Two ways of dealing with scientific fraud

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Science (and society) has only recently tried to come to grips with its exponential growth, most of which is bogus, false, pretentious, bold or irrelevant. Growing research on scientific fraud, of bogus and pseudo-science are telling signs that the need for criticism of science, or for a new sociology of science, has significantly increased in the recent years. But so far, this kind of debunking was considered to be an unglamorous enterprise. So far, criticism has mostly followed two paths: by debunking fraudulent research, or by debunking pseudo-scientific claims. Although both types are justified, they do not answer sufficiently the question: why so many people believe weird things. My contention is that people believe weird things, among other reasons reviewed by many scholars, because high- and hard-sciences give them reasons to. In other words, within high sciences (like astrology), scientists use the same unverified and wild claims as the pseudo-scientists do. Such claims unintentionally give credibility to pseudo-scientific claims of the same logical (and possibly epistemological) status. In order to substantiate the impartiallity of scientific criticism, I opt for a so called "symmetrical approach" to science and pseudo-science, promoted twenty years ago, in another context, by Edinburgh school of sociologists of science.

Key Words: *SCIENTIFIC FRAUD, PSEUDOSCIENCE, TRUST IN SCIENCE, UNVERIFIABLE HYPOTHESIS, NO-NONSENSE SCIENCE, SYMMETRY PRINCIPLE, CIRCLE OF CREDIBILITY*

In spite of the awareness of the existence of scientific malpractice and of irrelevance of a great body of scientific research, science has barely lost any credibility. This is mainly because it is still assumed that science is normally a self policing and self correcting practice, and that it is perhaps the only human endeavor where no external force of control is needed. According to that opinion, represented most notably by Robert Merton, bad methodologies, procedural mistakes, statistical make-up of data, false claims, fraud, will sooner or later be checked and thrown into oblivion.

But this self-correcting-science assumption has increasingly been put in doubt. In another classical book in the field, *Betrayers of the Truth,* by Nicholas Broad and Wade, authors cite numerous examples and reasons why this self correcting science assumption does not work. They begin their pathology of science with the following words: *According to conventional wisdom, science is a strictly logical process, objectivity is the essence... and scientific claims are rigorously checked by peer scrutiny and the replication of experiments. From this self-verifying system, error of all sorts is speedily and inexorably cast out. We began to doubt this view in the course of reporting some of the recent cases in which scientists had been discovered publishing results that were fictitious. (But) a more serious and general issue lay beneath these individual incidents: fraud was a phenomenon which the conventional ideology of science could not properly account for; therefor the ideology itself must be flawed or seriously incomplete... Fraud, we believe, offers another route to understanding science. By studying science through its pathology... it is easier to see the process as it is, as distinct from how it ought to be... Our conclusion in brief, is that science bears little resemblance to its conventional portrait... Science should not be considered the guardian of rationality in society, but merely one major form of its cultural expression* (Broad & Wade, 1982:7-8).

Let us for a moment approach the issue from another angle. In 1982 sociologist of science Derek de Solla Price claimed that out of the total number of existing scientists, which is 96% of all scientists that ever existed, we should take a square root, in order to get the number of relevant scientists. *(I)f you have published n papers in your life, the chance of your reaching 2n happens to be about 1 in 4. As an inevitable consequence of a completely lopsided skewed distribution of this sort, it follows that a small number of the men who write scientific papers happen to be responsible in the aggregate for half the production and a good deal more than half of the value if one weights each paper in any reasonable way. This small number, a hard core, may be estimated at about the square root of the total number of writing scientists, and together they constitute the heart of what has become known as the Invisible College of all the good people who really count in that neck of the scientific woods* (Price, 1982:166). Price's claim may be also used as a polite way of saying that the rest of scientists is active in something that can be called bad, bogus, imperfect or irrelevant science. Similar view was expressed by Martin Gardner in his classical book *Science. Good, Bad and Bogus*: *(W)e must not forget that for every example of a crank who later became a hero, there were thousands of cranks who forever remained cranks. We must not forget that for every outcast theory raised to respectability, there was thousands of crazy theories that permanently bit the dust* (Gardner, 1981:xiii). In view of such an estimate, science (and society) has only recently tried to come to grips with the its exponential growth, most of which is bogus, false, pretentious, bold or irrelevant. Growing research on scientific fraud, of bogus and *pseudo-science*, and also awareness of statistical estimates of Derek Price's kind, are telling signs that the need for criticism of science, or for a new sociology of science, has significantly increased in the recent years. But so far, this kind of debunking was considered to be an unglamorous enterprise. It has usually been treated with contempt, as a necessary by-product of big science, as a second-rate research. Recently, Stephen Jay Gould put forward the following opinion: *Skepticism or debunking often receives the bad rap reserved for activities, like garbage disposal, that absolutely must be done for a safe and sane life, but seem either unglamorous or unworthy of overt celebration* (Shermer, 1997:ix).

