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Refereed Journals: Do They Insure Quality or Enforce Orthodoxy?

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Introduction

I first became aware of the importance that many non-elite scientists place on "peer-reviewed" or "refereed" journals when Howard Van Till, a theistic evolutionist, said my book *The Physics of Immortality* was not worth taking seriously because the ideas it presented had never appeared in refereed journals. Actually, the ideas in that book *had* already appeared in refereed journals. The papers and the refereed journals wherein they appeared were listed at the beginning of my book. My key predictions of the top quark mass (confirmed) and the Higgs boson mass (still unknown) even appeared in the pages of *Nature,* the most prestigious refereed science journal in the world. But suppose Van Till had been correct and that my ideas had never been published in referred journals. Would he have been correct in saying that, in this case, the ideas need not be taken seriously?

To answer this question, we first need to understand what the "peer review" process is. That is, we need to understand how the process operates in theory, how it operates in practice, what it is intended to accomplish, and what it actually does accomplish in practice. Also of importance is its history. The notion that a scientific idea cannot be considered intellectually respectable until it has first appeared in a "peer" reviewed journal did not become widespread until after World War II. Copernicus's heliocentric system, Galileo's mechanics, Newton's grand synthesis—these ideas never appeared first in journal articles. They appeared first in books, reviewed prior to publication only by the authors or by the authors' friends. Even Darwin never submitted his idea of evolution driven by natural selection to a journal to be judged by "impartial" referees. Darwinism indeed first appeared in a journal, but one under the control of Darwin's friends. And Darwin's article was completely ignored. Instead, Darwin made his ideas known to his peers and to the world at large through a popular book: *On the Origin of Species*.

I shall argue that prior to the Second World War the refereeing process, even where it existed, had very little effect on the publication of novel ideas, at least in the field of physics. But in the last several decades, many outstanding scientists have complained that their best ideas—the very ideas that brought them fame—were rejected by the refereed journals. Thus, prior to the Second World War, the refereeing process worked primarily to eliminate crackpot papers.

Today, the refereeing process works primarily to enforce orthodoxy. I shall offer evidence that "peer" review is *not* peer review: the referee is quite often not as intellectually able as the author whose work he judges. We have pygmies standing in judgment on giants. I shall offer suggestions on ways to correct this problem, which, if continued, may seriously impede, if not stop, the advance of science.

The Peer Review Process

Since the 1950s, here is how the peer review process has worked: A scholar wishing to publish a paper in a journal would mail several copies of the paper to the editor of the journal. The editor would not make the decision himself whether to publish the paper in his journal. Instead, the editor would mail the paper to one or more scholars, whom he judges to be experts on the subject matter of the paper, asking them for advice on whether the paper is worthy of publication—their advice constituting the "peer review." Two or more experts in the same field as the author of the paper—his "peers"—are therefore to judge the worth of the paper. The editor asks the reviewers, often called the "referees," to judge the paper on such criteria as (1) validity of the claims made in the paper, (2) originality of the work (has someone already done similar work), and (3) whether the work, even if correct and original, is sufficiently "important" to be worth publishing in the journal. Generally, only if the referee or referees agree that the paper has met all three criteria will the editor accept the paper for publication in his journal. Otherwise, he will return the paper to the author, thereby rejecting it.

The peer review process was put in place after the Second World War because of the huge growth in the scientific community as well as the huge increase in pressure on scholars to publish more and more papers. Prior the war, university professors (who have always been the main writers of scholarly papers) were mainly teachers, with teaching loads of five to six courses per semester (as opposed to the one to two course load today). Professors with this teaching load were not expected to write papers. In fact, the Austrian/English philosopher Karl Popper wrote in his autobiography that the dean of the New Zealand university where Popper taught during Second World War said that he regarded Popper's production of articles and books a theft of time from the university!

But universities came to realize that their prestige depended not on the teaching skill of their professors but on the scholarly reputation of these professors. And this reputation could come only via the production of articles. So pressure began to be placed on the professors to publish. Teaching loads were reduced so that more time would be available to write papers (and perhaps do the research that would be described in the papers). Salaries began to depend on the numbers of papers published and on the grant support which well-received papers could garner. Before the war, salaries of professors of the same rank were the same (except perhaps for an age differential). Now salaries of professors in the same department of the same age and rank can differ by more than a factor of two.

