Lessons
From History

"Crackpots" Who Were Right

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Abstract

I'm going to run a series of posts at http://blog.vixra.org under the heading: "crackpots" who were right. It is surprising just how many times people have published ideas in science that were initially rejected by their peers simply because they went against the accepted wisdom of the time. These people submitted their work to journals only to have them repeatedly rejected with comments from the referees stating that the author simply could not be right. In all the cases I will mention, the idea has eventually been accepted, sometimes after many years and often only when another more influential scientist rediscovered it. Happily the original discoverers were not forgotten and are now recognised, but it is not just the matter of recognition that is of concern. The failure to evaluate the work correctly at the time has lead to delays in the progress of science that can last for decades.

Key Words: crackpot, Boris Belousov, Alfred Wegener, Ernst Stückelberg, Ignaz Semmelweis.

1. Introduction

I don't doubt for one moment that there are many other scientists with similar experiences whose work was forgotten and who did not get their place in the history of science that they deserved. Of course I cannot give examples and write about them because I don't know who they are. Preprint archives provide one way to ensure that in the future such scientists have the opportunity to be known about later when their work is re-evaluated. That is why such archives should be open and un-moderated rather than judging ideas on the preconceptions of the day when the work was done. viXra.org is one of the few general science preprint archives that adheres to this principle.

Of course many ideas that are described as "crackpot" will never turn out to be right. Some of them are obviously wrong from the beginning and are right to be rejected by scientists. I point out this obvious fact only because if I don't then other people will mention it as if we at viXra.org can't see it. The problem is that there is no clear line between the obviously wrong ideas and the crazy ideas that could just be right. If we tried to draw such a line we would either be to conservative and keep a few ideas that could never work, or we would be too harsh and risk rejecting something that actually has something valid in it. The solution we adopt at viXra.org is to reject nothing unless it does not even try to make a scientific statement or where there are potential legal issues.

The surprise is that most of the articles submitted here have a lot of good substance to them. Often they are of very good quality and it is not obvious why they would not be acceptable in other archives such as arXiv.org. In fact many of the articles in viXra.org have been accepted for publication in peer-reviewed journals. It seems clear to us now that archives such as arXiv.org have set their submission criteria on a line that excludes many good works of science. They assume that

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these people will find some other way to record their idea and often suggest submission to a journal. As my series of articles will show, this is not always possible. We are confident that one day the "crackpot" who was right will be someone whose contribution is recognised because they submitted their work to us.

2. Boris Belousov

In 1951 Borsi Belousov wrote a manuscript that opened a whole new field of chemistry with profound implications for physics and biology. Eventually the research would lead to a Nobel prize, but not for Belousov. His manuscript was rejected by the journals and by the scientific community worldwide. Belousov quit science, discouraged by the reaction from his peers and the early development of the subject was delayed for years.

Boris Pavlovich Belousov was a Soviet chemist who started his work after a distinguished military career. At the Laboratory of Biophysics in the USSR Ministry of Health he began to study the chemistry of reactions related to the extraction of energy from sugars in biology. While seeking an inorganic version of the cycle he stumbled upon a remarkable reaction that oscillated between states with different colour under only the constant influence of stirring. Astounded by the result he repeated the experiment very carefully while varying parameters such as concentrations and temperature to document how the reaction rates changed. His results were written up and submitted to a Russian Journal of Chemistry. At that time it was known that the rates of some reactions could vary but there were seemingly solid arguments that no reaction could oscillated in such a manner. The journal rejected the manuscript out of hand with the assertion that it was physically impossible so he must have made an error. Belousov made one more attempt to submit his article to peer-review but the result was the same.

