) (Hall, 1991), is by no means the first in modern times. One of similar magnitude developed at Yale during the late 1970's (Broad, 1980), and there have been others less publicized. Such peccadillo need not stem invariably from seeking a vested position, an interest in or requirement for large sums of research funding, or other material concerns. More often, perhaps, the basic motivation behind fraud, per se, is vanity, especially when vanity is offended by challenge, or fanatic commitment to a waning theory. We are reminded of some of the personalities who have "starred" in the history of evolution theory (Taylor, 1987), in particular.

Sources of Error

What is more frequent are unpremeditated errors, such as those engendered by quirky instrumentation, faulty reagents, contaminants, undiagnosed artifacts, flawed experimental design and/or data processing, including over-reliance on computer enhancement of raw data. The pressure to publish, generated by competition for date priority, an upcoming tenure decision or grant application deadline, can force hasty work that amplifies the potential for erroneous results. Or, interpretations, even experimental design, may be forced, in order to concur with a prevailing theory or the author's working hypothesis. A frequent example of the latter is encountered in radiometric dating, where it has been blatantly admitted that:

> In conventional interpretation of K-Ar (potassium-argon) age data, it is *common* to discard ages which are substantially too high or too low compared with the rest of the group or with other available data such as the *geological time scale.* The discrepancies between the rejected and the accepted are *arbitrarily* attributed to excess or loss of argon (Hayatsu, 1979, p. 974) (my emphasis).

Too often, studies that produce data contrary to the expected findings are considered failures and remain unpublished.

Thus are the hazards implicit in ". . . uncritical acceptance of data published in establishment journals . . ." (Matzko, 1991, p. 110); his critique of Van Till et al. (1988) is well-considered. Regarding the confidence that it is a "well-founded conclusion of science that the solar system is 4.6 billion years old" (per Matzko's 1991 citation, p. 111), I cite Chesterson, as quoted by DuPraw (1968, p. 11): "[There are things and there are theories] . . . and compared with [things] evolution and the atom* . . . are merely theories. " Here, evolution as fossils would be "things." For Van Till et al., the sun and planets are "things"; their age an educated opinion; how well educated is the question? Is there any scientific reason to doubt that figure of 4.6 billion years**? If so, the age of the solar system is not a conclusion of science, only perhaps a conclusion of some scientists. To a majority of the latter, 4.6 billion years is the favored presumption at this time.

Thus, unqualified generalizations, as found in secondary and tertiary sources of scientific information (i.e., textbooks, encyclopedias, commentaries, magazines, television documentaries) become a potential source of error, especially for teachers, students and lay persons. Simply reiterating a hypothesis does not make it more *factual.*

In essence, only well-defined observations constitute a cumulative heritage of reliability. The question is, when is anything *conclusively* demonstrated in science? Little of it is chipped in stone, and even stones change with time. The phrase "Science says . . .", often used by the lay press, educational television, and authors of introductory textbooks to connote monolithic authority for what follows, is vacuous.

During a debate with Duane Gish, evolutionist Steven Shore, an astronomer on the Hubble Space Telescope program, is alleged to have stated (Morris, 1992, p. 4) that ". . . one of the beauties of science is that it is often wrong."

Bronowski (1956, p. 82) notes, "There was never a great scientist who did not make bold guesses, and there was never a bold man whose guesses were not sometimes wild." Yet, ". . . such is the nature of science, (that) their bad guesses may yet be brilliant by the work of our own day."

Van Till and his co-authors' (1988, 1990) higher view of the scientific literature, essentially across the board, compared to Scripture, strikes me as almost humorously naive, at best. A tip-off should have been the rampant inconsistency in the scientific literature; Pauling and Watson/Crick (3 vs. 2 stranded DNA structure) could not have both been right. How much of the current literature dealing with the controversial evidence for/against the Big Bang, or Cold Fusion would it take to make the same point? "Science held hostage" indeed, but by whom? Van Till et al. might note that these conflicting bodies of scientific *data* are being generated in laboratories, not seminaries. Seminal *opinions* are another matter altogether.

By contrast, where are there any inconsistencies of this magnitude in Scripture? For that matter, where are there any real inconsistencies in Scripture (vs. various exegeses) ? Does the Bible really address "scientific" questions in a scientifically productive way

*Reference to atoms as theoretical entities may be a bit conservative. Chesterson, who published this view in 1925, even considered the solar *system* as theoretical, given what was known of its organization and organizational principles at the time. But his point is well taken.