As a consequence, the production of scholarly articles has increased by more than a factor of a thousand over the past fifty years. Unfortunately, the average quality of the papers also went down. Since earlier there was no financial reward for writing a scholarly article, people wrote the papers as a labor of love. They had ideas that they wished to communicate with their peers, and they wrote the papers to communicate those ideas. Now papers were mainly written to further a career.

Einstein's experience is illustrative. He published three super breakthrough papers in 1905. One presented to the world his theory of (special) relativity. A second paper showed that light had to consist of particles that we now call photons; using this fact, he explained the emission of electrons from metals when illuminated by light. Einstein was awarded the Nobel Prize for this explanation. The third paper explained the vibration of dust particles in air by attributing the motion to molecules of air hitting the dust particles. Einstein's explanation of this "Brownian motion" allowed properties of the molecules to be calculated, and it was Einstein's explanation that finally convinced physicists that atoms actually existed. Not bad for one year! And Einstein wrote these papers in his spare time, after he returned home from his paying job as a patent clerk in Bern, Switzerland.

All three papers were published in *Annalen der Physik,* one of the major physics journals in Germany. But none of the papers were sent to referees. Instead the editors—either the editor in chief, Man Planck, or the editor for theoretical physics, Wilhelm Wien—made the decision to publish. It is unlikely that whoever made the decision spent much time on whether to publish. Almost every paper submitted was published. So few people wanted to publish in *any* physics journal that editors rarely rejected submitted papers. Only papers that were clearly "crackpot" papers—papers that any professional physicist could recognize as written by someone completely unfamiliar with the elementary laws of physics—were rejected.

And if *Annalen der Physik* rejected a paper, for whatever reason, any professional German physicist had an alternative: *Zeitschrift für Physik*. This journal would publish *any* paper submitted by any member of the German Physical Society. This journal published quite a few worthless papers. But it also published quite a few great papers, among them Heisenberg's first paper on the Uncertainty Principle, a central idea in quantum mechanics. There was no way in which referees or editors could stop an idea from appearing in the professional journals. In illustration of this, the great Danish physicist Niels Bohr said, according to Abraham Pais (*The Genius of Science,* p. 307), that if a physicist has an idea that seems crazy and he hesitates to publish so that someone else publishes the idea first and gets the credit, he has no one to blame but himself. In other words, it never occurred to Bohr that referees or editors could stop the publication of a new idea.

Peer Review Today

Bohr would not say that today. If one reads memoirs or biographies of physicists who made their great breakthroughs after, say, 1950, one is struck by how often one reads that "the referees rejected for publication the paper that later won me the Nobel Prize." One example is Rosalyn Yalow, who described how her Nobel-prize-winning paper was received by the journals. "In 1955 we submitted the paper to *Science*.... The paper was held there for eight months before it was reviewed. It was finally rejected. We submitted it to the *Journal of Clinical Investigations*, which also rejected it." (Quoted from *The Joys of Research,* edited by Walter Shropshire, p. 109). Another example is Günter Blobel, who in a news conference given just after he was awarded the Nobel Prize in Medicine, said that the main problem one encounters in one's research is "when your grants and papers are rejected because some stupid reviewer rejected them for dogmatic adherence to old ideas." According to the *New York Times* (October 12, 1999, p. A29), these comments "drew thunderous applause from the hundreds of sympathetic colleagues and younger scientists in the auditorium."

In an article for *Twentieth Century Physics*, a book commissioned by the American Physical Society (the professional organization for U.S. physicists) to describe the great achievements of 20th century physics, the inventor of chaos theory, Mitchell J. Feigenbaum, described the reception that his revolutionary papers on chaos theory received:

> Both papers were rejected, the first after a half-year delay. By then, in 1977, over a thousand copies of the first preprint had been shipped. This has been my full experience. Papers on established subjects are immediately accepted. Every novel paper of mine, without exception, has been rejected by the refereeing process. The reader can easily gather that I regard this entire process as a false guardian and wastefully dishonest. (Volume III, p. 1850).