Towards what exactly should skepticism be addressed? Price, Gardner and Gould had only incorrect or irrelevant research in mind. But this is not necessarily all. Let us make a brief systematization. First, according to Price we have a vast body of irrelevant research and irrelevant scientists (estimated by Price to one half of all published scientific papers, and to a square root of the total number of scientists), who do not play a role on the scientific front. This however does not say anything about the truth of the other half. Second, only a fraction of "relevant" research may be true, or will yield some positive results. (I am not aware whether quantitative evaluation of this kind was ever made). According to the probability calculus, we may expect that at least a half of relevant research may prove to be incorrect. Studies of controversies in science, provided for instance by Engelhardt and Kaplan, or by Harry Collins for instance, may give us a rough picture that many results of scientific research are negotiated or established by consensus, and not by objective evidence. That in effect may be interpreted to mean that roughly only a half of scientists engaged in a controversy has originally claimed what in the end turned out to be true. Third, within a body of relevant science, there is a fraction of fraudulent research. This includes publication of false or produced facts, smoothing the curves, incomplete and unverified preliminary statements etc. Fourth, Bell has provided convincing cases of fraud and conflicts of interests while financing big science. Some of his cases, like the case of Edward Teller's X-ray lasers, describe research that has been sponsored by state agencies, but which will never be carried out. One of the morals of his cases for our evaluation is that a fraction of research that builds the research front, that belongs to science proper (and to Price's fraction of relevant science), will never yield any results. Here, we may perhaps add the category of *suppressed research*, which can be studied in its own right. Fifth, there is a body of pseudo-scientific work on pseudo-scientific phenomena (on UFOs, ESP, telekinesis and the like), provided by scientists and lay people,

which is not included in Price's evaluation. And finally, there is a growing body of pure scientific speculations, which belongs to the Price's positive fraction, because it is provided by "relevant" scientists.

This categorization is not meant to be extensive. But, even this may give us a rather grim picture. We see that debunking of scientific fraud is just a tiny fraction of bogus science research. I shall therefore extend the scope of the paper, in order to prevent us from believing that the only relevant part of *debunking business* in science is policing of fraud, or that we would establish the purity of science just by debunking fraudulent research. I shall use some of the categories above in order to show that even a fraction of allegedly "sane", proper and relevant science may be suspect, and that therefore researchers of bogus science should not restrict their scope of research only to those types of scientific research that are *prima facie* bad or suspect. And I shall claim that bogus science research is a research in its own right, since the exponential growth of scientific theories and hypothesis provides an increasing need to put speculations of fellow scientists in our and the neighboring disciplines under scrutiny.

Much of the enlisted work has been done so far. Some of the studies belong to the classics of sociology of science. Let me mention for example Diane Crane's *Gatekeepers of Science*, a criticism of the peer review system; studies on negotiations in science and studies of the reaching of consensus in scientific conflicts, works on ideological influences in science, monographs on Lysenko, Piltdown Man, Cyril Burt, etc. Several categories above have recently received a more thorough examination, like recent monographs on financial fraud in science (Bell), on suppressed research (Milton). Broad and Wade have started to extend the picture, and provided us with a broad pathological picture of science. Let me cite just a few additional categories:

1. Scientists do not repeat other scientists' experiments, because it does not pay (in terms of recognition), and it is normally not paid by sponsors.

2. Whistle blowing very often turns against the whistle blower, because it runs counter to the vested interests of the institutions (laboratory, university) where fraudulent work was performed, or sponsors; it is a sign of unreliability of a more general kind.

