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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.

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.

Cover Image: This 54-second-long, time-lapse image, taken on Sept. 26, 1996 by the Visible Imaging System on NASA’s Polar spacecraft in ultraviolet light, shows the trail of a small comet the size of a two-bedroom house that disrupted 5,000 to 15,000 miles above the Earth, according to University of Iowa physicist Louis Frank. The object was in sunlight but the Earth below was in darkness, so a map of the Earth has been superposed. Photo credit: NASA Goddard Space Flight Center and the University of Iowa.
Proposal For An Office of Public-Centered Science

Peter A. Sturrock and Wayne B. Jonas

The United States enjoys an enormously successful scientific status at the beginning of the twenty-first century. Our military strength and our economic wellbeing are derived in large measure from the technological development of our scientific accomplishments. For these reasons, and also in the interest of promoting knowledge for the sake of knowledge, Congress and taxpaying citizens have been very supportive of scientific research.

Agencies such as NASA and the NSF that fund scientific research have developed procedures to encourage scientists to share the results of their research with the broader community. These procedures typically involve an “out-reach program” comprising public lectures, participation in school programs, etc.

These efforts are useful and commendable. However, it is rather like a one-way conversation between the scientific and non-scientific communities, and one-way conversations are not the most engaging or productive. To convert this exchange into a two-way conversation, one should look for a complementary “in-reach program.”

To perceive an opportunity for such a program, one may note that the scientific agenda is set primarily by the scientific community and many topics of public interest are not itemized in this agenda. Hence it would be possible to create an effective in-reach program by developing a procedure whereby the scientific community could be encouraged to respond to topics that are of interest to the large non-scientific community but which are not part of the current research program of the scientific community. Since meaningful scientific research requires institutional support, an essential prerequisite is that one or more federal funding agencies should support such a program.

The Proposal

For these reasons, we recommend a dedicated program of federal funding of research in response to public interest. The justifications are:

1. It would encourage and enable the scientific community to be responsive to topics of public interest.
2. It would focus on topics that are not to be found in the conventional research program of the scientific community.
3. It would offer important challenges to science. Among the questions raised are: How does one investigate ephemeral and unpredictable phenomena? Are new experimental and observational techniques and procedures required? Are new data-analysis techniques required?
4. Such research may prove to have broad significance in calling into question some deeply engrained scientific beliefs.

It can be anticipated that a vigorous response to such challenges will lead to new scientific techniques applicable to other areas of scientific research, and possibly to a new scientific perspective that is broader than our current one.

For instance, scientists will find that their resistance to some topics is due to the fact that they are—or appear to be—incompatible with the “standard model of reality” of present-day science. The study of these topics may therefore lead scientists to question the standard model. This focus may lead scientists to consider more carefully, and in a different light, the role of science in the service of mankind.

For any such topic of research, there are three possible outcomes:
1. An informed consensus among all investigators that the phenomenon being investigated is completely devoid of reality.

2. Conclusive support for the popular interpretation of the phenomenon.

3. A conclusion that there is some reality to the phenomenon, but this reality is different from the popular interpretation.

For each topic, and whatever the outcome, science will have been responsive to the public need and responsible for an advance in human knowledge.

Implementation

We recommend that an agency such as the National Science Foundation be assigned responsibility for this program. The agency would create a new office, perhaps an “Office of Public-Centered Science,” with the mission of providing new forms of cooperation between the scientific community and the public.

The office would be provided with a budget, an appropriate staff, and an advisory committee. The advisory committee would be composed of successful, eminent scientists who are sufficiently open-minded to accept the mission of this office as a worthy goal for the scientific community and the agency.

By means of a webpage, the office would make a public announcement of the program, and it would invite members of the public to suggest topics for investigation. It would be the responsibility of the office to log in the recommendations, and develop a mechanism to prioritize the topics according to level of interest, availability of evidence, extent to which the topic is already funded by federal sources, and potential for investigation. This information would be submitted to the advisory committee, which would generate a list of topics for investigation. The office would then issue an “announcement of opportunity,” inviting proposals for research into these topics.

From this point on, the program would run like any other NSF-type program, beginning with peer review of the proposals. If an investigator makes progress, his or her grant would be renewed. If a topic is shown conclusively to be bogus, research would be discontinued. If the reality of a topic is conclusively established, the advisory committee might recommend that further research be conducted as part of some current area of mainstream science.

When a scientist has completed a study, he or she will, as usual, be expected to publish the results. It is to be hoped that a full report of the study would be made available on the investigator’s website; a short technical summary of the results would be submitted to a scientific journal with a wide scope such as Nature or Science; and a short summary, prepared in layman’s language, would be posted on the website of the office responsible for the program. The office would maintain on its website a historical summary of requests received from the public and the response and findings of the scientific community.

It is our opinion that this program would provide the scientific community an opportunity to respond positively to some of the topics that are of great public interest, but which currently do not find a niche in our current national program of scientific research.

We point to the Center for Complementary and Alternative Medicine in the National Institutes of Health as an example of a successful program in which topics of interest to the public but not—or at least not initially—to the relevant scientific community have been investigated successfully, to the advantage of both the lay and professional communities.

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Louis A. Frank, the Carver/James A. Van Allen Professor of Physics at the University of Iowa, died on May 16, 2014 at the age of 75. There was no obituary in the New York Times or the Chicago Tribune, though by all accounts there should have been. In the mid-1980s, before this mainstream scientist turned maverick, he was regarded as one of the most respected space physicists in the world; he was responsible for more scientific instruments aboard spacecraft than any other scientist, having been an experimenter, co-investigator, or principal investigator for instruments on 42 spacecraft.

A native of Chicago, Frank’s first professional research activities began in 1958 when he assisted James Van Allen in the calibration of the first U.S. lunar probes, Pioneers 3 and 4, as an undergraduate student at the University of Iowa. Later, Frank was the principal investigator for the auroral imaging instruments for the Dynamics Explorer Mission, the plasma instrumentation for the Galileo Mission to Jupiter, the U.S. plasma instrumentation for the Japanese Geotail spacecraft, and the camera for visible wavelengths for the Polar spacecraft of the International Solar Terrestrial Physics (ISTP) Program. His scientific accomplishments were many: he discovered the theta aurora, the remarkable configuration of auroral and polar cap luminosities that looks like the Greek letter theta hovering above the polar cap; he made the first measurements of the plasma ring around Jupiter and Saturn; and he was the first to measure solar-wind plasma funneling directly into the Earth’s polar atmosphere, as well as the belt of ions around the Earth known as the “ring current.” Frank was a Fellow of the American Physical Society, a Fellow of the American Geophysical Union, and the recipient of the National Space Act Award.

But everything changed for Louis Frank in 1986. After five years of analyzing the puzzling small black spots that appeared in the images from NASA’s Dynamics Explorer I spacecraft, which looked at the Earth in ultraviolet light, Frank and John Sigwarth, the graduate student who had been assigned the task of following this data wherever it led, concluded that the black spots were caused by water vapor remaining from the disintegration of small comets in the Earth’s atmosphere. The black spots were not transmission errors, not paint flecks on the imager, nor any other artifact, Frank concluded, but “atmospheric holes,” the remains of fluffy snowballs each weighing 20 to 40 tons and striking the Earth every few seconds.