**The most direct evidence for billion-plus year ages of the Earth, moon, and meteorites derives from geological radiometry (Badash, 1989). For a recent evaluation of these isotope dating methods and the data, see Austin (1988, 1992).

(Van Till et al., 1988, pp. 42-43)? See Morris (1984) and Gish (1991) for a number of specific examples. DeYoung (1991) provides an insightful review of Van Till et al. (1990), where their view of science and Scripture are concerned. DeYoung (1991, p. 73) asks ". . . how can incomplete, imperfect science theories be the final interpreter of Scripture?" Meanwhile, the "hard science establishment" is not greeting the compromises of the sort that Hugh Ross, Van Till et al., or the American Scientific Affiliation (Sheler and Schrof, 1991) would offer with much respect (Stone, 1992).

Secular science has its own problems with which to deal. Even the most carefully executed work, by brilliant and unhurried minds, is subject to a degree of uncertainty. Consider Albert Einstein's testimony (as quoted in DuPraw, 1968, p. 1): ". . . I believe in perfect laws in a world of existing things, in so far as they are real, which I try to understand with wild speculation." Meanwhile, Einstein's most sophisticated ideas, including his theory of general relativity, continue to be tested (Ruthen, 1992). I suggest that Einstein himself would have heartily applauded these experiments, irrespective of how they impact his theories. Note also Amberson's commentary on the "cutting edge" of scientific research (as quoted in DuPraw, 1968, p. 7):

> . . . in the world of science, fashion is a prevailing mode of thought or action determined by recent innovation . . . resulting in a wave of attention and emphasis . . . yet in [its] wake . . . critical questions are neglected, which often escape attention through several succeeding waves.

Just because an idea is new does not make it right.

Outright retractions of erroneous or misleading findings are uncommon. More often, one encounters a followup article that reconsiders the earlier report in light of new information. Exceptionally, one or more co-authors may withdraw from a paper, subsequent to its publication (Hall, 1991), to disavow and thereby call attention to an egregious error (or worse) committed by a recalcitrant cohort. But, at that point, one cannot go back and edit the data tables in the original paper, where they remain as a pitfall for the unwary who have not gotten the latest word from the author(s) on the subject, and unwittingly take a "high view" of the unqualified first version, hence the value of periodic reviews. Even in the best of cases, the progress of one's research and what one finally publishes is not co-variant (Figure 1). No matter how diligently one follows the scientific method, the truth of the matter may prove elusive. The hazard is in what may get published along the way.

There have been instances where inaccurate results nonetheless produced valid conclusions. An example is the classic work of Gorter and Grendel (1925) on the lipid bilayer structure of cell membranes. Their extraction method undervalued the total lipid content and their calculation of cell surface area was flawed; however, the errors in these "hard data" essentially canceled, and fortuitously, these authors derived what has proven by other means to be a correct model for this aspect of membrane structure.

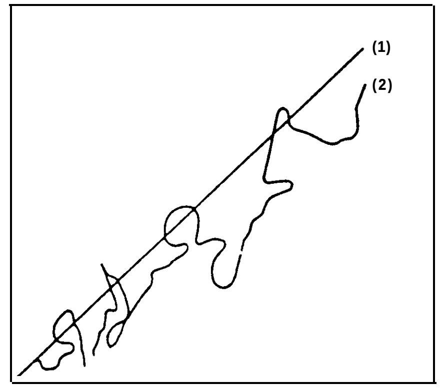

Figure 1. The sequence of research, as an author would have his readers perceive it in the publication (line 1), and of the same research as actually carried out in the laboratory (line 2). Note, interim publications may arise at any point along line 2. Derived from Szent-Gyorgyi (1900), courtesy Academic Press.

Editorial Screening

The security for the reader of a published research report rests largely on the quality of pre-publication peer review. By that standard, the term "reputable" journal is derived. Peer reviewers are for the most part highly competent, hands-on experienced in the subject area, and conscientious. But some papers are more equal than others. It is not exceptional that a reader (or a reviewer per se), faced with a paper that promotes an interpretation contrary to his theory, will most diligently critique the means by which the data were acquired. There is room for conjecture that the converse obtains, as well. When one of the authors of a submitted paper happens to be a Nobel Laureate, "peer" review may be even less rigorous. On the other hand, even a Nobel Laureate can find that challenging the establishment's position on a particular subject is tough going. From physicist Hannes Alfven (1988, p. 251): "If scientific issues always were decided by Gallup polls and not by scientific arguments science will very soon be petrified forever." And "With the referee system which rules U.S. science today . . . my papers are rarely accepted by the leading U.S. journals."

