Lessons
From History

"Crackpots" Who Were Right III

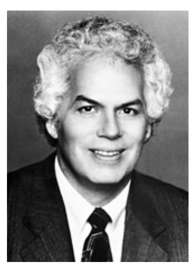
Philip E. Gibbs*

Abstract

This is the third instalment of the series entitled "crackpots" who were right. It is a collection of my postings at http://blog.vixra.org. The scientists covered are Stanley Prusiner, Georg Ohm, Robbert Goddard, Carl Woese, Galileo Galilei, Barbara McClintock and J Harlen Bretz. These are the scientists whose work eventually led to a paradigm shift in their discipline and in several cases the work was recognised with a Nobel Prize, though not always for the scientists who made the initial breakthrough. For every scientists who makes such a major advance in science there are many others who take smaller steps. Undoubtedly there must be many other independent scientists whose work was so completely rejected and ignored that it never garnered any recognition and has long been forgotten. Science suffers through such neglect and that is why we think viXra.org is so important.

Key Words: crackpot, who were right, Stanley Prusiner, Georg Ohm, Robbert Goddard, Carl Woese, Galileo Galilei, Barbara McClintock, J Harlen Bretz.

"Crackpots" Who Were Right 11: Stanley Prusiner



In 1982 Stanley Prusiner introduced the term "Prion" to describe a new kind of infectious agent formed from proteins. His hypothesis that diseases such as scrapie, BSE and CJD are spread by prions rather than a virus was met with extreme incredulity for more than a decade. In the 1990s he was proven right and his work brought him the Nobel Prize for medicine in 1997. His story is our most recent example yet of a scientist in the category of "crackpots" who were right.

Born in 1942 in Iowa, Stanley Prusiner followed a typical American education and graduated as a medical practitioner from the University of Pennsylvania. Already he was considering a career in biomedical research and after his internship he took up a residency in neurology. In 1972 he cared for a woman dying of Creutzfeldt-Jakob disease (CJD), a neurological disorder that was then attributed to the effects of a "slow virus" despite the observation that it evoked no response from

* Correspondence: Philip E. Gibbs, Ph.D., Independent Researcher, UK. E-Mail: phil@royalgenes.com

the body's immune system. Prusiner was struck with a keen interest in the unusual characteristics of CJD and its apparent relatives such as scrapie in sheep. In 1974 he set up a laboratory to study the diseases but funding was difficult to obtain because a number of very expensive programs to find the infectious agent had already failed. Prusiner persisted and was able to gain some NIH funding on the back of other work as well as some private sources.

When Prusiner and his co-workers sought to purify and isolate the agent transmitting the disease, all they could find was proteins. They had expected to see nucleic acids that would point to a virus but no trace could be found. In 1982 Prusiner published a manuscript in which he introduced the idea of a new type of infectious agent that he called prions to explain the lack of a virus.

At the time it was widely accepted that all infectious diseases were spread by a limited number of well-known types of infection that included bacteria, fungi and viruses. The power of the existing paradigm was so strong that other researchers could not admit the possibility of another cause despite the evidence that something very different was at work. Other researchers working on the same problem were very critical. Prusiner described the reaction in his Nobel autobiography:

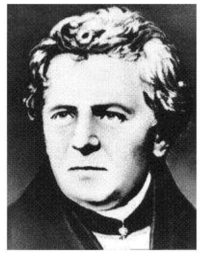
"As the data for a protein and the absence of a nucleic acid in the scrapie agent accumulated, I grew more confident that my findings were not artifacts and decided to summarize that work in an article that was eventually published in the spring of 1982. Publication of this manuscript, in which I introduced the term "prion", set off a firestorm. Virologists were generally incredulous and some investigators working on scrapie and CJD were irate. The term prion derived from protein and infectious provided a challenge to find the nucleic acid of the putative "scrapie virus." Should such a nucleic acid be found, then the word prion would disappear! Despite the strong convictions of many, no nucleic acid was found; in fact, it is probably fair to state that Detlev Riesner and I looked more vigorously for the nucleic acid than anyone else.