3. Scientific journals publish unverified data in order to sell. Overall, the scientific community is not rigorous about correcting mistakes. Being the first to publish is the highest priority even if it means making some mistakes. Errors introduced in the rush to publish may become a serious problem, if, because of competition, scientific journals start acting more like magazines and less like journals (Grinnell, 1992:114). Also, there is a high pressure on the scientists to publish unverified data.

4. Peer review system has increasingly become a nepotistic system, or rather a means for self promotion of the reviewer.

5. Career making very often collides with scientific ethic, and very usually the first one wins. Money (in the developed countries), political ideology, or religion (in the less developed) often provides the basic attitude for career making.

6. Scientists rarely check authenticity of colleagues' references.

7. Scientists often "polish" their data to fit into a preconceived hypothes.

8. Plagiarism is not rare among scientists, and in more pathological situations, there are cases of outright stealing of the whole research (while being in the peer review process).

Among scientists, particularly among the researchers of "impure science", there is a great disagreement about the purpose, as well as the methods of impure science studies. On the first, dominant or *asymmetrical* view, these studies should serve to avoid aberrations in science, to sharpen our critical abilities, to abandon false and pseudo-scientific claims, to debunk "cranks". This should be done for the sake of the proper work of science. According to

such a view, the episodes of fraud in science are marginal to the overall appraisal and general success of scientific method. Proponents of this view are highly committed to science, and they have high confidence that science normally "works". All one needs is a strong scientific methodology, which should be applied in order to test alleged scientific claims. When the test is performed, the false claim can be thrown overboard.

On the other, still marginal view, studies of bad and bogus science and of science in general, seek to provide evidence that there is either a) something fundamentally wrong with science as we know it today, or more usually b) that there exists no intrinsic demarcation between hypothesis in "hard sciences" and pseudo-sciences. According to the latter view there are no essentially different methods used in science and pseudoscience, there are no particular *a priori* reasons why we should have more confidence in a scientist than in a pseudoscientist with a wild imagination. Among bad and bogus science scholars, classical proponents of the first view, in slightly different ways, are Carl Sagan and Martin Gardner. Sagan is well known for his faith, popularization and achievement in science. Gardner, on the other hand, will probably be remembered rather for his debunking books, than for his mathematics. If science-debunking discipline ever gets established (as a part of sociology of science, or whatever), Gardner will definitely be one of its fathers. But unlike Sagan, Gardner sometimes directs his skeptical arrows towards authors and disciplines which rate high within scientific establishment. His closing chapters of his main book *Science: Good, Bad and Bogus*, are dedicated to astrophysicists who extend the scope of their scientific disciplines into domains only imagined. Also, Gardner says: *No one can define exactly what is meant by such words as pseudoscience, crank, and crackpot. The reason is simple. There is no exact way to define anything outside pure mathematics and logic... We all know there have been occasions when top scientists ridiculed ideas that later proved to be sound... I take for granted that all scientific hypothesis are conjectures to which scientists and laymen alike assign degrees of belief that vary between one and zero*. Nevertheless, Gardner believes in science, and his examples of scientific cranks rarely contradict usual opinions on who is justifiably a scientist and who is not. We shall treat Sagan's views in some detail later.

Classical proponents of the second view have so far been sociologists of knowledge, so called strong programmers, like David Bloor and Barry Barnes. They claimed that a proper investigation of science should be symmetrical. Symmetry requirement of their strong program says that we should study scientific beliefs without respecting our prior commitments and evaluations as to which scientific claims are true, and which are false, rational or irrational. Since we are not in a possession of absolute truths, since we are fallible etc.., there are strong reasons, why we should be impartial while investigating scientific beliefs and claims. This is the only way, says Bloor (1976), *to scientifically approach science, to emulate its own matter-of-fact, non-evaluative approach* (Barnes, Bloor, Henry, 1996:viii). Otherwise, we would commit the fallacy which science is supposed to avoid, and that is the prior evaluation of data. Among bad and bogus science researchers, the best example of such a view represent William Broad and Nicholas Wade.

