

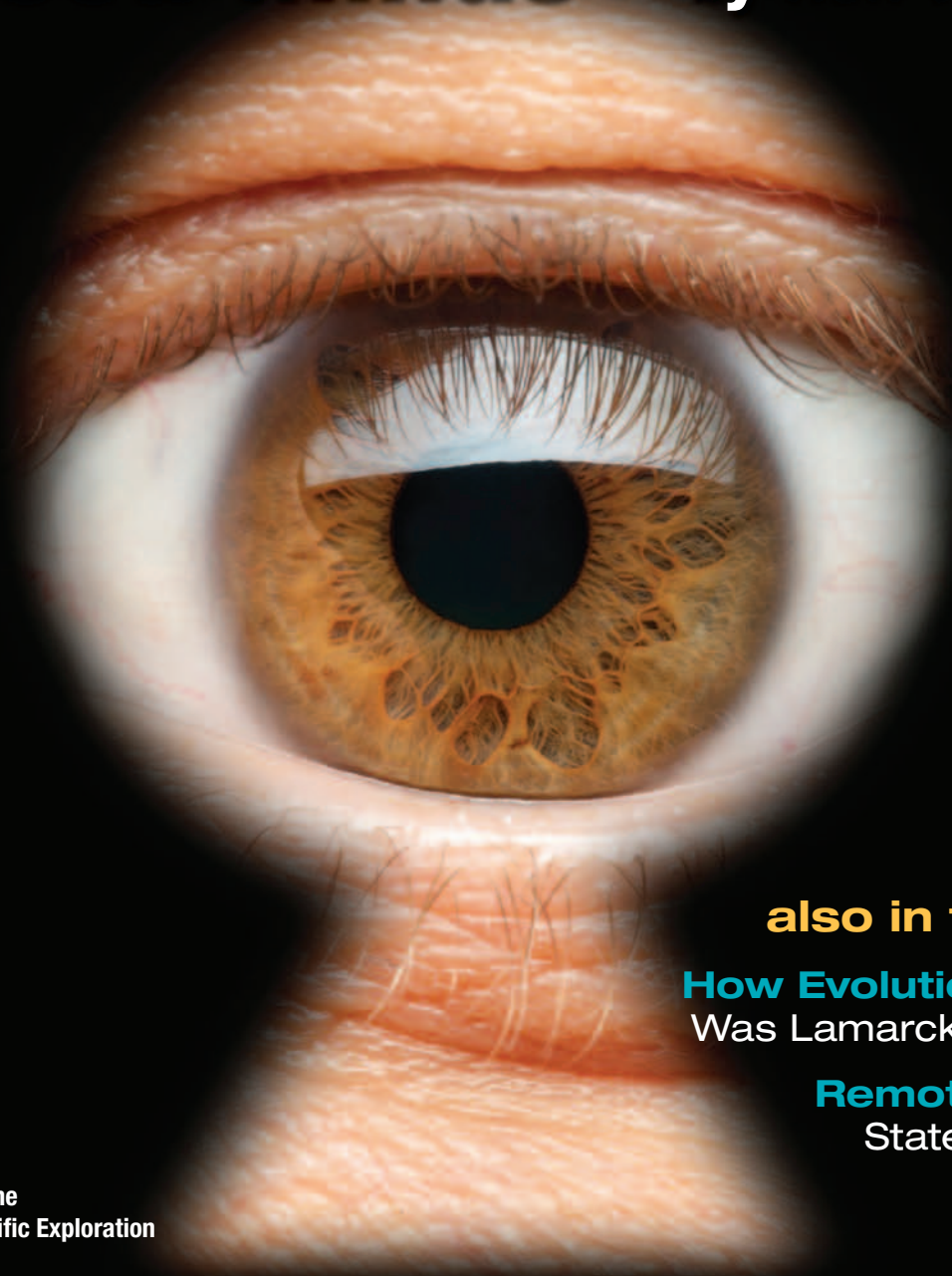
EdgeScience

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Current Research and Insights

Hidden Events and Closed Minds

by Ron Westrum



also in this issue

How Evolution Occurs:
Was Lamarck Also Right?

Remote Viewing:
State of the Field

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Why EdgeScience? Because, contrary to public perception, scientific knowledge is still full of unknowns. What remains to be discovered—what we don't know—very likely dwarfs what we do know. And what we think we know may not be entirely correct or fully understood. Anomalies, which researchers tend to sweep under the rug, should be actively pursued as clues to potential breakthroughs and new directions in science.

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The Society for Scientific Exploration (SSE) is a professional organization of scientists and scholars who study unusual and unexplained phenomena. The primary goal of the Society is to provide a professional forum for presentations, criticism, and debate concerning topics which are for various reasons ignored or studied inadequately within mainstream science. A secondary goal is to promote improved understanding of those factors that unnecessarily limit the scope of scientific inquiry, such as sociological constraints, restrictive world views, hidden theoretical assumptions, and the temptation to convert prevailing theory into prevailing dogma. Topics under investigation cover a wide spectrum. At one end are apparent anomalies in well established disciplines. At the other, we find paradoxical phenomena that belong to no established discipline and therefore may offer the greatest potential for scientific advance and the expansion of human knowledge. The SSE was founded in 1982 and has approximately 800 members in 45 countries worldwide. The Society also publishes the peer-reviewed *Journal of Scientific Exploration*, and holds annual meetings in the U.S. and biennial meetings in Europe. Associate and student memberships are available to the public. To join the Society, or for more information, visit the website at scientificexploration.org.

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Cover image by 4FR/iStockphoto

By Henry H. Bauer

Pseudo-Science in *Science*

The mail brought an unsolicited copy of a magazine—with, of course, the invitation to subscribe to it—that included among other things:

- A three-dimensional representation of HIV that had won first prize in a visualization contest.
- A reminder that a “tiny, perhaps 40-meter-diameter NEA [Near Earth Asteroid] . . . leveled 2000 square kilometers of Siberian forest in 1908.”
- Dwarfism of the Laron type is associated with a genetically controlled failure to respond to human growth hormone. Laron dwarfs rarely contract cancer or diabetes. Therefore researchers are looking into whether interfering with responses to growth hormone could protect healthy adults from cancer and diabetes.

All three of these pieces of information are in some important manner wrong or wrong-headed. Since they masquerade as scientific when actually misleading, they may be properly called “pseudo-science.”

So which magazine seeks new readers by promiscuously sending out unsolicited free copies that contain seriously misleading information?

Science.

The magazine is published by the American Association for the Advancement of Science (AAAS) and is often referred to—in company with the British journal *Nature*—as one of the world’s two leading, premier, or flagship journals reporting on science as well as publishing breakthrough science.

What’s wrong with these three pieces of information?

Three Strikes

The HIV depiction is seriously misleading because it is based on highly disputable inferences, as no pure authentic HIV virions have ever been isolated from purportedly infected individuals.

The cause of the Siberian (Tunguska) event remains in dispute. Perhaps it was an asteroid, but also perhaps a comet, or even a UFO, or something entirely unsuspected. The available evidence simply does not compel a conclusion, and it is distinctly misleading to use the asteroid assertion as an unqualified lead-in to an article about studying asteroids.

And the projected tampering with growth-hormone regulation in healthy people illustrates the pervasive mistake in modern medicine of presuming correlations to be causations.

Science and AAAS are not the only purportedly scientific publication and scientific society to behave like any popular magazine or popular group desperately seeking subscriptions

or members. A couple of decades ago or so, as a member of the American Chemical Society (ACS) I had been irritated when I began to be urged to renew my membership months ahead of the expiration date, with follow-up reminders every few weeks. My complaint to the newly minted MBA in charge of membership matters was met with the response that a study had shown that these tactics lead to a higher rate of renewals. My request for a copy of the study was ignored, and my comment, that this study must have been carried out with a popular magazine and not with members of a scientific society, received no reply.

The point I want to make here is that the pervasive commercialization of modern civilization has influenced scientific activity and medical research and medical practice to the extent that those activities, which used to be clearly distinct and distinguishable from other human activities, are no longer in any essential way different from any other social or political activities. Science began as a vocation populated by idealists, but it has morphed into just another human activity populated by a wide range of human actors whose personalities and behavior are largely indistinguishable from those exhibited by people in high finance, professional politics, or professional sports. The need to balance budgets and make profits now overwhelms every other consideration in academe and in research.

Of course the incidents I’ve described are in themselves trivial anecdotes. But there are plenty of other such happenings or circumstances that flesh out the general picture. That commercial considerations now outweigh others is demonstrated by attitudes toward advertising and conflicts of interest. Until rather recently doctors and hospitals did not solicit customers by advertising, but now they do. Even more telling is the direct advertising of prescription drugs to consumers, the enormity of which may be underscored by the fact that only one other developed country (New Zealand) has followed the United States in allowing this.

In 1995, the Director of the National Institutes of Health, Harold E. Varmus, had also loosened restrictions on outside consulting by NIH staff, citing the need to attract the most prominent people by allowing them to supplement their income, which in government service was said to be so much less than they could make elsewhere—a highly disputable assertion, for traditionally the view in the scientific community had been that scientists entered government service only if they couldn’t get into academe or make good in industry. The resulting scandalous happenings were described in a series of articles by David Willman in the *Los Angeles Times* (“Richard C. Eastman: A federal researcher who defended a client’s lethal drug”; “Ronald N. Germain: A federal lab leader who made \$1.4 million on the side”; “Jeffrey M. Trent: A government accolade from a paid consultant”; “Jeffrey Schlom: A cancer expert who aided studies using a drug wanted by a client”).

Traditionally there was a recognition that “pure” research, the quest for reliable knowledge, could be distinguished from “applied” research, the attempt to devise useful applications from already available reliable knowledge. The changed social environment has erased any such distinction, whereby the ethics and motivations of researchers have shifted from dedication to objective scientific truth toward loyalty to employers and clients.¹ That shift naturally threatens the reliability of what is produced.