While it is quite reasonable for scientists to be skeptical of new ideas that do not fit within the accepted realm of scientific knowledge, the best science often emerges from situations where results carefully obtained do not fit within the accepted paradigms. At times the press became involved since the media provided the naysayers with a means to vent their frustration at not being able to find the cherished nucleic acid that they were so sure must exist. Since the press was usually unable to understand the scientific arguments and they are usually keen to write about any controversy, the personal attacks of the naysayers at times became very vicious."

Within a few years a new form of the disease in cattle known as "Mad Cow disease" was spreading rapidly with farmers in the UK most severely hit. Despite the lack of understanding of how the infection could be spread it was claimed that humans could not contract it from eating tainted beef. This was proven wrong when people who had eaten poor quality meat were struck down by CJD. With a growing fear that many more people would be struck by the infection, more funding for research became available and by the 1990s it was generally accepted that Prusiner had been right.

Today a better understanding of prions is being developed. It is now thought that they propagate by refolding incorrectly into a structure that is able to convert normal protein molecules to the abnormal form. Even now a small minority cling to the belief that there must be a virus involved and continue to attack the prion hypothesis. In today's science, old paradigms die hard just as they have done for centuries.

"Crackpots" Who Were Right 12: Georg Ohm



The name of Ohm is well-known to electricians and physicists because of Ohm's Law and the metric unit used to measure resistance; the Ohm. The Law states that the current through a conductor is proportional to the voltage applied leading to the equation V = IR, a simple rule that is easy to test with some batteries, some wires and a current meter. It is therefore quite surprising to learn that when Georg Ohm published his work in 1827 it was highly criticised for several years.

Georg Ohm, born 1787 in Bavaria, was a clever student who was largely self-taught in scientific fields and mathematics. At 26 he enrolled in the University of Erlangen. He did not take his studies very seriously with the result that his father forced him to leave and take up a poorly paid job as a teacher. However he persisted with his studies privately and received a doctorate in 1811

Realising that he was in a poverty trap that meant he could never marry, Ohm aimed to make his name in science with the hope that it would lead to a better paid position. He read the texts of the leading mathematicians and physicists of France and set about his own electrical experiments. At the time it was not so easy to produce steady voltages and accurate measurements so little was known about the flow of currents. Ohm carefully prepared samples of wire of constant composition and width and used a thermocouple to control voltages with heat. With a galvanometer he could measure the current flow through the wires as a function of voltage and length of wire. Details of the experiments were published in 1825.

Unfortunately, the scientific philosophy of the time led by Hegel in Germany did not value experiment as highly as theory. Ohm therefore sought to bolster his work with a theoretical derivation and he turned to the work of Fourier on heat conduction as a basis for his theory. The completed work was published in 1927 as "The galvanic Circuit investigated mathematically" We now know that quantum mechanics is needed to properly treat the theory of conduction. Even primitive classical models based on kinetic atomic theory were not accessible to Ohm at the time and Ohm's theory could never be more than a sham.

Ohm's masterly empirical work was ignored while his theory was torn apart. His harsh critics in germany called it a "web of naked fancies". Another reviewer said the book's "sole effort is to detract from the dignity of nature." The hardest blow came from the German Minister of Education who said that "a professor who preached such heresies was unworthy to teach science." Ohm had hoped for a new position as a university professor but now he was forced to give up even his job as a lowly paid school teacher. His life was in ruins.

For six years he fell on hard times until he once again took up teaching at a polytechnic. Few of his German colleagues ever took back their stiff critical words, but in America and England where experimental work was more highly regarded he gradually found acclaim. In 1843 the Royal Society awarded him the Copley Medal. At that point he might have seen better fortune at home but he tempted fate with theory of acoustics that led to a dispute with the German scientist August Seebeck. It was not until 1852 that he finally appointed to the chair of physics at the University of Munich. He died two years later. In 1881 his name was imortalised when the Ohm was officially adopted as the unit of resistance.