Recent boom of bogus science research has not given an unanimous answer to the question: what is the purpose of such studies, of debunking and criticism of scientific practice. It is certain, though, that it provided arguments for both, pro-scientific and anti-scientific ideological views. In describing cases of fraudulent science, it has generally fulfilled the impartiality requirement of the strong program. Yet, the more general question remained. The simple division is slightly complicated by the vast amount of intermediaries, mostly represented by scholars who endorse *credo quia absurdum* view, by scholars who do believe in science, in spite our lack of reasons to support it, and in spite of scientists' inability to control the practice of science.

The key word for evaluating existing bogus science research that I shall use here is confidence and trust in science. While proponents of the first view have almost unlimited confidence in the present-day science, the proponents of the second are increasingly skeptical towards scientific claims and towards science in general, either for the reasons given above (by the Edinburgh sociologists of science who, for heuristic reasons, take everything people believe *cum grano salis*), or because they really believe that science is in the hands of bad people with bad intentions. Systematization of bogus science research would look like this:

Table 1. Categories of skepticism

	attitude: pro-science /field of study/ methodology: asymmetrical	attitude: anti-science /field of study/ methodology: symmetrical
confidence/trust	Sharmer / pseudoscience	strong programmers
	Gardner / pseudoscience	Wallis / "rejected knowledge"
	Sagan / pseudoscience	
	Rothman / pseudoscience	
	Perutz / science & war	
	American Skeptics / pseudoscience	
	Gross & Levitt / leftist "science"	
	Sokal / leftist science	
distrust	Broad & Wade / misconduct	Milton / "suppressed research"
	Lewontin / social use of science	Heideggerians
	Dewney / science classics	XX Century Marxists
	Grinnell / misconduct	Creationists
	Bell /science-financing, nepotism	
	Freeman / indiv. scientists	

In the first quadrant, there are researchers who attack various procedures in science. Their overall intention is to purify science from its malpractice and misconduct. In spite of their slings and arrows directed towards science, they generally believe that the demarcation between science and non-science exists. In the second quadrant, there are a number of scientists, philosophers and pseudo-scientists who distrust science in general. Their criticism of science is not intended to make science better, neither they believe that any kind of a more methodological approach could possibly make a progress. I especially emphasized Milton, because of his more recent account of science, and his rather paranoid belief that science is in the hands of a Mafia. Third quadrant is populated by Edinburgh-styled sociologists of knowledge. The conviction of the representatives of this sort is that we should take impartial view of science, in the name of science. It is named "antiscientific" because of the lack of respect towards the cognitive merits of scientific reasoning. Although it is rather doubtful whether there can be an anti-scientific attitude with "trust" and confidence in science, or whether the confidence in science is honest, I have allowed this combination to enter the picture. There is an important similarity of views in the first and the third quadrant, though. Both types of criticisms are primarily addressed towards science. Unlike *strong programmers*, representatives of the first quadrant are not necessarily committed to impartiality, neither to skepticism towards the strict demarcation line that divides science and non science. But I

shall claim that the arguments provided by representatives of the first quadrant may legitimately be used by the proponents of the *symmetrical approach*.