News of the discovery, published in two papers in Geophysical Research Letters (GRL), created a firestorm of controversy. Frank had to respond to 11 refutations of his claims in GRL from scientists in a half dozen disciplines, including astronomers who insisted that if the small comets were real, they would have seen them, and geologists who were convinced they could account for the Earth’s water balance very well, thank you, without the influx of Frank’s small comets.

But a few stifled a knee-jerk response to Frank’s outrageous claim and decided to search for the objects themselves. Within two years, Clayne Yates, a science and mission design manager at the Jet Propulsion Laboratory (JPL), convinced JPL to fund two nights on the Spacewatch Telescope in Arizona, the one telescope capable of detecting very small near-Earth objects, in an effort—which Yates thought unlikely—to find Frank’s small comets. After eliminating everything and anything in the images that might masquerade as a small comet, Yates found streaks on his images that he could not otherwise identify. They had the same motion in orbit, the same speed, about the same size and darkness, and they were within a factor of two in number of what Frank had calculated for the atmospheric holes seen by Dynamic Explorer. Still Frank’s critics were unconvinced.

But Frank had been hatching a plan to try to confirm his hypothesis using a different set of
instruments on an upcoming spacecraft. The story is told in the excerpt that follows, which is drawn from an unpublished 45,000-word postscript Frank prepared to a book he wrote entitled *The Big Splash* (Birch Lane Press, 1990) that detailed the discovery of the small comets. 

**SUCH IS THE NATURE OF SPACE EXPLORATION**

that scientists working in this field often must think about their next project years before their current project has gotten off the ground. And so it was for us with Dynamics Explorer. Even before its launch in 1981, NASA had begun selecting participants for a proposed spacecraft called Polar. The purpose of the Polar mission was to study the flow of energy and particles from the sun into the Earth’s atmosphere. So it was that we began working on our instrument for the Polar spacecraft several years before the small comets were even a gleam in the eyes of Dynamics Explorer. By the mid-1980s, however, we had the data from Dynamics Explorer, so we knew the small comets were real, but we were still a couple of years away from making our findings public. We began, however, to think about how we could modify our instruments on Polar to include the capability of looking for water clouds directly in the visible wavelengths. Opportunities like that do not come around often in space physics, but we made the changes to our instrument without any risk to the primary objective, which was studying the Earth’s auroral lights.

Basically, we made sure that our ultraviolet camera on Polar would have a very large field of view and very low noise so that there would be no question of instrumentation being responsible for the black spots in the images. And then for the visible-light cameras, it was a matter of choosing the right filters. We only had a limited number of slots available for filters. We were going to use the auroral filters obviously, but we were also able to put in a few others that had not been used on an Earth-orbiting space platform before. One, the hydroxyl filter, looked for light from the fragment of a broken water molecule, which has a hydrogen atom paired with the oxygen atom. The other was a sodium filter. These filters would eventually tell us about the dust, water, and sodium content of the small comets. Are they just like the known large comets, only smaller, or are they an entirely different class of objects?

Of course, we needed an ultraviolet imager to do our small comet work and there was already another ultraviolet imager on Polar. So we had to have a very good reason for having our own. But that was easy: it was a safety measure. Our ultraviolet camera would give us a view of the Earth that would enable us to protect our visible imagers. If the visible imagers were, in fact, pointed at the bright sunlight, they might be destroyed from such intense light. Of course, we used a filter on the ultraviolet camera that matched the Dynamics Explorer filter (1304 angstroms), which we knew from experience was ideal for looking for atmospheric holes.

Polar ended up on a Delta rocket that went up on the 24th of February 1996, but because we had to compensate for a wobble in the spacecraft it was not until the first week of April that we were able to get decent images out of Polar. By that time I was back in Iowa. One day shortly afterward, I was on the phone with John Sigwarth and he was reporting on all the engineering details he was monitoring at Goddard. And then, at the end of this rather long conversation, Sigwarth casually said, “Oh, yes, the atmospheric holes are there.” They were there and just the size we expected them to be. I was overwhelmed with joy.

**Confirmation**

When the time came to make our Polar findings public, we decided that the best forum for the announcement would be the Spring meeting of the American Geophysical Union, the same one in which we had announced the discovery of the small comets more than a decade previously. The big day came on Wednesday the 28th of May 1997. This American Geophysical Union meeting in Baltimore was not unlike any of the others I had attended during the past 20 or so years. But there was a distinct sense of anticipation in the air as I prepared to address the TV crews and dozens of reporters at the 10 a.m. press conference reserved for our Polar findings. Up on the podium with me was one of my critics, physicist Robert Meier, a situation that may have served as a clue to what was coming. Then, immediately after the press conference, I presented the findings to my colleagues and the public in one of the convention center’s largest auditoriums. Kathy Sawyer of the *Washington Post* would describe the crowd of colleagues there as “riveted.” There were so many lights from photographers that the audience could not see my slides. This was clearly not business as usual.

I began both the press conference and the public session with a little background for those who were not familiar with what we had found a decade earlier with Dynamics Explorer. I probably need not have bothered. But the point was that the Polar results confirmed our previous observations of atmospheric holes by Dynamics Explorer and did so in a very powerful manner. It involved a different instrument with better cameras that provided a better spatial resolution of the objects. Our new data confirmed the dimensions of the holes, their frequency of appearance, and their east-to-west motion across the sunlit atmosphere. Polar detects approximately several thousand atmospheric holes per day, which gives a global rate in the range of 5 to 20 per minute—the same sort of numbers we obtained from Dynamics Explorer. In some cases, the Polar UV camera caught the same atmospheric holes in consecutive exposures as it moved across the field of view. In other images, the spots were doubled, as they should be—because the spacecraft’s wobble is such that the same object was recorded twice in the same exposure. None of this is characteristic of noise, of course.

What surprised everyone the most, however, is Polar’s observation of an entirely new phenomenon—a large population of objects that had never been detected before. In some of our images we found bright trails left behind by small comets exploding at altitudes ranging anywhere from about 3,000 miles to 30,000 miles. The oxygen trails produced as sunlight dislodged the oxygen atoms from these water-bearing objects made for some very striking images. Dynamics Explorer could not see these trails as it was a different kind of imager and...
the water fragments, which is a standard proxy for water if you are on the ground looking at these large comets, was very powerful, independent evidence of the existence of these comets. These bright emissions allowed us to determine the amount of water in the small comet blow-ups and we found that the amount of water was the same as we had deduced for the atmospheric holes—an entirely different phenomenon, entirely different measurements, but the numbers were about the same. The bottom line is that we are indeed looking at small comets coming into the atmosphere in large numbers.