With hindsight, Ohm's poor level of recognition can be attributed to a number of unfortunate political and philosophical factors: The low regard for experimental science in his homeland, his status as a self-taught amateur, perhaps his reliance on the work of French mathematicians at a time of tension between Germany and France, and even some political wranglings involving his brother. But there is also a message from his story that is relevant today. Sometimes we may be distracted from seeing a great truth in a person's work because we are distracted by irrelevant errors. It is a lesson of importance for both the author and reviewer of any scientific work.

"Crackpots" Who Were Right 13: Robbert Goddard

Today the term "Rocket Science" is used as a metaphor for any kind of engineering endeavor that requires a mind of the highest caliber. In fact the standard technology for putting something into orbit is a liquid fueled rocket combining kerosene and liquid oxygen to propel a series of rocket stages, while using gyroscopes and steerable thrust to stabilise motion. It is remarkable then, that all these innovations were developed by one man working quietly without much support at a time when most people thought the idea of space travel was no more than a crazy dream to be found in science fiction. That man was Robbert Goddard.

As a young man in Massachusetts at the beginning of the twentieth century, Robert Goddard looked skywards and observed the flight of birds. At the time most people thought that controlled flight would never be a possibility for a man-made machine, but Goddard was one of those who thought differently, and he was no mere dreamer. Already he understood enough physics to work out the theory of flight. He was also an experimenter who had worked with kites and balloons to understand how flight could be possible. In 1907 at the age of 25 he published a paper in "Scientific American" about a method for "balancing aeroplanes".

His young age and lack of facilities meant that Goddard was not destined to contribute further to the achievements of early powered flight that were just getting under way. Instead he turned to idea that rockets might be used to take men much higher, even into space. In 1909 he made his first innovation when he realised that liquid fuels would be superior to solid fuels for high-powered rockets. While at Princeton a few years later he was struck with tuberculosis and returned to his home town. This gave him the time to turn his ideas into practice. In 1915 he launched his first prototype liquid fueled rockets.

Although his early flights did not get more than a few hundred meters off the ground, Goddard could see that the principle could be scaled up. In 1919 he published a book entitled "A Method of Reaching Extreme Altitudes" with the backing of the Smithsonian Institute. The book went far beyond what anybody else thought possible at the time. It culminated with the suggestion of an experiment to send a rocket beyond Earth's atmosphere and on to the moon where he hoped its impact could be observed through telescopes.

It is not surprising that such ideas were met with skepticism at the time, but the scale of ridicule and mockery from the US press in reaction to Goddard's work must have come as quite a shock. The New York Times was particularly vehement. The journalists thought that it would be impossible for a rocket to work beyond the atmosphere because it would have nothing to push against. They lambasted Goddard for his failure to appreciate basic high school physics. Of course it was them who were being ignorant.

The public backlash forced Goddard to retreat to more private research. despite very little funding he persevered and went on to develop many of the basic principles of rocket propulsion from both theory and experiment. While the US rejected his work, elsewhere in the world, and especially in Germany, others saw its value and quickly started to build on it. This gave the Germans a startling lead in rocket science that culminated with the V2 rockets launched against London during the war.

As the war ended Goddard died unaware that the Americans and Russians were secretly vying to capture German rocket technology. It was the beginning of the cold war which would be symbolised by the space race. Within less than 25 years military engineers with vast funds from government would go much further than even Goddard had dreamed. In 1969 Neil Armstrong stepped onto the moon. The technology used was scarcely different from what Goddard had proposed, except in scale.