In what follows, I shall be mainly concerned with the 4th quadrant. What seems apparently suspect in this picture is the fact that many debunkers do not treat scientific claims on a par. Consider the fourth quadrant. The representatives in this quadrant rarely treat scientific claims as subject to scrutiny. Their targets are only those claims that conform to the already established demarcation of right and wrong, true and false, of what is science, and what is not. They usually claim that there is something like scientific methodology, that will prove pseudo-scientific claims to be wrong. But they rarely question what this scientific methodology, or science consists of, and in such a procedure, they violate their own principles of scientific testing. Let me illustrate this kind of thinking, and quote one criticism of such a view, i.e. Lewontin's criticism of Sagan. *To Sagan, as to all but a few other scientists, it is self-evident that the practices of science provide the surest method of putting us in contact with physical reality and that, in contrast, the demon-haunted world rests on a set of beliefs and behaviors that fail every reasonable test. So why do so many people believe in demons?... The only explanation that he offers for the dogged resistance of the masses to the obvious virtues of the scientific way of knowing is that through indifference, inattention, incompetence, or fear of skepticism, we discourage children from science. He does not tell us how he used the scientific method to discover the "embedded" human proclivity to science... There is no attempt (in Sagan's work) to provide a systematic account of just what Science and Scientific Method consists in... The book is not meant to be a discourse on method... Nevertheless if the exhortation is to succeed, then the argument for the superiority of science and its method must be convincing, and not merely convincing, but must accord with its own demands. The case for the scientific method should itself be "scientific" and not merely rhetorical. Unfortunately, the argument may not look as good to the unconvinced as it does to the believer* (Lewontin, 1996:29). Sagan is an excellent example of an asymmetrical approach, not only because he commits an asymmetrical fallacy, but rather because he, just like many other prominent scientists, does not see any contradiction in debunking "pseudo-science" on the presupposed and unquestioned definition of science and an implicit demarcation criterion, while at the same time engages himself in speculation and procedures that are not consistent with his own requirements. The examples of the latter are his belief in extraterrestrial life and the rock from Mars fraud. Sagan is a very useful personification here. His work presents us with virtually all the problems faced by the scientific debunkers, and by many sociologists of knowledge. First, this sort of debunking is obviously biased and partial, since Sagan's attention is not directed towards all problematic beliefs equally. Second, his debunking is unreflective, because he does not apply the same rules and tests, he requires from his pseudo-scientific subjects, to his own views. Third, and this is the most important, it is radically asymmetrical. By this, I mean not only biased, or partial, but rather asymmetrical in the sense that he allows scientists to use procedures and speculations of the kind strictly and explicitly forbidden to his fellow-pseudoscientists.

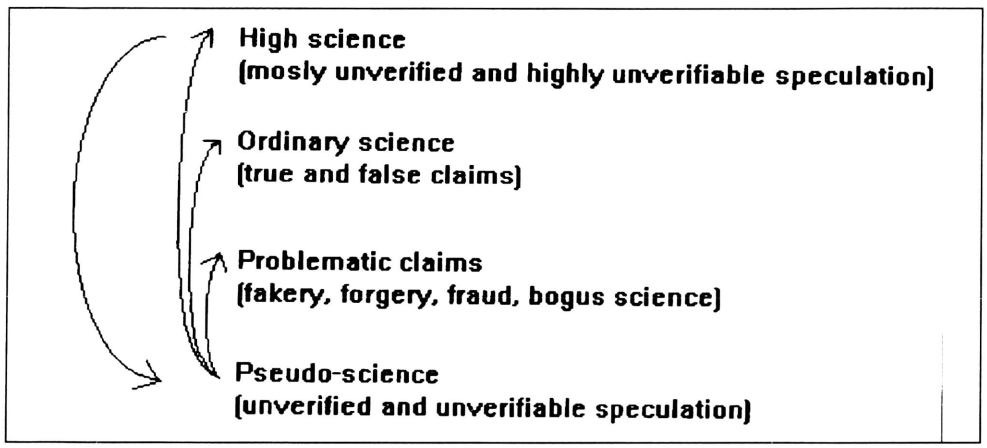
The simplistic normative picture of science and anti-science provided by writers like Sagan would be the following:

Table 2. Asymmetrical approach to scientific claims (case: Carl Sagan)

Scientific claims	**Pseudo-scientific claims**
speculation allowed	speculation not allowed
claims proven wrong are incorrect	claims proven wrong are irrational
fraud in the name of science allowed	fraud in pseudo-science should be illegal

Now we have a better picture of what is wrong with asymmetry, and what are prima facie reasons for endorsing the symmetrical stance. While the controversy around the strong program has centered around the ontological, and epistemological issues, here we see that the consequences of asymmetry might have a deeper moral significance. Instead of asking ourselves, does a symmetry requirement have a deep ontological and relativist commitment, which leads us in the end away from science and rationality, we may now ask the question: in the name of what should one and the same procedure by legitimate if it is labelled as scientific, and forbidden if it is labelled un- or pseudoscientific? Before I turn to the problem of asymmetry, let me draw a picture presenting my view.