Before Polar, we had no proof that it was water that was producing the black spots in the images. The Dynamics Explorer results merely allowed us to interpret the black spots as being produced by water vapor in the atmosphere. But the visible imaging camera on Polar showed that the water is there. This was a major step, because it could have been something exotic, some form of propane, or dust balls, or something else. Until we were able to use the same proxy as for large comets, there was always that uncertainty. But our reasoned guess that they are primarily water-snow was now confirmed. I think that is Polar’s most important finding.

The other filters on Polar told us there is no sodium in these small comets and no dust, or so little dust that you cannot see it. While we know that large comets are quite bright in dust and sodium, the small comets did not have any strong sodium signal or dust signal, which means they don’t have the
The contents of the libraries in the physical sciences”—Cook wrote in reply: “Get out the torches.” Richard Kerr, the reporter for Science called the results “a stunning turnabout.” And Robert Matthews of New Scientist saw it as “a wonderful tale of scientific David beating reactionary Goliaths.” Even the New York Times chimed in with an editorial a couple of days later that noted: “Just when you thought scientists had pretty well explored the far reaches of this planet and found everything significant worth knowing, along comes a discovery so startling and so potentially important that it seems hard to believe it was overlooked.”

Incoming

John Sigwarth thought we would hear from our detractors right away. But it took them a couple of days to recover from the shock. The messenger was the first to take the heat. Some people tried to pin the blame on NASA for endorsing such a controversial story, but NASA gets flack for everything they do, so they were rather impervious to this sort of attack. They

**INCOMING OBJECT:** This multiple exposure, composed of three one-second exposures each six seconds apart, was taken on Dec. 31, 1996, by the VIS camera on Polar. According to Louis Frank, this trail reveals the path of a water vapor cloud produced by an incoming icy comet breaking up over the Earth. The VIS camera used a filter that is sensitive to visible light from oxygen-hydrogen molecules, or the hydroxyl radical that originates from water molecules that have been stripped of a hydrogen atom. While the object at an altitude of less than 2,000 miles above the Earth was in sunlight, the Earth below was in darkness, so a view of the Earth at the time of the event has been superposed onto the image for reference. (Photo credit: NASA Goddard Space Flight Center and the University of Iowa)
of five miles, where commercial flights are common, the small comet is nothing but a cloud of water vapor being dispersed by the wind.

But that is not to say that one of these small comets could not—on rare occasion—strike a satellite or the shuttle. And indeed on the 1st of July 1997, I suspect that a disintegrating small comet may have struck the Mir space station. According to Dragonfly, Bryan Burrough’s entertaining chronicle of life aboard Mir, on July 1, 1997, the Mir commander, Tsibliyev, heard what sounded like two successive “muffled explosions.” When he looked outside he was startled to see a “floating blizzard of snowflakes” around the Spekr module. “His first thought is that the flakes are frozen bits of fuel,” writes Burroughs. “They look just like the little ice crystals that form when the station’s thrusters spit out drops of fuel. But no: there were too many.”

handled the Polar findings just like they handled the stories about life being found in a Martian meteorite. If NASA only paid attention to topics that were free of naysayers, they would not have much to talk about. Besides, as POLAR project scientist Bob Hoffman made perfectly clear: “NASA has not endorsed the interpretation of the images in any way whatsoever.”

Since our Polar results would be appearing in an official NASA press release, we were particularly cautious about what we said and how we illustrated it. We took care to note that these Earth-bound comets represented no threat to either people on the ground or in orbit, as these objects disintegrate far above the Earth’s atmosphere. We even prepared a diagram showing the relative heights of an incoming small comet and compared it to things that people might be familiar with. At an altitude of about 330 miles the comet vaporizes; this is about the altitude you will find some of our satellites. At an altitude of about 800 miles from Earth, the comet breaks up into fragments. Exposure to sunlight then rapidly vaporizes the water snow and the resulting cloud of water vapor expands to 30 miles in diameter. The water vapor absorbs sunlight reflected from oxygen in the upper atmosphere, causing the appearance of black spots in the images of the sunlit Earth taken in ultraviolet light. The cometary water vapor cloud slows as it descends and stretches out into a thin, wispy pancake. Upper atmospheric winds then disperse the cloud so that all traces of it essentially disappear. The cometary water vapor gets mixed into the normal meteorological system of condensing water that precipitates onto the Earth. The small comets are responsible for a tiny fraction of the annual rainfall, depositing only about one ten-thousandths of an inch of water on the Earth’s surface per year. (Photo credit: NASA Goddard Space Flight Center and the University of Iowa)
Months later Tsibliyev remarked: “Everyone was just scratching their heads and could not come up with an explanation.”

The Snowball Fight Resumes
There was no way to avoid the fact that small comets are a genuine geophysical phenomenon—or that my critics would not go away anytime soon. The basic outline of the critics’ arguments had changed little since the 1980s. Everyone was looking for mundane explanations for the black spots in the Polar images—but again no one could agree what they are. There were no showstoppers. Many of the objections to the Polar findings were again of the “we would have seen them if they were real” variety. I expected the backlash that followed our Polar announcement. Science is strange these days. New findings are usually well received by the press—and then the naysayers come out of the woodwork. While I welcome criticism in science, I do not think that the harassment of individuals who simply propose something new is justified. What is wrong with trying to advance science into the frontiers?

The critics did their best to pour hot water on the small comets, but all they did was steam up the place. An Associated Press story previewing the spring 1998 American Geophysical Union meeting in Boston noted that emotions about the small comet theory “have reached a red-in-the-face, fist shaking level.” One of the speakers would call this “science as contact sport.” Seven independent teams of researchers were prepared to assail the small comet hypothesis. The audience wanted blood—anyone’s.

The chairman of the session was Tom Donahue. In his introductory remarks, he warned, only half in jest: “I don’t want any hitting in the trenches. No fighting, no biting.” It didn’t help. The auditorium was filled with scientists eager to see this “intellectual slugfest,” as the Boston Herald called it. “I’ve never seen anything like this,” one NASA scientist told the Herald reporter.

Excess Water Vapor in the Atmosphere
There is, of course, hard data to back up the passage of this cometary water through the atmosphere. In August of 1997 an ozone-monitoring satellite trailing the space shuttle Discovery detected much more water vapor in the upper atmosphere at northern latitudes than current theories predict. This region is supposedly drier than the Sahara Desert. While water vapor is abundant in the lower atmosphere, very little of it manages to drift upward in the polar regions, so the stratosphere and mesosphere remain dry. Only in the tropics, where thunderheads push moisture into the stratosphere, does water vapor manage to reach the upper atmosphere. With the Polar confirmation still on everyone’s mind when this discovery made the news, the press quickly touted this preliminary finding as supporting the small comet theory.

The high readings came from an instrument called the Middle Atmosphere High Resolution Spectrograph Investigation (MAHRSI) operated by Robert Conway, a planetary physicist at the Naval Research Laboratory in Washington, D.C. MAHRSI found a surprising amount of our old friends, the hydroxyl ions, in a layer of the mesosphere at altitudes from 43 to 56 miles. The hydroxyl ions form at high altitudes when ultraviolet light splits apart molecules of water vapor. These results confirmed an earlier set of MAHSI measurements taken during a 1994 shuttle flight, but Conway had questioned them since they contradicted standard atmospheric theory.