Universities have succumbed no less than other institutions to making income and profit their overriding consideration, and no less than other social institutions, academe acts as though conflicts of interest were of no concern. Just a few decades ago, universities restricted severely the amount of consulting faculty were permitted to do and the conditions under which they could profit from businesses established by them in their own specialty. Nowadays it is a free-for-all with essentially no restrictions; indeed most universities encourage entrepreneurship by faculty as a way of bringing more resources under the purported rubric of the university’s own activity.

Beginning in the early part of the 20th century, the American Association of University Professors (AAUP) played a distinguished role in supporting, indeed pioneering, ideals of disinterested scholarship and teaching. As the size of academe exploded in the second half of the 20th century, just as did the research enterprise, attempts were made by existing national unions to recruit academics. AAUP felt the need to compete against them, and its central motivation changed away from concern for disinterested scholarship and teaching as it tried to avoid members joining a union, and in so doing AAUP became itself union-like in seeking secular benefits for all academic workers, including graduate assistants. AAUP lost members wholesale as it abandoned its ideals, and it has never recovered in size or prestige. Eventually an alternative group was formed to uphold traditional academic ideals, the National Association of Scholars (NAS), but it numbers membership only in the thousands. It is unions rather than the NAS that reflect the motivations of most academicians nowadays.

I’ve noted elsewhere² that *Science* and *Nature* try to perform two incompatible functions: to be the first with scientific breakthroughs and also to be the touchstone of reliability. Those functions are incompatible because real science, reliable science, isn’t news.³ Commercial considerations impel both journals, however, to seek the largest possible audience by being *the* place to go to for scientific information, fresh and old both. Thereby they place quantity of subscriptions over quality of readership.

A Broader Context

“Science” has a multitude of meanings and connotations. A common one is the reliable knowledge generated over the centuries by people looking into how the world works. But “science” can also mean the social enterprise of studying how the world works. Historians and sociologists are particularly aware that the social circumstances of scientific activity play a large role in determining how reliable or unreliable is the knowledge produced by that activity.

It has become increasingly evident that changes in the social environment of science in the second half of the 20th century have decreased dramatically the reliance that can be placed safely on official pronouncements about matters of science, especially matters of medical science.⁴ Quantitative expansion of “research” activity has brought lower quality, lower ethical standards, and conflicts of interest^{5–7} as researchers’ motives changed from truth-seeking to wealth-seeking, in collusion with unrealistic political and social demands for quick benefits from money spent. Institutional conflicts of interest are also pervasive, as academe places profit-seeking ahead of traditional ideals in intercollegiate sports and in commercial ventures with industry, drug companies in particular. Perhaps of greatest concern is that informed insiders have published many books and articles describing and decrying the corrupting influences and trends—without these publications having any discernable effect.⁸

REFERENCES

- ¹John Ziman, *Prometheus Bound: Science in a Dynamic Steady State*. Cambridge University Press, 1994.
- ²Henry H. Bauer, *Dogmatism in Science and Medicine: How Dominant Theories Monopolize Research and Stifle the Search for Truth*. McFarland, 2011; <http://henryhbauer.homestead.com/KnowledgeMonopolies.html>.
- ³Henry H. Bauer, “Real science isn’t news,” Science Writing Fellowships Program, Marine Biological Laboratory, Woods Hole (MA), 13 June 1996; <http://henryhbauer.homestead.com/RealSciIsntNews.pdf>.
- ⁴Henry H. Bauer, “Science in the 21st Century: Knowledge Monopolies and Research Cartels,” *Journal of Scientific Exploration* 18 (2004) 643–60—<http://henryhbauer.homestead.com/21stCenturyScience.pdf>; “Suppression of Science Within Science” — <http://www.lewrockwell.com/orig10/bauer1.1.1.html>; “The New World Order in Science” — <http://www.lewrockwell.com/orig10/bauer2.1.1.html>.
- ⁵Derek J. de Solla Price, *Little Science, Big Science . . . And Beyond*. Columbia University Press, 1963/1986 (the 1986 edition contains additional chapters).
- ⁶Sheldon Krinsky, *Science in the Private Interest*. Rowman & Littlefield, 2003.
- ⁷J. Klein, “Hegemony of mediocrity in contemporary sciences, particularly in immunology,” *Lymphology*, 18 (1985) 122–31.
- ⁸Henry H. Bauer, “Critiques Of The Commercialization of Science, Medicine, Academe,” http://henryhbauer.homestead.com/B-Corruption_of_academe.pdf.

HENRY BAUER is Professor Emeritus of Chemistry & Science Studies and Dean Emeritus of Arts & Sciences at Virginia Tech. Bauer has served as the editor of the *Journal of Scientific Exploration*. His latest book is *Dogmatism in Science and Medicine: How Dominant Theories Monopolize Research and Stifle the Search for Truth*, published by McFarland.

Dreams, Souls, and Memories

I read Andrew Paquette's article "Dreams Are Memories," [*EdgeScience* #7] and I thought his conclusions were insightful and scientifically sound, but wrong.



Here is where he is wrong. His conclusions require that there is no soul in order for his conclusions to be right. For if a soul exists his conclusions fall on their face. Have you ever seen something the night before and then seen the events the next day? How can you explain that?

— Chuck Moldenhauer

Andrew Paquette, author of Dreamer: 20 Years of Psychic Dreams and How They Changed My Life, replies:

I think Mr. Moldenhauer misunderstands me. I do not mean "memories" of past events, but memories of experiences of the astral body. These happen every night when we sleep and very much require the existence of a soul, as he suggests. It brings a smile to my face to read this particular critique, since the majority of my book is about the reality of precognitive dreams and other related types of experiences. •

I appreciate Andrew Paquette's critique of the usual, generally accepted views of the origins of dreams but cannot fully accept his own interpretation: he claims that all dreams are memories, that is that they originate in real experiences that the individual had or will experience in the past, present, or future. I agree that his view of academia's mechanistic orientation is essentially correct, but cannot agree with his notions wherein he discards the "soulless" Freudian view of the origin of dreams as involving only repressed desires brought into contemporary life. As a practicing psychiatrist of considerable experience and with a reasonable knowledge of the literature, I know of nothing in either personal clinical psychotherapeutic experience or reading that indicates that psychoanalysis considers humans as "soulless" and that the sources of dreams are only "repressed desires."

Andrew Paquette claims that the existence of only one case of a dream whose content is other than personal will make his case. Here he cites several dreams that he had of major future global events. Yes, I agree that such dreams are possible; the literature on paranormal events is loaded with such pre-cognitive dreams. However, the presence of these kinds of dreams, which apparently to him are evidence of the "soul," does not exclude other possible sources of dreams in other

kinds of dreams, whether they be random processes in the mind or, indeed, repressed desires. Dreams seem more than anything else to be multi-determined, including experiences of the soul as it traverses the realms of carnate and incarnate life.

— Richard Blasband, M.D.

Andrew Paquette replies: My characterization of Freudian ideas on dreams is necessarily circumscribed for space. Everything I've read about Freud, and he crops up often in my reading, indicates that his most commonly cited principle regarding dreams is that they are repressed desires. On the origin of these desires, he frequently, if not always, traces them to some physically explicable event in the subject's past. Although it is not clear whether Freud believed that psi is genuine, his denial of non-physically explicable causes for emotional disturbances and the dreams these might inspire, argues against the idea. Therefore, because the "soul" is a supernatural entity with non-physical properties, it would not seem to fit comfortably with Freud's theories. Whether other psychiatrists feel the same way is a different question, but again, this may well be a matter of individual differences within a consensus. While some psychiatrists may agree that a soul exists, this is not the most common "Freudian" view.

The point about "multi-determined" dreams is a fair one, but loses some of its luster when repressed desires are added back into the subject. As I write in my book, I see our dreams as memories of what are the travels of our mind. Call it an astral body or soul if you like, but the point is, they are real travels and they are related to real things and places. It often happens that a certain amount of mental confusion is present during these experiences, and when trying to remember them. Both of these problems interfere with the integrity of a dream record.

In a dream about a visit to certain person, one may become violently agitated and strike the person, even though he is normally well regarded by the dreamer. This can appear to be a repressed desire based on some deep-seated but never expressed dislike for the person. When I have had the opportunity to track down these dreams, that is, when they are veridically psychic and can be tracked down, I have always found a different explanation. They tend to be out of body experiences (OBEs) where I try to interact with the physically present participants. I become frustrated when they fail to react to me, and increasingly step up my efforts to get their attention. This is hardly a repressed desire, even if it might look like one.

I cannot respond to every possible explanation for these things here, but will add that I agree that some dreams are indeed a reflection of our thinking. However, this is an obfuscating layer that blocks our view of potentially far more interesting material. My book does not deal with non-psi dreams, but I believe that many of the examples I give shed light on all dreams. The reason is that veridicality in the psi dreams has given me a way to concretely measure whether or not a given explanation is correct. Having done this, I have found that these explanations apply to many non-psi dreams as well. •

Robyn A. Lindley

How Evolution Occurs: Was Lamarck Also Right?

The scientific study of evolution has always been carved by emotionally charged debates. It seems that these have now reached a crisis point, forcing a revolution in our thinking about how evolution occurs. We now have an expanding body of scientific evidence suggesting that nature has evolved a number of mechanisms that enable all organisms to evolve as they adapt to their environment. Yet, acceptance of the need to update our ideas on how evolution occurs is still thwarted by powerful emotions that carve out a rather fractured tale of how evolution occurs. While many mainstream scientists continue to pay their respect to Charles Darwin in a series of very public encores, a far more sophisticated view of how evolution occurs is rapidly emerging.

The Outsider

Our story begins in the 1790s, when the renowned French naturalist and zoologist Georges Cuvier produced the first highly ordered classifications of living things based on anatomical differences. In *Philosophie Zoologique* (1809) Jean-Baptiste de Lamarck used these to argue that if all of “nature’s productions” were arranged linearly from the simplest forms to the most complex, then all life could be traced to a single form. Lamarck had decided that once the difficult step of “spontaneous” generation of the first life forms was made, nature then had the means to produce all other forms of life from it. He reasoned that mental functions and physical traits such as the long neck of a giraffe are all acquired characters. He was

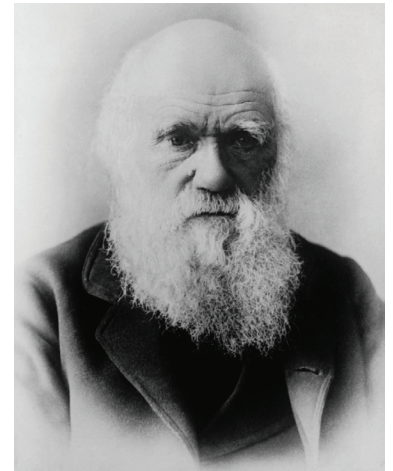


convinced that *organic change followed a change in habit*.

