

## CORRESPONDENCE

Madam,

I am repeatedly astonished at the complex design of basic experiments entered into by scientists seeking to determine the nature of supposed "paranormal" events such as metal-bending. As a layman, I have not found it difficult to originate basic, simple test procedures that satisfy all criteria except one—they invariably lead to negative results. And that result does not seem acceptable to the researchers who spend valuable time, intellect and money on this work.

As an example, I designed a direct test of PK powers that used simple Perspex tubes, sealed permanently at both ends and containing security-check devices that would immediately indicate tampering through force. A piece of nitro-cellulose "paper" was included which matched up to another piece, retained by me. Any large rise in temperature that would possibly lead to metal-deformation if applied, was thereby obviated. The seals applied consisted of parts of a printed currency bill, the remainder of which was retained as well. Thus, any substitution was prevented. Invisible (but fluorescent) markings were also applied, and the tubes were carefully and accurately weighed.

These devices, containing ordinary rods of aluminium chosen in various non-harmonic lengths, were presented to John Taylor of King's College, with the provision that they be returned a year from that date to me. The tubes had been prepared and documented at King's College Dept. of Biophysics under my direction. That was years ago. I have never seen the tubes again, nor am I able to determine what happened to them. Not only that, but the conditions under which they were issued were violated from the very beginning, so the test was invalidated.

One of the provisions insisted upon by Professor Taylor was that air-access be provided in the tubes. This was done by means of "blind" vents, so that tampering was not possible. Why, I may ask, is a similar system not used, for example, in Hasted's experiments? I venture to guess that such security, simple though it is, results in negative results.

Design of adequate experiments is not at all difficult. The materials need not be ponderous, expensive or complicated. Consider the experiments I attended at Bath recently at the invitation of Harry Collins. A Miss Judy Knowles was highly touted as a spoon-bender, and I was there bearing my well-worn cheque for US \$10,000 as an

inducement for her to perform. She sat at a table, encouraged by Professor Hasted, for hours while attempting to bend a simple teaspoon, and nothing happened. We had designed a very direct system. A videotape set-up viewed Miss Knowles, both into a diagonal mirror and directly (to provide simultaneous views from two directions) and the bowl of the spoon was lamp-blacked to prevent her applying pressure there without detection. Nothing happened. . . .

Earlier, a similar videotape arrangement was used on Jean Pierre Girard in Grenoble, France. While I, in company of two representatives of this Committee, sat patiently, Girard attempted to bend a bar of the same metal which he had previously bent successfully, but under very poor observing conditions. We sat for 3½ hours. Nothing happened. . . .

All these experiments, and many, many others I have designed and sat through, were easy to design and met with the approval of all concerned, though several "psychics" have balked and refused the tests when told of the conditions. The Taylor Tubes cost less than £1 apiece, and were quite simple.

It has been said repeatedly in your pages that these matters are difficult to test, and that experiment design is complicated. Not at all. If I, a layman, can design inexpensive and direct, secure tests for the existence of paranormal powers, surely the scientific body can do likewise. The truth of the matter is that the simple, direct tests just do not produce positive results. Is it not time that we began to doubt the existence of these wonders?

I will not accept the cries of learned man that are directed against my supposed naiveté in these matters. True, I possess no high degree of learning. But my experimental design will stand up against that of any scientist who would care to dispute me. My offer is still open: I will award US \$10,000 to any person who can produce ONE paranormal demonstration or event under satisfactory conditions. I await comments from performers or researchers.

**James Randi**

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The writer is a Member of The Inner Magic Circle, London, an Executive Board Member of The Committee for Scientific Investigation of Claims of the Paranormal, and author of many articles on the paranormal as well as a book "The Magic of Uri Geller" (Ballantine, 1975).

Madam,

Mr. Randi's astonishment at scientists' complex designs is matched only by the scientists' astonishment at Mr. Randi's naiveté. He imagines he can disprove the existence of a phenomenon without even beginning to understand what it is.