Conway, however, was less than convinced that the small comets were responsible for the presence of water vapor in the polar atmosphere. On the one hand, he told Richard Kerr of Science: “You have to give the man [Frank] credit for predicting something we’re now seeing.” But he then told Richard Monastarsky of Science News: “…I can’t believe that his explanation is the right answer.”

This was not the first report of excess water in the mesosphere. Nor was it the first time that such results had encountered disbelief. Not long before the surprising MAHRSI results, James Russell of Hampton University in Virginia had also found a water spike in the upper atmosphere in their data from the Halogen Occultation Experiment (HALOE), which has been flying on a satellite since 1991. The HALOE instrument measures water vapor directly by peering through the atmosphere while the Sun rises and sets behind the Earth. While reprocessing their data late in 1996, Russell and his colleagues were shocked to find that their instrument had discovered 50 percent more water vapor than they expected to find at an altitude of about 45 miles above the sunlit poles.

Of course, the critics replied that even if the mesosphere is wet, it was not nearly as wet as it should be if the small comets were bombarding the Earth. But they forgot one thing. You just cannot stop this incoming cometary water. There is so little atmosphere up there, the water vapor comes in like a freight train. It travels at Mach 10 as it passes through the upper atmosphere. How could it possibly stop there? Sure, some of the “skin” gets left up there, but who knows what percentage that is? It could be one percent or ten percent.

The important thing is that an excess of water has been measured up there which no one can account for as being from an Earthly source. If they could totally account for the water vapor up there, that would make a strong statement about there being no significant extraterrestrial influx. Sorting out water fluxes and why the water abundance varies with latitude and season will need close monitoring of the variation in the small comet influx. “It’s when we get challenging observations like this that we start to rethink all of our assumptions,” John Olivero told Science reporter Richard Kerr. While at Penn State, Olivero had made microwave measurements of water bursts in the upper atmosphere and had found more than 100 instances of “water events” that could not be interpreted as volcanic events or other injections from the surface.

A Snowball’s Chance in Hell
Science once believed that Earth was an isolated little well of water, that Earth was a unique circumstance. All the water was here, and there was next to nothing anywhere else. But
“crime,” John Sigwarth, who by then was a senior scientist in the Heliophysics Division at NASA’s Goddard Space Flight Center, died of an aortic aneurysm at home at the age of 49. Now, with Frank’s passing this spring, the small comets have lost their primary advocates. Is Louis Frank correct about the existence of the small comets, or is he mistaken? No one seriously denies the data; only his interpretation is in question. But until now no one has been able to put together a viable alternative explanation that fits all the physical characteristics of the phenomenon observed. So perhaps, as Frank liked to say in response to his critics, these snowballs do stand a chance in hell.

— Patrick Huyghe

The 10th Annual European Conference of the Society for Scientific Exploration
Leiden University Medical Center, Netherlands
November 13–15, 2014
For more information see scientificexploration.org.

Postscript:
The presentation of Louis Frank’s Polar findings in the late 1990s unleashed a flurry of activity. NASA asked Bob Hoffman, the Polar project scientist, if anything could be done to quickly determine if the small comets were real or not. Hoffman put together an ad hoc advisory group to coordinate and identify activities that could be used in a confirmation or refutation of small comet activity. But Hoffman soon realized that nothing could be done quickly and certainly not cheaply, and so the effort fizzled. Other scientists decided to conduct searches for the small comets, but all turned out negative. In one case, Frank saw evidence of the objects in an image in one team’s presentation; Frank tried unsuccessfully to get them to acknowledge this evidence. In another case, a team of scientists who had conducted a new telescopic search for the small comets, and had announced negative results, refused to share their data with Frank. Insults were swapped. Others even accused Frank of tampering with the Polar data. Frustrated with his colleagues in the scientific community, Frank lashed out. A newspaper reporter wrote that Frank had accused a colleague of fraud. Frank vehemently denied it, and pointed out that the reporter had not put the word in quotes. Nonetheless the colleague filed a complaint with the University of Iowa, which began what Louis Frank called a “witch hunt” against him.

Then in March of 2005 Frank suffered a stroke that effectively ended his career. In December of 2010, Frank’s partner in small comet
If 500 or so of the twentieth century’s great scientists faced the same constraints their modern contemporaries do today, we might be denied the advances that include the laser, the electronic and telecommunications revolutions, nuclear power, biotechnology, and medical diagnostics galore we now take for granted. Had they applied throughout the twentieth century, it is unlikely that the work leading to the most radical discoveries would have been funded simply because the mavericks who made them were necessarily out of step with their peers, and life today would be unrecognizable.

Research is a global initiative. Ideally, an article about science policy should therefore include contributions from the policies of every nation with an involvement in research, which would not only be difficult but also unreasonably tax the reader’s patience. However, there is a thread that is common to almost every funding agency—the peer review of proposals, a relatively new development for most researchers dating from about 1970. Before that, researchers largely determined science policy, but by about 1970 the scale of academic research had become too large for governments to leave undirected. Today, the numbers of universities have been considerably expanded. Most academics are publicly funded, and before researchers are allowed to lift the proverbial test tube they must convince their peers that the effort would not only be the best use of the required resources but would also lead to some sort of national benefit. Proposals usually take months to prepare, yet average success rates are low, about 25%, and there are strict policies on resubmissions. These policies lead to frustration and colossal wastes of time and energy.

Peer Preview

Before 1970, policies in the United States, the United Kingdom, and other countries that support research were unconstrained. At that time, with the exceptions of government defense departments, research spending was relatively small because there were fewer universities, the major source of new ideas in science. The preparation of research proposals was largely restricted to those few scientists who needed exceptional items of equipment, which were then subjected to peer review—or peer preview, as it should be called. The rest had time to reflect and consider what they wanted to do free from peer or other pressures so long as their requirements were modest. Astonishingly post about 1970, considering that academics are noted for their individuality, policies on who should be funded turned out to be virtually the same everywhere. Common themes are that research selection should be as free from favoritism as possible. Such fairness-based policies have been easy to sell to the public and academics generally, as they can be presented as being above suspicion and the best way to allocate scarce resources. Everyone submitting a proposal should have the same chance of getting funded, of course, but fairness is a social concept. It can be achieved only by collective decisions, and science is not democratic.
evaluating NSF proposals, reviewers will be asked to consider what the proposers want to do, why they want to do it, how they plan to do it, how they will know if they succeed, and what benefits could accrue if the project is successful. These issues apply both to the technical aspects of the proposal and the way in which the project may make broader contributions.” The 2010 strategic plan of the UK’s largest research council, the Engineering and Physical Sciences Research Council (EPSRC), states: “Our strategy is to deliver greater impact than ever before. It will focus research on the needs of the nation and commit greater support to those scientists and engineers who are world-leading.” Peer review of proposals is now known as the “gold standard” of research selection in the US, the UK, and probably everywhere; researchers cannot escape it.