While Cuvier received ovations for his work and ideas on the spontaneous generation of new species, he made it clear that he did not believe in Lamarck’s idea of evolution. From this point on, Cuvier’s rise was mirrored by Lamarck’s scientific demise. Lamarck was greatly saddened by the growing disdain of his peers. At the heart of the bitterness was a battle for scientific status and recognition. His ideas continued to polarize scientists, politicians, and religious leaders. Even when others eventually completed a more detailed fossil record revealing the evolution of life, there were only a few who openly supported Lamarck’s views. It remains an interesting footnote in the history of science that if Lamarck’s views were so totally wrong, it is odd that Sir Charles Lyell and others put such sustained and emotive efforts into refuting him. Yet, even the fear of living in a revolutionary Paris under The Reign of Terror was not enough to convince Lamarck to alter his views. He stood by his convictions until his death in 1829. He died blind, almost friendless, and without enough money to pay for his funeral. His work remains the subject of ridicule.

Enter Darwin

About ten years after Lamarck’s death, English naturalist Charles Darwin set out to explain his ideas on how evolutionary change occurs over millions of years. He had read Lamarck’s theory, as well as *Zoomania* (1794), which was written by his grandfather, Erasmus Darwin, who described similar theories. Darwin accepted Lamarck’s propositions that all life has evolved from a simple form and the concept of the inheritance of acquired traits.



At the end of the very first chapter of *The Origin of Species* Darwin made his views on Lamarckian inheritance apparent: “Changed conditions of life are of the highest importance in causing variability, both by acting directly on the organism, and indirectly by affecting the reproductive system... The greater or less force of inheritance and reversion, determine whether variations shall endure... Some, perhaps a great, effect may be attributed to the increased use or disuse of parts.” (Darwin, 1859)

Darwin was also very puzzled about how natural instincts arose. In fact he saw these and other examples, such as the evolution of the eye, as potentially fatal to a theory based on the forces of natural selection alone.

Like Lamarck, Darwin also faced some serious opposition. Although many Victorian thinkers gradually grew to accept the concept of evolution, Darwin's opponents challenged his work by associating it with that of Lamarck. Cartoons depicting man as descending from gorillas flourished. It also didn't help when the influential Scottish geologist Sir Charles Lyell referred to his work as a modified version of Lamarck's theory. To build support for the acceptance of a less emotive form of evolutionary theory, Darwin did not draw direct attention to Lamarck's work—for instance, making no direct reference to Lamarck in *The Origin of Species*. He does, however, acknowledge Lamarck in his introductory historical sketch as he continued to develop his own ideas about acquired inheritance effects.

To ensure that his own ideas would be accepted, Darwin was also careful to build and maintain a scientifically and politically influential phalanx of supporters—something that Lamarck failed to do. He praised the work of Professor Thomas Huxley. He carefully nurtured his relationships with Alfred Wallace. He also became a close friend and mentor of the renowned geologist Sir Charles Lyell.

While several pre-eminent scientists of the day helped to garner broad support for Darwin's ideas on evolution, none anticipated the development of the neo-Darwinian view later adopted in the twentieth century.

The Rise of the Neo-Darwinians

When the first reports of the rediscovery of Mendel's work were published around 1900, the idea of particulate inheritance based on genes having fixed positions on a chromosome became a foundation concept for twentieth century genetics. Mendelian genetics was also used to put another nail in the coffin of the nineteenth century Lamarckians.

The final blow to nineteenth century ideas on acquired inheritance was delivered by August Weismann when he published his "theory of the germplasm." Weismann's hypothesis was that genetic information in somatic cells (normal body cells) and the reproductive germline cells (ova and sperm) do not mix. Acceptance of the concept was designed to promote the importance of Darwin's idea of evolution based solely on "survival of the fittest." It meant that mainstream scientists had effectively constructed an intellectual chastity belt around the germline genes. By uniting these views, the neo-Darwinian model of how evolution occurs became widely accepted.

The current form of neo-Darwinism based on natural selection, random point mutations, and Weismann's barrier arose sometime after the 1920s. Since then the neo-Darwinians have demanded the total exclusion of Lamarckian acquired inheritance effects.

Yet research has continued to support the Lamarckian view. During the 1890s and the 1900s, Ivan Pavlov, a highly regarded Russian scientist, conducted experiments on the

conditioning responses of dogs. Pavlov reasoned that the dogs learned to "associate" the sound of a bell with the food as a reward, and that the new memories established were somehow inherited. Although reports of Pavlov's experiments were not made available to western scientists until 1927, they became incredibly influential in the field of psychology by introducing the concept of associative learning. Guide dogs, hunting dogs, sniffer dogs, retrievers, and guard dogs are all bred for different behaviors, and based on the principles that Pavlov used in his early experiments.

In the 1920s, Austrian Paul Kammerer conducted experiments in an attempt to prove that Lamarck was right about inherited physical characteristics. Kammerer believed that he could take a variety of "midwife toads" that normally mate on dry land and cause genetic changes in their offspring by raising them in water. The idea was that successive generations would develop so-called nuptial pads, typical of water-dwelling toads. However, his political views were out of favor with the emerging Nazi party, and the British geneticist William Bateson of Cambridge also accused him of scientific fraud. When the test specimens were examined, Bateson claimed that they had been altered using an ink dye. Although Kammerer protested his innocence, he committed suicide soon after.

Another Lamarckian study that received a lot of attention was reported by William McDougall using rats. The first report of McDougall's Lamarckian experiments appeared in the prestigious *British Journal of Psychology* in 1927. Initially McDougall was interested in repeating Pavlov's experiments, using white rats in a small, but well equipped, animal room at Harvard University. McDougall wrote to Pavlov and was surprised to receive a rather odd reply from him, stating that he "no longer held his deductions from his experiments to be valid." However, McDougall continued with his research plan undeterred. He reported that the first generation of rats made an average 165 mistakes on their first run through a maze. After some practice they seemed to learn the route. Once they learned to do this, McDougall bred them and tested their offspring. This next generation of rats made an average of only 20 mistakes on their first run. He wrote that it appeared to him that Lamarckian transmission is "a real process in nature."

A decade later, the USSR's Trofim Lysenko attempted to prove Lamarckian inheritance. In the 1930s, Lysenko promoted the idea that crops could inherit acquired characteristics using vernalization. Vernalization is the process of sowing winter wheat in the spring snow. Lysenko argued that the process of germinating wheat in the snow before planting would lead to greater crop yields. At the time, the need for improved wheat production in Russia was acute. Lysenko had full state backing for his work; numerous scientists who opposed his findings were executed. Even so, the results promised by Lysenko were never delivered and no one else was able to replicate the findings he claimed to have made. Despite this, Lysenko became an unassailable figure in the Soviet regime; he retained extraordinary intellectual authority until President Khrushchev's dismissal in 1964.

During the post-war period and up until Khrushchev's demise, there was an assumed close alliance between one's

political views and one's scientific views. Western biologists who were members of a communist party were expected to support Lysenko's Lamarckian view of how evolution occurred. French geneticist Marcel Prenant was such a party member. But when he initially attempted to steer clear of Lamarck and Lysenko, he was condemned by communists, non-communists, and political commentators alike. In 1949 he felt obliged to state that only "proletarian science" (i.e. that which supported Lysenko) could be right, but even that did not prevent him being expelled from the Communist Party in France.

One of the pioneers of genetic inheritance effects in plants was Barbara McClintock. Her research began in the 1940s and the Lamarckian nature of her conclusions meant that she was also treated as an "outsider" for several decades. However, mainstream scientists finally acknowledged that plant genes have the ability to rapidly adapt, and that the seeds that produced the next generation of plants carried at least some of the environmentally induced genetic changes produced in the somatic cells, which are the progenitor of germ cells. She was awarded the Nobel Prize in Physiology or Medicine in 1983.

The geo-political fear barrier between western and communist countries drew a clear boundary between the type of science that was deemed acceptable—depending on what political system you happened to be a part of. It was during these years that neo-Darwinism truly reached its zenith. It had become the most dominant scientific dogma of the twentieth century.

Molecular evidence for acquired inheritance

In the post Cold War era, the demarcation between what was permissible by mainstream scientists was absolute. The passage of ideas through publication, the awarding of research grants and senior academic positions were all carefully controlled to ensure that the "heretics" (aka "communists") who held Lamarckian views were excluded. Those who transgressed paid the penalty.

This meant that when Australian immunologist Ted Steele and his Canadian colleague Reg Gorzynski reported acquired inheritance effects in the immune system in the late 1970s, the results were met with disbelief (Gorzynski, and Steele, 1980). As further evidence was later independently published showing acquired inheritance effects in the immune system, some fierce and very public debates on the mechanism for acquired inheritance were again ignited. The exchanges were embittered.

But the ideas embodied by Steele and Gorzynski's work were certainly not ignored. A decade later, John Cairns and a group of other scientists at Oxford University published a paper in the prestigious journal *Nature* (Cairns et al, 1988), in which they concluded that they had found evidence suggesting that bacteria could somehow select which mutations to produce. In the experiments, John Cairns and his group took a sample of the bacteria *E. coli* that was unable to consume lactose and placed it in an environment where lactose was the only food source available. They observed that the genetic makeup of the bacteria rapidly changed so that the

original bacteria and the next generation of bacteria were able to use lactose.

When Steele read Cairns' paper, he became outraged that his prior work had been ignored. It was another example of powerful emotions playing a key role in the game of status seeking science. The dispute on Lamarckian inheritance was again ignited globally, and after an acrimonious and very public exchange between Steele and Cairns, a joint letter of compromise was brokered by senior scientists and published in *Nature* (Steele, and Cairns, 1989).

