

---

## From Our Readers

---

### Dowsing tests deficient?

As a recent subscriber to your publication I must say that it is quite unlike any other magazine I have seen and is enlightening in many respects. However, the article by James Randi on dowsing in the Fall 1979 issue seems to sidestep many of the requirements of a controlled scientific experiment. In fact the experiment seems to be as biased toward disproving dowsing as those in the "popular press" are biased toward "proving" such paranormal abilities.

Although the dowsing experiment was performed under controlled conditions—that is, the site selection, preparation, etc., were chosen by the experimenter rather than by the subjects—it was definitely not a controlled experiment. A controlled scientific experiment would have a *control group* of subjects, and conclusions would be based on the *difference* in performance of the avowed dowsers and the control group of "ordinary" people. As any high school science student knows, the purpose of the control group is to cancel out the effect of outside influences on the experiment and thus isolate the effect of the parameter being studied. Experimenters often go to great lengths to ensure the equality of test and control groups, using such techniques as double-blind studies, where even the experimenter does not know which group is which.

Another objection is the poor design (by modern scientific standards)

of the dowsing experiment. One shortcoming is that the pipe layouts were determined by one human being and the test subject was another human being. This actually favors successful dowsing because human beings are remarkably similar in their conception of randomness. Ask any two people to mentally conceive a sequence of random digits and they will be not only statistically similar but decidedly nonrandom. The experiment would have been much better if flexible hose had been used and a computer had randomly selected the paths (within certain constraints, such as a constant hose-length). An added benefit would be the ability to compare dowsing results statistically with chance results. Also the numerous excuses made in the article for lack of time, lack of water, last minute changes in the plot size, change in procedure based on preliminary results, etc., did little to build confidence that the experiment was well designed.

The most serious objection however is the lack of *sensitivity* in the experiment and the utter impossibility that even an incredibly accurate dowser could have won the prize. To prove this point, let's assume that the pipes were made of iron and that instead of a dowser we have a man with a metal detector. Given the conditions of the experiment (8-cm-diameter pipe buried 50 cm underground with a tolerance band 20 cm wide) it is unlikely that this man could have won the prize either. My trigonometry says that there would

be only a 2 percent difference in signal strength (the pipe is a *line* source, so the inverse square law does not apply) between the center of the pipe path and the edge of the tolerance band, assuming the detector's head was very small. The difference would be even less with typical head diameters of 30 cm. If the detector's signal pickup determined the loudness of a tone, that 2 percent would be only 0.17 dB, considerably below the 1 dB differential threshold for humans. If one hypothesized a "psychic signal" from water flowing in the pipe, not only would the dowser have to be sensitive to differences less than .17 dB, but the psychic signal-to-noise ratio would have to be greater than 34 dB just to get 50 percent of the pegs within limits!

In conclusion, the only point that the dowsing experiment proved is that dowsers grossly overestimate their capabilities. One cannot say that significant dowsing was not demonstrated, only that the sensational type of dowsing seen in old western movies was not demonstrated. Further conclusions will require performing properly designed experiments of greater sensitivity.

Lest one conclude that I am a firm believer in dowsing, I wish to make a couple of additional points. ESP, if it exists at all, is going to be a very small effect when observed under controlled conditions. If it was a large effect, it would have already been accepted as a common everyday event rather than being immersed in controversy. Being a small effect, it will take sensitive experiments to observe it. To give an idea of what a sensitive experiment is, consider the thousands of *tons* of water and other liquids used in neutrino and gravity-wave detection experiments or the great effort being expended to determine if saccharin is really a carcinogen. To me, if ESP experiments can consistently show a statistically irrefutable performance level above chance, even if it is 50.001 percent, an explanation is required as surely as if the performance level was 100 percent.

Such an explanation might very well hypothesize "second order" statistical phenomena, much like the quantum mechanical and relativistic effects in physics, which are extremely small under normal circumstance, rather than ESP.

Hal Chamberlin  
Manchester, N.H.

*James Randi responds:*

*Mr. Chamberlin makes a number of interesting points, some of which resulted from my necessarily incomplete account of the total protocol employed. All of this is a matter of record but was condensed for SKEPTICAL INQUIRER use.*

*"The experiment seems to be . . . biased toward disproving dowsing . . ." Really? The dowsers had by far the advantage, providing that a power exists to do what they claim. They at all times said that they were confident of success, that they were proceeding successfully, and, afterwards, that they had succeeded. They offered no complaints about protocol, approved each and every step of the adopted procedure, and in fact agreed that no "control" nondowsers were required because it would be impossible to tell if these controls were also using dowsing powers. That all persons possess this power, they agreed. Thus, controls were useless.*

*"[In] a controlled scientific experiment . . . conclusions would be based on the difference in performance of the avowed dowsers and the control group of 'ordinary' people." Not so. There was no ambiguity at all about the rules, and careful and agreed parameters were established in advance, well beyond those suggested by the dowsers, in order to give them an advantage. The "difference" would only be a criterion if the rules allowed any uncertainty in judging. If an archer says he can hit a bull's-eye 7 times out of 10, it is not at all difficult to decide whether*

he makes good his claim. It is a matter of simple numbers. We counted the percentage of pegs that were placed by the dowser within certain limits of the buried pipes. It was a matter of mathematics, a digital measurement rather than an analog.

Chamberlin complains of the "poor design" of the tests. He says the layouts were "determined by one human being." True, they were, but in a random manner. A heavy ball was rolled from outside a delineated square on a large board and transferred to the master chart. It was a practical and efficient method. And it worked. The eventual pipe layout was "rounded off" to accommodate the materials used, of course; and hairpin turns had not been allowed by the dowsers, so these were cut out for that reason.

