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Discerning Pathological Science

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Abstract

Pathological science is the science of things that aren't; that is, cases of pathological science involve studies of phenomena that are falsely believed to exist. In these instances, researchers are engaging in self-deception. Since this type of flaw is dangerous, to both the credibility of the researcher and the sanctity of the scientific institution, it should be eschewed. To better understand the nature of pathological science, three historical episodes are examined: the cold fusion fiasco of 1989, polywater of the late 1960s and early 1970s, and N-rays at the turn of the 20th century. In the hopes of obtaining insight into the trends in pathological science, these episodes and others from the history of science have been reviewed with an eye to commonalities and those characteristics that seem to have a particularly insidious ability to breed "pathology" in the research environment. The commonalities and characteristics have been formulated as additions to Irving Langmuir's seminal work, in which he introduced six "symptoms of pathological science." The additions are critical of the personal practices of researchers, the institutions and environments in which they perform, and the science establishment. It is recognized that these symptoms are not an absolute indicator of the pathology of a research field, because some of the symptoms can be applied to instances of good science. Thus the utility of the symptoms in "diagnosis" is limited. Their use should, perhaps, be constrained to the purpose of indicating a need for further inquiry rather than to issuing an absolute condemnation of a research field. It is hoped that an understanding of these symptoms will prevent researchers from succumbing to pathological thinking.

Discerning Pathological Science

In the modern era, science has risen to become an important part of our everyday lives. Science enjoys an esteemed position, resting with prestige among the achievements of civilization. The rewards of scientific advancement have undoubtedly left an impression on our lives, for it is our current state of living that we owe, at least in part, to science. Life enhancing technologies, our common health, and our understanding of the world stem from the research efforts of scientists. Thus, it stands to reason that if we desire to maintain the present state of benefit stemming from science achievement, then the current system of research must also be maintained by ensuring honesty and integrity in the research process. There are problems confronting scientists and threatening to undermine the integrity of science research. These include insidious threats such as outright fraud along with more subtle ones that involve self-deception on the part of the researcher. It is this latter threat that shall be the focus of this paper.

"Pathological science" is a phrase that has been used to describe instances of self-deception that have occurred in the history of science. In these instances, the proper functioning of science breaks down and becomes "pathological." The proper function of science is to add to the body of knowledge that explains how natural phenomena actually occur. When this function breaks down in the sense that science is no longer explaining existent phenomena but rather phenomena that are mistakenly believed to exist, "pathology" occurs. "Pathology" and "pathological science" are terms that will be used as metaphors to describe a particular form of science, further defined herein, that has become "sickly," and that by its proliferation sickens the body of scientific knowledge. One can envision the effect pathological science could have. The product of pathological science is erroneous research results. With erroneous results infiltrating the scientific lexicon, we will develop a misguided understanding of phenomena. Additionally, especially among the folk unacquainted with the nature of science, and who, through beliefs that conflict with the underlying rational upon which science is based, harbor a mistrust of science, the image and reputability of science may suffer. Pathological events in the history of science seed a mistrust, or provide support for a framework that attacks the validity of science or the reputation of researchers, who all too often can become the target of generalizations about assumed universal character traits among scientists. Pathological science should be eschewed, for its effects are undesirable.

Pathological science is of particular personal interest to the aspiring researcher whose concern is to make significant contributions and build a reputable career. The conscientious researcher can avoid fraud outright. However, the subtle clasp of self-deception could prove easier to fall prey to because by its nature self-deception is not consciously recognized. To avoid self-deception requires vigilance on the part of the researcher. Ways that self-deception can be avoided are by knowing limitations on the accuracy of measurements and by having a familiarity with the pitfalls into which other researchers have fallen, those instances of pathological science that have occurred throughout the history of science.

The purpose of this paper is to outline the characteristics of pathological science. It is hoped that recognition of characteristics will help in the identification of pathological research by bringing suspicious-looking research into examination. The characteristics were gathered through a review of available literature on known, popularized incidents such as the 1989 cold fusion fiasco. The progression of events during a pathological episode is described, and pathological science is differentiated from pseudoscience and fraudulent science. The list of characteristics is examined in the context of several of the major incidents including the cold fusion fiasco, polywater, and N-rays. The list encompasses factors stemming from the researchers themselves and from the research atmosphere and research pressures.

Distinguishing Pseudoscience, Fraudulent Science, and Pathological Science

Pseudoscience, fraudulent science, and pathological science all threaten the scientific institution. These forms of "bad science" diminish the reputability of science by producing results that are based upon faulty methods, false, or lacking evidence. To better understand pathological science, fraudulent science and pseudoscience will be defined and a contrast will be made with pathological science in order to distinguish it.

Fraudulent science is the most blatant. Fraudulent science involves a deliberate and conscious attempt on the part of the researcher to falsify data. This is typically motivated by personal gain that the researcher will receive for succeeding—money, acclaim, prestige, award, recognition, tenure, or a grant for instance. Perhaps a researcher will commit fraud to establish primacy before a rival publishes, fabricating data to fit what the researcher anticipates will be the inevitable results. Fraud is a result of misrepresentation or fabrication, deliberate acts intended to deceive.