It is another rather curious by-line in the history of science that Cairns later felt obliged to distance himself from the Lamarckian implications of his work. He referred to the processes described as "adaptive mutagenesis" so that the work could be viewed from an ontogenic perspective. In other words, he wanted the results to be viewed from the perspective of changes that occur in an individual cell rather than a population that changes over a much longer period of time (phylogeny).

While no one is now inclined to deny the Lamarckian nature of the acquired inheritance effects of the immune system originally reported by Gorzynski and Steele, the use of the word "Lamarck" is still strictly taboo in most fields.

New molecular evidence for acquired inheritance effects

To the surprise of many, new molecular evidence for Lamarckian effects is now forcing evolutionary biologists to make a sharp u-turn. Thanks to several advances in gene sequencing technologies, there is now extant evidence supporting the idea of acquired inheritance (Lindley, 2010/2011).

The science of epigenetics is providing some of the most compelling evidence yet in support of the idea of acquired inheritance. In the last decade, a number of molecular mechanisms have been discovered that are used to write additional heritable information onto the surface of our genes—without altering the genomic sequence code. Put simply, epigenetic markers are like little footprints left on our genes as we interact with the environment. The markers are considered to be a "soft" form of acquired inheritance, as epigenetic changes are not hard-wired into the genome. Inheritance of this type of modification can occur rapidly and enable a species to adapt to sudden environmental change. There are now many studies showing that what we eat—and even some subtle changes in behavior, such as whether or not a rat mother arches her back when feeding her young—can be inherited by first and subsequent generations of offspring.

The science of epigenetics is also creating a new conundrum for scientists. How was such a sophisticated "library" of genetic alternatives and cross-gene linkages created as we evolved? To answer this question, we need to invoke ways for environmental feedback to result in adaptive mutational changes being introduced into the genome. This implies that there are mechanisms for the creation and integration of new nucleic acid sequence information into the germline.

We know that there are a range of environmentally

directed mutations that are both time dependent and loci specific on the genome. The Lamarckian and Darwinian mechanisms involved in updating immune system genes are well studied. The core mechanism involves a process known as somatic hypermutation (SHM). The SHM is a mechanism responsible for producing the inherited genetic variability in the antibody genes when our immune system is challenged by a foreign pathogen. Recent research provides the first molecular evidence that the same SHM processes are active in a range of cancers arising in a wide range of previously normal somatic cells (Steele and Lindley, 2010). It may well be that the SHM mechanism is an important mechanism that is co-opted in some way to update the DNA of a large number of genes in healthy tissues and germline cells. While the idea that the DNA of normal somatic and germline cells are updated as one interacts with the environment remains controversial among many scientists, there is now an expanding body of research suggesting that these complex molecular processes do play an important role in generating adaptive mutations in a number of genes. Further work is needed to understand the underlying mechanisms.

What does this all mean? It appears that the rapidly expanding areas of research into epigenetic and adaptive mutation phenomena are now the main vehicles driving a grudging acceptance among mainstream scientists that Lamarck may have been right after all.

REFERENCES

- Bowler, Peter (1996). *Charles Darwin: The Man and His Influence*. Cambridge University Press.
- Cairns, J., Overbaugh, J. and Miller S. (1988). "The Origin of Mutants," *Nature*, Vol. 335: 142-145.
- Darwin, C. (1959). *The Origin of Species*. New American Library, Mentor Edition, 1958.
- Darwin, C. (1872/1998). *The Expression of the Emotions in Man and Animals*. New York: Oxford University Press.
- Gillman, Mark (1996). *Envy as a Retarding Force in Science*. Avebury Series in Philosophy.
- Gorczynski, R.M. and E.J. Steele (1980). "Inheritance of acquired immunological tolerance to foreign histocompatibility antigens in mice," *Proc. Natl. Acad. Sci. USA*, 77(5) 2871-5.
- Honeywill, Ross (2008). *Lamarck's Evolution: Two Centuries of Genius and Jealousy*. Pier 9, Murdoch Pty Ltd.
- Lindley, R.A. (2011). "Born to evolve: How mutational and epigenetic changes enable adaptive evolution," *G.I.T. Laboratory Journal Europe*, Vol. 15 20-2.
- Steele, E.J. (1979). *Somatic Selection and Adaptive Evolution: On the Inheritance of Acquired Characters*. Williams and Wallace, Toronto.
- Steele, E.J., R.A. Lindley and R.V. Blanden (1998). *Lamarck's Signature: How retrogenes are changing Darwin's Natural Selection Paradigm*. Perseus Books, Reading, Massachusetts.
- Steele, E.J. and J. Cairns (1989). "Dispute resolved," *Nature*, Vol. 340: 336.
- Steele, E.J. and R.A. Lindley (2011). "Somatic mutation patterns in non-lymphoid cancers resemble the strand biased somatic hypermutation spectra of antibody genes," *DNA Repair*, Vol. 9: 600-3.

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 Ron Westrum

Hidden Events and Closed Minds: The Case of Battered Children

I believe that it is important to understand the realities of what I have called “hidden events.” A hidden event is some unusual phenomenon that is widely experienced, but is counterintuitive. Because it doesn’t make sense, the persons who experience it shrink from treating it as real. They don’t want to believe that they had the experience, and they don’t want to talk about the experience to other people. And so, because they don’t want to talk about it, other people also tend not to want to talk about their experiences. So while many people may have this experience, each believes that his or her experience is unique. This state of “pluralistic ignorance”—the term was coined by social psychologist Floyd Allport—of observers is the first part of the reality of hidden events.

*What we are willing to see is
what we think we can handle.*

A Case Study

To make this concept concrete, let’s take the discovery, by medicine, of what we today call “the battered child syndrome.” While other examples might be brought forward, this example has the advantage that we now know the full story, whereas with other hidden events, the story is yet to be completed. One key to understanding why the battered child syndrome remained hidden for so long is that admitting it was simply too scary.

In 1950 the medical world did not have a “battered child” concept. Rather, when physicians observed children with bruises or bone breaks, they were likely to seek routine causes for these injuries, such as falls or other kinds of accidents. They did not want to see what was literally in front of them. So if children were injured, doctors did not look suspiciously at the parents or other caretakers. Battered children were a hidden event because nobody wanted to think that parents would intentionally hurt or injure their own children, unless the parents were alcoholics or psychotics.

So pluralistic ignorance about battered children was in part a flinching from observation. Remember what Sherlock Holmes tells us: it is easy to see but hard to observe! Observation means understanding. With battered children, we did not want to understand. But also when we do not want to know, we label things incorrectly. We cannot talk about battered children unless we first observe them. So when we

mislabel, we cannot check our understanding with others, because there is nothing to talk about. Pluralistic ignorance, then, is supported by lack of communication about what we *could* observe, but choose not to. So the initial stage of hidden events is often uncorrelated sightings.

Even by 1950, however, there had been an article about this phenomenon by a pediatrician and radiologist named John Caffey. A pioneer in pediatric radiology, Caffey would later literally “write the book” on pediatric radiology, a book destined to go through many editions. In 1946 Caffey had published an article on “Multiple Fractures in the long bones of children suffering from chronic subdural hematomas.” (A hematoma is a blood blister, and a subdural hematoma is one on the surface of the skull.) By that time Caffey had become aware, through studies of breech births by S.T. Snedecor, that subdural hematomas were caused by trauma. So if kids had hematomas, they must have suffered trauma. But in the mid-1940s Caffey wanted to see pediatric radiology succeed, so this is one reason that he played down, in print, a suspicion that he had had for many years—that this syndrome linking bone fractures with hematomas in children might be due to hostile parents. So his article also downplayed two other features of interest: the lack of relevant medical “history” for the injuries and the apparent lack of affection for the children suffering these injuries.

Yet Caffey was aware of the probabilities, and two of his radiology residents, Frederic Silverman and Bertram Girdany, were convinced that parents or other caretakers of the children had caused the injuries. But Caffey did not put in print what for many years he imparted in person; both of the residents later became strong champions of the battered child idea. Then in 1953 Silverman would write a paper himself, “The Roentgen manifestations of unrecognized skeletal trauma in infants,” and publish it in the *American Journal of Roentgenology*. The paper created quite a stir in X-ray circles. Even so, Silverman’s comment that “individuals responsible for the care of infants and children...may deliberately injure the child and deny it,” is pursued only in one of the three cases studies of the paper, and the thought barely appears in the conclusions at the end.

The work of Caffey and Silverman received support from a pediatrician/radiologist team, who in 1955 published an article in the *Journal of American Medical Association (JAMA)*. In their article Paul Wooley, Jr., and William A. Evans considered 25 infants diagnosed with subdural hematoma. They separated the skeletal injuries of the children into three groups:

(1) those caused by random accidents, (2) those caused by an “injury-prone environment” (neglect), and (3) intentionally inflicted injuries. Wooley and Evans thus laid out the situation clearly, but others were slow to take up the cause. Some physicians suggested “bone fragility” as an explanation.

A Class of Events

Meanwhile, other forces were in play. A social worker named Elizabeth Elmer was working at Children’s Hospital in Pittsburgh in 1957 and was told about an interesting case up on the Infant Floor. The case involved a seriously injured five-month-old baby. The parents did not wish to discuss the case and had left the hospital quickly. When she went to the infant floor and described the case, the nurse brought out the cards on six infants. Later, Dr. Elmer would write: “If I had thought in such terms at all, I would have considered the original case unique, but in a few minutes’ revealing talk with the nurse, abuse had become a class of events with ramifications far beyond one infant, one family.”

So if there is more than one event, then we can name the class of events. But as soon as we do, the reality and the explanation are likely to be contested. In the case of the battered child syndrome, some doctors doubted what others were gradually coming to believe. And then, how many of these events were there? Were there dozens, hundreds, or thousands of battered children? Today we know the answer is more like “millions.” But there was, for a while, a lot of controversy.

In the early stages, the estimates of the prevalence of a hidden event may be off, not by a simple factor, but by orders of magnitude.

