

Pathological Disbelief

Brian D. Josephson

Department of Physics,
University of Cambridge

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WARNING

Readers may find some of the ideas in this lecture disturbing; they may conflict with various deeply held beliefs.

What is the issue?

The ‘generally accepted view’ regarding a phenomenon can be wrong in two ways:

(a) a non-existent phenomenon is considered real (e.g. N-rays, polywater); or

(b) a real phenomenon is considered non-existent (e.g. continental drift, meteorites)

Langmuir’s much-quoted lecture on Pathological Science dealt with case (a); the interest here is in case (b). What makes the scientific establishment, in some cases, vehemently deny phenomena for which there is strong evidence?

Case 1: meteorites

The issue: do meteorites have an extra-terrestrial origin?

Argument in favour: visual sightings, stones found at site of apparent landing, often warm

Incorrect argument against: 'objects falling from space contradicts laws of mechanics'

Alternative explanation offered: optical illusion, stone struck by lightning.

Cause of capitulation: massive meteorite falls near Paris

Case 2: continental drift

Arguments in favour (Wegener, from 1912):

- fit of S. American and African coastlines (Bacon 1620)
- matching fossils, rocks
- coal found in the Antarctic

Argument against: claimed phenomenon is impossible

This case shows how effortlessly the scientific community can dismiss 'bizarre ideas', even if they are supported by very strong evidence.

Cause of eventual capitulation: other geological observations led to theory of plate tectonics

Case 3: 'cold fusion'



In 1989, Pons and Fleischmann claimed to have measured excess heat in a Pd-D electrolytic cell in quantities too great to be accounted for except by a nuclear process.

But others failed to reproduce the effect:



“And what we see in our laboratory is, no evidence for any unusual nuclear or chemical reaction.”

In just a few weeks it was all over — apparently.

“My conclusion is that we are suffering from the incompetence and perhaps delusion of Drs. Pons and Fleischmann.” — Steven Koonin (American Physical Society spring meeting, Baltimore, Maryland, May 1, 1989).

“For all practical purposes, the cold fusion episode ended a mere five weeks after it began on May 1, 1989. All three scientists from Caltech executed between them a perfect blocked shot that cast cold fusion right out of the arena of mainstream science.”
— David Goodstein, Caltech

Huizenga and Maddox give their views ...



“We learned a very negative report, and concluded that the results that were being presented to us were contrary to everything we had found out about nuclear physics over the last 50 years.”



“I think that, broadly speaking, it's dead, and it will remain dead for a long, long time.”

The committee's argument appeared to be the following:

1. We know of no process which can generate the amount of heat claimed that does not at the same time generate far more radiation than was observed.
2. The observations have not been reproduced by others.

Therefore, the observations must be flawed.

They did not however, as they should have done, investigate the experiments in any detail.

In fact, the nuclear measurements carried out in order to try to establish what the process was that generated the excess heat were inaccurate. But the assertion of Pons and Fleischmann that heat was generated in excess of anything that could be accounted for in terms of chemistry depended on their calorimetry, which has never been successfully challenged. The DoE committee seemed not to have appreciated this fact. And the Pons–Fleischmann excess-heat observations have been replicated many times since, in many different laboratories.

Cold Fusion Reproducibility

from a survey conducted in November 2003 by Steven Krivit
(24 out of 43 of those contacted for the survey responded)