Luckily a biochemist Simon Schnoll came to hear of Belousov's work and persuaded him to submit to an obscure non-reviewed journal to ensure that the work would be recorded. Had he not done so we may never have heard of this seminal research. Even as it was, the development of the subject was delayed by several years. Schnoll assigned a project to one of his graduate students Anatol Zhabotinsky to reproduce the reaction, which he did. It was too late for Belousov who has been so discouraged that he had ended his research. Even while the reaction was being studied in further detail in Russia, Western scientists continued for years to publish refutations. Instead of trying to replicate the result they simply claimed that the reaction was not consistent with the laws of thermodynamics and that some outside contamination must be affecting the results. Their arguments were wrong because they assumed that the reaction reached a stage of thermodynamic equilibrium, but of course it did not. Eventually evidence for the phenomena became overwhelming and was studied in great detail. Similar reactions became the basis for the study of self-organisation in biology and were a key influence on the study of chaotic behaviour in dissipative structures. In 1977 another Russian chemist Ilya Prigogine received the Nobel prize in chemistry for work in this field, seven years after Belousov's death. Three years later Belousov was posthumously awarded the Lenin prize for his work.

3. Alfred Wegener

The story of Alfred Wegener and his theory of continental drift is one of the most cited instances of an outsider who proposed a radical theory that was dismissed by the experts in the field. Of course he turned out to be right. Wegener was a conscientious scientist who had gained a doctorate in astronomy, but he was also a daring explorer who made expeditions in the arctic and held the record for the longest hot air balloon ride. This meant he observed the geology of the Earth first hand but he was not a trained geologist influenced by the favoured theories of the day.

Some time before 1903 he had noticed that the coastline of the American continents matched the shape of Africa and Europe in surprising detail. His theory based on this was simply that the continents had once been joined together in a supercontinent he called Pangea. In fact this had been remarked upon by many others before him and there had been plenty of theories to explain it. Some had thought that the earth had originally been fully covered in a crust, but the earth expanded and it broke apart with water filling in the cracks to form the oceans. Superficially such an idea looked right at the time, but science requires proper investigation based on theory and observation and the expanding earth theory just did not hold up.

If Wegener had just stopped there he would have been just one of many people with the same idea but he started to look for evidence. He noticed that the geology of the continents actually coincided at the points where he imagined they had broken apart, even to the extent that coal seams on either side of the Atlantic can be matched up. His confidence in his theory was boosted.

Another unsolved problem of the day was how animal life on Earth had spread to continents that seemed disconnected, especially Australia. From the fossil record it seemed that similar species existed on different continents at the same time as if they had somehow crossed over the wide ocean. Wegener saw this as strong evidence for continental drift but two other competing theories sprang up. One said that there had been land bridges that joined the continents before collapsing. The third theory was that the continents had not changed much at all, and the animals had spread via existing land routes that were sometimes frozen over. Some similarities were attributed to convergent evolution or just plain coincidence.

With hindsight we can see that Wegener had the best evidence in his favour, but he was not regarded as an expert in geology. The people who were regarded as experts were not ready to accept the new idea and so they attacked it. They criticised continental drift on the grounds that the land could not float on the ocean crusts as if it was a fluid. Indeed Wegener did not have a fully formed theory of how continental drift worked but he had considered it beyond the point at which he was being attacked. He was aware of the mid-ocean ridges and suspected that the oceanic crusts were spreading out from there, as indeed they were.

Some of the attacks on Wegener were quite vehement. His theory was called preposterous, antiquated, a serious error, footloose and dangerous. He won support from some lesser geologists but his opponents were considered the authorities and no amount of evidence or reason was ever going to convince them at that time. Wegener died young when an Arctic expedition turned to tragedy in 1930. After that, little progress was made until the 1950s when people started to look at how rocks were magnetised. This provided almost indisputable evidence that the land masses had moved and in 1953 Samuel Carey developed the theory of plate tectonics that finally explained the mechanism behind continental drift.

The moral of this story is that the experts in a subject are not always the best authorities. Sometimes they are too versed in current theories to see the truth of a new idea even when the evidence comes up in its favour. Of course this does not mean that every crazy idea is going to be right, most are not, but ideas have to be judged on the best observational evidence and not on dogma. This is why when you learn something you should always question it. Just how good is the evidence? Don't accept it because your teacher says it is right, but don't reject it just because you don't understand it either. The truth lies in reason and evidence and the mainstream view is sometimes still open to question. When new observations come along they sometimes show that earlier accepted ideas were wrong. Often we are left wondering why we were so sure of those previous ideas in the first place. The answer is sometimes just because they were written in the textbooks.