Earlier in the same volume on 20th century physics, in a history of the development of optical physics, the invention of the laser by Theodore Maiman was described. The result was so important that it was announced in the *New York Times* on July 7, 1960. But the leading American physics journal, *Physical Review Letters,* rejected Maiman's paper on how to make a laser (p. 1426).

Scientific eminence is no protection from a peer review system gone wild. John Bardeen, the only man to ever have won *two* Nobel Prizes in physics, had difficulty publishing a theory in low-temperature solid state physics (the area of one of his Prizes) that went against the established view. But rank hath its privileges. Bardeen appealed to his friend David Lazarus, who was editor in chief for the American Physical Society. Lazarus investigated and found that "the referee was totally out of line. I couldn't believe it. John really did have a hard time with [his] last few papers and it was not his fault at all. They were important papers, they did get published, but they gave him a harder time than he should have had." (*True Genius: The Life and Science of John Bardeen*, p. 300).

Stephen W. Hawking is the world's most famous physicist. According to his first wife Jane (*Music to Move the Stars: A Life with Stephen Hawking,* p. 239), when Hawking submitted to *Nature* what is generally regarded as his most important paper, the paper on black hole evaporation, the paper was initially rejected. I have heard from colleagues who must remain nameless that when Hawking submitted to *Physical Review* what I personally regard as his most important paper, his paper showing that a most fundamental law of physics called "unitarity" would be violated in black hole evaporation, it, too, was initially rejected. (The word on the street is that the initial referee was the Institute for Advanced Study physicist Freeman Dyson.)

Today it is known that the Hawaiian Islands were formed sequentially as the Pacific plate moved over a hot spot deep inside the Earth. The theory was first developed in the paper by an eminent Princeton geophysicist, Tuzo Wilson: "I … sent [my paper] to the *Journal of Geophysical Research*. They turned it down…. They said my paper had no mathematics in it, no new data, and that it didn't agree with the current views. Therefore, it must be no good. Apparently, whether one gets turned down or not depends largely on the reviewer. The editors, too, if they don't see it your way, or if they think it's something unusual, may turn it down. Well, this annoyed me, and instead of keeping the rejection letter, I threw it into the wastepaper basket. I sent the manuscript to the newly founded *Canadian Journal of Physics*. That was not a very obvious place to send it, but I was a Canadian physicist. I thought they would publish almost anything I wrote, so I sent it there and they published it!" (Quoted from *The Joys of Research,* p. 130.)

The most important development in cloning after the original breakthrough of Dolly the Sheep was the cloning of mice. The result was once again described on the front page of the *New York Times*, where it was also mentioned that the paper was rejected for publication by the leading American science journal, *Science*.

Everyone knows today that the dinosaurs were wiped out 65 million years ago when a giant asteroid hit the Earth. *Science* did publish the article presenting this theory, but only after a fierce fight with the referees, as one of these referees later confessed. On the Nobel Prize web page one can read the autobiographies of recent laureates. Quite a few complain that they had great difficulty publishing the ideas that won them the Prize. One does not find similar statements by Nobel Prize winners earlier in the century. Why is there more resistance to new ideas today?

Why Does Peer Review Suppress New Ideas Today?

Philip Anderson, a winner of the Nobel Prize for Physics opines that "in the early part of the postwar [post-WWII] period [a scientist's] career was science-driven, motivated mostly by absorption with the great enterprise of discovery, and by genuine curiosity as to how nature operates. By the last decade of the century far too many, especially of the young people, were seeing science as a competitive interpersonal game, in which the winner was not the one who was objectively right as [to] the nature of scientific reality, but the one who was successful at getting grants, publishing in *Physical Review Letters*, and being noticed in the news pages of *Nature*, *Science*, or *Physics Today*.... [A] general deterioration in quality, which came primarily from excessive specialization and careerist sociology, meant quite literally that more was worse." (*20th Century Physics*, pp. 2029).