The scientists find apparently paranormal time-variations of nominal strain, residual stress, dislocation loop density, micro-hardness, grain structure, electrical resistance, specimen dimensions, etc., under well-witnessed conditions using precise measurements and often without human touch. (Sometimes, but not always, spontaneous bending and fracture can occur as a result of these things). But Mr. Randi merely gets other scientists to build a toy with "security check devices" within which he *demand*s that metal be bent in sealed perspex tubes; *why does he expect it will be, and if it does not, why should that disprove "a phenomenon"?*

It is true that it would be interesting to know just how large a hole, or "blind vent", must be left in laboratory glassware before metal could be bent inside. But this is only an interesting peripheral question, and is given different answers by different investigators. For example, I myself would answer, "for a certain subject I worked with nearly two years ago, 2mm diameter holes; baffles, "blind vents" of my design, sintered glass plugs and collodion seals inhibit". Dr. Wolkowski of the Université de Paris, France, would answer: "Girard has bent a nail, a metal strip and a spring inside identified laboratory-sealed glass tubes without orifice". (Photographs are widely distributed in Europe and are in my possession).

Another "adequate experiment" mentioned by Mr. Randi consisted in video-tape witnessing a "highly-touted" child bend a simple teaspoon under rather complicated protocol, which no doubt did not greatly assist the spontaneous phenomenon; that "nothing happened" is rather a bold statement to make without any tests other than matching against another teaspoon (!) being performed. Nevertheless, the "experiment" as such was negative.

I refused to act as judge in this "experiment" because of the crudity of the technique (pace Harry Collins, who is interested in the sociology and not the physical accuracy). Even the protocol I could fault on two counts, although I deliberately kept quiet; the spoons were unlabelled, and *only one side* of the bowl was lamp-blackened, so that a finger could be inserted underneath; I mention these things in order to deflate Mr. Randi's claim that he is a better witness than scientists. The "very poor observing conditions" in successful experiments with Girard have been, in my personal experience (July 23, 1977), better than those of Mr. Randi. At least the specimens were of precisely defined dimensions and yield strength, and were labelled by engraving. Incidentally the

Grenoble scientists indignantly deny allegations, as reported in the *New Scientist*, that this was not the case in their experiments.

The limit of human strength for production of a typical bending moment with both hands is  $\approx 25$  Nm (male) and  $\approx 15$  Nm (female) with children varying between  $\approx 10$  and  $\approx 20$  Nm. Whereas cutlery bends fall in the range  $\approx 5 - \approx 15$  Nm, many tougher specimens, requiring moments as high as 80 Nm have been bent under witnessing, with precautions against substitution. I myself published in this journal successful results of witnessed shear epoxy-resin fractures requiring draws of thousands of newtons.

Experiment design had better be left to professional experimenters and not to professional deceivers.

J.B. Hasted

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#### A CASE OF ESP

Madam,

On 31 May 1977 my daughter Anna visited an amateur psychic in Perth, who makes only occasional appointments and does not ask for fees. Anna had arranged this by telephone and did not give her name to the psychic before or afterwards. Anna has visited my new home, 10 miles from Perth, only for a few short visits. I had never heard of the psychic.

On returning home, Anna spent well over an hour telling my wife and myself things which the psychic had reported seeing in her crystal ball. We were astonished by the number of "hits". Some of them were verified only later, or their reference was recognised only later. I shall describe only one, which concerns myself.

Anna said that the first thing that the psychic reported seeing was a hospital operation; she had then seen the patient being roughly handled by nurses and left sitting in a bedside chair with a black cloud round his head. I had returned home the day before after a major operation in which a neuro-surgeon had reconstructed my neck with the use of a bone graft. I then told Anna about the most traumatic experience of my fortnight in hospital. Two days after the operation, three bad-tempered nurses had man-handled me out of bed, *one of them pushing the back of my head from behind!* When I had nearly reached a vertical sitting position, I had a black-out and fell backwards. I was left, dazed, fixed in a bedside chair feeling like death slightly

little is known about ESP precisely because findings are so inconsistent between one research and another. A determined attempt to tackle the "experimenter effect" in the manner suggested would not only help to win over some sceptics, it would likely provide a better understanding of what really goes on in ESP experiments.