According to Hines, in being a pseudoscience an area of investigation possesses two definitive features (1988), the first of which is that the hypothesis put forth is non-falsifiable. Pseudoscientific claims are set-up in such a way that evidence cannot be gathered to disprove them. An example presented by Hines is the Creationist argument. Here, an omnipotent being created the world some recent time in the past, more recently than conclusions drawn from natural laws and geological and biological principles would tell us. Let us say for instance that the Creationist purports the Earth to have been created 10,000 years ago. When confronted with scientific evidence to disprove that belief, the Creationist argues the Earth was created with geologic and biologic states having already "advanced" to a latter stage, giving only the semblance that the Earth is older when in actuality it was instantaneously birthed into being. This argument, for the Creationist, renders geologic and biologic evidence invalid. Hines points out that an analogous argument can be made: the argument that an omnipotent being willed the universe into existence 30 seconds ago. When the reply is made "but I have memories that go back farther than 30 seconds," a person of the Creationist mindset might reply "But you were made with those memories." This argument is non-falsifiable. Since the omnipotent being left no evidence of instantaneous creation, we would have no means of discovering the moment of creation. In the absence of evidence, a hypothesis cannot be disproved, although it is intuitive that lack of proof does not lend proof to the non-falsifiable hypothesis either. There lies only the possibility that it could be true, letting the hopeful believer continue to cling to their belief.

Hines cites a second feature of pseudoscientific claims. The feature, contrasting with the behavior of scientists, "is a general unwillingness on the part of promoters of pseudoscientific claims to look carefully at the evidence they put forth to support their claims" (Hines, 1988). This seems obvious, since if the promoter of the claim did indeed subject the claim to scrutiny, then the promoter would likely realize the faulty structure of the claim and would recant his/her belief or support. A discrepancy is notable here. When a pseudoscientific claim is supported by a believer despite the existence of evidence that falsifies the claim, a significant "block" exists in the believer's mind. Regardless, the believer will continue to believe even in the face of refutation. Once the block is overcome, then the believer can realize the nonfalsifiability of the hypothesis and thus the erroneous nature of the hypothesis. If the block ceased to exist, then there would be no problem because the believer would realize the evidence disproves their belief and would no longer believe. One of two situations can exist—either the hypothesis is nonfalsifiable or evidence exists to disprove the hypothesis and the believer is recalcitrant. It is more appropriate to view Hines' two criteria as exclusive—only one can appropriately apply, and if one applies then the claim can be labeled as pseudoscience.

Hines distinguishes science from pseudoscience using the criteria Karl Popper outlines (1953). Popper says, "the criterion of the scientific status of a theory is its falsifiability, or refutability, or testability." Popper recognizes that contradictory evidence must be possible in order for a hypothesis to be scientific. Popper also recognizes that "some genuinely testable theories, when found to be false, are still upheld by their admirers—for example by introducing *ad hoc* some auxiliary assumption, or by re-interpreting the theory *ad hoc* in such a way that it escapes refutation." Pseudoscience suffers from one of two possible plights: either its claims are non-falsifiable or its supporters are in denial of evidence that debunks their claims.

Pathological science is distinguished from fraudulent science by personal intent. In cases of fraud in science, a deliberate attempt to falsify data is made in order to support an outcome that is perceived to be beneficial. With fraud, a researcher succumbs to the selfish desire for personal gain and lets this desire supercede the selfless desire to maintain the integrity of the scientific institution that is necessary if knowledge is to be valid. On the contrary, in cases of pathological science the researcher does not realize that the phenomenon they are studying or purporting to exist is in reality non-existent. Although the research results are false, the researcher has no awareness of the falsity and has not made an attempt to deceive. Put simply, fraudulent science is deliberate falsification whereas pathological science is unwitting error that goes unrecognized for a period of time.

Pathological science differs from pseudoscience. In many cases, pathological science presents a workable, falsifiable hypothesis whereas pseudoscience presents a non-falsifiable hypothesis. Thus, pathological science, judging from the nature of the hypothesis, is workable from the start (from the presentation of a hypothesis) but goes awry at some later stage due to deficiencies in the researcher. Pseudoscience is doomed from the start because its foundation, the hypothesis that will guide the direction of the research, is shaky.

Pathological science can be described further. While it is conducted as science, pathological science differs from normal science in that it is science conducted poorly. In other words, the researchers are doing sloppy work. This trend is seen in many instances.

Although these distinctions are clear in definition, when a case occurs it can appear to include a combination of aspects from the three types of bad science. For instance, it has been suggested that the cold fusion fiasco of Fleishmann and Pons, although an overall case of pathological science, included an element of fraud. In their initial data, a peak of a spectrum measured from the emission of their apparatus did not match with what would be expected from a fusion event, but in a later presentation of the data, the peak had been allegedly fit to what would be expected as the correct peak. While this seems fraudulent, it may not be rightly described as such. It is questionable whether this is a case of fraud or simply data massaging, perhaps a lesser infraction. Whereas the differences in the forms of bad science are clear in definition, when cases are examined the distinctions are not always clearly demarcated.

General Nature of Pathological Science Episodes in History

Three of the more popular episodes of pathological science, taken from the history of science, will be described to build an understanding of the general nature of the episodes and factors involved. The three described are: the cold fusion fiasco of 1989, the polywater incident of the late 1960s and early 1970s, and N-rays of the early 20th century.

Cold Fusion

Several books have been published that deal solely with this case and the events surrounding it (Close, 1991, Huizenga, 1992, Taubes, 1993) while it is treated in other books simply as a chapter (Dewdney, 1997).

On March 23,1989, the researchers Martin Fleishmann and Stanley Pons of The University of Utah announced to the press that they had discovered a way to perform cheap, table-top cold fusion. The announcement came as a surprise. The scientific community had considered fusion to be impossible, and certainly not anywhere near development. What made their announcement so incredible was the repercussions that the discovery could have on the developed world: if feasible, cold fusion would provide cheap, clean energy in abundant quantity, thereby eliminating the burden of energy production and concurrent pollution. As the story unfolded, it became clear that

Fleishmann and Pons were in error. Other researchers in laboratories across the country had failed to reproduce their results. Criticisms abounded from the physics community. Excitement had gotten out of hand, and Fleishmann and Pons had jumped the gun. In retrospect, we can identify points where the two had gone wrong, where they had acted sloppily and had committed some grievous errors in the conduct of their research.