Furthermore, the experts may not know anything about it. But experts’ ignorance may lead to them pooh-poohing the anomaly. If you let it be known that you think the anomaly is impossible, people will not come to you with reports. So arrogance begets ignorance.

Resolution

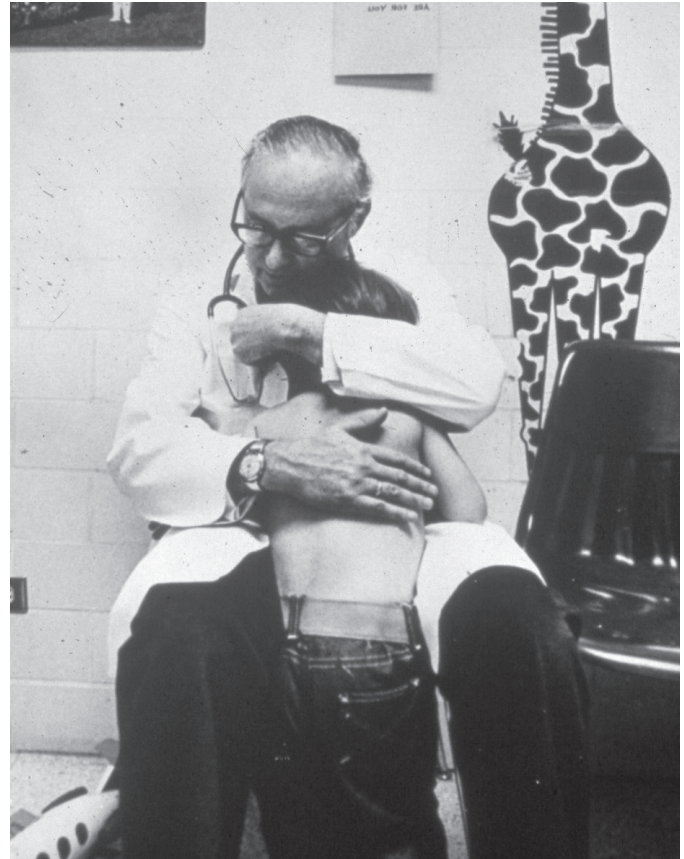
Previously I have suggested that information about anomalies goes through three phases. These phases are

One: Uncorrelated observations

Two: Controversy

Three: Final acceptance OR dismissal

For some anomalies there is a resolution, an absolute acceptance or absolute denial. For the battered child syndrome, the resolution took place in Chicago in 1961. C. Henry Kempe, head of pediatrics at the University of Colorado, had resolved he was going to blow the whole thing wide open. His chance came in 1961 when he was chairman of the American Academy



Henry Kempe, courtesy University of Colorado Health Sciences Center

of Pediatrics. The chairman got to choose one panel on any subject for the annual meeting. Kempe decided he would talk about child abuse. He set aside the grand ballroom at the Palmer House and invited an interdisciplinary panel. The hotel ballroom held 1,000 people. And then, in 1962 Kempe and some colleagues published an article in the *JAMA* on the “battered child syndrome.”

How did Kempe become the person to “blow it wide open”? Kempe was a highly creative physician, and he had developed an interdisciplinary team consisting of a pediatrician, a radiologist, and a social worker. Pediatricians were the official “keepers of the problem.” But social workers could reach beyond the hospital, which the physicians typically could not do. This allowed the problem to be seen, because now something could be done about it. The structural change was needed to get the perceptual change.

But Kempe also needed to connect the anomaly to things known and accepted. The purpose of the interdisciplinary panel used by Kempe was to connect the battered child syndrome to the professional specialties that would have to deal with it.

An anomaly will seldom be accepted based simply on physical evidence. It must be connected up to what we already know.

Certainly we know this to have been true as well with the acceptance by science of the existence of meteorites, “the stones that fall from the sky,” at the beginning of the 19th century. It was not enough simply to collect the testimonies, or even the stones. A plausible hypothesis had to be developed. It was. The falling stones were believed to come from the moon! Obviously the hypothesis doesn’t have to be correct, just plausible to people at the time.*

With the battered child syndrome, again there had to be an explanation. While Kempe and company’s 1962 *JAMA* article was open-minded, most physicians initially pinned the abuse on psychotics and alcohol abusers. Sadly we know now that child abuse is much more widespread, and that many abusers are often neither psychotic nor inebriated. But at the time the “psychotics and alcohol abusers” were easier to accept than the actual reality.

And finally we can note that the *JAMA* article of 1962 included a primitive survey of hospitals and police departments, suggesting hundreds of cases. As I have noted, even “thousands” would have been many orders of magnitude too low.

Lessons

And so the lessons we learn about hidden events from this case include the following:

One: Hidden events may be far more widespread than initial impressions would suggest.

Two: People are often unwilling to see or to report on what doesn’t make sense, especially if they do not know about others’ experiences.

Three: Experts are often as ignorant as everyone else, but do not know they are ignorant.

Four: When the dynamics of the reporting system change, the prevalence of the anomaly may seem very different.

Five: It is important to connect the dots before one can get resolution, and to connect up, as it were, the anomaly to knowledge already existent.

*Ron Westrum, “Science and Social Intelligence about Anomalies: The Case of Meteorites,” *Social Studies of Science*, Vol. 8, 1978, pp. 461–473. And Ron Westrum, “Social Intelligence about Anomalies: Implications for Scientific Research and Social Policy,” *Knowledge, Creation, Diffusion, and Utilization*, Vol. 3 #3, March 1982, pp. 381–400.

More Hidden Events

A final matter to think about would be other kinds of anomalous events that lie “under the radar.” This was true, for instance, regarding the ozone hole, whose existence the British kept secret for many years, out of concern that the Americans would think badly of them for seeing “something that wasn’t there.” The American Nimbus 7 satellite in particular should have shown such a hole, if it existed. It was only later that the Americans recalibrated their Nimbus 7 computer program and so were able to process the data that clearly showed the ozone hole. Today I believe that a major hidden event is UFO abductions, apparently widespread, but also highly counterintuitive. So, of course, there can’t be UFO abductions, can there?

REFERENCES

- John Caffey, “Multiple Fractures in the Long Bones of Infants Suffering from Chronic Subdural Hematoma,” *American Journal Of Roentgenology*, Vol. 56, No. 2, (August 1946), pp. 163–173.
- Frederic N. Silverman, “The Roentgen Manifestations of Unrecognized Skeletal Trauma in Infants,” *American Journal Of Roentgenology*, Vol. 69, No. 3 (March 1953), pp. 413–427.
- Frederic N. Silverman, “Unrecognized Trauma, the Battered Child Syndrome, and the Syndrome of Ambrose Tardieu,” (Rigler Lecture), *Radiology*, Vol. 194 (1972), pp. 337–353.
- Paul V. Wooley Jr. and William A. Evans, “Significance of Skeletal Lesions in Infants Resembling those of Traumatic Origin,” *Journal Of The American Medical Association*, Vol. 158, No. 7 (18 June 1955), pp. 539–543.
- C. Henry Kempe, Frederic N. Silverman, Brandt F. Steele, William Droegenmueller, and Henry K. Silver, “The Battered Child Syndrome,” *Journal Of The American Medical Association*, Vol. 181, No. 1 (7 July 1962), pp. 17–24.

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Paul H. Smith

Remote Viewing: State of the Field

When Ingo Swann introduced the basic remote viewing protocol* and gave it its name in December 1971, he thought he was just creating a new way of doing a parapsychology experiment. He had no idea that he was not just suggesting a new research paradigm, but in the process planting the seeds for what would become a new community and, even, a new industry that actually sought to *apply* our extra-sensory perceptual capabilities.

Forty years later remote viewing is still on the roller coaster ride that has taken it through the doors of science, the military and intelligence establishments, and now into the homes and businesses of fascinated amateurs worldwide. It's hard to come up with solid numbers, but there are perhaps several dozen individuals, businesses, and investors who in some fashion derive income from remote viewing. Thousands have taken training either directly from one or another of the original members of the government's Star Gate remote viewing team or from distance-learning products offered by some of these same folks. Still more have learned their remote viewing chops from a second generation of instructors. Add to that the millions who have heard remote viewing discussed in the media, and it's easy to see why the field continues to expand and find new converts.

Since becoming a public commodity, remote viewing has been applied in various countries around the world to finding missing persons; for investment, business intelligence, and archaeological research; and in psychotherapy and medical practice (on a limited basis); while still continuing to be solicited, on occasion, by the military or intelligence services. But remote viewing has its challenges, and the science aspect has been largely neglected due to limited funding and misunderstanding of the field by its critics.

Breakthrough

Though it made its publication debut in the journal *Nature* in 1974, remote viewing was put on the map by Hal Puthoff and Russell Targ's seminal 1976 article "A Perceptual Channel for Information Transfer over Kilometer Distances," in the journal *Proceedings of the IEEE*. Parapsychologists and skeptics alike greeted the paper excitedly, and over the following several years numerous studies reporting replications joined other articles and commentaries disputing the results and the methodology.

For any of the remote viewing work to see the light of day required that the SRI team work out an agreement with the Central Intelligence Agency, for whom they were performing the work. SRI argued that, given the need for outside contractors and research subjects, keeping secret the fact that SRI was doing parapsychology research was impossible. Ironically, getting the CIA to agree to disclose secretly held data turned out to be the easy part. The real challenge proved to be getting the article published.

Both Targ and Puthoff were fairly well known by the community of the Institute of Electrical and Electronics Engineers (IEEE) and had published on laser physics and applied engineering principles in its journals. The journal's editor, Robert Lucky (then Director of Communications for Bell Labs), was automatically cautious about the subject matter of the proposed article. But he agreed to farm out copies for peer review. Two of the reviews came back recommending publication, but the third gave a thumbs down with the comment, "This is the sort of thing I wouldn't believe, even if it were true."