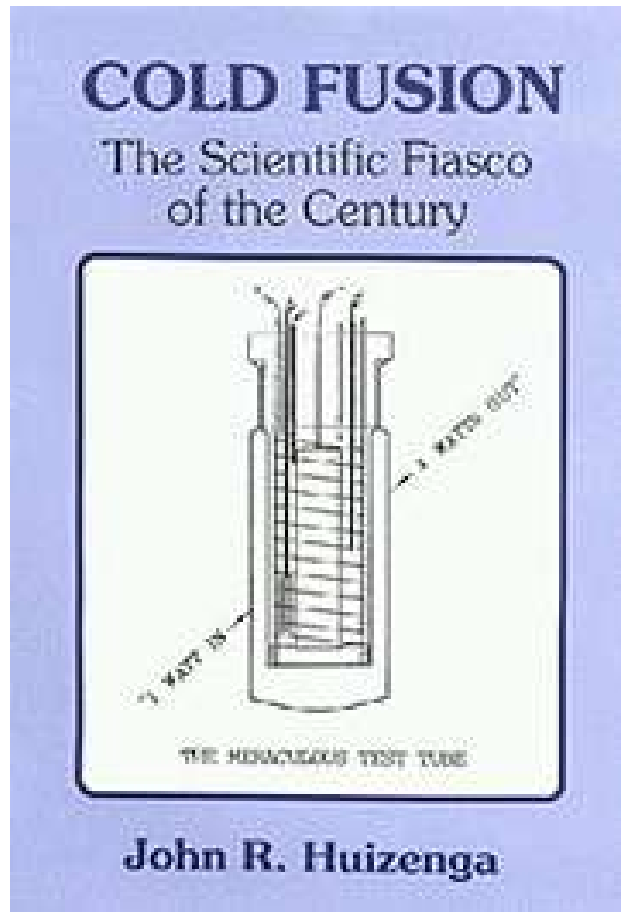
Researcher's Nationality	Field of Degree Obtained	Years of Cold Fusion Research	Years of Hot Fusion Research	Estimated Number of Experiments Performed	Reproducibility Rate 5 Years Ago	Reproducibility Rate Last 12 Month	Do You Conclude That Nuclear Activity is Occurring?
Italy	Chemical Engineering	na	yes	na	na	50	na
Russia	Condensed Matter Physics	18	na	1,000	na	60	Yes
Italy	Physics	14	16	300	40	75	Yes
United States	Mass Communications	13	no	6,000	25	75	Yes
United States	Physical Chemistry	14	no	200	10	80	Yes
United States	Metallurgy	14	no	3,000	50	90	Na
Japan	Nuclear Engineering	14	20	20	70	100	Yes
Romania	Atomic Physics	10	no	40	70	100	Yes
United States	Radiochemistry	14	no	700	50	100	Yes
Russia	Nuclear Rocket Engineering	13	2	3,500	na	100	Yes
TOTAL ESTIMATED EXPERIMENTS				14,720			
AVERAGE REPORTED REPRODUCIBILITY					45%	83%	

na = Not Available

Q. how do you persuade the scientific community to believe that something is the case when there is insufficient evidence to make a proper case?

- state that the claim being made contradicts scientific understanding
- claim the experiments are faulty
- **MAKE YOUR POINTS LOUDLY**, and make them before time has shown them to be incorrect; with any luck, the major journals will then refuse to publish the relevant information when it becomes available.

Write a book with a title such as '**cold fusion: the scientific fiasco of the century**', and get the right people to give it glowing reviews:



'An authoritative, frank, hard-hitting account of the cold fusion fiasco.' **GLENN T. SEABORG**

'As a distinguished nuclear chemist he is uniquely qualified to evaluate the field. Cool, dispassionate scientists and policymakers will receive his book, I trust, with the respect it deserves.' **FRANK CLOSE, NATURE**

The Department of Energy said sceptically:

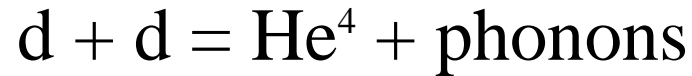
“Nuclear fusion at room temperature, of the type discussed in this report, would be contrary to all understanding gained of nuclear reactions in the last half century; it would require the invention of an entirely new nuclear process.”

Comment: it does happen from time to time in science that a discovery is made that is ‘contrary to all present understanding’ in the field concerned (e.g. the discovery of the acceleration of the expansion of the universe and the non-zero cosmological constant). It also happens occasionally that a new process is discovered!

“A shortcoming of most experiments reporting excess heat is that they are not accompanied in the same cell by simultaneous monitoring for the production of fusion products. If the excess heat is to be attributed to fusion, such a claim should be supported by measurements of fusion products at commensurate levels.”

Comment: it is legitimate to conclude that fusion is involved if alternative mechanisms cannot account for the amount of heat produced, and not necessarily a shortcoming for fusion products not to be measured (cf. the way strong diamagnetism is used as a diagnostic for superconductivity, since superconductivity is the only known mechanism giving rise to strong diamagnetism).

Can one make sense of the claims? The ‘entirely new nuclear process’ may well be



X.Z. Li et al. (next slide) argue that tunnelling, probably augmented by a resonance, is the relevant process, instead of thermal activation as in high-temperature fusion.

Hagelstein, and Chubb and Chubb, argue that coherence effects are of importance in transfer of energy to the lattice. Some combination of these ideas may explain the observations, though quantitative computations have not, as yet, been executed.