Shortly after, the New York Times issued a correction to its editorials of 49 years earlier that had mocked Goddard:

"Further investigation and experimentation have confirmed the findings of Isaac Newton in the 17th Century and it is now definitely established that a rocket can function in a vacuum as well as in an atmosphere. The Times regrets the error."

Even within the apology the journalists show an incredible ignorance and arrogance. How could they truly believe that Newton's laws had not been understood or tested earlier? It raises deeper questions: why did no American physicists speak up for Goddard at the time? They must have seen the basic errors in the criticism against him. Did no scientist of authority think to write a letter to point out how Newton's laws worked? Were they really so scared on the power of the papers that they did not want to risk their reputation in defense of Goddard?

We shall perhaps never know the truth but we should not forget how dire the consequences nearly were. With a little more work on rockets the Germans would have had weapons of incredible power that might have led to a different end ot the war.

In 1959 the Goddard Space Flight Center became one of several facilities to be named in his honour, so that his legacy may live on in our memory for many years.

"Crackpots" Who Were Right 14: Carl Woese

The taxonomy of life first set out by Linnaeus is familiar to anyone who has taken a basic interest in natural history. At the finest level life on Earth is divided into species that are grouped together in genera. Higher level groupings define families, orders, classes, phyla and then kingdoms. The original system of 1735 Linnaeus listed just two kingdoms for animals and plants. With the invention of the microscope it was realised that there are also bacteria that do not fit into either of these kingdoms. They are distinct because their DNA is not held in a nucleus. Over time the taxonomy system was refined until the 1969 classification of Whittaker with five kingdoms became accepted by biologists.

Initially classification was based on simple morphology. Later the chemical composition became more important. For example, fungi were placed in their own kingdom distinct from plants because their cell walls contain chitin instead of cellulose. Then in the 1970s a few scientists started to look at the RNA in the cells. One of them was Carl Woese who wanted to use the technique to help classify bacteria. When he was asked to apply his method to specialised bacteria that live in extreme conditions he found that the RNA was so different from other bacteria that it was wrong to put them in the same kingdom. At first he called them Archaebacteria, but then he decided that a more radical change to the top levels of taxonomy was needed. In the new system there would be three domains, bacteria, archaea and eukarya with the latter containing the more familiar kingdoms.

Carl Woese was born in 1928 in Syracuse and still holds a professorship at the University of Illinois. His scientific training was more in biophysics than traditional microbiology so to some extent he was an outsider in the field he was revolutionising. As we have seen before it is sometimes such people who come along with a different outlook on a subject, untethered by the ruling dogma. These are the people with a mind open enough to shift the paradigm, but they should expect a lot of resistance from the authorities on the subject. This was certainly the case for Woese.

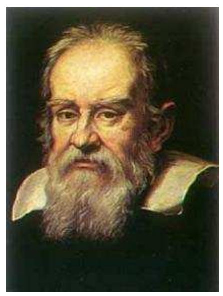
Although Woese's research was published in a top scientific journal, most biologists knew of it only through newspaper accounts. Unfamiliar with the new techniques used they immediately set about criticizing it. Nobel Laureat Salvador Luria openly derided any possibility of a three domain taxonomy believing it to be based on a classification game. Privately he contacted Ralph Wolfe, a colleague and supporter of Woese and told him " you're going to ruin your career. You've got to disassociate yourself from this nonsense!'" Others followed with a hostility that shocked Woese. Because they saw him as a physicist rather than a microbiologist they did not hesitate to call him a crank. They did not believe that the RNA studies he had carried out could be used to classify bacteria. They did not even bother to look at the data. Ernst Mayr, an icon of evolutionary biology could never accept the new conventions even when they became more widely accepted in 1990.

Woese was never allowed to defend his work on the conference circuits and his funding remained at a scathingly low level. Try as he might he could never get the money from the NFS Systematics panel increased even though with retrospect it was by far the most important research they ever funded.