Lucky accepted Puthoff and Targ's offer to come to New Jersey from California to brief an assembly of Bell engineers on their research. After the briefing the engineers were clearly enthusiastic, busily debating amongst themselves what mechanism might account for the impressive results the SRI scientists had shown them. Lucky was still hesitant, so a deal was struck. He would run his own in-house experiment, and if it failed the paper would be rejected. If, on the other hand, it succeeded, then the paper would be published in the *Proceedings*. Lucky's experiment, done according to the SRI protocol but with no other assistance from the SRI scientists, was successful and the Puthoff/Targ paper appeared in the *Proceedings* of March 1976.

This vignette of how the "Perceptual Channel" paper came to be published illustrates both the ambivalence and the enthusiasm that met this newly emerged parapsychology research protocol, and which still confronts remote viewing today.

The Beginning

For several years after the appearance of the *Proceedings* article, the remote viewing protocol underwent notable refinement and expansion, from tweaking blind judging methodologies, to improving quantitative evaluation techniques, to developing precognitive remote viewing protocols, and so on. The majority of studies attempting either exact or conceptual replications were successful, and many of the attempts that were unsuccessful strayed noticeably from the successful original protocol.

* The "remote viewing" protocol essentially involves the mental perception and description of a distant or hidden location under laboratory-controlled conditions.

One benchmark survey of remote viewing research, published by Targ and Keith Harary in 1984, reported 28 published studies, of which 15 produced statistically significant results (when just over one study should have been significant by chance alone). Of the 13 “failed” replications, my analysis showed that the majority of these actually strayed considerably from the original protocol, and so it is perhaps unsurprising that they produced non-significant results. Maybe just as interesting was that this review also uncovered 18 unpublished remote viewing studies, eight of them reporting statistically significant results (which has something to say against the hoary “file-drawer” objection to successful remote viewing statistics). Despite criticisms, it seemed that the attention the remote viewing protocol received was relatively unprecedented in the parapsychology field.

Why? By the time Ingo Swann formalized the basic remote viewing protocol, some four decades had passed since J.B. Rhine had created, to all intents and purposes, the field of scientific parapsychology. In that time, relatively few new ESP research paradigms had been developed. Other than Montague Ullman’s new dream research lab at Maimonides Medical Center and Chuck Honorton’s just-emerging Ganzfeld work—two research paradigms that lacked the drama of being played out publicly in the pages of journals such as *Nature* and the *IEEE*—the field was sparsely populated.

Compared to the card guessing experiments and attempts to influence the roll of a dice that were the norm in parapsychology at the time, remote viewing promised a richer, more exciting environment for researcher and research subject alike. Instead of long runs of bare hits or misses, this extra-sensory perceptual process produced more detailed verbal or written reports, or even easily elaborated sketches, drawings, or even three-dimensional models.

Important as well was that the remote viewing task involved a vast array of possible targets—in principle, anything in the world could be the focus of a remote viewing

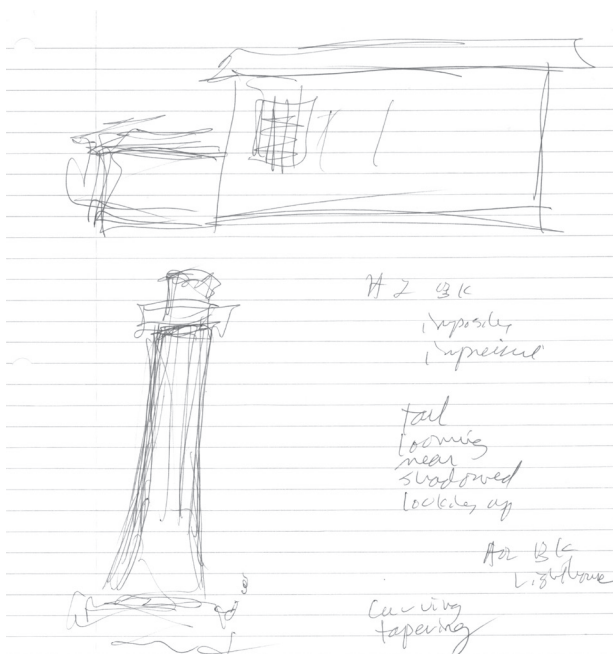
attempt. As a consequence, a number of advantages accrued to the remote viewing paradigm that were not present in earlier forms of parapsychology research. Among these were, for example, the more interesting experience for both subjects and experimenters (a “boredom effect” was much less likely to occur); objectively demonstrable results; adaptability of protocols to principles of human psychology; and obvious potential for real-world practical applications.

It also helped that remote viewing experiments were relatively easy to do. While one could employ Faraday cages, brain monitoring equipment, and sophisticated randomizing devices to augment remote viewing experiments, none of this was necessary. Experiments could be easily performed with materials available around the average household, so long as suitable care was given to maintaining a clean scientific protocol. This meant that even modestly funded institutes and scientists could undertake their own exploration of the phenomenon.

There were, to be sure, downsides to the new research paradigm: even if done double blind, subjectivity in the process of judging the results seemed unavoidable. As a consequence, objectifying and quantifying the results was more problematic than it was for Rhine’s earlier experiments.

Classified Applications

From the mid-1980s through the early 1990s, the bulk of remote viewing, both research and applications, was taking place *sub rosa*, “behind the Green Door” of the classified military world. To be sure, remote viewing applications work was still being explored in the civilian world by Stephan Schwartz and others, and the Princeton Engineering Anomalies Research lab was conscientiously pursuing its “precognitive remote viewing protocol” under the leadership of Robert Jahn and Brenda Dunne.



Double-blind remote viewing session done at the 2003 remote viewing conference. Target was randomly selected just prior to session start.

But the fact that much of the most fruitful remote viewing research remained unavailable to anyone outside of the military security system perhaps hampered the fertile exchange of ideas that feeds robust development of a research program most of all. The government involvement was a true two-edged sword for remote viewing. On the one hand, the enforced secrecy made the exchange of information, ideas, and results difficult. On the other hand, without government involvement, it is questionable whether any of this research would ever have even been undertaken.

How problematic this could be was illustrated by the 1988 investigation and report, “Enhancing Human Performance,” commissioned by the Army Research Institute and performed by the National Research Council (NRC). With the participation and contributions of noted skeptics Ray Hyman and James Alcock, the NRC presumed to find little value in the body of remote viewing research. Unavailable for review was the large body of classified research that had been conducted to that point, of which the NRC was purposely kept unwitting. Would this have made a difference to investigators who were determined to find no smoking gun? It’s hard to say, but not having it accessible certainly didn’t help.

Secrets Revealed

All this changed, of course, with the now well-known public revelation in late 1995 that for 23 years the U.S. intelligence and military establishments had fostered a relatively vigorous parapsychological research and applications program. Remote viewing became the focus of a virtual firestorm of public interest. Once again, though, the old ambivalence was very much in evidence. That it was the *government* that had pursued ESP-related objectives, and that the program had been going on for nearly a quarter-century, served on the one hand to confirm in many people’s minds that there was something to the phenomenon after all.

And it didn’t hurt that over time new confirmation of remote viewing’s efficacy began to trickle out. For example, one 1989 survey by Ed May and colleagues sequestered among the 90,000 pages archived from the now-declassified CIA’s Star Gate program is a comprehensive review ordered by the Defense Intelligence Agency (as required by congressional oversight) reporting 25,449 remote viewing trials in 157 experiments performed by 227 different individual participants yielding successful results (to the tune of $p = 6.12 \times 10^{-14}$).

Unfortunately, little new research from professional parapsychology developed from the 1995 revelation, with a few exceptions. Among these few exceptions were James Spottiswoode’s work on local sidereal time, Jahn and Dunne’s continued exploration of their precognitive remote perception model, and a smattering of publications from the remnants of the Star Gate research team headed by Ed May.

Where the post-1995 interest in remote viewing did grow was among the lay population. For several years there was almost a feverish excitement on the subject of remote viewing, reflected by books, videotapes, and many late-night radio interviews with various remote viewing “celebrities.” People flocked to practitioners to take courses of varying quality,

duration, and price. The upshot was that remote viewing enjoyed a popular vogue that was, unfortunately, not echoed in scientific or academic progress.

It appears that the *kind* of notoriety that accompanied remote viewing’s public emergence played a role in its scientific marginalization. The circus-like atmosphere generated—which to some degree still continues today—by certain of its early proponents no doubt poisoned the well for researchers who might otherwise have been willing to look into the phenomenon.

Another possible drag on remote viewing’s development was competition from other emergent ESP research paradigms. But perhaps overshadowing even this was that remote viewing reemerged at a time when support for parapsychological research was starting a long decline. Funding for such research was growing harder to come by, and the presence of academicians in American research institutions who were interested in engaging in it was decreasing. Those who remained were under increasing pressure from their more orthodox colleagues to abandon the work. We have reached the point today where funding from public and government sources has dried up, and even that from private sources is in short supply.

Remote Viewing Today

But the picture isn’t all gloomy—some relief has come from a perhaps unexpected source. Mostly embracing the conceptually simple Associative Remote Viewing (ARV) process,* amateur researchers have been making progress in a variety of new experiments involving remote viewing. Thanks to some of the characteristics of ARV (easily executed protocols, precognitive targeting, and so on), even sufficiently conscientious non-specialists can generate credible results. One feature of ARV that serves as both an incentive and as a control to help insure proper execution of the experimental protocol is that it can effectively be used to make money (and when the protocol is not properly executed money is lost—hence the control). As reported at the 2010 conference of the Society for Scientific Exploration, an undergraduate class used ARV to correctly predict seven stock market trades in a row, yielding a \$16,000 profit to an investor who had joined in support of the project. Many of these experiments are of sufficient quality to be publishable in appropriate peer-reviewed journals.

I’m convinced we are moving into a new era of remote viewing sobriety, which I hope will lead to scientific progress as well. There are many unexplored research avenues into foundational remote viewing principles that would be relatively inexpensive, at least from a funding perspective. These would help increase our knowledge of human consciousness in

* In associative remote viewing the viewer attempts to describe a target, such as a cup or a pair of eyeglasses, that has been linked, without knowledge of the viewer, to the outcome of a future event, such as a football game. What the viewer is describing is the feedback of the winning target that is presented to him/her after the event.

general and remote viewing functioning in particular, as well as expand our understanding of how to employ remote viewing in practical applications.