4. Ernst Stückelberg

Baron Ernst Carl Gerlach Stückelberg was one of the most accomplished theoretical physicists of the middle twentieth century. He ranked alongside such greats as Feynman, Dirac and Fermi, but you could be forgiven for not knowing it. His name appears in physics text books only when attached to some relatively minor phenomena such as the Stückelberg mechanism. Even in popular physics books that recount the glorious history of that golden age of discovery in physics, he is rarely mentioned. Yet Stückelberg made prior breakthroughs in at least three developments that led to Nobel prizes for others, and he contributed to a wide range of other research topics in particle physics and quantum theory.

Here is a short list of some of his greatest achievements (taken from Wikipedia):

» 1934: He devised a fully covariant perturbation theory for quantum fields that was more powerful than other formulations of the time.

» 1935: He gave vector boson (meson) exchange as the theoretical explanation of the strong nuclear force. This is normally credited to Yukawa who discovered it independently at around the same time, and who was awarded the Nobel Prize.

» 1938: He recognized that massive electrodynamics contains a hidden scalar, and formulated an affine version of what would become known as the Abelian Higgs mechanism.

» 1938: He proposed the law of conservation of baryon number.

» 1941: He presented the evolution parameter theory that is the basis for recent work in relativistic dynamics.

» 1942: He proposed the interpretation of the positron as a negative energy electron traveling backward in time, an observation often attributed to Feynman.

» 1943: He came up with a renormalization program to attack the problems of infinities in quantum electrodynamics (QED). This was a precursor to the fully renormalized theory of QED completed in the 1940s which netted a Nobel prize for Feynman, Schwinger and Tomonaga.

» 1953: He and Andre Petermann discovered the renormalization group, but it was Kenneth Wilson who took the Nobel Prize for work that demonstrated its full worth in critical phenomena.

So why is Stückelberg not more widely recognised for these achievements? There seems to have been a number of factors at work. Firstly he had some bad luck with publications. He did not publish his work on the meson simply because Pauli said it was ridiculous. His work on the renormalization program was rejected by the Physical Review who said it was more of a program outline than a paper. Sadly no copy of this work was preserved. He is said to have gone on to develop a full theory of QED by 1945 which is recorded in the thesis of one of his students but the credit went to others.

Another element may have been his isolation in Switzerland before and during the war when he did some of his best work. However this seems unconvincing when you consider that he established good friendships with other well-known physicists of the time. He could be considered less isolated than physicists working in Japan such as Tomonaga whose work on QED was recognised later. One other contributing factor that is given part blame for his lack of credit is that he invented unusual notation for his work that made it difficult to read.

Whatever the cause, he ended his life feeling lonely and rejected. When Feynman gave a lecture in Switzerland in 1965 he spotted Stückelberg after the lecture leaving quietly from the back. Pointing to Stückelberg, Feynman remarked "He did the work and walks alone toward the sunset; and, here I am, covered in all the glory, which rightfully should be his!"

The story of Stückelberg shows just how easy it is to be overlooked in science. There is no convincing reason why he was not given the full credit he deserved for his work, but it would have helped if he had presented his work more clearly and fully. While people like Feynman gave seminars and wrote books, Stückelberg seems to have quietly accepted his rejections and left it to others to speak up for him. But that was something they did not do enough. There is a lesson to be learnt here. Most of us cannot claim achievements comparable to those of Stückelberg so if he can be overlooked the rest of us should take nothing for granted. It does no good to make a discovery and bury it so deep that nobody pays any attention until it is rediscovered by someone else who is better at presenting it. Research needs to be explained clearly and publicly or it sinks into obscurity..

5. Ignaz Semmelweis

Like many people these days I have experienced the thrill of tracing my ancestors using some of the online resources and public archives available. In my case a large number of my ancestors that I can trace lived in Victorian London and in following their lines I am struck by the high mortality rates, especially among children and mothers in childbirth. It is particularly sad to learn that a significant number of those deaths could have been prevented if medical practitioners had paid attention to the work of Ignaz Philipp Semmelweis. That makes this entry in our series about "crackpots" who were right the most shocking case that I am aware of.