One might first note that Fleishmann and Pons were experienced in the field of electrochemistry, not nuclear physics. This fact would call into question the reliability of their research in the field of fusion, since they likely wouldn't have the background knowledge necessary to make accurate interpretations of their results and wouldn't be familiar with the trappings into which inexperienced researchers in the field can fall. However, the apparatus involved elements that an electrochemist would be well qualified to construct and theoretical aspects were within the bounds of what Fleishmann and Pons could reasonably be expected to comprehend. But they did not have the background in theoretical physics that would be required to fully explain and understand the phenomena, the fusion of hydrogen nuclei, they were claiming to measure. Perhaps the incident would have never happened had they been experienced in the field and had been witness to the false claims of fusion discoveries that had occurred over the years.

The function of the peer review process is to assure consensus before research results are published. This allows claims to be "checked" by experienced researchers in the field prior to dissemination and acceptance into the body of scientific knowledge. Fleishmann and Pons neglected the peer review process, announcing their "discovery" to the popular press prior to review. There was purpose to their fault though—given the immense returns that the discovery could bring, The University of Utah needed to stake a claim on the discovery to ensure primacy. Besides, it would be difficult to contain the discovery once the reviewing scientists had read the manuscript. By foregoing the peer review process though, they ensured their fate. Had they not gone public and had awaited peer review, their error would have been exposed quietly within the peer review system, without the brouhaha, and sparing their reputations. By adding to the hype surrounding the episode, involving the university administration, and catching the attention of scientists around the globe, they had raised the stakes considerably. The consequences of being wrong were magnified, and by the time the consensus was established that they were wrong, their reputations were lost. In this case, the peer review system would have functioned as a prophylactic, but even peer review is not always preventative of pathology, as the polywater case proves.

Interestingly, Fleishmann and Pons were not the only ones that believed they had witnessed the cold fusion phenomenon. Another researcher, Steven Jones, located at nearby Brigham Young University, had been performing similar work. The two groups became aware of each other when Jones had been assigned as a reviewer for a grant proposal that Fleishmann and Pons had submitted to DOE. In the interest of fairness, the two groups agreed to publish simultaneously, and had agreed upon a time and a particular FedEx post to meet at in order to mail the manuscripts. As the agreed upon time for mailing the manuscripts approached, communication between the groups broke down. Fleishmann and Pons had announced their discovery in advance of the mailing, and Jones considered this to be a breech of agreement. Serving his interests, he sent his manuscript before the agreed date. Science research is competitive, as this attests, and when the potential rewards of discovery are high those involved are more prone to become suspicious of those whose actions could potentially deprive them of the credit they are due. Preempting, mistrust, and accusation are unprofessional, since these could be overcome with functioning channels of communication. Unprofessional behavior evidences incompetence on the part of the researcher, and therefore their proclivity to pathological research.

But one of the surprising things about pathological science episodes is that the researchers involved are respected in their usual field of study. This is intuitive, because if a researcher had displayed incompetence or sloppiness in their research, any claims of outstanding discoveries would be given little regard, being dismissed as just another product of their incompetence. Naturally, pathological science will emerge from respected researchers, just as was the case with Fleishmann and Pons.

The experimental apparatus was simple and easy to construct. It was so simple in fact that an undergraduate team at MIT had attempted to replicate the experiment shortly after the announcement. Simplicity allows the experiment to be replicated by many groups and increases the chance that systematic error will be replicated as well. Ease of replication proliferates pathology. If the experiment were very difficult to perform, it would subdue the spread of the pathology because only the few scientists who possess the knowledge, facilities, and equipment to perform the experiment will be susceptible. A common trend with pathological science is that the experiments are simplistic.

An important consideration that it appears Fleishmann and Pons failed to take into account is that, should they have succeeded in producing fission, the radiation from the process would have been harmful to themselves and those working around them. This is further sloppiness, and it evidences their inadequate experience in the field by their lack of precautionary measures.

Had Fleishmann and Pons been right (and they certainly believed they were right at the time) the consequences for them would have been magnificent. They would have received monetary benefits and would have been likely candidates for a Nobel prize. Once they realized this, they lost the disinterestedness that is essential to a scientist's work. By being disinterested in the outcome of their work, the scientist is working to prevent bias in interpretation of results. Knowing that if their experiment were to be successful they would benefit greatly, Fleishmann and Pons were more apt to interpret their results in favor of the hopeful outcome.

Once the "discovery" had been announced, replication attempts were made immediately. But details about the nuances of the experimental set-up were hard to find. And those that were fortunate to obtain copies of procedures and apparatus descriptions found the descriptions still inadequate. Perhaps the phenomena of inadequate procedural descriptions is not unique to pathological science, but would also be found in good science if instances of good science were to be scrutinized to the degree that the cold fusion fiasco was. The difference is that with pathological science there is a focus on replicating the experiment to either support or disprove the alleged results, inevitably revealing discrepancies.

It is evident that Fleishmann and Pons had not conducted a literature review as they should have prior to publication and as part of their research. If they had, they would have found that many false claims had been made in the field of cold fusion research, which perhaps would have quelled their enthusiasm and encouraged them to err on the side of caution. The very process they were demonstrating had been attempted before many decades previous by a German researcher hoping to discover a method for making cheap hydrogen to fuel Germany's dirigibles during their heyday.