Table 3. Circle of credibility



Instead of having a continuum where "high science" of astrophysics presents one side, and pseudoscience of telekinesis the opposite side of the spectrum (the picture drawn from Sagan), the studies of fraudulent and bogus science draw another one. According to this picture, there is a scale of "verisimilitude", of truthfulness, or rather, of fakery. But also, perhaps even more importantly, high science can be as bold as pseudoscience. The opposites match, and complete a circle. In his text *Scientific Elites and Scientific Illiterates*, David Goodstein claims that there is no hope of having a no-nonsense science. We have no money, says Goodstein, to bring about the kind of education among laypeople, in order for them to asses the merits of certain theories and speculations. The same claim is uttered by James Randi, in his article *Time for Science To Take a Stand Against Popular Superstitions*. At the same time both authors claim that we should stand against popular superstitions.

This strikes me as being barely consequent. A consequential picture would be the following. Either (1) you claim that speculations (of scientific or pseudo-scientific kind) should be tested, or at least, require that a test should be designed in advance, in order to assess their potential empirical content or you (2) allow speculations to float in the world of weird ideas, unless proven untrue.

If you take the first option, which is nowaday almost universal, on what grounds do we accept wild speculations of astrophysicists as being scientific, and of ESP pseudoscientists as unscientific? They are both untested and, seemingly, untestable. If you take the second option there is no need to design a demarcation criterion for separating pseudoscientific and scientific beliefs in advance. As a matter of fact, in this case a separation *via* demarcation cri-

terion is inconsequent, illogical and unscientific. (It is obvious that I am following the proposals given by Karl Popper.) In both alternatives above, we have a symmetrical approach to science and pseudoscience. If, on the other hand, we design an apriori criterion of separation, which allows scientific speculation, and rejects sc. pseudoscientific speculation, we shall use an asymmetrical approach, the most common among scientists and so called American Skepticists.

Why do people believe weird things, indeed? Apart from the obvious and notorious reasons, quoted all-too-often, like religious convictions, search and need for emotional consolation, ideological indoctrination, social recognition, and belonging to a group, which allegedly lead people to believe in UFOs, telekinesis, and other similar things, there is one serious additional reason I want to discuss. It is the fact that "high" science has increasingly become an anti-common sense science. Mostly it cannot be understood by the broad public, not even by the fellow scientists, but rather only by the few. It follows that it cannot be properly appraised by the public whom science should serve. It follows, that the body of science has increasingly become an uncontrollable enterprise. Also, it follows, that science, taken from the sociological point of view, increasingly becomes a dogmatic, taken-for-granted enterprise, in both respects, as an ideology, as well as concerning its contents. And these are the reasons for discontent, reasons which stand behind and which generate studies of fraud, pseudoscience and criticisms of scientific practice in financing, and political decision making. And these same reasons may generate weird beliefs. As a matter of fact, it may even be claimed that precisely wild speculations of the high science give rise to pseudo-scientific beliefs. Consider for example Einsteinian twin paradox, or EPR paradox, or duality of light, or paradoxes of infinity in mathematics. These examples are obviously beyond common sense. But, even if we admit the merits of such posits, theoretical entities, thought experiments and hypothesis, there is a constant danger, in which astrophysicists are nowadays especially engaged, to treat them as more than they are - real, instead of a mere speculation.

Before I turn to weird examples taken from high science, I just want to add: what I mean by no-nonsense science is the testable science, and not only testable in principle, but with an already given design of a crucial test that would make it possible to overthrow the speculative claim. This kind of trial was suggested by James Randi for ESP, but it is strikingly absent from present-day evaluations of speculations in physics.

Let us consider several quotations from highly respected scientists. The first one is from Roger Penrose: *I don't know how clear cut these experiments are, but there are experiments done on the timing of consciousness, and they seem to lead to a very odd picture which doesn't quite make a consistent sense... It does look like there is something odd about consciousness, somehow as if the future affects the past over a very tiny and limited scale, something in the order of a fraction of a second. And there is no reason to believe that one's conscious experience shouldn't be a part of somebody else at some stage. I do not know what happens when one dies, but it's a plausible picture that you could be somebody else. And that somebody else could be somebody who lived in the past, not in the future* (Brief History of Time, Paramount Pictures, directed by Errol Morris, 1991).