Applying these constraints to perhaps the leading maverick of them all, Max Planck, would probably in such a hesitant person have terminated his interest in the relatively unknown field of thermodynamics. In 1900, Planck discovered the quantization of energy and initiated the subsequent avalanches of scientific and technological discoveries that transformed modern life. Without it, the world would now be completely different. As a scientist and student of Planck’s work, I doubt if he would have agreed at the beginning of his career to submit his ideas for his peers’ critical consideration. However, assuming he had, he might have said that his sole objective for the next three years say, or indeed for the rest of his life, would be to strive to increase understanding; but such an undefined plea would be unlikely to cut any ice with funding agencies today whatever the standing of its originator. Planck was renowned for his absolute honesty and integrity, but these qualities are not unique to Planck.

Nowadays, funding regulations reward credible dissembling (though many still avoid it) and would put a modern Planck at a serious disadvantage. While Planck was still in his early 20s, he had to deal with the indifference, to say the least, of the two most famous members of his university department—Herman von Helmholtz and Gustav Kirchhoff—to his early work without deflecting in the slightest from his chosen path. But indifference does not include the power of veto, as it usually does today. Nowadays, our representatives seem determined to make selection ever more restrictive in their relentless quests for value for money. Maybe they have not read Planck’s words commenting on another scientist’s (Ludwig Boltzmann) struggles: “This experience gave me also an opportunity to learn a fact—a remarkable one, in my opinion: A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it.”

Planck’s self-imposed task was to prove that entropy—then a virtually unknown concept—was, after energy, the most important property of a system. Its relationship with probability led him to introduce a new universal constant, the elementary quantum of action, which expressed the relationship between the energy of an oscillator, $E$, and the frequency of the radiation it emits, $v$, as: $E = h\nu$, where $h$ is Planck’s constant. Planck announced his now ubiquitous constant to the German Physical Society in Berlin on December 14, 1900, and a new era was born.

The Planck Club
One should not stop with Planck, of course. Moving through the twentieth century, similar remarks could be made about the work of such mavericks as Joseph John Thomson, the “father of the electron”; Ernest Rutherford, the “father of the nucleus”; Albert Einstein, the “father of relativity”; Niels Bohr, the “father of the atom”; Wolfgang Pauli, the discoverer of the exclusion principle; Werner Heisenberg the discoverer of the uncertainty principle; Oswald Avery, the discoverer of the true role of DNA, a discovery ignored by the Nobel Committee; Barbara McClintock, who dedicated some 30 years to the discovery of “jumping genes,” despite the disdain of her colleagues but which today are at the heart of genetic engineering; Charles Townes, who was told to stop his work by Isidor Rabi and Polykarp Kusch, his senior departmental colleagues, but who went on to discover the maser, the precursor to the now ubiquitous laser; Carl Woese, who working alone discovered a new domain of life—the archaea; Peter Mitchell, who discovered how ATP—Nature’s universal energy molecule—is synthesized and promoted his idea in the teeth of international opposition; and Harry Kroto, who together with his American colleagues discovered the fullerenes, a new chemical species based on $C_{60}$. Kroto proposed in the 1970s a laboratory simulation of conditions thought to prevail in red-giant stars that might be responsible for synthesizing the long carbon-chain molecules detected by radio astronomy, thus his chances of winning support under today’s rules might not be very high.
These twentieth century scientists together with about 500 of their similarly courageous colleagues constitute what I call “the Planck Club.” Almost all won Nobel Prizes, but how many of them would, if they were setting out today, be free to turn their fresh and fertile minds to some of the twenty-first century’s intractable scientific problems? Youthful ambition (at any age) must be allowed freedom of expression, but today such ambition would probably be dismissed as arrogance.

Today, peers are usually allowed to express their opinions anonymously, which means that they can say what they think about proposals without fear of the consequences, a laudable aim. However, scientists are also people, and when asked to comment on a close rival’s (or would-be rival’s) bid for the funds they might also be bidding for, it should be expected that some might be unable to resist the chance to put the boot in if they can get away with it. In fact, these well intentioned but misguided policies are having disastrous consequences and are, in effect, unprecedented, global-scale gambles with future prosperity.

**The End of Open-Ended Search**

Scientists, including the mavericks among them, know that eventually they must submit to their peer’s judgment if their ideas are to be accepted into the scientific lexicon. Before about 1970, acceptance of radical results was usually delayed, but no one could alter the fact that their work had been done. It could speak for itself. The scientific and technological harvest produced by this approach was spectacular. It included, as mentioned previously, the laser and a myriad of its spin-offs, countless components of the electronic and telecommunications revolutions, nuclear power, biotechnology, and medical diagnostics, all of which are now indispensable parts of everyday life. Nowadays, funding agencies, public and private, favor targeted, prioritized objectives selected by consensus and discourage the open-ended searches for new perspectives that had earlier proved so successful. It seems to have been accepted that pragmatism should rule. But science is perhaps the only human endeavor dominated by absolutes. Scientists strive to understand some aspect of the universe in its many manifestations and complexities, an understanding that is indifferent to human institutions or values. New science should be equally applicable on Alpha Centauri, for example. The rules governing science’s administration should therefore be as absolute and as free from considerations of survival as we can make them.

Although much excellent work is done within the current mainstreams, this is a far cry from the research that is possible today. However, revolutions are not required to put it right. Among a wide range of solutions, by far the cheapest is “Venture Research,” an initiative that seeks to identify the scientists who have become aware of areas of ignorance that are serious obstacles to progress. It does not use deadlines, peer
review, milestones, or any of the now-ubiquitous criteria used by the conventional funding agencies. Initial applications are easy to make and selections are made by face-to-face discussion with a small group of non-competing, non-specialist scientists who create the critical and trusting environments within which applicants can argue for as long as it takes that they should be funded. If applicants trust those making the selections, they will reveal what they really want to do. The aim should also be to give researchers maximum freedom; that would only be done if in turn they were trusted.

**Venture Research**

In the Venture Research initiative sponsored by British Petroleum during the 1980s and 1990s, we found, before its untimely closure, some 26 groups of researchers whose proposals (with the possible exception of one) had been rejected by the conventional funding agencies. We were the agency of last resort. The research, in Europe and North America, was initially well outside the current mainstreams, and scientists worked on the most basic and fundamental problems chosen by themselves. Despite rejection by peer review, the initiative led eventually to at least 13 breakthroughs; that is, the researchers succeeded in radically changing the ways we think about important subjects. For example, Ken Seddon, now at Queen’s University Belfast, had in the mid-1980s submitted a proposal to a precursor of the EPSRC to study the chemistry of ions in an ionic environment. Hitherto, chemistry had been almost exclusively concerned with the study of covalently bonded molecules in a molecular environment—a description that virtually covers the entire field of organic chemistry. But Seddon had identified an unexplored domain. Astonishingly, after the usual trial by peer review, it was rejected with a gamma rating; the Council’s lowest possible. But Seddon knew about Venture Research, having been a junior participant in the scheme a few years before, and brought the same proposal to us, which we gladly funded. His Venture Research not only turned out to be scientifically very successful (as usually happens when new questions are asked in important fields), but it also transformed the field of green chemistry, and Seddon became the UK’s most cited chemist.