The cold fusion fiasco is unique among episodes of pathological science because of its brevity. In a matter of weeks doubts emerged and were confirmed, spelling demise. Other episodes of pathological science were not dispelled as quickly. The brevity of the cold fusion fiasco can be attributed to new, quicker modes of communication such as email, which facilitated dissemination of the results of replication trials.

Polywater

The author Felix Franks has given a good recounting of the polywater incident in his 1981 book Polywater. The discovery of polwater is attributed as beginning with the work of the Russian scientist Boris V. Deryagin, although its origins can be traced farther back to the work of a lesser-known Russian scientist, Nikolai Fedyakin. Polywater was believed to be a previously uncharacterized form of water, one whose properties differed from that of normal, everyday water. It took a while for the research to catch the attention of western scientists, but when it finally did, polywater research blossomed more so than in Russia. This new form of water was polymerized (hence the "poly" of "polywater") by the interaction of water molecules with solid surfaces. The water could only be formed in very confined spaces, such as capillary tubes. Because of this, no more than a few microliters could be formed at a time. Franks sums up the incident: "millions of dollars (and rubles?) were spent in the pursuit of anomalous water, and thousands of pages were filled with polywater stories, scientific and otherwise, over a period of several years. A warning that polywater… might be the most dangerous material on earth alerted the news media, and from then on much of the scientific debate was carried on in the pages of newspapers."

In the end, the properties of this supposedly new form of water were found to be due to the presence of contaminants such as silica from the walls of the capillary tubes. This turn-around left many scientists that had become involved with the incident disillusioned. Fortunately, as the trend is with pathological science cases, the research had been relatively inexpensive to perform, a factor that contributed to its spread among scientists.

As with cold fusion, points where the researchers had gone wrong can be shown. Additionally, political factors are cited as contributing to the incident.

Water is such an important, widespread substance that it has implications for many disciplines of science. Biology is concerned with the role of the substance in living organisms while physics and chemistry are concerned with study of the substance to gain an understanding of its structure and properties. Despite the significance of water research for many scientific disciplines, research tended to be compartmentalized. Franks describes the isolated research groups:

> [T]hey operate in ignorance of the existence of other groups with identical interests. Each group publishes in its own journals, organizes its own conferences, develops its own jargon, and has its own "experts," few of whom are aware of similar activities in other groups.

This failure to communicate across disciplines is a dilemma that still plagues science. The expertise of many scientists from varied backgrounds adds new insight to problems and when research is open and collaborative throughout the entire process, this helps to mitigate the impact of false results by allowing doubts to be voiced as claims are made. If research is closed and secretive, then a when claim is finally released, doubts arise after the full impact of the claim is received by the scientific and lay community.

Open communication is conducive to collaborative efforts and the resolution of dilemmas. If scientists become polarized in their beliefs about whether a phenomenon exists or does not exist and have difficulty communicating across their differences of belief, then they are much less apt to resolve the difference objectively and resort to arguments that are lacking in objectivity. One frequently encountered argument has been dubbed the "golden hands" argument, where researchers are said to have a special touch in getting experiments to work. According to this line of argument, a researcher must be amply experienced and must possess a degree of prowess before an experiment can be properly performed. Although this is rightly applicable in instances, it can become an excuse for inreplicable results. Pathological science is littered with researchers who vindicate themselves with this argument. One could be skeptical of the golden hands, for they may be less apt at performing experiments right and more apt at consistently reproducing systematic error.

As with cold fusion, the simplicity of replication encouraged scientists to jump on the bandwagon. The inexpensiveness and ease of the experiment allowed researchers to undertake the additional study without having to compromise their current studies.

Context could be key to the polywater incident. In the political climate of the time, the west had just lost the space race to the Russians who successfully put Sputnik into space. When word of the work the Russians were doing on polywater reached the west, the west was not going to be left behind again. Polywater research proliferated in the west, with ultimately more articles published than in Russia. Pressure to outcompete the Russian enemy may have encouraged discoveries that never really existed.

Only microgram quantities of the substance could be produced. Such scarce quantities are difficult to work with. As the phenomenon measured becomes less pronounced, or nears the limit of detection, errors are more prone to occur. Furthermore, the experimenter effect proliferates under such conditions. The experimenter effect occurs when a person taking data has a desired outcome in mind and selectively records or interprets data in a way that is supportive of the desired outcome. Ideally, a researcher would be lacking of desire or bias for any outcome, recording and interpreting data as it really is rather than how it is desired to be.

N-rays

Mary Jo Nye has written an insightful account of the N-rays episode (1980).

The French physicist René Blondlot announced the discovery of a new form of radiation in 1903. He dubbed the radiation N-rays after the University of Nancy where he held a professorship. The N-rays, when shone upon an electric spark, were purported to change the brightness of the spark. This constituted the method of detection for the rays. The changes were so faint, however, that they could only be observed in a darkened room by a person with sufficiently sensitive eyes. Blondlot pursued his characterization of the rays so far as to characterize a spectrum for the rays. Numerous researchers in France confirmed Blondlot's discovery and even discovered the rays to be emitted by various organic substances. Of course, all of the work was later found to be bogus.

The definitive moment in the downfall of N-rays came when an American scientist, R. W. Wood, visited Blondlot's laboratory to observe the new radiation.

Blondlot demonstrated his techniques under Wood's observation. Wood could not detect the changes Blondlot was claiming to observe, and Blondlot explained to Wood that his eyes were not sensitive enough to detect the changes. For Wood, the matter was put to rest when Blondlot was observing a spectrum of N-rays emitted from a spectroscope. A thread covered with luminescent paint was moved through the spectrum. The spectrum was created by passage of the rays through an aluminum prism. Blondlot read off measurements as the thread was moved through the spectrum. Wood asked Blondlot to repeat the measurements. Meanwhile, Wood surreptitiously removed the prism, eliminating the spectrum, while Blondlot continued to make observations when in fact there was nothing to observe. Wood reported his findings in a letter to *Nature*. Although Blondlot continued to believe in the existence of his rays, this marked the downfall of N-rays.