We have, nonetheless, the ideal to which even these editorial boards would subscribe, if not always practice. From Bronowski (1956, pp. 87-88):

> The society of scientists has a directing purpose: to explore the truth . . . It must encourage the single scientist to be independent, and the body of scientists to be tolerant [of dissenting views].

Tolerance ends, however, at dishonesty (see Bronowski 1956, pp. 73-76) .

Scrutiny

Matzko (1991, p. 111) suggests that ". . . most published data is (sic) never replicated, some of it (sic) never even read . . . research grants are given to produce new findings, not rehash old ones." The latter is, to a degree, true enough (given the granting agencies' budget-imposed priorities for funding, and how, in the "rehash," new findings might actually emerge). And, in the process of reviewing a grant application, tenure or promotion candidacy, etc., papers may be counted before (if ever) read (hence, perhaps, so many ambiguous titles !). However, if the work is in an active area of major interest—prerequisite to grant fundability in the first place—the findings *will* be read, certainly by the competition, and to a degree repeated—not necessarily to check their veracity, but as aspects of the protocol become incorporated into the experiments of other investigators, results compared. One of the reasons why scientific fraud is considered so heinous is that one begins his own work with the usually well-founded assumption that according to the methods and materials used, the published data were in fact obtained as published. However, the methods and their accuracy may be questioned, and accordingly, the conclusions drawn from the results challenged. Where the reliability of the results is questionable, the work is usually stigmatized as less than careful or thorough, not fraudulent. Given the pressure to publish or perish, one can risk publishing *and* perishing as a result of sloppy or ill-considered work, or otherwise by publishing prematurely (see Figure 1). For a recent example of premature publication, at best, and its consequences, see the latest developments in the "Cold Fusion" controversy (Taubes, 1991).

Because of coincidental scrutiny, at least, fraud is as remarkable for its foolishness as its abuse of trust. Consideration of the risk:reward ratio alone should be a deterrent, as cynical as that may sound. Frankly, while fraud may elude an editorial board, it is hard to hide in the long run, and its disclosure is devastating for the perpetrator. First reactions to the Imanishi-Kari et al. debacle included the question "how could they have been so stupid?", or arrogant (see Hall, 1991). Baltimore's alleged lack of oversight, at best, drew sardonic expressions not only about his administrative abilities, but because it implied he did not know the details of what he was publishing from his own laboratory! The situation of a Nobel Laureate, who founded and subsequently served as Director of the Whitehead Institute for Biomedical Research and then became President of Rockefeller University,* who cannot evaluate "his" own work (as co-authorship implies) sounds oxymoronic, but should serve as a warning flag to those of the Van Till et al. (1988, 1990) persuasion insofar as their unbridled confidence in the pronouncements of science.

In stark contrast to the Baltimore, Imanishi-Kari et al. imbroglio is the circumstance of Andrew Lyne of Jodrell Bank's University of Manchester Astronomy Laboratories. Like Baltimore, Lyne is a paragon in his field, at an eminent institution engaged in collaborative research. Last year, Lyne and his colleagues published evidence for the existence of a planet outside this solar system (putatively one orbiting a pulsar in the constellation Sagittarius) —a spectacular, first-of-its-kind discovery, enthusiastically received by the editors of *Nature,* and heralded by the *London* and *New York Times,* et al. On January 15, 1992, Lyne rose at a meeting of the American Astronomical Society and announced apologetically, with no self-serving qualifications or innuendoes directed at others of his team, that they (collectively) had been wrong (Flan, 1992)—"in a moment of awful comprehension one night last week I realized [that we had committed an inadvertent procedural error in analyzing the radio wave data] . . ." and, when the flawed analysis was corrected ". . . the planet just evaporated" (Ritter, 1992). The response? A justly deserved standing ovation from his peers—not for having made a mistake, of course, but out of respect for an honest mistake honestly corrected and in timely fashion, since it has now alerted others pursuing this kind of research to a potential error. In the week prior to Lyne's retraction, Alexander Wolszczan, of the Arecibo Observatory, announced the detection of two, possibly three, putative planets around a different pulsar (Wolszczan and Frail, 1992). Most importantly, the Jodrell Bank's mistake was *self*-detected and immediately self-corrected, by a scientist who patently values integrity above the fame and fortune that attends a major, though in this case spurious, discovery, and who clearly wasted no time pondering the embarrassment or other possible material consequences of its rectification. Meanwhile, Wolszczan, while acknowledging the Lyne phenomenon, has stated that ". . . it does not change my thinking about what I have found" (Ritter, 1992). "I am 99.9% certain [that what I have detected] . . . are planets" (Sawyer, 1992).