Or consider another one, taken from a popular textbook on science: *If physicists regard the universe as having, not the familiar four dimensions, but instead, an additional spatial dimension, the properties of light emerge naturally from the equations describing this five dimensional world. Adding even more dimensions gives rise to still other forces of nature. Indeed, the number of dimensions that appears to best explain nature is 10... The notion of a 10-dimensional universe may be impossible for ordinary folks to comprehend, but it is a key part of a newly ascendant model of the universe known as the superstring theory... Steven Hawking argues that time travel must not be possible, since if it were, modern civilization would already have been overrun by tourists from the future... To escape apparent randomness (of quantum mechanics),*

some researchers suggest that in fact all the possible scenarios exist simultaneously, but in multiple, parallel universes that are independent of each other... It might be possible for people to travel through time and slay their grandfathers, but these events would merely trigger the creation of a parallel universe where the person was not born, leaving the other, more familiar universe intact... The idea that these mini-universes might be connected to each other via wormholes gives Kaku hope that some future civilization might escape the Big Crunch, the theoretically possible collapse of the universe (William Allman: *Beyond the Top Quark*, Science Horizons Yearbook 1995, Collier).

Consider further a quotation from astrophysicist dr. John Gribbin, from his book *White Holes* (quoted from Gardner): *The spectacular production of bent spoons produces the wave of astonishment from the audience, releasing a flood of tachyons which travel backward in time to cause the spoons to bend just before they are produced to cause the surprise. If such a process could be triggered deliberately, it would explain telepathic phenomena, the direct tachyonic communication between minds... Perhaps this tachyonic link even provides a clue to such mysteries as poltergeists!* In the similar tone, let me mention John Taylor, mathematical physicist at the University of London, chosen by the journal New Scientist in 1975. among 20 top world scientists ever. In his book *Black Holes* (1973) he thought it possible that Earth was visited in the distant past by extraterrestrials who may have come in spaceships driven by black-hole power generators. Speaking about the end of the Universe, Taylor further speculates: *The only chance of immortality then is in an oscillating universe. Even in that, everlasting life will not be of the usual form but one in which there may be no relation at all between one cycle and the next due to enormous re-scrambling of matter in the collapsed phase.*

All of these quotations were uttered by "respectable" scientists (those Price would include into his tiny fraction of relevant science) who talk nonsense, or apparent nonsense. The quotations above give implicit justification for wild imagination of the lay people. Since broader public, as well as scientists in other fields have no specialized knowledge allegedly required to combat such apparent nonsense, it is only natural that wild imagination flourish. Therefore, in a sense, it is not difficult to answer the question why people believe weird things. They believe in weird things because their beliefs are not so far of the mark in comparison to the scientists' ways how to build hypothesis. Consider for instance an account of Stephen Hawking's philosophy: *I do not think thought should be restricted at all. Why shouldn't you go on thinking the unthinkable. Think how many things were unthinkable a century ago, and yet people have thought them. And they also seemed quite unpracticable. So not all things Steven is saying are to be taken as a Gospel of Truth... he is a searcher... and he sometimes talks nonsense. Well, don't we all? People must think, they must go on thinking, they must try to extend the boundaries of knowledge.*

This claim is justified, but only provided we did not make an *a priori* distinction between "thinking the unthinkable" of scientists and non-scientists, only if we simultaneously hold that such scientific "thinking of the unthinkable" has the same problematic status as the "thinking of the unthinkable" of the lay people. The additional complication arises when scientists are so convinced in the just role of science in "extending the boundaries of knowledge", that they even feel compelled to fake the evidence in the name of the cause. The most recent and convenient example (since it involves Carl Sagan, who is at the same time one of the main skeptics when pseudo-science is concerned, and NASA, a respected American scientific agency) is the rock from Mars. As one critic comments: *Someone found a rock somewhere that had some fossils of germs on it. Normally it would seem a terrible stretch, but these people now tell us that they can not only tell that the rock is actually a meteorite, but to top it off, they can actually identify it's origin as coming from Mars... And as if that were not enough, the germ fossils that they supposedly found there are supposed to prove that there was at one time*

Life on Mars. And of course, it naturally follows that we need to rocket right up there and check it out.