As with the aforementioned episodes, there are factors that can be cited as having contributed to the N-rays episode. Blondlot's "discovery" came at a time when new forms of radiation were being discovered. X-rays had recently been recognized by the Germans. Alpha, beta, and gamma rays and "blacklight" were additional discoveries of the time. Amid this atmosphere of new discovery, Blondlot's "discovery" was nothing unusual. At the University of Nancy itself there had been a flourishing climate of intellectual development that had gained recognition and admiration. The climate seemed to lend sanction to the N-ray discovery as if any product of the university was credible because of its reputation.

Blondlot was respected in the scientific community. This respect may have led his lessers in the field to have confirmed his results without due scrutiny, believing that his findings were correct because his prestige warranted it.

The rivalry between Germany and France had created nationalistic tensions. Since Germany had hosted the discovery of X-rays, France was left out. When Blondlot's discovery arrived, the French community welcomed it. France had finally received the credit it had wanted.

Perhaps Blondlot's biggest mistake was his use of a subjective measuring device, the human eye. The human eye has limited reliability as an instrument for certain types of data acquisition, especially when making observations at the limit of detectability, where the experimenter effect can be more pronounced. Blondlot wanted to see the N-rays, and under the conditions he was making observations, he clearly believed that he was indeed observing them.

Blondlot's attitude was a major contributing factor. Generally, he was defensive of his discovery and had constructed a series of excuses to defend against criticisms. Nye details characteristics of Blondlot's attitude: "Blondlot seemed to critics irrational, even perverse;" "When experiments went badly, Blondlot and others pleaded tiredness or the difficulties of the observations;" and "Blondlot refused control experiments on the grounds that the observer must regulate the emission of N-rays and their detection in order to avoid fatigue" (1980). Critics wanted to collaborate with Blondlot to resolve their doubts, but Blondlot "never agreed to any cooperative venture with them" (Nye, 1980). In addition, Blondlot's papers were lacking in details, an issue which he failed to resolve.

According to Nye, "Blondlot's contemporaries in the physics community were former classmates; his colleagues among physiologists and medical doctors were friends or acquaintances since students days in the Latin Quarter" (1980). In the French community, those who had established reputations (partly through their prestigious education) led the scientific community, and generally their work wasn't questioned. Having had an established reputation for good work, and being surrounded by old friends who are less critical, Blondlot wasn't met with criticism from those sources that might have been most influential.

Besides cold fusion, polywater, and N-rays, the history of science records other episodes not detailed here. By analyzing episodes, the emergence of pathological science can be linked with particular characteristics of researchers, their work, and the contextual situation. Commonalities between the episodes become evident.

Characterizing the Nature of Pathological Science

Irving Langmuir is perhaps the person most associated with the characterization of pathological science because of his seminal study of its occurrence in the field of physics. On December 18, 1953, Langmuir held a colloquium on pathological science, thereby establishing his association with the subject. The colloquium was held at General Electric's Knolls Atomic Power Laboratory. His transcribed lecture appears as an article in *Physics Today* (Langmuir, 1989), published, appropriately, just months after Fleishmann and Pons boasted their discovery of a process to perform table-top cold fusion. The important product of Langmuir's inquiry into the nature of pathological science was his characterization of six "symptoms of pathological science" that were presented in Langmuir's lecture. These are the recurring characteristics of pathological science as Langmuir saw it in the examples he reviewed, coming mainly from physics:

1. The maximum effect that is observed is produced by a causative agent of barely detectable intensity, and the magnitude of the effect is substantially independent of the intensity of the cause.
2. The effect is of a magnitude that remains close to the limit of detectability or, many measurements are necessary because of the very low statistical significance of the results.
3. There are claims of great accuracy.
4. Fantastic notions contrary to experience are suggested.
5. Criticisms are met by *ad hoc* excuses thought up on the spur of the moment.
6. The ratio of supporters to critics rises up to somewhere near 50% and then falls gradually to oblivion.

Episodes that fit these symptoms nicely have occurred since Langmuir's lecture, polywater and the cold fusion fiasco being the popularly recognized ones. The continuing emergence of episodes that fit his "symptoms" supports his ideas.

An examination of cases of pathological science reveals other possible symptoms in addition to those outlined by Langmuir. It should be noted that these symptoms do not all have to be present in order for a science to be considered pathological. The additions are considered to be descriptive of pathological science as opposed to good science.

Additions to Langmuir's Symptoms of Pathological Science[1]

1. Criticisms are met by ad hoc excuses thought up on the spur of the moment.

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[1] The following abbreviations will be used to identify the episode from which the symptom was derived: CF, cold fusion; PW, polywater; NR, N-rays; and OT, other.

    a. Golden Hands (CF, PW, NR)
    b. General Defensive Attitude[2] (NR)
2. The ratio of supporters to critics rises up to somewhere near 50% and then falls gradually to oblivion. (CF, PW, NR)

    —Polarization (PW)
3. Researcher could have a vested interest in a particular outcome (CF)
4. Ego or personality of the researcher is conducive to denial or risk-taking (CF, NR)
5. Plagued by unprofessional behavior (CF, NR)
    a. Publishing in their own journal (OT)
    b. Unwillingness to collaborate (NR)
    c. Working in secrecy or isolation (CF, PW)
6. Research occurs in a political climate or a climate that otherwise promotes achievement that glorifies the institution (CF, PW, NR)
7. The researchers themselves will have a history of reputable work, perhaps encouraging others to fudge (CF, PW, NR)
8. Simplicity of the experiment propagates pathology (CF, NR, PW)
9. General lack of detail in experimental procedures (CF, NR)
10. Will more likely emerge from the work of one or a few scientists rather than a large group (CF, NR, PW)
11. There is outside sanction of some form (NR)
    a. Riding a wave of new discovery or enthusiasm
    b. Fellow researchers are old friends and uncritical colleagues
12. Inadequate replies are made to critics (NR)

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[2] The first two additions are two of Langmuir's original symptoms with added details.