*The Rockefeller University Board of Trustees accepted Dr. Baltimore's resignation as President on December 3, 1991.

Quality Assessment

When debating a creationist, an evolutionist will not uncommonly resort to *ad hominem* remarks about his opponent's scientific credentials or "his" journals. Creation scientists, as a group, have been pejoratively characterized by the establishment variety as talking (or writing) about science while doing very little of it themselves, though when pressed, the establishment will admit to some striking exceptions to this generalization. In any case, it is not uncommon to find creationist papers structured entirely of data elsewhere acquired. At that point, there may be some substance to the view that one who has no direct experience himself with the techniques, the study object/system, raw data acquisition and subsequent analysis, is not in the best position to evaluate these data or draw conclusions from them. However, this admonishment should not be directed solely at creation "armchair" (or library vs. laboratory) scientists. We have, for example, the copious treatises on biology and its evolution from *astronomer* Carl Sagan, who, in my estimation, seldom lets details, data or otherwise, stand in his way. He is a classic case of the perilous transfer of expertise to which Ph.D.'s are so prone.* Defenders of this principle might, on the other hand, identify some who practice it as Renaissance men. In any event, in defense of armchair science, sound theoretical reviews and re-assessments can be more valuable contributions than a handful of original "nature notes."

I am a relatively recent convert to the creationist (vs. evolutionist) persuasion, and as the Editor already knows, have a lot of creationist literature yet to read. I find that much of what I have read, in or ancillary to my scientific discipline (biochemistry, biophysics, cell biology), is altogether solid. Especially exemplary are the publications of Frair, Gish, Marsh, Ouweneel, Thaxton, Wilder-Smith, and Emmett Williams, among

*Among Dr. Sagan's latest exploits are Gifford Lecturer in Natural Theology at the University of Glasgow, and co-chairman of the Joint Appeal by Science and Religion for the Environment.

others. I have encountered exceptions to the contrary. As is the case for the establishment literature, there would seem to be peaks, valleys, and in-between plateaus of scientific quality in the creationist literature as a whole. One should be instinctively wary of conclusions supported only by references to secondary or tertiary sources (e.g., textbooks, encyclopedias, the "science news") irrespective of their putative authority. In particular, I concur in our *CRSQ* editor's view that the credibility of creation science is not well served by the plethora of self-published works, vs. peer reviewed monographs and journal articles (see, e.g., DeYoung's comments, 1991, p. 70). Painful as it can be, a rigorous pre-publication review may bless the author as much as the reader, certainly the former's scholarly reputation, once the manuscript is in print.

On Faith and Metaphysics

Evolutionists belabor the putative omnipotence of time and random chance, to the chagrin of probability mathematicians and information theorists. What has been particularly distressing for me in reading a discourse about purely natural phenomena, when violation of one or more physical laws is requisite to support a creationist's paradigm, is to find the creationist author invoking some nebulous biblically unrecorded act of supernatural intervention in the process. In some cases, this paraphrases the foppery of "theistic" evolution and Gaia-ism. Otherwise, constructions such as "the Creator *must* have . . ." startle me by their imperative presumption. By no means do I have a philosophical or theological argument with the reality of Divine miracles, but . . .

Walters (1991, p. 129) addresses this issue, noting:

> . . . any creationist models that require violation of physical laws . . . should be viewed critically . . . It is true that the Creator can override the 'laws' of nature as we know them, but it is also true that He rarely chooses to do so.

Who, after all, created these laws, set them into action, and for what reason, in the first place?

Harrison (1933, pp. 319-320) would warn us creationists, as he was warning his fellow embryologists in context, to beware the ". . . anthropomorphisms and relics of our demonology*. . . which may lend a false sense of security to our explanations but may also suggest foolish questions that can never be answered."

Ideally, a scientist does not "believe in" a theory, but either accepts or rejects it on the weight of the evidence. Yet, one always begins his work with a biased view–the hypothesis is just that. Ham (1987) states the matter more strongly (p. 8) —"Scientists are not objective truth-seekers; they are not *neutral.*" There is a sense of comprehensive finality about that statement with which I am uncomfortable. I would suggest, in agreement with Ham, that as a human being, a scientist (like anyone else) may hold a philosophy that varnishes his concept of ultimate truth. But, in the actual practice of his science, on a specific problem (if not world-view!), what we call "scientific discipline" will, in most cases, prevail. This is much of what the "training" of a Ph.D. is all about. The investigator who lacks that quality will, in all likelihood, suffer a short career as a practicing scientist, irrespective of the grant or two he may acquire or the handful of papers he may get published in the interim. However, he may ascend to great heights in the teaching profession, or as an author of commentaries. And, given the tenet of "academic freedom," he may remain there indefinitely.