D. J. WEST

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Madam,

In replying to my letter (the *Journal*, December 1977) Professor John Hasted incorrectly represents the stand of this Committee and my personal stand. Says Prof. Hasted, "He imagines he can disprove the existence of a phenomenon . . ."—but the CSICP has never claimed this, nor have I. To disprove a claim is not our purpose; to insist upon logical and scientific reasoning and procedure, is our aim. Nor have I ever claimed, as Hasted says, that I am ". . . a better witness than scientists". I *do* claim that I am a better witness than *some* scientists . . .

Nowhere in the discussion do I find any support for his statement that scientists have found various miracles "under well-witnessed conditions". In his own work, in particular, he constantly refers to incomplete observations and poor viewing conditions. When his experiments in Grenoble were conducted under good conditions, as usual, nothing happened.

I have never "demanded" that metal be bent in sealed perspex tubes; I cannot understand Hasted's use of that term. I have merely gone along with his claim that such a miracle is possible, but having my \$10,000 on the line, and not just some grant money, I find it essential to do a *proper* experiment, not one in which holes large enough to admit trickery are allowed, *and* the subject is not watched at all. I am not that much a fool.

We have seen the marvellous Wolkowski photos, and are not at all impressed, though more naïve observers were. Knowing the tendency of para-investigators to omit data and engage in semantic evasion, I would like to see the *evidence*, not an account of a fictitious event. The rest of the Girard "proof" was of such a ridiculous nature that I have

lost all respect I might have had for his supporters in France. I had once thought them to be honest men.

As for the Judy Knowles test, I remind Prof. Hasted that I was invited to Bath *by his wife*, and Hasted himself attended willingly. All who officiated there agreed in advance that the protocol was correct, sufficient, uncomplicated and proper in all respects. The method of testing—and we said in advance that we were looking for a *bend* of a certain degree—was as perfect as could be hoped for. Unknown to Miss Knowles and Hasted, the spoons *were* marked, and the under-side of the spoon was not blackened, since we agreed *in advance* that Judy would hold the spoon in a certain manner, and determined all possibilities of trickery. Prof. Hasted, *I do know what I'm doing*. Apparently you do not, since you are unaware that *experimental aims are announced in advance, as well as the protocol*, and not altered after the results are in!

As for the professor's reference to the "crudity" in tests I have performed, I must inform him that the Girard samples *were* marked, not only by stamping, but by striping, to prevent rotation and substitution, something that did not occur to Prof. Hasted. The samples were supplied by Pechiney, and examined and catalogued by myself and a scientist fellow-member of CSICP and an independent scientist. It was a scientific test. It was a negative test—as was a subsequent one with Girard conducted by Bernard Dreyfus, using the same protocol.

Hasted closes his comments by saying, "Experiment design had better be left to professional experimenters and not to professional deceivers." I agree. But it had better not be left to incompetent and gullible experimenters who fail to observe the simplest of precautions. When scientists get back to science, we "deceivers" can get back to the business of entertaining, rather than explaining the real world to the academics.

JAMES RANDI

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Madam,

I am very pleased to read in Mr. Randi's letter that he does not claim to be able to disprove the existence of the metal-bending phenomenon.

Although in my published work I sometimes (although not "constantly") "refer to incomplete observation and poor viewing conditions", I make no such reference so far as the children's dynamic strain data are concerned. Most of my own observations in Britain

have been conducted under well-witnessed conditions, just as they were in Grenoble; on that occasion nothing happened, no doubt because in the television production the child merely acts the part of a metal-bender, and spontaneous phenomena fail to appear. Indeed, the very concept of ability to produce phenomena to order is not yet to be accepted. Girard and Geller frequently fail.