13. Researchers haven't reviewed the literature thoroughly (CF)
14. The research was not planned (CF, PW)
15. Hype surrounds the research (CF, PW, NR)
16. Data has been manipulated (CF)
    a. Forging—Recording data that never were
    b. Trimming, massaging, or fudging data—Data is manipulated to make it look better
    c. Cooking—Choosing only data that fit the researcher's hypothesis best and discarding those that do not (Kohn, 1989)
17. There is a history of false claims in the field (CF)
18. The research is in a field where researchers are motivated for less than ideal reasons, such as monetary gain (OT)

The first symptom is one of Langmuir's to which more detail is added. Often, the "golden hands" argument is encountered, alleging that only a researcher experienced in the techniques of a particular experiment will succeed in getting the experiment to work because of the difficulties involved that the inexperienced researcher simply can't overcome (Segerstrale, 1990). This argument is presented on the spur of the moment to counter criticisms. As in the case of N-rays, Blondlot exhibited a generally defensive attitude. He simply would not accept the possibility of being wrong. He evaded attempts to resolve the debate by establishing control experiments, controls that would inevitably have proven him wrong. The trouble with excuses is that no follow-up is done to resolve the issue. The excuses stand alone in defense of a position. Here, the excuses exist as explanations for why something occurs a certain way. In order to provide a defense, the proposition of the excuse should be tested. If it holds true, for instance, that only an experienced researcher can replicate results, then collaboration between an inexperienced and an experienced researcher should evince the difference.

Polarization. In the polywater episode, there were few researchers who took the middle ground. Either those involved believed that polywater was a real phenomenon or they didn't. Scientists were divided into camps, and a debate was maintained between them. Pathological science episodes are contested from the start—by their nature they are controvertible.

Researcher could have a vested interest in a particular outcome. The presence of motivating factors can encourage the experimenter effect. The knowledge that discovery will yield reward in the form of acclaim, money, etc. drives the desire for the discovery to be real, and under those circumstances researchers could be prone to the psychological trickery of self-deception, letting down their rational skepticism for something that will grant them a more favorable psychological reward.

Ego or personality of the researcher is conducive to denial or risk-taking. Fleishmann and Pons have been described as conducting high-stakes research. Their risk-taking research ventures are said to have paid off for them in the past, but with the cold fusion fiasco it had caught up with them. They had frequently stepped outside the bounds of the conventional, which implies they were looking, or hoping, for something out of the ordinary. With N-rays, Blondlot seemed to suffer from denial. Rather than disprove criticisms, Blondlot made excuses and ignored them. These are personal traits of the researchers themselves.

Plagued by unprofessional behavior. The researchers conduct their work sloppily, neglecting fundamentals of science research. They might neglect controls as Blondlot did or exhibit poor communication skills. In a case not detailed involving pathological research on the chemical transfer of memory in worms, a researcher had created a journal in which he published his work (Collins and Pinch, 1998). One must wonder about the effectiveness of the peer review process in such a journal. Publishing in a journal for which the author serves as editor doesn't imply pathology, it just compromises one of science's ways for restraining pathology, critical review. Working in secrecy or isolation keeps new insight and criticism out of the research process. Doubts expressed at the initiation of research are likely to have more of an impact than doubts expressed after a researcher has become convinced of results.

Research occurs in a political climate or a climate that otherwise promotes achievement that glorifies the institution. Western researchers wanted to outdo the Russians. The French wanted to outdo the Germans. The University of Utah had to establish its primacy quickly or risk losing glorification. Institutional pressures are felt by researchers. The need to perform to fulfill the subtle, implicit goals of the institution motivates discovery of the non-existent.

The researchers themselves will have a history of reputable work, perhaps encouraging others to fudge. This is somewhat obvious, since if a researcher lacks a reputable history, their work, if sensational, will be discarded as a product of incompetence or will be altogether ignored. This symptom is a more widely applicable one.

Simplicity of the experiment propagates pathology. The experiments of pathological science are simple, cheap, and easy to perform. Therefore, the pathology can spread to become a significant episode. Were the apparatus complex, it would limit the ability of others to confirm or disprove claims.

General lack of detail in experimental procedures. Those researchers replicating the experiments, although involving simple apparatus, found necessary details lacking. This prevented them from definitively settling debate, caused delays, and allowed the defending researcher to cite the excuse that the experiment was not being performed properly by critics. Perhaps it would be found that even good science suffers from lack of detail in recounts of experimental procedures, but since good science is not often replicated because it is not controversial, it is difficult to know.

Will more likely emerge from the work of one or a few scientists rather than a large group. When research is conducted by many individuals, chances are greater that if the research product is indeed dubious, it will be recognized early and proper measures will be taken before the pathology is propagated.

There is outside sanction of some form. In the case of N-rays, the wave of achievement and recognition experienced by the University of Nancy had established the researchers there as respectable. Their collective reputation gave sanction to Blondlot's work. Blondlot's prestigious Parisian education sanctioned his work. His colleagues knew and respected him and would back up his work because of their connection to him. With the cold fusion fiasco, The University of Utah supported the press conference "publication" of Fleishmann and Pons' work. Outside sanction supports research claims for reasons unrelated to how compelling the research is.