As one example of a conceptually simple, yet relatively inexpensive remote viewing-related experiment I offer a pilot study I helped organize in 2008. The goal of the study was to determine whether a correlation could be shown between certain stages of controlled remote viewing sessions and excursions from randomness of a Psyleron random event generator. Six remote viewers under carefully controlled conditions performed 24 sessions. Statistical analysis is still ongoing, but a clear correlation is emerging between periods of non-randomness displayed by the REG and sensory-experiential portions of the remote viewing sessions. (For a full account see <http://www.crvreg.org/>.)

This experiment was supported by the Gabrielle Pettingell research fund administered by the International Remote Viewing Association, and cost less than \$2,000. (This low cost was facilitated by many hours of donated time on the part of the various participants.) Much of the cash for the Pettingell fund was generated through a stream of micro-donations I dubbed “guerilla funding.”

The International Remote Viewing Association (IRVA), founded in 1999, has suffered all the vicissitudes typical of volunteer non-profit organizations. But thanks to the foresight and leadership of its current president, John Stahler, IRVA has increased in both organizational effectiveness and fiscal soundness to the point that the organization was this year able, in conjunction with a new French foundation, IRIS Intuition Consulting, to sponsor a competition for innovative remote viewing research projects. Dubbed the “Warcollier Prize” after Rene Warcollier, who conducted pioneering research in the 1930s and 1940s that laid the groundwork for modern remote viewing, the competition this year offered a \$2,000 prize to defray expenses in carrying out the experiment from the winning proposal. (See Warcollier prize competition at <http://www.irva.org/news/warcollier.html>.)

Most of the problems and the promise of remote viewing are still with us. There is still too much re-inventing the wheel—amateur researchers and enthusiasts leaping into projects without sufficiently studying what had been done before. Contributing to this problem is the fact that much of the trove of remote viewing lessons-learned languish in obscurity in relatively inaccessible corners of the bibliographic morgues of academia. Another IRVA initiative hopes to remedy this, by attempting to capture all the published remote viewing research literature, with links to any article now digitally available on the internet. (The bibliography is available at <http://www.irva.org/library/>.)

There are many other issues great and small affecting the development of remote viewing, from ethical concerns, to how best to represent and promote it to the public, to funding various research and applications initiatives, to the question whether training improves remote viewing performance, to the head-butting that goes on between factions in any emerging field. For me, all this contributes to the interest and excitement—and provides ample evidence that remote viewing is indeed a living, breathing field with a promising future.

REFERENCES

- Druckman, D. and Swets, J. A. eds. (1988). *Enhancing Human Performance: Issues, Theories and Techniques*. National Academy Press, Washington, D.C.
- Marks, D.F. & Kammann, R. (1980). *The Psychology of the Psychic*. Buffalo, New York: Prometheus Books
- May, E.; Utts, J. and Trask, V., et al (1989). Review of the Psychoenergetic Research Conducted at SRI International 1973–1988. SRI International (declassified).
- Puthoff, Harold E. (2011). Telephone Interview, 7/12/2011.
- Puthoff, H. E. and Targ, R. (1976). “A Perceptual Channel for Information Transfer over Kilometer Distances: Historical perspective and recent research.” *Proceedings of the IEEE*, 64(3), 329–354.
- Smith, Christopher C., Darrell Laham, and Garret Modell. (2010). “Stock Market Prediction Using Associative Remote Viewing with Untrained Viewers.” 29th Annual Meeting of the Society for Scientific Exploration, 11 June 2010.
- Smith, Paul H. (2009a). Is Physicalism “Really” True?: An Empirical Argument Against the Universal Construal of Physicalism. Austin: The University of Texas. December 2009.
- Smith, Paul H. (2009b). “Associative Remote Viewing: The Next Candidate for the Paradigmatic Psi Experiment?” 52nd Annual Parapsychological Association Convention, 8 August 2009.
- Smith, Paul H. (2008). “Guerilla Funding.” The International Remote Viewing Association. Accessible at <http://www.irva.org/research/funding.html>
- Smith, Paul H. (2005). *Reading the Enemy’s Mind: Inside Star Gate, America’s Psychic Espionage Program*. New York: Tom Doherty Associates.
- Targ, R. and Harary, K. (1984). *The Mind Race: Understanding and Using Psychic Abilities*. New York: Villard Books.

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Mass Bird Deaths

Stories of mass bird death are not uncommon, but the spate of events that occurred in January of 2011 had more than a few people, scientists among them, scratching their heads and wondering just what was going on. The scenes, widely reported by newspaper and television, were evocative of a Hitchcock movie.

On New Years Day, residents of Beebe, Arkansas, awoke to find about 5,000 dead red-winged blackbirds scattered across their homes, cars, and lawns in a 1.5-square-mile area.

On the 3rd, just 300 miles away from Beebe, another 500 blackbirds, starlings, and sparrows were found littering a highway in Labarre, Louisiana

On the 5th, nearly a hundred jackdaws were found dead on a street in Falkoping, Sweden.

On the 7th, more than a thousand turtle doves fell like “little Christmas balls” from the sky in the northwestern Italian city of Faenza.

On the 8th, more than 100 dead birds were found clustered together just off Highway 101 in Geyserville, California.

On the 13th, some 300 grackles, a common blackbird, were found dead along I-65 in Alabama.

Officials insisted that the incidents were coincidental and unrelated. That made for explanations that were as imaginative as they were varied. The Arkansas birds were found to have died from trauma, primarily to the breast tissue, with blood clots in the body cavity and internal bleeding. Officials blamed New Year’s Eve fireworks for startling the birds and forcing them to plunge to their death. An ornithologist ventured that the birds had been asleep, roosting in a single tree, when a “washing machine-type thunderstorm” sucked them up into the air, disoriented them, then fatally soaked and chilled them. The dead birds found in Louisiana were thought to have flown into a powerline, causing their broken beaks and broken backs, but no one could explain what it was that could have caused them to fly into the powerlines in the first place. A veterinarian suspects that the birds in Sweden had been disoriented by fireworks and then run over by a car. Likewise, the Alabama birds were thought to have died in a collision with a large object like tractor trailer rig. The turtle doves in Italy, on the other hand, had a strange blue splotch on their beaks, which scientists said might indicate poisoning or lack of oxygen.

Aside from these mostly off the cuff remarks, no one puts too much effort into trying to explain mass bird deaths, which are viewed as a fringe phenomenon at best. “Despite their lowly status,” comments William Corliss in *Science Frontiers*, “bird falls can be very impressive, and the causes of death enigmatic.” Corliss points out that the record for mass bird falls occurred on March 13–14, 1904, in southwestern Minnesota



Lapland Longspur

Photo Credit: U.S. Fish and Wildlife Service

and Northwestern Iowa. A worker for the Minnesota Natural History Museum counted 750,000 dead birds on just two lakes. The causes of death included crushed skulls, broken bones, and extensive internal hemorrhages. And very one of them was a Lapland Longspur.

“Science is struggling to explain these things,” Nick Nuttall, spokesman of Nairobi-based U.N. Environment Program, told Reuters. “These are examples of the surprises that nature can still bring. More research is needed.”

SOURCES

- “First Falling Birds, Now Dead Fish in Arkansas,” *CBS News*, January 4, 2011
- Josh Levs, “Officials probe ‘unusual’ mass deaths of birds in two states,” *CNN*, January 4, 2011
- Matt Gutman and Jessica Hopper, “Bird Death Mystery: More Dying Birds Fall From Louisiana Sky,” *ABC News*, January 5, 2011
- “Mass bird deaths rare, not apocalyptic: experts,” *Reuters*, January 6, 2011
- Travis Walter Donovan, “300 Dead Birds Fall From Sky In Alabama,” *Huffington Post*, January 13, 2011
- William Corliss, “Mass Death of Birds in Flight,” *Science Frontiers*, No. 194, May–June 2011

Road to Mystery

Travel brochures on Easter Island all feature gorgeous photographs of the giant, grim faced stone statues staring out to sea. There are nearly 900 of these Moai, as the stone heads are known, on the island’s 63 square miles. Nearly half of them remain at the quarry near Rano Raraku, the island’s major volcano crater, where Easter islanders hewed these massive statues, the tallest of them being 33 feet high and weighing 75 tonnes. Once roughed out, the statues are thought to have been transported using manpower along the island’s 20 miles of rarely publicized roads to their designated sites and erected on stone



Photo Credit: Artemio Urbina

platforms along the coast. The statues, erected between 1200 and 1500 AD, appear to be yet another example of the powerful human urge to construct large ceremonial structures like Stonehenge and the Pyramids, though what the statues are looking out for, other than perhaps visitors from their homeland far to the west in central Polynesia, is not known.

But those rather unusual “roads” suggest that the standard accepted scenario doesn’t tell the whole story. To begin with the roads are not flat, as would have been practical for dragging heavy stone statues. In cross section, the roads are V-shaped, even when they had to be carved through solid rock. In some places, the roads are flanked by curbstones and pits, whose purpose is unknown. And along the roads are found recumbent statues, which never made it to the coast, if indeed that was their destination, interrupted perhaps by some environmental event or some religious or political revolution.

But one thing is clear: some of the stone heads were never destined to be erected *along the sea coast*. Using ground-penetrating radar, Sue Hamilton and Colin Richards of

University College London discovered that near some of the toppled roadside statuary are stone platforms that have been covered by soil. It appears then that some of the giant stone heads were supposed to have been erected *along the roads*, adding mystery to a mystery. Of course, no one depends on travel brochures for the whole story.

SOURCES:

“The Secrets of Easter Island’s Fallen Idols,” *World Archeology*, Issue 42, 2010.

William Corliss, “Easter Island’s Sacred Roads,” *Science Frontiers*, Nov.–Dec. 2010.