Sub-barrier fusion and selective resonant tunneling

Xing Zhong Li, Jian Tian, Ming Yuan Mei, and Chong Xin Li
Department of Physics, Tsinghua University, Beijing 100084, China

(Received 2 August 1999; published 19 January 2000)

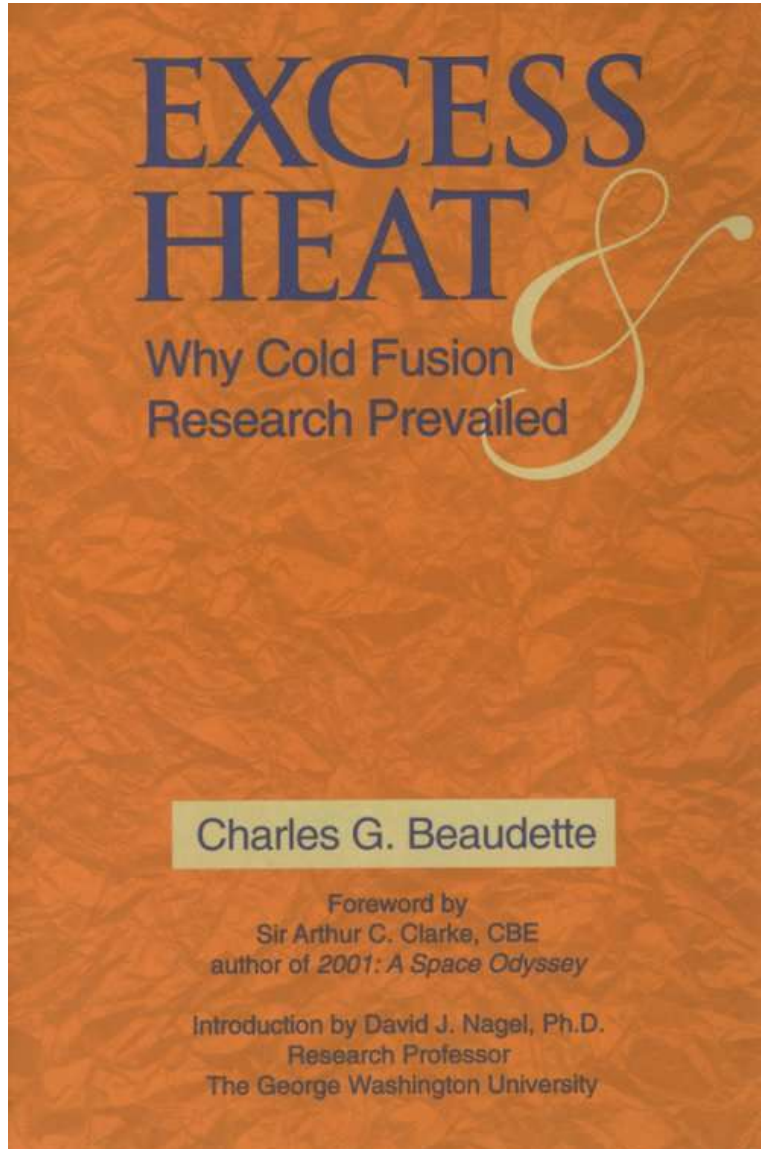
The cross section of deuteron-triton sub-barrier fusion is calculated using the selective resonant tunneling model with the assumption of a square-well nuclear potential. A complex potential is assumed to describe the absorption inside the nuclear well. The surprisingly good agreement between the theoretical calculation and the experimental data implies that the compound nucleus model might not be applicable to the light-nuclei sub-barrier fusion. Instead, the selective resonant tunneling model is proposed.

(and again, from X.Z. Li, *Fusion Science and Technology* 41, 63–68 (2002):

“... resonant tunneling [is usually] treated as a two-step process [with] decay independent of tunneling. ... this is not true in the case of light nuclear fusion. ... the wave function will reflect back and forth inside the nuclear well.

“In conclusion, the nuclear physics for subbarrier fusion provides a new approach toward nuclear fusion energy with no strong nuclear radiation.”)

and now for a rather different view of the matter:
(not reviewed in Nature!)