Over time acceptance of his work grew and he started to collect some honours. Finally it was the possibility to sequence genomes that confirmed the verity of his work in 1996. Now it is widely understood and Woese has the recognition he always deserved. His work has been absorbed into the textbooks without any embarrassing mention of just how bitterly it was opposed for nearly twenty years. In some cases the name of Woese is conveniently left out when his work is described by those who attacked it for so long.

The taxonomy of the simplest lifeforms is tied into the study of early evolutionary biology and is a field that continues to develop. The groupings into domains and kingdoms will no doubt be modified again, but the work and techniques of Woese that recognised the differences of archae will remain as a revolutionary step that advanced microbiology to its modern form.

"Crackpots" Who Were Right 15: Galileo Galilei



I have tried to keep to recent and relevant cases in my stories of "crackpots" who turned out to be right, but to celebrate one year of viXra I'm including a much older one. The plight of Galileo Galilei is well-known so I'll be brief, but it is always worth retelling. Actually it is the story of a number of heretics and a hundred years of history that saw centuries of dogma finally pushed aside.

The event that made it possible was undoubtedly the invention of the Gutenberg printing press in 1450. Before then any book had to be copied laboriously by hand which meant that scientific knowledge in Europe was controlled by a small and elite group based around the church. The possibility to print mass copies of books meant that new ideas could be distributed rapidly and beyond the control of the select few who wished to censor it. (I like to think that viXra.org plays a similar role today in its own small way.)

Less than a hundred years later one such book was printed in the name of Nicolaus Copernicus. "*De revolutionibus orbium coelestium"* was about the revolution of the planets, but it also started a revolution of scientific thought. Copernicus had formulated his idea that the Earth and planets revolved around the Sun 30 years earlier but he was reluctant to publish openly until near the end of his life. It seems he need not have worried. The heliocentric theory was at first welcomed by the church and by scholars despite its potential contradictions with catholic teachings. These were happier times before the unsettling influence of the reformations took hold.

Until then cosmology was based on the ancient work of Aristotle and Ptolemy who taught that the Earth was fixed at the centre while the sun, moon and planet revolved on system of celestial spheres. After Copernicus the first direct evidence that this could not be quite right came from observations of comets by Tycho Brahe. In 1577 he noticed that comets passed through the spheres, something that was not supposed to be possible. Further cracks opened when Giovanni Benedetti attacked Aristotle's laws of motion. it had been believed that all bodies tend to a natural state of rest but Bendetti noticed that a law of impetus better fitted observations. In 1592 this helped Galileo formulate his principle that the laws of physics in the heavens was the same as on Earth. A year later Johannes Kepler published his astonishing observation that the motion of the planets was best accounted for in a heliocentric system with the planets following elliptic orbits.

So far there had not been too much controversy as the old scientific dogma fell away, but that was to change dramatically. In 1600 Giordano Bruno was burnt at the state for his heretical view that the stars are distant suns with other Earth's in orbit. The revolution had gone too far for the church's liking.

When Galileo famously turned his telescope to the night sky he saw things that would finally overturn the old dogma. Jupiter had moons that revolved around it. The surface of the moon was scarred with craters. The sun, thought to be a sign of perfection, was blemished with dark spots. Hostility towards his ideas grew and Galileo knew he had to tread carefully, but he also had to speak up for truth. His book *Dialogue Concerning the Two Chief World Systems* published in 1630 led to him being brought before Pope Paul V. He was deeply suspected of heresy but a partial recant saved him. Nevertheless, he spent the rest of his life under house arrest watched closely to ensure that no more of his dangerous ideas could be published. Of course it was too late. The genie of truth was already freed and with the wide distribution of printed books it was no longer possible to rebottle it.

Many people see Galileo's battle as a fight against religion. It is possible to find passages in the bible that say the Earth is fixed, an idea that Galileo contradicted and which was central to the case against him. However, I see his struggle as something directed against a scientific dogma that happened to be firmly tied to the authority of the church. Many of the people involved in overturning it were themselves priests, cardinals and bishops and they did not seem to see any contradiction with their beliefs. It was only when the authority of the church over scientific teaching was threatened that opposition arose. In this sense it is not so different from most of the more modern stories we have encountered of "crackpots" who were right.