Today, companies and philanthropists could initiate their own Venture Research schemes. Universities can also seek to eliminate peer review’s influences. University College London (UCL), my own institute, set up in 2008 a Provost’s Venture Research Fellowship open to all members of staff. Its first Fellow was Nick Lane whose 2009 proposal, entitled “Chemiosmosis and the foundations of complex life,” proposed to examine the broader implications of chemiosmosis for the evolution of complexity. All three domains of life rely on the counter-intuitive mechanism of chemiosmosis to generate energy in the form of ATP. Despite its nearly universal occurrence, the advantages and disadvantages of chemiosmosis were largely unstudied. However, Lane had only an honorary position at UCL, as he was intending to commit himself full time to his chosen path, he needed a salary, and worse still, he could not credibly point to specific benefits that might come from his research. It is therefore most unlikely that this maverick would have qualified for funding from the usual agencies. During the three years of his fellowship, he published some 20 peer-reviewed papers, the highlight of which was a prestigious Nature “Hypothesis” paper, in which he and his colleague Bill Martin showed that prokaryotic genome size is constrained by bioenergetics. In prokaryotes, ATP synthesis takes place on the plasma membrane, whereas in eukaryotes ATP synthesis (also through chemiosmosis) takes place on mitochondria dispersed throughout the cell. Eukaryotes arose from the endosymbiosis of two prokaryotes, an endosymbiosis that turned out to be the defining event in the creation of complex life. The eukaryotic mitochondria thereby created restructured the distribution of DNA in relation to bioenergetic membranes, permitting a remarkable ~200,000-fold expansion in the number of genes expressed. This vast leap in genomic capacity was strictly dependent on mitochondrial power and was the prerequisite to eukaryotic complexity. It was also the key innovation en route to multicellular life. The success of Lane’s work led the Provost to renew the scheme.

**Are Applications Necessary?**

But Venture Research’s weakness is that scientists must apply. If that were not the case, only the famous and well connected would be selected. The current funding crisis could be solved without this minor drawback if research sponsors were to empower a small number of universities to take responsibility for funding the choices of appointed academic staff, as they did before about 1970. Proposals would not be required. This is an expensive solution, but a university selected for this role would not need the substantial numbers of staff apparently necessary to administer proposals, oversee research, and ensure compliance with the rules. John D. Rockefeller, for example, was a philanthropist who made his huge fortune from oil and in real terms was one of the richest people ever, including the present day. At the turn of the twentieth century, there were, of course, no antibiotics, and there was constant fear of childhood diseases and the “microbial agents” that cause them. In 1901, Rockefeller’s eldest grandchild died from scarlet fever and he finally agreed to found the institute he had been considering for years—the Rockefeller Institute of Medicine, now the Rockefeller University. It was to be dedicated to improving health through purely disinterested scientific inquiry. Its researchers would be supported to do whatever they thought necessary, an astonishingly altruistic and visionary decision. Rockefeller did not require that the Institute should concentrate on any specific disease, such as scarlet fever, for example, as philanthropists tend to do nowadays, but should be as wide ranging as possible. Andrew Carnegie and Howard Hughes also founded institutes that were similarly unconstrained. In all other respects, these philanthropists were hard-nosed businessmen, but how many of today’s super-rich have their unbounded altruism?

Although the changes needed to restore scientific endeavor to full health are tiny, funding agencies and their institutional supporters do not concede that the peer review of proposals has
any flaws. Universally accepted as the gold standard of research selection, it therefore rules absolutely. It is strangling science. Before about 1970, universities were trusted to support the research that will lead to economic benefit for all. Through the actions recommended here, and others that will come when the funding agencies and their supporters encourage them, we must restore trust in scientists wanting radically to challenge conventional wisdom.

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2. Green chemistry, also known as sustainable chemistry, is the design of chemical products and processes that reduces or eliminates the production of hazardous substances. The use and production of these chemicals may involve reduced waste products, nontoxic environments, and improved efficiency. Green chemistry is a highly effective approach to pollution prevention because it applies innovative solutions to real-world environmental situations.
5. Non-academics now constitute roughly 50% of university staff in the US and UK.

**DONALD W. BRABEN** Following 16 years as a researcher in nuclear structure and high-energy physics, Braben was at the Cabinet Office in Whitehall, the Science Research Council in London, and the Bank of England. In 1980, he created and ran Venture Research, an initiative sponsored by British Petroleum that was fully operational for ten years (1980-90), and ran until 1993. He is now Honorary Professor in University College London’s Department of Earth Sciences and also in the Office of the Vice Provost for Research. In addition to papers on nuclear and elementary-particle physics, and articles on science policy, he is the author of the new book, *Promoting the Planck Club: How defiant youth, irreverent researchers and liberated universities can foster prosperity indefinitely* (2014), as well as *To Be a Scientist* (1994), *Pioneering Research: A Risk Worth Taking* (2004), and *Scientific Freedom: The Elixir of Civilization* (2008).

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On Social Psi and the Fatima Apparitions

It is difficult to support Eric Ouellet’s attempt to explain the Fatima apparitions via the Model of Pragmatic Information (EdgeScience 18). The social-psi hypothesis is an expansion of William G. Roll’s finding that RSPK may be due to living agents. That Mararian apparition is the product of social-psi is untenable; however, higher-order apparitions may be leaving a trail in social psi.

True apparitional occurrences have three components: non-physical (apparition), physical (apparitional experience), and social events. Social-psi/collective faith contributes to the social events, but several features at Fatima point towards a true apparition. The events had a preparatory phase: an angel manifesting to percipients three times before Our Lady appeared (1916–17). It was a collective experience and cannot be explained by the telepathic theory (Stevenson, 1982). Collective percipience, the apparition’s quasi-physical features, the apparition’s strong motivation, and the percipients’ loyalty and obedience to the psychokinetic-like manifestations point towards an apparitional manifestation.

How could fulfilled predictions be a social psi anomaly? Those most involved at Fatima were people who could not foresee world events reliably. When our lady mentioned the conversion of Russia, Sister Lucy even thought that Russia was a prostitute as she had never heard the name. Among the fulfilled Fatima predictions are these: the short lives of Jacinta and Francisco; the long life of Lucy; the end of the First World War; the global spread of communism; the Second World War; Papal suffering; and the (partial) conversion of Russia.