Medical knowledge in the early 19th century was very limited. The theory of diseases spread by germs was not understood until after the work of Louis Pasteur from 1864 and effective treatments for infections were not available until the discovery of the medicinal effects of penicillin much later. The leading theory of diseases was dyscrasia based on the ideas of an imbalance of the basic "four humours" and the usual treatment was bloodletting or extreme forms of hydrotherapy which often did more harm than good. It was thought that disease was spread by bad air until the 1854 Broad Street cholera outbreak when John Snow identified contaminated water as the source of the cotangent. Such advances dramatically improved the prevention of diseases, but an earlier discovery could have saved many more lives in London and other cities if it had been accepted more widely.

In 1847 Ignaz Semmelwies was a physician working at an obstetrical clinic of the Vienna General Hospital where his duties included inspections, teaching, supervision of difficult cases and record keeping. When he took on his responsibilities the clinic had a particularly bad record for maternal mortality due to puerperal fever which was causing the death of 10% of new mothers. A second clinic had a better rate of only 4% so women would beg to be admitted there instead. The situation was so bad that many would prefer to give birth at home with no medical supervision and indeed the survival rates were probably better under such circumstances. Naturally Semmelweis was not happy with this situation and he set about looking for the cause by carefully eliminating possibilities and keeping the best possible records of all cases. He soon found that the cause of the problem was related to cleanliness so he instructed the doctors and midwives to wash their hands with chlorinated lime solutions which were most effective at removing smells. The result was a dramatic ten fold decrease in mortality rates.

News of the breakthrough spread round Europe via lectures and reports delivered by students of Semmelwies. Given the clear evidence for the effectiveness of the washing procedure and its easy reproducibility you might expect that it would have been adopted quickly. But sadly there was considerable resistence and only a few hospitals in Germany followed the practice. As a result it can be estimated that some tens of thousands of mothers died needlessly following child birth.

In part the problem was that Semmelweis offered no explanation for why his procedure worked. It was a purely empirical observation that could not be explained until the theory of germs became current some twenty years later. At the time doctors believed that such deaths had numerous causes because autopsies seemed to show significant variations of the decease. Reactions to Semmelweis were very mixed. In England doctors thought that the fever was contagious and they mistakenly took the new result as simply a confirmation of this theory with nothing new to report. In part the fault lay with Semmelweis himself because he did not publish an explanation of his results himself and information passed secondhand via his students. Nevertheless it is clear that the failure to change hygiene practices was not just through misunderstanding. There was considerable resistence, not least because the egos of the top physicians of the time would not allow them to accept that their own uncleanliness could be a cause of disease. In 1956 Jozsef Fleisher, an assistant to Semmelweis reported supporting evidence from another clinic in the Viennese medical Weekly. The editor remarked sarcastically that it was time people stopped being misled about the theory of chlorine washings. Such reactions were not atypical. Semmelweis's doctrine was finally rejected at a conference of german doctors which included the celebrated Rudolf Virchow who was considered a scientist of the highest authority at the time. It was the ultimate blow from which Semmelweis could not recover.

In 1861 Semmelwies's apparently suffered a breakdown through depression. He would turn every conversation to the topic of childbed fever. By 1965 he was considered an embarrassment to his colleagues and was tricked into entering an asylum where he was held in a straightjacket against his will. His bad treatment there led to his death from gangrene that year and his work was conveniently forgotten. Some people speculate that he may have suffered from Alzheimer's, bipolar disorder or some other mental ailment we recognise today. But consider this. He knew that each day mothers were dying needlessly at the moment that should have been their families' greatest joy. It was an unnecessary tragedy perpetuated by the arrogance of doctors and could be stopped if only people would listen to him. Through his work in his own clinic he would have seen first hand the hurt that this caused. He was unwilling to accept that, and they called it madness.

5. Conclusion

As illustrated in these four initial cases, many people have published ideas in science that were initially rejected by their peers simply because they went against the accepted wisdom of the time. These people submitted their work to journals only to have them repeatedly rejected with comments from the referees stating that the author simply could not be right. In these cases, the idea has eventually been accepted, sometimes after many years and often only when another more influential scientist rediscovered it. Happily the original discoverers were not forgotten and are now recognised, but it is not just the matter of recognition that is of concern. The failure to evaluate the work correctly at the time has lead to delays in the progress of science that can last for decades.