“a masterful presentation, clearly reasoned and argued.”
—Michael McKubre,
SRI International

“a monumental work of scholarship”
John O’M Bockris,
Texas A&M University

Characteristics of scientific sceptics, according to Beaudette:

1. They do not express their criticisms in those venues where it will be subject to peer review.
2. They do not go into the laboratory and practise the experiment along with the practitioner.
3. Assertions are offered as though they were scientifically based when in fact they are mere guesses.
4. Satire, dismissal and slander are freely employed.
5. When explanations are advanced ... ad hoc reasons are constantly advanced for their rejection. These reasons often assert offhand that the explanation violates some conservation law.
6. Evidence is rejected outright if it does not answer every possible question at the outset.

“The sceptics avoided learning the necessary skills, refused the measured data, did not care how well the data was measured, refused invitations to the laboratory to experience the gathering of data, demanded that nuclear effects be found, and when they were found rejected each such instance.”

Marcello Truzzi asserted at one time that ‘extraordinary claims require extraordinary evidence’, but later decided the concept was incoherent.

In fact, the evidence that changes minds is often quite ordinary: what is of relevance is the psychological factors that may inhibit acceptance of the evidence.

The use of a Bayesian statistical analysis, setting a very low value for p_{init} (prior probability), is merely a way of glossing over the fact that one is highly prejudiced, by retreating into the world of mathematics. (e.g. setting $p_{init} = 10^{-30}$ is effectively synonymous with ‘I wouldn’t believe it even if it were true!’).

Pathological Disbelief

The behaviour of the scientific community as described has a pathological component in that the conclusions that were arrived at are, in all probability, different from those that would have been arrived at if the evidence available had been examined more objectively (had the committee members had not taken a predominately negative attitude to the evidence). It may be of value to consider what the various causative factors ('risk factors') might be.

1. the claims were dramatic, and appeared not to accord with existing knowledge.

2. the critics were, by and large, workers in paradigms different from those relevant to the actual research (e.g. in nuclear or plasma physics, as opposed to electrochemistry and calorimetry). This may lead to problems with evaluating experimental methodology, and to a focus on irrelevant details (+ ‘the tribal factor’).

3. replication was not straightforward, success being sensitive both to methodological details and to material factors.

4. aggressive attitudes soon prevailed.

All these factors are liable to lead to a situation where a wrong conclusion is reached and adhered to. Once the idea is reached that the field as such is pathological, all papers in the field tend to be barred from the journals, and the normal processes of assessment break down.

Which leads us to the issue of myth ...

What happened with cold fusion (and what happens in other cases as well) was the creation of a *myth*, the myth that the phenomenon was unreal. Such a myth consists an elaborate story, which in principle might be true. The two main responses of interest are

- (i) accept without serious questioning
- (ii) question the story before accepting it.

(i) occurs when there is a strong disposition to accept, *because it confirms a belief system*
(ii) is the more scientific response, which sometimes gets overridden, especially when strong emotions are involved.

Some scientists are especially prone to whip up emotion ‘in the cause of science’ (or so they believe).

This leads us to the key question:

... where else might such a situation prevail?

Case 4: memory of water (Benveniste effect)

The claims:

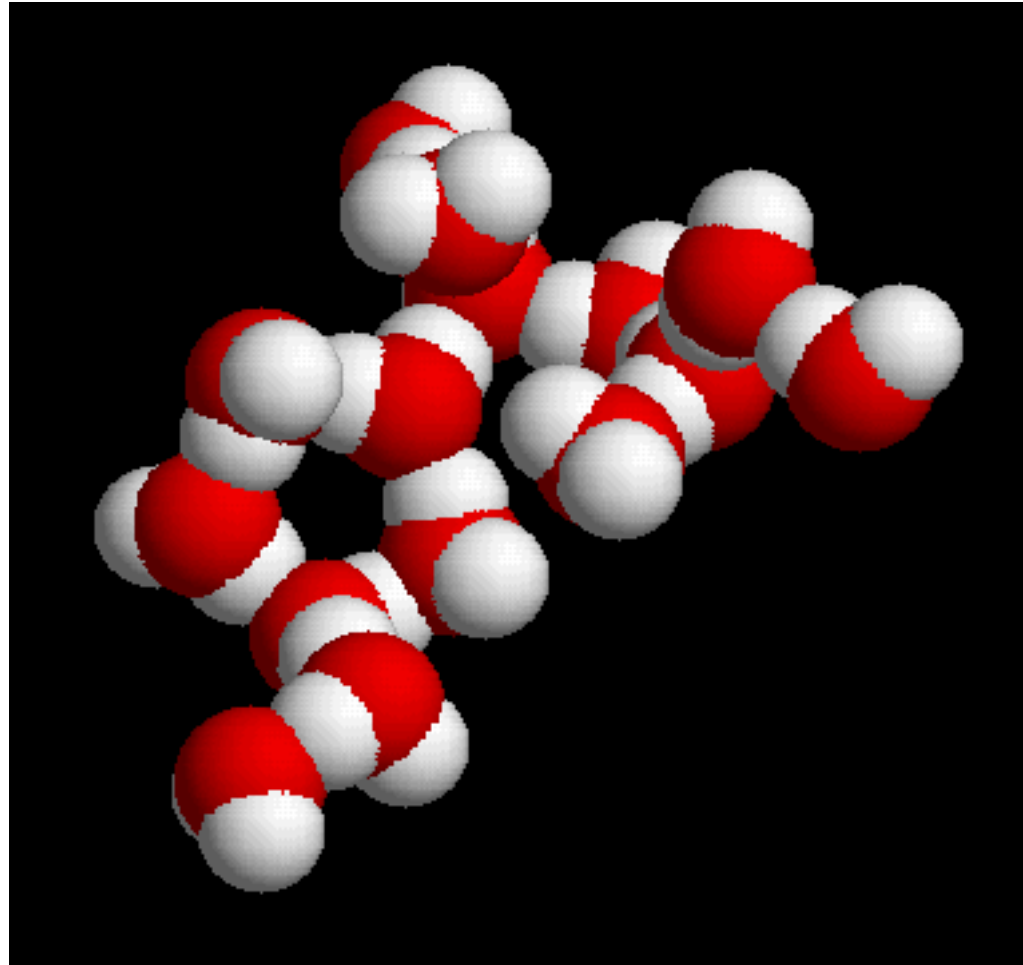
1. biological activity present in highly diluted solutions
2. electromagnetic induction of biological activity

conventional objection: highly diluted solutions are just 'pure water'

most people think of liquid water as just a collection of H_2O molecules wandering around randomly

but experts on water know that 'pure water' is really not so simple ...

The complexity of water (simulation by Errington and Debenedetti)



15-molecule cluster
 $q_c = 0.85, T = 240 \text{ K}; \rho = 1 \text{ g/cm}^3$

Nature's referees could not find any fault in Benveniste's research.

In the end, the Editor proposed a curious deal: we publish your paper, and then you let us come and investigate and discover what is wrong.

And so three non-biologists (Maddox, Stewart and Randi) set out to Clamart one day to observe (for a week), and pronounce judgement.

They think they have pinned down some 'errors', and publish an article about it (non-refereed, à la Beaudette).

A biologist might have found the investigators' horror at the fact that the research was funded by a medical firm with an interest in the outcome rather amusing.

As non-biologists, the investigators could well have misinterpreted things Benveniste told them about the experiment. Who knows whether the various criticisms of the techniques made in the article regarding the techniques were adequately informed?

A watchful referee would have suggested, I think, that the investigators had accumulated insufficient data to justify their very strong conclusions.

But who cared about those minor difficulties? The investigators had accumulated enough data to satisfy the editor (who was, conveniently enough, also one of the investigating team). And so, in accord with Beaudette's 1st. and 4th. precepts and my own 3rd. principle, an article duly appeared in Nature under the title

High-dilution Experiments a Delusion

A subsequent experiment by Benveniste using blind counting, thereby refuting the explanation given by Maddox, Stewart and Randi for his past successes, was refused publication in Nature.

Benveniste continued to develop his experimental techniques, and is currently investigating the question of the optimal conditions for obtaining the memory effect. The conventional journals refuse to publish any of this research.

We have the same phenomenon as before: a field of research can be given what is, in effect, a fatal blow, by determined attackers.

Power may be more helpful than being right!

Let us examine two important ‘vehicles of power’:

1. The physics preprint server, arxiv.org

2. The Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP)

First, the *good* features of the preprint server (*all quotes taken from articles on the arxiv.org site*):

“Until recently, there were few effective options for physicists to break into an intellectually void closed loop involving only publisher and library systems ... the on-line electronic format will allow us to transcend the current inadequate system for “validating” research in a variety of ways. No longer need we be tied to a one-time all-or-nothing referee system which provides insufficient intellectual signal, and a static past database. We eagerly anticipate a vastly improved and more useful electronic literature, taking advantage of the flexibility afforded by the electronic medium and unhindered by artifacts of its evolution from paper.