"Crackpots" Who Were Right 16: Barbara McClintock

All 15 scientists featured in this series so far have been men and it has been very hard to find a women that fits the criteria for a "crackpot" who was right. Of course there are plenty of cases of women who have found it difficult to gain acceptance as scientists, but here I am looking for something else; scientists whose work was regarded as wrong for many years before it was eventually found to be correct. The lack of women in this situation could be regarded as a good or bad sign for women in science. Is it just a reflection of the general problems that women have had entering into the male dominated domain of science, or is it because when they do succeed their work is more likely to be accepted? Perhaps they have just avoided more controversial subjects, or maybe there is some other factor at play. Whatever the correct explanation, I'm glad at least to be able to document one example to prove that it is possible for women to do science that is so revolutionary that it is at first treated as a crazy idea.

There have been only two women who won a Nobel prize in physics. In chemistry the count is four but in medicine there have been ten female Nobel laureates. Barbara McClintock is one of those ten. By 1940 McClintock had already become recognised as an accomplished geneticist since graduating with a PhD in botany from Cornell University in 1927. Working mostly with Maize she used microscopic analysis to demonstrate many fundamental genetic ideas such the recombination mechanisms by which chromosomes exchange information. Early in her career her advisor Lowell Randolph became very irritated when she solved a problem he had been struggling with for his whole scientific life. McClintock had to depart when Randolph could not tolerate her success. This was not atypical of her relations with her colleagues. As one of the best in her field it is difficult to account for her failure to find a tenured position for so long, unless it was her gender. Finally nothing could stop her being elected a member of the national Academy of Sciences in 1944. After several unsettling moves she found a place at the Cold Spring Harbor Laboratory and stayed.

Even then her best work was ahead of her. In the 1940s she discovered the process of transposition in which sequences of DNA can move to different positions within the genome causing mutations. It was for this work that she was made a Nobel Laureate in 1983, but it is a later series of discoveries that led to controversy.

From 1948 she investigated the role of Activators and Dissociators in the DNA sequence. She observed that these elements could control the transposition of genes. This discovery ran against the concept of the genome as a static set of instructions passed from generation to generation. She published her ideas in 1953 and undertook lecture tours to speak of the work, but the reception was one of puzzlement and hostility. It was in that year that the double helix structure of DNA was discovered, changing the way people worked in genetics. Sensing that she risked alienating the scientific mainstream and damaging her career, she stopped publishing such work and moved onto other things.

In 1961 two French geneticists Jacob and Monod discovered the genetic regulation of the lac operon. McClintock subsequently published an article showing that the mechanism was similar to her work on controlling elements. In 1973 she described the situation,

"*Over the years I have found that it is difficult if not impossible to bring to consciousness of another person the nature of his tacit assumptions when, by some special experiences, I have been made aware of them. This became painfully evident to me in my attempts during the 1950s to convince geneticists that the action of genes had to be and was controlled. It is now equally painful to recognize the fixity of assumptions that many persons hold on the nature of controlling elements in maize and the manners of their operation. One must await the right time for conceptual change."*

Even now not everyone accepts that hers was a prior discovery of genetic regulation. Some people even prefer to say that the work of Jacob and Monod proved her wrong!

"Crackpots" Who Were Right 17: J Harlen Bretz

At the beginning of the 20th century, the prevailing view amongst geologists was that all features of the Earth developed over millions of years in gradual processes of erosion and rock-forming. For most geological features this is indeed true, but today we know that there are other important processes of change that can take place over just a few years or even days. These include volcanic eruptions and meteoric impacts that shaped the land and had dramatic consequences for life on earth and the process of evolution.