So, what is wrong with American Skeptic Society led by Shermer and Sagan? In both cases, the absence of skepticism towards science is obvious. American Skeptics (Sagan, Shermer) are wrong in not applying the same methods of criticism to both, science and non-science. They operate with an implicit definition of science and the demarcation line that divides first from the second, and put only "pseudo-science" under scrutiny. We are therefore naturally led to question: Why is scholarly and public scrutiny not directed towards science itself, but rather to the obvious absurdities only? Is our belief in science and scientists so unreserved, that we should not put it in question? To quote Lewontin again: *Our willingness to accept scientific claims that are against common sense is the key to an understanding of the real struggle between science and the supernatural. We take the side of science in spite of the patent absurdity of some of its constructs, in spite of its failure to fulfill many of its extravagant promises of health and life, in spite of the tolerance of the scientific community for unsubstantiated just-so-stories, because we have a priori commitment, a commitment to materialism. It is not that the methods and institutions of science somehow compel us to accept a material explanation of the phenomenal world, but, on the contrary, that we are forced by our a priori adherence to material causes to create an apparatus of investigation and a set of concepts that produce material explanations, no matter how counterintuitive, no matter how mystifying to the uninitiated.* Now, it may be true that we have prior materialistic commitment. It may also be true that this commitment more probably generates true results. But this commitment does not mean that it necessarily generates true results. There is such a vast array of error possible even within this "commitment". And as a matter of fact it does generate false results, in spite of our commitments.

This is precisely the reason why, while evaluating other people's beliefs, we should take impartial stance. The morale for a researcher of bogus science, and his methodological requirement should therefore be: **in spite of our prior commitments, of our metaphysical convictions and values, of our scientific way to approach science and beliefs in general, we should analyze science impartially and symmetrically.**

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DVA NAČINA RAZOTKRIVANJA ZNANSTVENE PRIJEVARE

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Sve veći broj istraživanja o prijevarama u znanosti, o bespotrebnim i pogrešnim istraživanjima i o pseudo-znanosti, u uvjetima eksponencijalnog rasta znanosti, rječiti su znakovi potrebe za novim, i kritičnijim pristupom znanosti (unutar ili izvan sociologije znanosti). Dosad je, međutim, "raskrinkavanje" pseudo-znanosti i prijevara u znanosti bilo područje istraživanja koje je rijetko donosilo nagrade njihovim praktičarima. Dosad je kritika uglavnom slijedila dva pravca: 1) raskrinkavanje prijevara u znanosti (pri čemu su "veliki znanstvenici", mitske figure poput Galileja, Newtona i drugih često bili izuzeti od kritičkog povecala); 2) raskrinkavanje pseudo-znanstvenih uvjerenja i hipoteza. Premda su obje vrste pristupa potrebne i legitimne, nijedna od njih nije uspjela na zadovoljavajući način odgovoriti na pitanje koje često postavljaju brojni suvremeni "skeptici": kako to da tako mnogo ljudi, znanstvenika i neznanstvenika vjeruje u lude i pogrešne stvari. Moj je stav da ljudi vjeruju u "lude" stvari - pored ostalih razloga o kojima govore istraživači loše i prevarantske znanosti (o kojima je riječ u ovome članku) - i zbog toga što ih na to navode neke "čvrste" znanosti, poput današnje astrofizike. U nekim područjima visoke znanosti (kao što je astrofizika), znanstvenici se koriste istom vrstom nedokazanih, ludo-hrabrih hipoteza, kao što to čine i pseudoznanstvenici. Bez svjesne namjere, takve znanstvene tvrdnje pružaju legitimnost pseudoznanstvenim tvrdnjama istog logičkog (a katkada i epistemološkog) statusa. Kako bih potkrijepio pravilnost nepristranog pristupa u kritici znanosti i pseudoznanosti, u ovome članku zalažem se za tzv. "simetrični pristup" znanosti i pseudoznanosti, koji su pred dvadesetak godina, u drukčijem kontekstu, koristili sociolozi edinburške škole, kao i "falsifikacionisti" u filozofiji znanosti, koji su tvrdili da su ludo-hrabre hipoteze legitimne (u oba slucaja) ako unaprijed navode uvjete vlastite empirijske opovrgljivosti.