But the interesting question is, what caused the "excessive specialization and careerist sociology" that is making it very difficult for new ideas to be published in peer review journals? There are several possibilities. One is a consequence of Anderson's observation that, paradoxically, more scientists can mean a slower rate of scientific advance. The number of physicists, for example, has increased by a factor of a thousand since the year 1900, when ten percent of all physicists in the world either won the Nobel Prize or were nominated for it. If you submitted a paper to a refereed journal in 1900, you would have a far greater chance of having a referee who was a Nobel Prize winner (or at least a nominee) than now. In fact, a simple calculation shows that one would have to submit three papers on the average to have an even chance that at least one of your papers would be "peer" reviewed by a Nobel Prize winner. Today, to have an even chance of having a Nobelist for a referee, you would have to submit several hundred papers. Thus Albert Einstein had his revolutionary 1905 papers truly peer reviewed: Max Planck and Wilhelm Wien were both later to win the Nobel Prize in physics.

Today, Einstein's papers would be sent to some total nonentity at Podunk U, who, being completely incapable of understanding important new ideas, would reject the papers for publication. "Peer" review is *very* unlikely to be peer review for the Einsteins of the world. We have a scientific social system in which intellectual pygmies are standing in judgment of giants. (See P. Stephan and S. Levin, *Striking the Mother Lode in Science,* chapter 7 for a detailed discussion of the Pygmy Effect.)

One could argue that because the number of Nobel Prizes awarded is permanently fixed at one per year in three scientific disciplines (physics, chemistry, and medicine), the relative decrease in Nobelists does not mean a similar decrease in the number of giants to pygmies. The data contradict this proposal. The American Chemical Society made a list of the most significant advances in chemistry made over the last 100 years. There has been no change in the rate at which these breakthroughs in chemistry have been made in spite of the thousand-fold increase in the number of chemists. In the 1960s, U.S. citizens were awarded about 50,000 chemical patents per year. By the 1980s, the number had dropped to 40,000. Finally, although the number of people *awarded* a Nobel Prize is fixed, the number *nominated* is unlimited. Yet the data show that the number of scientists nominated for the Prize has increased by at most a factor of three in the past century—despite the thousand-fold increase in the number of scientists. (Robert Root-Bernstein, *Discovering*, Harvard University Press, 1989, pp. 39-40.) Unquestionably, there has been a huge drop in the ratio of giants to pygmies over the last century.

Another possibility is that the increasing centralization of scientific research has allowed powerful but mediocre scientists to suppress any idea that would diminish their prestige. All great advances in science have by definition the effect of reducing the prestige of the "experts" in the field in which the advance is made. The expert's expertise is necessarily invalidated by a radical change in the underpinnings of a scientific discipline. Laymen rarely appreciate how centralized scientific research has become in the last fifty years. Funding for my own area of physics, general relativity, is located in one and only one division of one and only one bureau of the federal government, the National Science Foundation. If the referees for a grant proposal submitted to this division of that bureau happen not to like your work, your grant proposal will not be funded—period. In the first part of the 20th century, a grant rejection, like a paper rejection, would not stop an idea from being presented or from being developed. In this earlier period, a tenured professorship came with a small amount of research funds. Since the universities of the time were not dependent on government grant money, tenure decisions were not dominated by whether a scholar up for tenure obtained a grant.

Now most American universities, even the liberal arts colleges, are desperately dependent on government grants. A typical National Science Foundation grant, for example, has an "overhead" charge, which can amount to fifty percent of the grant. This "overhead" charge goes directly to the university administration; the scientist never sees a dime of this part of his grant. If the total amount of the grant is $1,000,000, and the overhead is fifty percent, the scientist who secures the grant has $500,000 to do his research. The other $500,000 goes to the university bottom line. A university is strongly motivated to hire only those scientists who can obtain large grants. Pushing an idea that is contrary to current opinion is not a good way to obtain large grants.