Lauren Schenkman, “Scared Roads,” *Science*, May, 21, 2010

Things We Thought We Knew for Certain Department

It now appears that the call to limit our salt intake has little basis in science. An article by Melinda Wenner Moyer entitled “It’s Time to End the War on Salt,” which was published in *Scientific American* on July 8, 2011, reports: “This week a meta-analysis of seven studies involving a total of 6,250 subjects in the *American Journal of Hypertension* found no strong evidence that cutting salt intake reduces the risk for heart attacks, strokes or death in people with normal or high blood pressure. In May European researchers publishing in the *Journal of the American Medical Association* reported that the less sodium that study subjects excreted in their urine—an excellent measure of prior consumption—the *greater* their risk was of dying from heart disease. These findings call into question the common wisdom that excess salt is bad for you, but the evidence linking salt to heart disease has always been tenuous.” Always? Really?

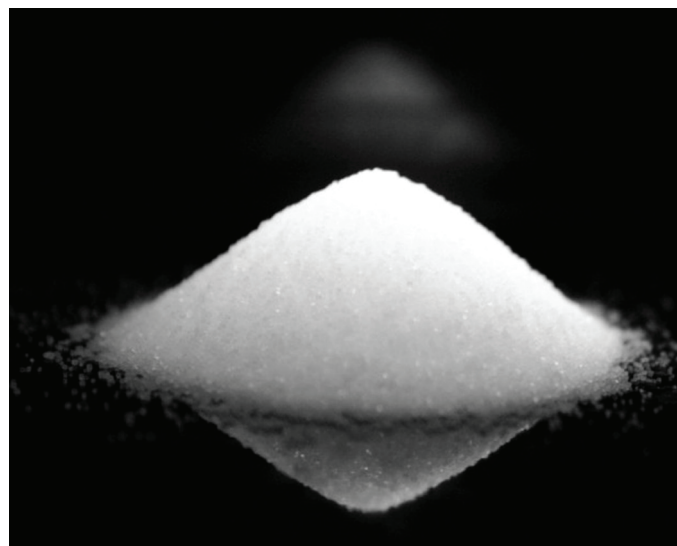


Photo Credit: Katie Sloan Dornblaser

Book Review by Randy Moore

Science, Politics, Love, & War

This book by Peter Pringle—the former Moscow bureau chief for the British newspaper *The Independent* during the collapse of the USSR—is an original, important, and compelling account of the life of Nikolai Vavilov (1887-1943), one of the most innovative geneticists in history. Pringle’s excellent book is the first full-length biography of Vavilov published in English, and is exceedingly well-researched; Pringle’s sources included government records, family papers, first-person interviews, and Vavilov’s remaining files and correspondence. However, *The Murder of Nikolai Vavilov* is more than an excellent scholarly book; it’s also a moving book about science, politics, love, and war that you will not forget.

Vavilov was born into a wealthy family, and became interested in Mendelian genetics soon after he enrolled in 1906 in the progressive Petrovskaya Agricultural Academy. While at the academy, Vavilov began using genetic selection as a way of improving crops as part of his commitment to “work for the benefit of the poor” and the “enslaved class of my country.” During a subsequent visit to Britain, Vavilov met William Bateson, who coined the term genetics and wrote the first genetics book (*Mendel Principles of Heredity*, which was published in 1909). Vavilov was convinced that Mendel’s work was the foundation for improving crops. By the time Vavilov produced his *Law of Homologous Series in Variation* (1920), which claimed that traits occur similarly in the various evolutionary stages of related species, his work was being recognized with awards such as the Order of Lenin. However, by 1930, Vavilov was mired in administrative work (“these layers of rubbish on all sides”), at one point holding 18 different positions (e.g., Director of the Institute of Genetics of the USSR Academy of Science).

Vavilov understood that genetic variability is important for improving crop production, and he correctly concluded that the greatest amounts of variability would be found in a crop’s “center of origin”—that is, where the crop was originally domesticated. To find and document this variability, Vavilov organized more than 100 expeditions through more than 60 countries to collect seeds of progenitor species and undomesticated strains unavailable in Russia. His first plant-hunting trip to find the genetic birthplace of the foods we eat—in 1916 to the Pamir Mountains—convinced Vavilov that he was on the right track. During this and subsequent trips, Vavilov endured much hardship, but he was successful—his “World Collection” of more than 250,000 seeds of cultivated



**THE MURDER OF
NIKOLAI VAVILOV**
THE STORY OF STALIN'S PERSECUTION OF ONE OF THE
GREAT SCIENTISTS OF THE TWENTIETH CENTURY
PETER PRINGLE

The Murder of Nikolai Vavilov: The Story of Stalin's Persecution of One of the Great Scientists of the Twentieth Century by Peter Pringle.
Simon and Schuster, 2008.

plants and their varieties (from five continents) was the most extensive collection in the world. In the 1930s, Vavilov’s institute distributed millions of packages of seeds and helped begin the production of more than 250 new varieties of plants. Vavilov’s *The Centers of Origin of Cultivated Plants* (1926) remains a classic.

There was a clear urgency to Vavilov’s work—“Life is short, we must hurry” was a favorite, if not prophetic, phrase. Vavilov had been Theodosius Dobzhansky’s mentor, and Vavilov pleaded for Dobzhansky to return to Russia from California (where Dobzhansky was working with Thomas Hunt Morgan) to help him “lift the country” in a “mission for all humanity.” Dobzhansky stayed in California.

The Murder of Nikolai Vavilov is an important reminder of the high costs incurred by individuals and society when leaders allow science to be perverted by politics and ideology. The bourgeois, well-dressed Vavilov had been supported by Lenin, but he began to fall out of favor when Stalin became leader. Stalin favored Trofim Lysenko, who used neo-Lamarckism as an ideologic basis for promises of immediate

improvements in crop yields. Lysenko—whom *Pravda* described as a “barefoot scientist”—claimed that he could direct heredity and denounced Mendel’s work as “rubbish and falsehood,” telling Vavilov “I do not recognize Mendelism.”

Not surprisingly, Lysenko’s use of Lamarckism to improve crop-yields failed, and the USSR soon needed a scapegoat for its food shortages. Vavilov, unable to meet Stalin’s demands for immediate results, was arrested in August 1940, while collecting plants in the Ukraine and was taken by the Soviet secret police to Moscow. He was prosecuted on trumped-up charges for sabotage and spying (e.g., “Treason to the Motherland,” “wreckage” of the economy). Following more than 1,700 hours of interrogation over 11 months, Vavilov was sentenced to death by firing squad, but that sentence was later commuted to 20 years in prison. Sadly, this commutation remained a death sentence, for while in a Saratov prison, Vavilov—the man who had hoped to use genetics to feed the world—was starved to death.

The Murder of Nikolai Vavilov is fascinating, thoughtful, and at times horribly sad. You’ll pause when you read that Yelena Barulina—Vavilov’s lover, companion, and former student—unknowingly took up residence only a few miles from where Vavilov was imprisoned; you’ll be angered when you

(continued on page 20)

BACKSCATTER

Roger Nelson

Why Don't the Data Move Like They Should? A Global Consciousness Project* Update



Photo Credit: Robbie Dale/Flickr



Photo Credit: ChiefHira/Wikipedia



The network of random sources that make up the Global Consciousness Project (GCP) has shown a correlation with interconnected human consciousness on a global scale. We have seen a highly significant overall effect on random sources during special times we identify as “global events” which bring great numbers of people to share consciousness and emotions. The effect is a tiny deviation from what’s expected, but the patient replication of tests has gradually created very strong statistical support for the reality of this subtle correlation of human consciousness with deviations in random data.

A basic assumption of the GCP is that the bigger the event, the greater will be the movement of the GCP data. So, when the GCP looked at a horrendous natural disaster like the 9.1 earthquake and the accompanying tsunami in Japan, we expected a major “hit” in the GCP data. Although our Egg in Meiji, Japan deviated powerfully during the time we specified for the formal hypothesis test, the network as a whole didn’t show a significant departure from expectation. Why not?

And why was the network apparently unexcited by William and Catherine’s Royal Wedding, or the beatification of Pope John Paul II? After all, these drew the attention of millions, or, according to some, a billion or more people in the case of the wedding. The result for the death of the enormously popular (though often criticized) Indian guru, Sai Baba, actually went backwards, yielding a Z-score of -2.6, and reduced our 12-year bottom-line statistic from 6.5 to 6.3 Sigma.

Perhaps the most mystifying event of all is the data for the killing of Osama Bin Laden. That surely must have stimulated any reasonable sort of global consciousness. Yet, while the cumulative deviation trace has some long steady trends, the final test statistic is just at the average level. What’s going on?

Why have these recent major events shown null results?

The short answer is that our signal to noise ratio is too small for single events to produce reliable statistics. There appear to be other factors (differences between types of events, level of emotion, depth of engagement . . .), but this is

the important one. We are searching for a goldfish jumping amidst ocean waves. The grand average effect size over the history of the Project is about $\frac{1}{3}$ of a standard deviation. It is only the accumulation of more than 360 formal tests of the GCP hypothesis that allows us to see the importance of this otherwise unimpressive 0.33 Sigma departure from expectation. The fact is that the outcome for single events can’t be reliably interpreted. Put another way, the noise in our measurements may obscure real effects (and of course, noise may also masquerade as effects).

This is why the GCP is a planned series of replications. When we patiently repeat the question many times, the average answer becomes very reliable. A modest 70% of the events we test actually show the positive deviation we predict, but when it should be 50/50, that percentage in 360 events is very unlikely by chance. We are looking at a subtle question, or as one of my colleagues, George deBeaumont, put it years ago, we are chasing a wily beast. To learn where it lives and what it eats—that is, what drives the effects—patience is our best experimental strategy. •

REFERENCE POINT, continued from page 19

read that Vavilov succeeded in bringing Georgy Karpechenko and other colleagues back to the USSR, after which they were arrested and executed; and you’ll be moved when you read about how workers at Vavilov’s Leningrad Institute protected their precious seeds during the German siege, despite the fact that they were starving.

In 1955, a branch of the USSR Supreme Court overturned Vavilov’s conviction, and in 1968 the Research Institute of Plant Industry (which Vavilov headed from 1921–1940) was renamed the N. I. Vavilov Institute of Plant Industry. Vavilov’s World Collection in St. Petersburg, Russia, remains an important resource for conservation biologists.

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* See *EdgeScience* #1 for a complete description of the Global Consciousness Project.