Flexible hose, preferred by Chamberlin, was not usable because one of the dowsers had said that rubbery substances inhibited his powers. He had tried a flexible material once, and failed. We were required to go along with that requirement. And as for objections concerning our excuses re lack of time and water, and change in plot size, these were unavoidable. The test, as it was, cost many thousands of dollars, and, most important, all the subjects agreed to the changes without reservation. An exception was the depletion of the water, which served to prove that Professor Borga continued to imagine he was dowsing long after the water had ceased to flow.

Next, Chamberlin raised serious objection to the "lack of sensitivity" in the tests. The fact is that all subjects told us what they could do, and within what limits. We then increased the diameter of the pipe, raised the depth to half what they declared suitable, provided twice the volume of flow, and gave them one-third more leeway in approaching the actual pipe path in their determination of its position. The actual conditions thus gave them many times the optimum conditions and parameters, and all agreed we had been most generous. One declared that I was a fool

to be so liberal, since it was obvious (to him) that he had won the prize.

Remember, the dowsers were able to detect the pipe very positively in an exposed condition and also as it went beneath the ground to the junction point, at which position it was 50 cm deep. Only when they did not know the location did they fail, though they still got strong reactions.

Furthermore, none of the dowsers agreed on where the natural "underground streams" were in that designated area. And when a strong (very strong) magnet was buried just 3 cm in the ground during a subsequent test (unreported), neither of the two dowsers who said they were very good at that test could find it in a one-meter-square area. However, when the location was given, the sticks twisted and dipped appropriately.

In conclusion, Chamberlin says that, if dowsing powers exist at all, they are doubtless very small in order. Agreed. But according to the experts in this subject, dowsers can even "unfailingly" detect small currents flowing in a copper wire. Surely that is an easy test, and simple to conduct? Yet the president of the American Society of Dowsers, through his spokesman, Z. V. Harvalik, says that tests of dowsers are "superfluous" and that dowsing is "amply established." I invited this convinced dowser to win my \$10,000 prize in tests that he would consider adequate. I await a response.

### Tests of "thoughtography"

Scott and Hutchinson committed several errors in their article in your Spring 1979 issue concerning tests of Masuaki Kiyota's "thoughtography." Included among them is Hasted's hailing the production of a "whitey," only for the sales representative of Polaroid to point out a hole in the camera. Scott and Hutchinson, who were not present, are not to know the several gross inaccuracies in that statement, which comes close to being

legally actionable.

Morris Smith is supposed to have told Scott and Hutchinson that he wasn't exactly sure how a film pack can be taken to pieces, each frame being exposed to light, and reassembled. But, in fact, he explained this in detail to Hasted, with a description of the traces of evidence that would remain and the statement that he had searched the pack for such traces and not found them. (Hasted has since searched other, more recent Masuaki packs and found nothing.) This differs from Scott and Hutchinson's statement that Morris Smith twice said it was not possible to tell if the film pack had been tampered with. Hutchinson has more recently claimed to have found a method without leaving traces, but Polaroid has not investigated this claim to my knowledge.

Finally, journalist David Tharpe's claim that the camera and the film had not left his or his girlfriend's sight during the two-hour lunch-break should not be dismissed. No doubt he will communicate, Sir, with you.

John B. Hasted  
Birkbeck College  
University of London  
London, England

Michael J. Hutchinson replies:

Although the article in the SKEPTICAL INQUIRER was a joint effort between Christopher Scott and me, I alone was responsible for gathering the information concerning events that took place before our arrival at Granada's tests. It is therefore only right that I alone answer Professor Hasted's letter, which I do with the full knowledge and approval of Christopher Scott.

I find it difficult to respond to the professor's first paragraph for its lack of information. Just what "errors" is the professor referring to? His "hailing" the production of the whitey, or reference to a hole in the camera? In an attempt to

find out I tried to contact Jeremy Fox, the TV producer, and Morris Smith, the technical (not "sales") representative from Polaroid, to obtain their comments on the "whitey" incident. Unfortunately, due to a labor dispute that affected Granada Television for nearly three months, it was not possible to contact Mr. Fox. I have subsequently learned that he has now left Granada and is working in the United States.

However, Morris Smith has written to me as follows: "... Your report on the 'whitey' incident is exactly as I remember it, with one slight [my emphasis] difference. It was not a hole that had been left, but the base of the diode was not completely opaque and when the connection was removed light filtered in." This is a more technical explanation than the one given to me by Jeremy Fox. The main point is the same. It was the removal of the connection that allowed light to filter in.

Perhaps the professor is objecting to the use of the word "hailed," which seems to be the only other possible error. (Unless Hasted had not found the day to be as monotonous as suggested by us.) Both Jeremy Fox, immediately after the event, and Morris Smith, some thirteen months later, have spoken of Hasted's "excitement" upon production of the "whitey." In order not to cause him undue embarrassment, we chose to use the word "hailed." The professor's poor attempt at pointing out "gross inaccuracies" that are "close to being legally actionable" has only highlighted the events of the day.

Morris Smith's method for exposing each frame of the film involves the removal of a staple that holds all eight frames in the pack and prevents them from being pulled out together. Therefore, examination for tampering means looking to see if the staple has been opened. (The staple is fairly wide and is very firmly fixed. Do not imagine that it is like the staples which hold the pages of this journal.) Whilst not impossible, it would be