I have experienced this form of discrimination first hand. When I came up for tenure at Tulane in 1983, I was already controversial. At the time I had proposed that general relativity might allow time travel, and I had published a series of papers claiming that we might be the only intelligent life form in the visible universe. At the time, these claims were far outside the mainstream. (They are standard claims now. Kip Thorne of Cal Tech has argued for the possibility of time travel, using the same mechanism I originally proposed. The scientific community is now largely skeptical of extraterrestrial intelligence, if for no other reason than the failure of the SETI radio searches.) But my views made it very difficult to get an NSF grant. One reviewer of one of my grant proposals wrote that it would be inadvisable to award me a grant because I might spend some of the time working on my "crazy" ideas on ETI. I didn't get the grant.

It began to look as if I wouldn't get tenure. I had a large number of papers published in refereed journals—including *Physical Review Letters* and *Nature*—but no government grants. For this reason, and for this reason alone (I was told later), the initial vote of the Tulane Physics Department was to deny me tenure. But I had another grant proposal under consideration by the NSF. I called Rich Isaacson, the head of the Gravitation Division of the NSF, and told him about my situation. Rich called me a few weeks later, and told me that the referee reports for my proposal were "all over the map"—some reviewers said I was the most original relativity physicist since Einstein, and others said I was an incompetent crackpot. Rich said that in such a circumstance, he could act as he saw fit. He saw fit to fund my proposal. I had grant support! I also had tenure; the physics department reversed its negative vote.

But even at the time I worried that this sequence of events boded ill for science. Rich was the head of the only government agency that supplied funds for research in relativity physics. He knew that an influential minority of physicists thought well of my work (especially John Wheeler of Princeton, who is really the father of most relativity research in the U.S.). But what if I was engaged in a long-term project that had not definitely established itself? Except for the lack of a grant, I had impressed many of my colleagues as a capable physicist. But in today's science, this is not enough. It is absolutely essential to obtain a government grant. I got the grant—and tenure—only because a single man thought well of my work. If he did not, then I would not have gotten tenure. Nor would I have gotten tenure at any other American university. I have always had a high opinion of Rich Isaacson. But no man is God. No man should have the effective power to deny or award tenure for an entire field over the entire United States. But the current grant support system has created such research czars. These individuals are discouraged from supporting radical ideas.

The most radical ideas are those that are perceived to support religion, specifically Judaism and Christianity. When I was a student at MIT in the late 1960s, I audited a course in cosmology from the physics Nobelist Steven Weinberg. He told his class that of the theories of cosmology, he preferred the Steady State Theory because "it *least* resembled the account in Genesis" (my emphasis). In his book *The First Three Minutes* (chapter 6), Weinberg explains his earlier rejection of the Big Bang Theory: "[O]ur mistake is not that we take our theories too seriously, but that we do not take them seriously enough. It is always hard to realize that these numbers and equations we play with at our desks have something to do with the real world. Even worse, there often seems to be a general agreement that certain phenomena are just not fit subjects for respectable theoretical and experimental effort."

I have now known Weinberg for over thirty years, and I know that he has *always* taken the equations of physics very seriously indeed. He and I are both convinced that the equations of physics are the best guide to reality, *especially* when the predictions of these equations are contrary to common sense. But as he himself points out in his book, the Big Bang Theory was an automatic consequence of standard thermodynamics, standard gravity theory, and standard nuclear physics. All of the basic physics one needs for the Big Bang Theory was well established in the 1930s, some two decades before the theory was worked out. Weinberg rejected this standard physics not because he didn't take the equations of physics seriously, but because he did not like the religious implications of the laws of physics. A recent poll of the members of the National Academy of Sciences, published in *Scientific American,* indicated that more than ninety percent are atheists. These men and women have built their entire worldview on atheism. They would be exceedingly reluctant to admit that any result of science could be valid if it even suggested that God could exist.

I discovered this the hard way when I published my book *The Physics of Immortality.* The entire book is devoted to describing what the known laws of physics predict the far future of the universe will be like. Not once in the entire book do I use anything but the known physical laws, the laws of physics that are in all the textbooks, and which agree with all experiments conducted to date. Unfortunately, in the book I gave reasons for believing that the final state of the universe—a state outside of space and time, and not material—should be identified with the Judeo-Christian God. (It would take a book to explain why!) My scientific colleagues, atheists to a man, were outraged. Even though the theory of the final state of the universe involved only known physics, my fellow physicists refused even to discuss the theory. If the known laws of physics imply that God exists, then in their opinion, this can only mean that the laws of physics have to be wrong. This past September, at a conference held at Windsor Castle, I asked the well-known cosmologist Paul Davies what he thought of my theory. He replied that he could find nothing wrong with it mathematically, but he asked what justified my assumption that the known laws of physics were correct. At the same conference, the famous physicist Freeman Dyson refused to discuss my theory—period. I would not encounter such refusals if I had not chosen to point out my theory's theological implications.