Non-survivalists have considered ESP combined with psychokinesis an alternate explanation for all generic apparitional experiences. In Mararian ones, the percipients’ ESP powers are presumably enhanced by the apparition’s PK-like powers. Super-ESP powers plus some form of refined PK power may hypothetically result in auto-apparitional sightings. The social-psi hypothesis brings the psi faculties of witnesses, naive and critical observers, and the local community into the equation along with the percipients’. Refined PK stipulates intense ESP monitoring of the outcome (Braude, 2003) and poses huge practical problems to facilitate the occurrence of collective apparitional experience. The super-psi and social psi hypothesis do not fit with three percipients being simultaneously involved, and it is difficult to explain the apparent externally controlled psychokinetic-like manifestations.

One of the Fatima witnesses confirmed the external inaudibility of the visionaries’ voices. Maria Carreira (Walsh, 1947) reported that when the children started running, they began to hear something like a high-frequency voice, but no one could understand a word. It was reported that while Lucia saw, spoke to, and heard the Lady, Jacinta saw and heard her, and Francisco simply saw her, but they could not hear each other (Johnston, 1980)—this may be termed “internal inaudibility.”

The voice blackout of Mararian apparitions may be an RSPK-like activity, arguably controlled by the apparition (Pandarakalam, 2006). Parapsychologists observe and report on RSPK action on physical objects. Sound production is a physical phenomenon and the vanishing of voices is an externally generated and controlled psychokinetic-like phenomenon or transcendental psychokinetic-like activity.

Within the social-psi hypothesis, we must conjecture that the visionaries’ super-psi in combination with social psi generated an identical auto apparition and their ESP powers constantly monitored the voice phenomenon. Such a multi-process social psi and super-psi has to take place without invasion of the witnesses’ mental activity. There is no probability of such orchestrated PK activity in Fatima. If multi-process social and super-psi of percipients and witnesses is implicated, which agency coordinated this? Exclude an apparition of a higher order and we have a psi-orchestra without a conductor.

The partisans of social and super-psi have to identify a master trainer who enabled the visionaries to develop higher psi faculties. They were unaware of such things. Their experiences were spontaneous, initially in the form of angelic visions, and they demonstrated no unusual psychical faculties. Are we to attribute superhuman powers to the visionaries and witnesses? I contend that where collective and controlled RSPK-like activity is involved, even the presence of social and super-psi does not exclude apparitional manifestation.

If we accept the ESP-PK hypothesis of apparitions, we may conjecture that an inter-actionist psi is involved in Mararian apparitions and nonverbal cognitive exchanges between apparition and percipients (Pandarakalam, 2006). There is no evidence that percipients’ psi/super-psi controls the apparitional events, but it may aid visionary experiences. The percipients’ selectivity, their unique and matchless voice phenomena, the fulfilled predictions made to percipients and aerobic solar phenomena (October, 18th 1917) as predicted in advance argue against the social and super-psi hypothesis of living agents.

The late Dr. Ian Stevenson identified several features that distinguish RSPK due to living agents and discourse agents (Stevenson, 1972). The term RSPK, when used in cases involving discourse agents, may be a misnomer as it simply delays the search for a convincing explanation (Broughton, 2002); use of the terms “RSPK-like activity” and “PK-like activity” is justifiable. Social-psi may be a good tool to explain away several large-scale anomalous experiences, but I think Ouellet has chosen the wrong example. Using the social-psi hypothesis to explain Fatima apparitions would be a canibalization of Mararian mysticism.

In the ongoing apparitions of Medjugorje (1981 to date), the apparition states that she is manifesting there to fulfill her Fatima promises—a period of peace. The apparition, who self-identified as the Queen of Peace, seems to target a build-up of collective faith/collective social-psi—to complete her peace mission.

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REFERENCES:

*Dr. Eric Ouellet replies*: I would like to thank Dr. Pandarakalam for his letter, as intellectual debates are useful and necessary for the advancement of scientific knowledge. However, I do not agree with the arguments proposed in his letter.

The notion of “true” apparition is rather simplistic and out of phase with research in parapsychology. As Nandor Fodor noted already a long time ago, spontaneous macro psi events are never “pure” and are always a mixture of anomalies, hoaxing, misperceptions, or make-belief. Research in parapsychology also tends to show that strong and sincere beliefs, and this includes religious faith, are an important factor in enhancing the probability of having a psi event.

In the case of Fatima, there was clearly a sociological dynamic that led to a particular religion-based social construction attached to the anomaly. Such construction was very much woven into the political dynamics of 1917 Portugal between the Roman clergy and the republican authorities. One side was stronger and won. This does not mean that such interpretation is necessarily unanimous or even widely shared; it just means that a particular interpretation of the events became the dominant one because of the social power behind those who were supporting the Marian interpretation. The individual accounts on the last day at Fatima show a rather wide variety of perceptions and interpretations. Furthermore, collective percipience is just what it is, a collective perception. In itself it does not confirm the source or origins of the anomaly. As with individual psi-related events, how the situation is interpreted by the percipient is not confirmation that such interpretation is correct. RSPK events can be construed as demonic manifestations, but such interpretation does not make it so. The same can be said for the so-called alien encounters linked to UFO incidents.

For matters related to premonitions, once again there is no obligatory need for a non-human entity to be involved. As well, many of those premonitions were interpreted as they were not sent in the “clear” (symbolic visions). A number of them were already discussed among educated people at the time (end of the war, Russia to sink into socialism, further conflicts in the future, etc.). Some of those interpretations were also produced after the fact. Pre-cognition is a known phenomenon in parapsychology, and technically only requires someone with psychic abilities. A social psi fervor may have improved the conditions for such pre-cognition, although this is not the gist of my paper.

The key issue, however, is thinking about psi only in individual terms. This is a problem particular to parapsychology, as its mother discipline is psychology, which essentially conceptualizes everything in individual terms. This is an ontological problem with epistemological consequences. The concept of social psi is actually designed to de-personalize some psi experiences, and to provide a sociological level to understand psi. A personal psi event could co-occur with an impersonal one. There is no logical reason why it would be impossible. To make a comparison, if someone is part of a demonstration against some governmental policies, people are collectively angry. And yet, one or more individuals in that crowd can have a nice chat with one of the riot police officer because they know each other. Others will stop to buy a coffee and have a discussion about the weather with the cashier. The same demonstration can be overall quite calm and non-violent, and yet have some hooligans causing troubles in the margins. Human reality exists concurrently at several levels, individual, small groups, large groups, nations, etc. If psi is a human reality, then it should co-exist at different levels, too. The events of Fatima were both very personal to the girls and some of the faithful, and yet could have had an impersonal anomalous dimension at the very same time. One does not preclude the other. To use individual-psi examples to declare collective psi impossible is epistemologically untenable.