The first evidence that such sudden changes could take place was presented in the 1920s by J Harlen Bretz. After careful exploration he realised that the unusual topography of the Scablands in North-Western America could only be explained by a sudden catastrophic flow of flood water across the landscape.

Bretz was originally trained as a biologist and worked as a highschool teacher. Later he moved to geology in which he earned his PhD. When he first published his theory in 1923 he was pitting himself against the established works of geologists supported by the authority of respected Ivy-League professors. The idea was quickly labelled as outrageously wrong and his opponents set to work to discredit it.

Another geologist, Joseph Thomas Pardee who worked for the US Geological Survey had identified a large glacial lake that had occupied a region of Montana known as the Missoula Lake. Pardee had written a paper that explained the Scablands as glacial erosion.It was just the kind of formation theory that the geologists preferred, but Bretz looked more closely at the geology and realised that it could not be true. When Pardee heard the evidence he agreed and wrote in support of Bretz. HE thought that the Missoula Lake could have been the source for such a flood, but others would not hear of it. Pardee was dissuaded from supporting Bretz. Under threats to his own livelihood from his employers he had no choice but to be quiet.

The matter came to a head when Bretz was invited to present his theory at a public forum to the Geological Society of Washington in 1927. Bretz was unaware that six authoritative geologists had been lined up to oppose him with the objective of utterly humiliating him and his theory. Bretz went well prepared and defended his case with good evidence from the field. He pointed out that the channels and bars of the area were too large to have been carved out by the Columbia river over millions of years as others thought. Only a sudden great wave of water could be responsible. But all of Bretz's potential supporters had been silenced and the debate inevitably went against him.

The conclusion stood for another 13 years before a field trip was organised that was designed to establish the erosion theory beyond any doubt. Before then Bretz himself was the only one who had explored the Scablands as a geologist. Bretz was invited to contribute but his disillusion after so many years of ridicule kept him away. Following the trip a meeting was organised for the geologists to present their findings. Seven geologists described how the landscape fitted in with their uniformitarian views, even though these were not always consistent. Finally Pardee stood up as the eighth geologist and quietly described gravel ripples in the area that stood 15 meters tall and 150 meters apart. They could only have been formed by a vigorous flow of huge amounts of water. The development and end of the ice ages was beginning to be better understood and Pardee believed that the ice of the Missoula Lake has melted but had been held back by an ice dam. When the dam finally broke some 2000 cubic kilometers of water broke free in one catastrophic event, sweeping through the landscape. Pardee stopped just short of making the obvious conclusion that the flood was the cause of the features of the Scabland. Perhaps he still felt intimidated by his colleagues, or perhaps he just wanted to leave it to others to acknowledge their error themselves. The game should have been up there and then as the evidence started to fall into place. In fact the debate continued for some 30 more years before veryone could be convinced.

It was not until 1979 when Bretz was in his 90s that he was at last publically recognised by being awarded the prestigious Penrose Medal.

The most thought-provoking aspect of the case of J Harlen Bretz is the extent to which geologists ganged up against him and tried to publically humiliate him. They used heavy tactics to ensure that anyone who might have supported him was silenced. When we look back today we see this as shameful behaviour. In my view this is very similar to the tactics used by the arXiv today where they sideline work they don't like into archive categories that they euphemistically label as "general physics" and "general mathematics". Everybody knows that this is done to imply that the articles are of no value and it is used as a system of ridicule. Furthermore the arXiv uses public threats directed at their endorsers designed to stop them helping outsiders who want to submit to arXiv when their work may not agree with the prevailing consensus. Of course this is even more true where people want to label papers in viXra as the work of cranks and dissuade anyone of submitting to viXra if they want their work to be taken seriously. In my opinion people will one day look back on this kind of behaviour as both harmful and shameful. The only difference with the attempts to suppress the work of Bretz is that they attack a whole community of independent scientists rather than just a few individuals.