“What then is so essential about the arXiv to its users? The immediate answer is ‘Well, it's obvious. It gives instant communication, without having to wait a few months for the peer review process.’ ”

Now the serious *problem*. To quote from Ginsparg's article again:

“From the outset, a variety of heuristic screening mechanisms have been in place to ensure insofar as possible that submissions are at least of refereeable quality. That means they satisfy the minimal criterion that they would not be peremptorily rejected by any competent journal editor as nutty, offensive, or otherwise manifestly inappropriate, and would instead at least in principle be suitable for review (i.e., without the risk of alienating or wasting the time of a referee, that essential unaccounted resource).”

Regrettably, the archive's administrators appear to not to make any distinction between 'manifestly inappropriate' and 'unorthodox'.

In an attempt to promote proper discussion of the issue, I tried in 2002 to upload a survey by Storms (see <http://pw1.netcom.com/~storms2/>) to the archive, but the moderators frustrated this intent by deleting the review declaring it, as is their habit, "inappropriate" (chemists, being a more robust species than physicists, were permitted to see it on their own server chemweb.com).

The official word from the Librarian at Cornell:

“I am comfortable with our policy that the contents of arXiv conform to Cornell University academic standards.”

begs a number of questions!

Very occasionally, the archive's moderators will respond to complaints, but not however in a way that suggests a rational or responsible attitude. The response to my querying an assertion that "the submission was removed as inappropriate for the cond-mat subject area" was this:

"The answer above appears correct. If it is research in nuclear fusion then it would necessarily be classified as Nuclear Physics. If it is not research in nuclear fusion, then it is neither Nuclear Physics nor Condensed Matter Physics. In either case it is inappropriate for the cond-mat subject area."

Aristotle might have had a little difficulty with this logic!

Case 5: the paranormal

on the one side: the people who quietly get on with investigating the paranormal, gradually improving the methodology.

on the other, those who **SHOUT** their objections, e.g. (some) members of the organisation *CSICOP*, but don't actually take proper note of what is going on.

They have been successful in almost entirely blocking access of parapsychologists to the regular journals, thereby preventing others from having a clear idea of what is happening in the field.

CSICOP is the name of an organisation whose fundamental aim is to influence minds by attacking ‘incorrect beliefs’. As Hansen’s review notes, its magazine The Skeptical Inquirer “nearly always presents only one side of a controversy in its articles. Although SI sometimes publishes letters of complaint, full papers from CSICOP’s critics almost never appear.” Now we will see some of its propagandists (or mythmakers) at work.



James Randi



Nicholas Humphrey

Randi and Humphrey have refreshingly different styles, as you can hear in the discussion of ‘the Royal Mail Nobel Centenary Stamps affair’ (for details of this affair, see <http://www.tcm.phy.cam.ac.uk/~bdj10/#stamps>),

which can be heard on the BBC’s web pages:

<http://www.bbc.co.uk/cgi-bin/radio4/today/listen/audiosearch.pl?ProgID=1002031547>

Here (see 2nd. paragraph) is the article that generated all the excitement:

PHYSICS AND THE NOBEL PRIZES

(included in a booklet accompanying Royal Mail's Nobel Centenary stamps):

“Physicists attempt to reduce the complexity of nature to a single unifying theory, of which the most successful and universal, the quantum theory, has been associated with several Nobel prizes, for example those to Dirac and Heisenberg. Max Planck's original attempts a hundred years ago to explain the precise amount of energy radiated by hot bodies began a process of capturing in mathematical form a mysterious, elusive world containing 'spooky interactions at a distance', real enough however to lead to inventions such as the laser and transistor.

“Quantum theory is now being fruitfully combined with theories of information and computation. These developments may lead to an explanation of processes still not understood within conventional science such as telepathy, an area where Britain is at the forefront of research.”

This is CSICOP representative Randi in full swing:

“There is no firm evidence for the existence of telepathy, ESP or whatever we wish to call it, and I think it is the refuge of scoundrels in many aspects for them to turn to something like quantum physics, which uses a totally different language from the regular English that we are accustomed to using from day to day, to merely say, oh that's where the answer lies, because that's all very fuzzy anyway. No it's not very fuzzy, and I think that his opinion will be differed with by the scientific body in general ...”