Hence, super-psi and social psi should not be confused. Super-psi is still related to individuals and their role in the psi events as individuals. To be clear, social psi describes psi events that are not individual-based; they are experienced by individuals as impersonal events (namely, felt as imposed on them by an external force). The impersonal nature of social psi is certainly a direct challenge to proponents of the existence of non-human entities engaged in human affairs. The centrality of their argument is built on the notion that such anomalous events are experienced as impersonal forces, which would therefore constitute some sort of proof for non-human entities’ responsibility. But they have no answer for this logical gap: an impersonal force does not require the existence of a super-personal entity. Sociologists have long figured out that social forces affecting us as an impersonal force are a fact of large group dynamics. Why should large-scale anomalies be any different?
This book describes an accomplished scientific revolution which, however, and as usual, awaits recognition by the mainstream. Water, it turns out, does some extraordinary but well-attested things that have never been explained and which have been largely ignored for many decades. Gerald Pollack studied these anomalous phenomena in detail and presents explanations that stem from radical new insights. Thomas Kuhn’s description of scientific revolutions applies perfectly here: Anomalies are ignored by the mainstream. Their resolution requires a fundamental change of mindset. The mainstream does not engage because it thinks so differently (the new and the old theories are “incommensurable”). Time has to pass before the mainstream incorporates the new understanding.

The conventional wisdom acknowledges that water has some unique properties: very high surface tension, very large latent heat, and that the solid phase is less dense than the liquid. All these are explicable as consequences of uniquely strong hydrogen bonding between water molecules. I learned that many decades ago as I studied chemistry to the doctorate level. Then I carried on research on electrochemical phenomena in aqueous solutions for several decades, and had no occasion to doubt the conventional view—until I came across this book.

I had not known about some things water can do that are well-attested and long-known—but known only to those who are familiar with specialist literature, some of which dates to more than a century ago. For example, there is Kelvin’s water-dropper: Water drips from a container through two separate outlets into two metal beakers, each of which is attached to a rod ending in a metal sphere. The two spheres are placed near each other. After a while, a spark bridges the gap between the spheres, even though no electrical voltage or current has been applied. And, of course, everyone knows that pure water doesn’t even conduct electricity. Still, take two beakers of water whose lips are touching, apply a voltage across them through immersed electrodes, and a bridge of water will form between the lips and the beakers can then be slowly moved apart while the bridge remains, without even drooping, as the separation between beakers becomes as great as several centimeters. Explained by hydrogen bonding?

Start reading this book not at its beginning but at Chapter 1, where these and other astonishing phenomena are described, and you’ll be hooked. Little if any technical background knowledge is needed to follow the descriptions and explanations in this volume, but you may need to read it quite slowly, as I had to, because the basic insights on which explanations build are so unfamiliar:

**In the presence of any hydrophilic surface, water spontaneously undergoes a separation of charges, thereby storing energy that can be drawn off. Incident electromagnetic radiation provides the energy needed for the initial charge separation.**

These assertions seem so bizarre that I would have rejected them out of hand if the book had declared them at the outset. Instead, the text begins with evidence. Following descriptions of well-attested anomalies such as the water bridge and the Kelvin dropper comes an account of yet another extraordinary phenomenon. Inside a tunnel through a gel, place water filled uniformly with microspheres: After a while, the microspheres move to the center of the tunnel, leaving the space near the gel completely free of microspheres—they have been excluded from that space, which was therefore christened the “exclusion zone” (EZ) by early investigators.

The water inside EZs is unlike bulk water: For example, it is more dense, more viscous, it absorbs electromagnetic radiation at about 270 nm—and it bears a negative charge. It is less acidic than the solution outside the EZ.

That EZ water is unlike bulk water brings recollections of “polywater”—the claim, originally by Russian scientists but subsequently confirmed by others, that water in narrow tubes differs from bulk water, for example in being more dense and more viscous. Polywater was eventually dismissed as a mistake stemming from the presence of impurities leached from the glass walls of the capillary tubes, but Pollack cites personal sources to the effect that the distinguished Russian chemist, Boris Derjaguin, did not believe that contamination was the whole explanation, even as he agreed publicly with that explanation for political reasons.

Pollack infers that EZ water is composed of a stack of planar networks of water molecules interconnected in hexagonal arrays. Forming the necessary bonds ejects protons, which generate the hydronium ions that make the bulk water more acidic and leave the EZ less acidic as well as negatively charged.

The book’s argument becomes even more radical in Chapter 8, which explains how “like attracts like,” the very opposite of what everyone knows. Actually there is no contradiction: Spheres with negatively charged EZs surrounding them, suspended in water, attract one another. Even though their like charges do repel one another, the geometry of the charge in the liquid between the spheres brings the spheres...
closer together. This illustrates why the book cries out to be read *slowly*: Several phenomena are explained on the basis of unfamiliar axioms of the sort that “like attracts like” under particular circumstances.

My mind was further stretched as Pollack points out that the terms “heat,” “temperature,” and “energy” are ill-defined, ambiguous, and even mutually incompatible as encountered in common usage, including in the technical literature. Thus it requires energy input to bring order to water molecules as entropy decreases in the formation of EZs; yet EZs radiate less infrared energy than the bulk water, which would normally be interpreted as being at a lower temperature: What then happened to the input energy? Pollack discusses a wide range of phenomena in convincing fashion: Brownian motion, diffusion, osmosis, water as a lubricant; why car batteries regain a bit of charge after standing for a while; properties of clouds; radio transmission around the globe with only slightly attenuated signal strength; why “steam” comes off hot coffee in puffs; how bubbles form in liquids, and the exact and detailed mechanics of boiling; Kelvin’s water dropper, of course; the exact nature of water’s “surface tension,” explaining some astonishing structures found even in open ocean waters and to amazing depths. How water is able to rise hundreds of meters inside tall trees. Why warm water can be made to freeze faster than cold water, or why adult mosquitoes can survive the winter as “water memory” that homeopathy works because water can somehow “remember,” “retain” the structure of substances earlier dissolved in it.5–8

Chapter 18 reviews the chief tenets of Pollack’s insights:
1. EZs constitute a genuine fourth phase of water, not solid or liquid or gas, and perhaps best described as a “liquid crystalline” phase.
2. Water stores energy in the form of charge separation and ordered structure.
3. Water gains energy from light, electromagnetic radiation, and not only at those wavelengths where infrared radiation is strongly absorbed.
4. Likes attract likes via intermediate unlikes.

No further explanation is needed than those assertions, as to why Pollack’s insights have yet to become part of mainstream discourse. But several contributing factors are pointed to in the book:
1. The polywater episode left the conviction that any claims of unusual water structure and properties must be owing to impurities. The aftertaste of that 1960s episode was further strengthened circa 1988 and in subsequent years by claims of “water memory,” that homeopathy works because water can somehow “remember,” “retain” the structure of substances earlier dissolved in it.5–8
2. Water is so common, surely everything about it must have been understood long ago.
3. Scientists always resist startling novelty.
4. It is dangerous for scientists’ careers to follow unconventional paths.

I’ve corresponded intermittently with Gerald Pollack over some years, not about this work or this book but because of his interest in finding ways to fund non-mainstream research. This volume illustrates why such funding could pay enormous dividends.

This is a one-in-a-million book for learning entirely new things. It exemplifies the approach that the Society for Scientific Exploration stands for and wants to see manifested in the *Journal of Scientific Exploration*. It is a rare exemplar of truly empirical, evidence-based science. It is a book to savor, to read and re-read, to urge on your best friends.

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