In the foreword to *The Physics of Immortality,* I included the standard acknowledgment of grant support. The government official (of Austria in this case) who provided funds to partially support my research told me that he had received enormous criticism from his fellow bureaucrats. They were outraged that a defense of Christianity was being supported by a respectable science organization. The California Skeptics Society founder, Michael Shermer, informs me that a proposal to the NSF to fund the publication of all of Isaac Newton's to-date unpublished work on theology was rejected even though the proposal was made by one of the world's leading Newton scholars. The reason given, according to Shermer, was that it would be bad for science if it became generally known that the greatest scientist of all time actually believed in God. Clearly, the scientific community is not open to any evidence or any theory that might even hint that God really exists and might actually act the in physical universe.

Intelligent Design

The most radical scientific theory with religious implications is Intelligent Design. It is impossible to get any member of the National Academy of Sciences to consider it seriously. The typical reaction of such scientists is to foam at the mouth when the phrase "intelligent design" is mentioned. I have recently experienced this. In the fall of 2002, I arranged for Bill Dembski to come to Tulane to debate a Darwinian on the Tulane faculty. (This faculty member was appropriately named Steve Darwin!) Bill presented only the evidence against Darwinism in the debate, while Steve's response unfortunately had quite a few ad hominem remarks. Steve has continued to be friendly to me personally. But ever since the Dembski/Darwin debate, another evolutionist on the Tulane faculty—who shall remain nameless!—glares at me every time he sees me. Before the debate he and I were friends. Now he considers me a monster of moral depravity.

Yet if the religious implications of Intelligent Design are ignored, if the theory is called something besides "intelligent design," then the scientific community is quite open to intelligent design. The evolutionist Lynn Margulis, a member of the National Academy of Sciences, has made much the same criticism of modern Darwinism that Michael Behe and Bill Dembski have made. She has put her arguments in a book titled *Acquiring Genomes: A Theory of the Origins of Species,* written with her son Dorion Sagan. The book has a foreword written by Ernst Mayr, a retired professor of evolutionary biology at Harvard, who agrees with Margulis that Darwinism has the problems she discusses. Now this is especially significant since Mayr is not just an ordinary evolutionist. He has been called the "Dean of American Evolutionists," and he is one of the founders of the Modern Synthesis, which is the modern version of Darwinism. Mayr does not think that Margulis has resolved the problems with Darwinism (and I agree with him). I should mention that to her credit, she cites in her book Michael Behe's *Darwin's Black Box.*

The problem that Behe, Dembski, and Margulis address is that random mutation is simply too slow and too undirected to generate the enormous change we see in the fossil record. I shall not discuss the evidence for this; the other essays in this book do an excellent job. (Or if you want a presentation by someone with impressive credentials who never uses the dreaded expression "intelligent design," read Margulis's book.) I do, however, want to make two points not raised elsewhere in this collection of essays.

The first is that "intelligent design" could have been called "Asa Gray Darwinism." Asa Gray was a 19th century botanist at Harvard. He was a friend of Charles Darwin long before the publication of *On the Origin of Species by Means of Natural Selection*. He arranged for the *Origin* to be published in America. He was Darwin's greatest supporter in America, writing many articles in support of Darwin's theory of evolution. But in one crucial respect he disagreed with Darwin. Not with the mechanism of natural selection. Everyone, every creationist, believes that natural selection operates in nature. The question is, and has always been, where do the superior gene complexes, the genes that are going to win the struggle for existence, come from.