*Harrison uses the term *demonology* here in the sense of great energy, urgency, or skill, not in the spiritual sense.

Separating the Wheat from the Chaff

There is, then, "good" science and "bad" science (in the sense of its execution and veracity, not morality), irrespective of its source. Where that source is the evolutionary camp, a creationist scientist should never "throw out the baby with the bathwater." The virtue of a partial truth is that it contains some truth. Even mistakes can be revealing. As I have been telling my students for years, there is no such thing as "wrong data" (as long as they are not fraudulent!), though data may have been improperly acquired, or misinterpreted, or be contradictory to the expected result. When the latter obtains, one should reconsider the basis for the expectation. Thus, no one in my laboratory ever threw away—with my concurrence—a printout from the scintillation counter, the X-ray spectrometer, or notes on an experiment that "did not work." That was not necessarily being super-ethical or even paranoid.

In any event, when using data, theirs or others, creationists *must* be as rigorous in guarding against their commission of "investigator interference" (i.e., following a protocol that forces data consistent with a preconceived conclusion) and biased screening of data, as the evolutionist *should* be.

The Lord blesses "good" science as the discoveries reveal His creation, His Being, and so glorify Him (Romans 1: 19-20). On the other hand, authors and teachers [authors when published become teachers, if they are not already] should also heed keenly Matthew 18:6,

> But whoso shall offend [by errant teaching, etc.] one of these little ones [the naive student or reader] . . . it were better for him that a millstone were hanged about his neck and that he were drowned in the depth of the sea.

How, ultimately, does one discern the truth? See Proverbs 3:5. If in some minds I, like George Matzko (1991, p. 111) exhibit, in the process, a "hopeless naivete when approaching the Scriptures," so be it.

Conclusions

What is the basis for the present shortcomings in science? Is it, at least where the secular establishment is concerned, the pain of riches (or deficit of same) and reputation . . . or technical limitations? In part, yes. But consider also Lammerts' sage view (quoted by Meyer, 1991, p. 85) that

> . . . all research should be undertaken *prayerfully* with the objective of helping one's fellow man to better understand, enjoy, and thankfully appreciate . . . the evidence of the intricacy of His creations (my emphasis).

When asked for bread, does He give a stone (Matthew 7: 9)? As a reviewer of grant proposals for the National Institutes of Health, the National Science Foundation, etc., I never read a prospective research protocol that began that way—i.e., that the principle investigator might seek and then be guided by a Wisdom beyond his own or his peers. Might it be that the "wise," otherwise, would be caught in their own craftiness (1 Corinthians 3: 19)?

Acknowledgements

I thank Drs. Richard Bliss, Edward Boudreaux and Duane Gish for a critical reading of the manuscript and their helpful comments, and Gaynell Lumsden for her valuable assistance with the literature search and manuscript preparation.

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Addendum

As reported in the July 16, 1992 issue of *Nature* (p. 177), the Imanishi-Kari / Baltimore saga continues. The technical reliability of an earlier forensic analysis of Imamshi-Kari's data books by the U.S. Secret Service has been challenged in a reanalysis commissioned by Imanishi-Kari's defense attorneys, and Baltimore has claimed ". . . extensive confirmation" (unspecified) of the *Cell* paper. Identifying the controversy as one of a very complex scientific nature that a grand jury "were clearly incapable of understanding" the U.S. Attorney is no longer seeking an indictment in the case. Baltimore has retracted his retraction, which some observers feel was pressured in the first place by other problems he was having at Rockefeller University. Imanishi-Kari, interpreting these events as exonerating, says she will be requesting NIH to release funds from a 1989 grant frozen during the course of the Congressional, NIH and Justice Department proceedings. Has a matter of principle devolved to one of principal? Meanwhile, Congressman John Dingle, who chairs the Oversight and Investigation Subcommittee of the House Energy and Commerce Committee, has observed that ". . . the decision not to prosecute does not change the fact that the *Cell* paper was retracted (by the other four original co-authors) because of serious, and extensive, irregularities."

Reference

Anderson, C. and T. Watson. 1992. US drops Imanishi-Kari investigation; Baltimore withdraws *Cell* retraction. *Nature* 358:177.

QUOTE

It has long been assumed that preserved sedimentary rocks record primarily normal or average conditions for past epochs but this uniformitarian assumption must be challenged.

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