Inadequate replies are made to critics. This is evidence of the avoidance that allows the researcher responsible for the pathological science to continue his/her belief. Willingness to collaborate on the part of critics is met with avoidance by the defending researcher. Were the defending researcher to face critics, resolution of the problem would ensue more quickly. Whereas the magnitude of an episode is augmented because avoidance extends its longevity, the episode might have turned out to be just another simple error brushed away with other minor incidents limited to and forgotten in small, closed circles of science had the error been recognized early.

Researchers haven't reviewed the literature thoroughly. In the cold fusion fiasco, a very similar experiment had been attempted earlier in the century. The researcher's intent was to develop a source of helium for dirigibles. Needless to say, the work did not culminate in success. Had Fleishmann and Pons been aware of this, as veteran researchers in the field who are familiar with the literature would have been, they wouldn't have made their mistake. Inadequate review is a consequence of working in a field for which you are not trained.

The research was not planned. In the case of cold fusion and polywater, the experiments were simple and easy to perform. This allowed researchers to stop their planned research activities and devote time to attempts at replicating the pathological research. When the research is simple to replicate, then researchers who are inexperienced in the field are be drawn in. Inexperienced researchers will more easily fall prey to mistakes that are a result of their ignorance. Premeditated research will generally be performed by researchers who have "done their homework," especially if their research is the product of a grant, for which they would have to demonstrate convincing knowledge of the field before receiving the grant.

Hype surrounds the research. Hype exists when controversy or high stakes are at play. These factors are not conducive to objective research. There is a caveat to this symptom—the three cases examined in this paper, and which form the basis for many of the symptoms and conclusions, were studied because they are some of the more well known examples, so naturally they are more well known because they were hyped. Perhaps lesser-known examples of pathological science, if analyzed, would not exhibit the degree of hype that cold fusion, polywater, and N-rays did.

Data has been manipulated. Data manipulations are common in research, but sometimes data that is invalid will be manipulated to make it appear supportive of research claims. Researchers can do this intentionally and commit fraud, or they can engage in self-deception and manipulate data without realizing that their manipulations invalidate their results. Anytime heavy manipulations of data have occurred, one should be suspicious of the research. As Collins and Pinch point out, there are examples where research has been labeled as good science when in fact it is based on dubious practices. An example Collins and Pinch give is Eddington's solar eclipse observations that helped prove the theory of relativity (1998). Eddington threw out an entire data set from his experiment that disproved relativity. However, history records Eddington's work as proof of the theory of relativity. Ultimately, further experiments have confirmed the theory of relativity, but were Eddington to have been on the wrong side, his data manipulations would be cited as a basis for his pathological research. Caution must be exercised in relying on the symptoms as decisive indicators.

There is a history of false claims in the field. Physics researchers had witnessed many false claims in the cold fusion field over the years. Numerous false claims are perhaps evidence of the influence of factors that stimulate false claims. These factors could include the expectation of high rewards for discovery.

The research is in a field where researchers are motivated for less than ideal reasons, such as monetary gain. The field of biomedicine receives comparatively more funding than other fields. Salaries are higher in the field partly because there is more money to go around and there is a need for more, better researchers. The competitiveness in the field coupled with the money to be gained (a reward) lead to higher rates of fraud in the field (Segerstrale, 1990). Although there is no concrete evidence to support the existence of pathological research in the field of biomedicine, it seems likely that the same factors that are conducive to fraud might also be conducive to pathological science.

There are three characteristics of the scientific institution itself that predispose research to pathology. Since these encompass the whole institution rather than applying to its individual constituents, such as the researchers, they will be treated separately.

1. Publish or Perish encourages sloppiness

   —Emphasis on accrediting the first to print encourages quick publication to preempt other researchers ("eureka effect")

2. Negative results often aren't published
3. Emphasis in the teaching of students to get the "correct" result in laboratory courses.

Publish or Perish encourages sloppiness. The current system of university research places emphasis on production, which comes in the form of publication. If a researcher doesn't publish, then he/she will not obtain tenure and will, essentially, perish in the system. Not only does a researcher have to publish, but also he/she must publish prolifically, and the work must be original. The trouble with this is there are limits on how much original work there is to be done, and time is another limiting factor. This type of competitiveness is conducive to sloppiness. A researcher needs original work to his/her credit and will devote less time to carrying out experiments that may prove the work to be false. It is widely known in science that "the first to publish a finding, not the first to discover it, tends to get most of the credit for its discovery" (Committee on Science, Engineering, and Public Policy, 1995). The emphasis on being the first to print with an idea, called the "eureka effect," encourages researchers to hastily preempt others, as Fleishmann and Pons did, without fully evaluating their results.

Negative results often aren't published (Segerstrale, 1990). Negative results are valuable to scientists. They can serve as a guide for what not to attempt, saving time and mistakes. Unfortunately, negative results aren't published, leaving out information that is of potential value to researchers. Perhaps if procedures that proved useless were accumulated in some systematic form or were in some way retained and recognized in the disciplines, it would have prevented some of the episodes from occurring.

Emphasis in the teaching of students to get the "correct" result in laboratory courses (Segerstrale, 1990). It is feared that the current generation of science students are being improperly trained in a manner that will lead to a heightened incidence of pathology. There is an emphasis in laboratory courses on achieving the "correct" or the "right" answer when working on a laboratory assignment. This is clearly not the message that should be ingrained in aspiring researchers. Emphasis should instead be placed on earnestness in reporting results. Students should learn to be objective and honest, not to fit their results to match an outcome that is expected or believed to be correct.