Asa Gray believed that the superior mutations, the irreducible complexity in the genome, came from God. He rejected the idea that superior mutations appeared by random variation. Instead, the mutations appeared in a sequence in the genome in a way intended and actively directed by God. Once a build-up of what we would now call a gene complex was complete, this sequence would be turned on, and a new species would appear. Then, and only then, would natural selection operate.

Remarkably, Charles Darwin himself gave the strongest argument for Asa Gray Darwinism and against his own random variation version of Darwinism. Darwin pointed out in the last few pages of his second most important book, *The Variation of Animals and Plants* *under Domestication*, that his own version of Darwinism could not be an ultimate theory. His own version of Darwinism could only be an approximation. In actuality, at the most fundamental level, random mutations did not, and could not, occur. The reason, wrote Darwin, is simple. At the most fundamental level, the laws of physics govern everything, and the laws of physics are deterministic. Therefore, at the most fundamental level, there are no random events. The cosmic ray that causes a particular superior mutation in the genes was not a random, undetermined event but was determined by the laws of physics and the initial conditions of the universe. If we knew the ultimate laws of physics and knew the initial conditions of the universe, we could predict which mutations would occur and when they would occur. We would be able to predict the entire future course of biological history.

Charles Darwin got it exactly correct. He had a much deeper understanding of evolution than any 20th century evolutionist. As a physicist, I am aware that quantum mechanics, the central theory of modern physics, is even more deterministic that was the classical mechanics of which Darwin was aware. More than this, quantum mechanics is actually teleological, though physicists don't use this loaded word (we call it "unitarity" instead of "teleology"). That is, quantum mechanics says that it is completely correct to say that the universe's evolution is determined not by how it started in the Big Bang, but by the final state of the universe. Every stage of universal history, including every stage of biological and human history, is determined by the ultimate goal of the universe. And if I am correct that the universal final state is indeed God, then every stage of universal history, in particular every mutation that has ever occurred, or ever will occur in any living being, is determined by the action of God. In other words, if the laws of modern physics are correct, then Darwin has actually given the strongest argument for Asa Gray Darwinism. Charles Darwin was actually an Intelligent Design Creationist!

If my Tulane University ex-friend ever reads these words, he would want to do more than glare. He would want to strangle me for writing such a heresy! He definitely would not approve of these words were he to be the referee of a paper of mine wherein this argument is repeated. He would definitely reject any grant proposal I would make that contains these words.

Suggestions to Make Science More Open to New Ideas

I shall make two recommendations that, if adopted, would make science more open to new ideas. One concerns a way of opening up the refereed journals to new ideas, and the other, a way of breaking the centralization of research funding.

The problem with the referee system for papers is that in the post-WWII period, the referees are almost never the "peers" of the scientific genius. The size of the scientific community makes true peer review impossible. Most referees are "stupid" (to use Nobelist Blobel's adjective), at least relative to the authors whose breakthrough work we would most like to see published in the leading journals. But I will grant that these "stupid" referees serve a useful purpose if the scientific community remains as large as it is today. Most papers written by most members of the scientific community are worthless. (Most papers are never cited by other scientists.) These trash papers are written because of the "publish or perish" rule imposed by universities. A referee, even a stupid one, can at least keep out the worst of the trash papers from the journals. But we don't want to misidentify works of genius as trash. Which is exactly what the typical referee in fact does.

So I propose that the leading journals in all branches of science establish a "two-tier" system. The first tier is the usual referee system. The new tier will consist of publishing a paper in the journal automatically if the paper is submitted with letters from several leading experts in the field saying, "this paper should be published." Crick and Watson followed this procedure in the case of their famous paper on the double helix structure of DNA. The paper was never sent to referees (*New York Times,* February 25, 2003, p. D4). Instead the paper was submitted to *Nature* with a "publish" covering letter from Sir Lawrence Bragg, the head of the Cavendish Laboratory at Cambridge University, and also a Nobel Prize winner (James Watson, *The Double Helix*). Charles Darwin's first paper on evolution was published in the *Journal of the Linnean Society* upon the recommendation of several leading members of that society. A journal could list on the web the experts, and would-be authors would be advised to contact them by e-mail only. As long as the number of experts is "large"—in physics, several hundred would be sufficient—the chance of a "stupid" referee being able to stop the publication of a breakthrough paper is small. A genius could interact directly with another genius. I would think a single letter of recommendation to publish would be sufficient if the letter were from a Nobelist or an NAS member.