One wonders ... can Randi seriously be suggesting that explanations based on quantum mechanics are, on that account alone, automatically inadmissible?

... while Humphrey simply dismisses the whole business by *fiat*:

“Well, I think the idea that quantum physics explains the paranormal is an unnecessary idea, because there's nothing to explain.”

He did, it is true, write a book about the subject. Unfortunately, as it happens, my examination of this book failed to find any arguments for his thesis that stood up to careful analysis: see

<http://www.tcm.phy.cam.ac.uk/~bdj10/psi/humphrey.html#humphrey>

Let's see how other people reacted:

Herbert Kroemer:

'I am highly sceptical,' said Professor Herbert Kroemer of Santa Barbara University. 'Few of us believe telepathy exists, nor do we think physics can explain it. ... if the US postal services did something like this, a lot of us would be very angry.'

In fact, surveys have shown that the proportional of scientists who think telepathy is likely to occur is not inconsiderable. Why exactly people get so angry in such circumstances is an interesting psychosociological phenomenon (violation of a taboo? Or what?).

Anger seems to be a common feature of situations of this kind. Why that might be? Let us speculate, following up the question with an imaginary dialogue ...

In essence, because something has been done that is contrary to the scientific ethic, one might say a disgrace to science.

Why exactly is mention of telepathy, etc. impermissible in a scientific context?

Because telepathy is known not to exist.

Has that been proved scientifically?

Not with complete rigour, but it is generally recognised in the scientific community that those who believe telepathy exists are deluded, and thereby worthy of censure. It is reprehensible even to suggest that it might be a real phenomenon.

and now let us hear another CSICOP member:

‘It is utter rubbish,’ said David Deutsch, quantum physics expert at Oxford University. ‘Telepathy simply does not exist. The Royal Mail has let itself be hoodwinked into supporting ideas that are complete nonsense.’

Comment: where did the usual precautionary phrase ‘in my opinion’, normally prefixed to statements of this kind, get to?

And again: ‘If engineers or doctors accepted the level of proof that is accepted by paranormal supporters, bridges would be falling down round the country, and new medicines would be killing more than they cure.’

It may be relevant to note that Deutsch, has never, as yet, responded to any emails people have sent him asking how much he has actually studied the parapsychological literature. And, in defiance of his assertions, the CIA's 'remote viewing' group ['remote viewing' is the name given to a human capacity believed to exist involving the ability to 'see' what is happening at a distance], when asked if they might be able to help locate some missing H-bombs, in a plane that had crashed somewhere in Africa, and which people seemed keen to locate, fulfilled their brief in less than a day's focussed attention (see Joe McMoneagle's account in his book *The Stargate Chronicles*).

And, in regard to the matter of drugs, drug testing was an unfortunate case to choose. It would in fact be closer to the truth to state the following:

“Many people end up taking drugs that are of no benefit to them at all, because drug trials are not carried out with the same degree of rigour as is the norm for experiments in parapsychology.”

The sociology of attitudes to the paranormal

Usually, experiments and their analysis determines what the scientific community thinks about a subject.

With parapsychology a dominant factor is editor power, (the ability to control journal content), combined with the ease of making denunciations if the situation is such that, as is typically the case, assertions that are made do not have to be properly substantiated.

Here we have an extract from an unusually candid letter from a *Nature* editor:

*“We are not keen at all on considering an article about the paranormal, but if you think there is something significantly new to be said on this well-worn and **antiscientific** topic and want to submit an article ... I will read it, discuss it with my colleagues and let you have our views.”*

Conclusion: *why bother with facts, when it's so much easier to be an armchair critic?*

Now what about the argument 'if X were true, we'd have to start all over again?'

I have news for such people: physicists *did decide* they needed to start all over again (string theory, M-theory, quintessence, cosmological constant ...). Anything goes among the physics community (time travel, cosmic wormholes ...), just as long as it keeps its distance from anything remotely mystical or New Ageish, because we, the keepers of that special kind of knowledge we call science, are *quite certain* that such people have it all wrong

In conclusion:

“We think that we think clearly, but that’s only because we don’t think clearly”.

I am grateful to Charles Beaudette and Steven Krivit for their assistance with the preparation of his talk. Some of the pictures used were extracted from the video *Fire from Water* (www.infinite-energy.com)