Mr. Randi's only answer to Dr. Wolkowski's photographs of three major bends inside sealed laboratory glassware is to claim that they are a "fictitious event". On what grounds, other than that of mere abuse? To be sure, these tubes were not sealed to good vacuum standards by drawing and rotation, but instead were softened and pressed together; but they are still sealed glassware; and Dr. Wolkowsky has other tubes, sealed by drawing and rotation, with minor bends inside. Girard has also achieved a minor bend (0.5 per cent strain) in a tube sealed in the Pechiney laboratories by Dr. Bouvaist. But since Mr. Randi does not now believe Girard's "supporters" in France to be honest men, no doubt this claim also fails to satisfy him. Recently a child has returned one of Dr. Beloff's sealed tubes to me, with a  $1.55 \pm 0.04$  mm bend in the brass strip (not the first minor bend I have seen in a sealed glass tube given to the children). The subtleties of what is possible to fabricate through small holes in glass are too great to consider in a short letter, but I may mention that my use of periodic photography and inspection of a complicated glass-enclosed scrunch during production is a powerful protocol which has not been broken.

Data of differing quality continue to accumulate, but Mr. Randi merely continues to impugn the honesty of all and sundry. Since legal action is repugnant to most researchers, his \$10,000 is absolutely safe, putting temptation in the way of already stressed families, and serving only to distract attention from the conjurors' inability to duplicate the effects which have been observed.

Mr. Randi assures us that he does know what he is doing, and that apparently I do not. Alas, his real intentions are becoming apparent to all.

My criticism of Mr. Randi's observations of Julie Knowles is simply that by modern standards they were crude, and had not the capability of measuring small effects. Even if the spoon had curled upwards, the protocol would have forbidden its acceptance. I would not accept responsibility for such an experiment although I came to be involved, and sat with the subject and Dr. Pinch, thereby not being "privy to the spoon-marking arrangements" as the protocol reads.

The possibility of rotation and substitution of Girard's bar samples occurred to me at my very first viewing of a Girard television performance in 1976, as those research students present will testify. Indeed

I critically discussed these elementary principles in public at the Reims *Rencontre de Parapsychologie* in December 1975. Stripping the bars is unnecessary provided that a good rolling test is conducted on a flat surface in camera, as it was in the successful NBC television production I witnessed in Paris.

The protocol devised by Mr. Randi and others was subsequently used by Professor Dreyfus and others on 18 June, 1978; this has been reported as a negative test because Dreyfus alone amongst the witnesses and in disagreement with the conjuror present, judged that manual force had been used by Girard in producing the 14 Nm bend. Fortunately, the videotape is in my possession and it can readily be seen by all how contentious this judgement is. It is precisely because such situations arise that the more sensitive instrumental methods of investigation—without human touch—are preferable. Out of sociology, into physics.

Mr. Randi closes his comments by hoping that scientists can get back to science so that he can get back to the business of entertaining. But he has never left it; nor have metallurgists left their metallurgy, nor the instrumentalists their instruments. Paranormally induced structural changes in metal, without bending, are claimed; an account of some of the French observations appear in the *Revue de Metallurgie*, March 1978, and I am preparing a further report myself.

J. B. HASTED

Madam,

I have been away from Britain for a considerable part of the last few months, and I have only just opened my *Journal* of December 1977. This contains a mis-statement of fact which should, I think, be corrected before it passes into the history of psychical research. On page 694, it is claimed that Adrian Parker was awarded the first doctoral degree ever to be awarded for a research project in extrasensory perception. Even if the term "doctoral degree" is, as seems to be assumed here, taken to exclude the Ph.D. degree, this is not the case. S. G. Soal was awarded the D.Sc. at London for a research project in ESP in 1948. If we adopt the more ordinary usage of including the Ph. D. in the term "doctoral degree", the earliest doctoral degree would seem to be that of Albert Coste who received his Ph.D. from the University of Montpellier in 1893 for a thesis on "Les phénomènes psychique occultes".

The further statement that Adrian Parker received the first Ph.D. in parapsychology since the war is also incorrect. White and Dale, in their *Parapsychology: sources of information*, list no less than 14 Ph.D.s awarded for parapsychological theses between 1944 and 1972. Of