In all the cases mentioned above, the genius papers (as we now regard them) would have been published immediately. The chaos genius Feigenbaum, for example, mentions by name a few of his pre-publication supporters, and some of these are universally recognized as geniuses themselves. Feigenbaum had the advantage of being known to these men personally. The unknown patent office clerk has a problem. For him the physics community has the lanl database (http://xxx.lanl.gov), which is the modern equivalent of the early 20th century *Zeitschrift für Physik*. Anyone can place a paper on the lanl database. There is no referee to stand in the author's way. Of course, a great deal of nonsense is placed on the lanl database, but in my own field of general relativity it seems no worse then the huge amount of nonsense that appears in the leading refereed journals, including *Physical Review Letters.* An unknown author first has to put his paper on the lanl database and then persuade a leading physicist to read it. If a leader can be persuaded to read it, and take it seriously, my recommendation would ensure that it would be published in a leading journal.

The grant-funding problem is more difficult to solve. The ideal solution would be to abolish federal support of science altogether. In the "golden years" of physics in Germany in the first 30 years of the 20th century, the national German government provided very little support for physics, or for science of any sort. Instead, the regional German governments (the German equivalent of states in the U.S.) provided the funds for sciences through their funding of the universities. It was impossible for one small group to control thought by means of a stranglehold over a centralized funding agency. All this changed when Adolf Hitler rose to power in 1933. Hitler sought conformity of thought by centralizing all areas of intellectual endeavor in Germany. The universities were even compelled to dismiss professors whose opinions were not to the liking of the central authorities. Unfortunately, as a consequence of the Second World War and the Cold War, the United States is now enforcing a similar conformity through its science policy.

What's needed now is the "trust-busting" philosophy of the late 19^{th} century. If it was bad to have Standard Oil control ninety percent of the oil refining capacity of the U.S., it is equally bad for the federal government (or a few universities like Harvard, Princeton, MIT and Cal Tech, which disproportionately influence federal support of science) to control the production of scientific results. Monopoly is bad, both in the economy and in science.

But as I said, there are now too many special interests involved in federal science funding to abolish the system altogether. I would therefore recommend, as a second-best alternative to abolishing the system entirely, that "earmarked" funding be increased ("pork barrel" funding in the language of the monopolists). Individual senators and representatives would designate these grants to go to particular universities in their own states and districts. Such grants would bypass the centralized referee system. The individual congressmen can consult the referees they themselves regard as "expert." The funding decisions will indeed be based on politics. But the important thing is that the politics will be coming from outside a narrow, self-selected group of "experts." If my recommendation were followed, science funding would be spread out among the states and congressional districts more or less as it was in the golden years of physics. It would be much more difficult for a small group to control the generation of new ideas in science.

My own state of Louisiana has a model program that I hope could be emulated by the other states. A decade ago, Louisiana had a billion-dollar windfall arising from a settlement with the federal government on the division of revenues from the sale of oil leases in the Gulf of Mexico. The citizens of Louisiana voted to establish an educational foundation with the money. The foundation awards grants to Louisiana scientists and only to Louisiana scientists. The foundation sometimes solicits opinions about the worth of a Louisiana scientist's work from scientists outside Louisiana, but it is not required to do so. In this way, a source of research funding not centrally controlled by the federal government has been established. If the federal government were to decrease funding to the federal government labs, and use the money saved to set up foundations analogous to Louisiana's in all states, we would see an increase in scientific breakthroughs. The astronomer Martin Harwit pointed out in his book *Cosmic Discovery* (pp. 260-261) that in the period 1955 to 1980, national astronomy labs absorbed seventy percent of the federal research funds in astronomy but made none of the astronomy breakthroughs of that period. Shutting down the labs would not decrease the number of great scientific advances.

The federal government must not impose constraints on what is "valid" research. In particular, if a state foundation chooses to fund research in Intelligent Design, then it should be allowed to do so.