Limitations and Criticisms of the Symptoms

The scope and utility of the symptoms are limited. It is important to note that in some instances the symptoms can apply to cases of good science, which blurs the distinctions between pathological and good science.

Some symptoms require a firsthand knowledge of the research or the personality and history of the researcher. In order to make an evaluation of a claim on the basis of those particular symptoms which require a firsthand knowledge, it would be necessary to either know the researcher personally or to conduct an extensive background investigation (a lengthy and difficult endeavor). This limits the usefulness of those symptoms to the select few who happen to have insider information.

Pathological science is often better defined retrospectively. This limits the utility of the symptoms for determining that a research claim is pathological at the time of its emergence. For instance, determining the progression of the ratio of supporters to critics can only be accomplished after the episode is over. The symptoms of pathological science are limited because the ultimate test of whether a science is pathological is whether the claim proves to be true in the end. This is determined by the efforts of those replicating the experiments and only comes once a consensus is reached in the scientific community. It is recommended that the symptoms be used as indicators that a claim warrants further scrutiny before it is accepted.

Most every researcher has some degree of interest in the outcome of their work. Scientists are motivated by rewards like persons in many professions, which serves some good in science by encouraging researchers to achieve more to win acclaim, better positions, and, with prizes like the Nobel, money. Thus, even good researchers conducting good science will have a vested interest in succeeding, which will only come through novel discoveries. It is when this vested interest causes the researcher to produce erroneous results or become blinded to the faults of their research that vested interest becomes pathological.

"Bad" egos and personalities are not limited to pathological researchers. Scientists hailed as geniuses who contributed significantly to our understanding have been remembered, probably more accurately, as possessing a human, biographical element that was less impressive than their myth. Newton is one that comes to mind.

It can be found that some of the symptoms apply to what has been recorded as good science. Abuse of data manipulations is probably the symptom most common to both good and pathological science. The Eddington solar eclipse observations and the Millikan oil drop experiment are examples. The Eddington solar eclipse observations were hailed as proof of Einstein's theory of relativity (Collins and Pinch, 1998). Both the theory of relativity and Newtonian theory predict that strong gravitational fields will affect light rays. However, Einstein's theory predicts a greater effect than Newton's. When light from a star passes near to the sun, the light will be bent by gravity. The star will appear to be in a slightly different position in the sky when near to the disk of the sun. The theories are tested by measuring the displacement of a star near the disk of the sun during a solar eclipse because otherwise the intensity of the sun's light will not allow observation of the star. The difference in position was minutely small, so like in pathological science Eddington was working near the limit of detection. Eddington's equipment had to be precisely set up and fine-tuned or else the data would be distorted. Two parties were sent out to make observations, one to a site in Brazil and the other off of the coast of West Africa. Some plates that were recorded of the eclipse were better than others, and those that were of a lesser caliber were tossed out. The data from one expedition was supportive of the Newtonian prediction whereas data from the remaining expedition supported Einstein's prediction. Eddington chose to discard the data that supported the Newtonian prediction. One must keep in mind that Eddington knew all along what the data values should be in order to support Einsteinian predictions. It appears he was biased, and his choosing of data shows he acted on his bias. But in the end Eddington's experiments have become often cited and respected proof of relativity. This case demonstrates the similarity that can exist between pathological science and cases of what has been deemed good science.

In the Millikan oil drop experiment, it was later discovered that Millikan had thrown some of his data out, despite the fact that the result, the value for the charge of the electron, was said to be determined as a result of the average of all drops (Collins and Pinch, 1998, Segerstrale, 1990). However, Millikan was "exonerated on the basis that he was "right"" (Segerstrale, 1990), and he was later proven to be right by researchers replicating his experiments. Good science isn't always ideal.

Political influence has had its place in good science. Just as Blondlot had a reputation and influence that helped maintain belief in his N-rays among many of his contemporary Frenchmen, Pasteur had similar influence that led to acceptance of his experiments disproving spontaneous generation. Pasteur was a top ranking, well-respected scientist during his time. It has been argued that acceptance of Pasteur's disproof of spontaneous generation theory had more to do with his influence in the scientific community than with how well-performed his experiments were (Collins and Pinch, 1998). Other researchers had supported the theory of spontaneous generation at the time, and they even had experiments that ostensibly supported the theory and were not fully accounted for by Pasteur. But, as Collins and Pinch describe, "the opposition were crushed by political maneuvering, by ridicule, and by Pasteur drawing farmers, brewers, and doctors to his cause." However, we now know the reasons why the spontaneous generation theorists' experiments worked in their favor (likely due to the presence of spores which resisted killing) and know that spontaneous generation is not a real phenomenon. But at the time, the acceptance of Pasteur's proof was more of a result of factors other than the convincing nature of his experiments.

Thus, it has been shown that there are instances where the symptoms are weak. But overall the symptoms are descriptive and elucidate many of the faults that researchers can suffer from. So let the reader be forewarned, the symptoms, although useful in gaining a perspective on pathological science, are not without caveats.

Science is Inherently Self-Correcting

Although pathology may plague science from time to time, science is inherently self-correcting. While individual scientists may be duped, eventually science will discover the truth. Pathological science may be an unfortunate but necessary accompaniment to normal science. Inevitably, mistakes are made or scientists are deceived. This is unavoidable. To circumvent the few cases of pathology that occur would require draconian measures that would stifle creative thought and scientific intuition. It would prevent many excellent researchers from making important discoveries. More instances of pathological science will continue to emerge in coming years. This is not a tragedy. It is to science's credit that the system inevitably cures its own pathologies.

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