Dowsing Successful

H.-D. BETZ

In a recent issue of SKEPTICAL INQUIRER, J.T. Enright (1999) repeats criticism (Enright 1995) on one of our dowsing studies, the Scheunen-Experiment ("nothing but random results") and generalizes his negative opinion to all dowsing effects.

In fact, our initial analysis of 843 double-blind trials with forty-three operators yielded considerable statistical significance (p=0.0007; see Betz 1995), although the individual hit-rate turned out to be exceedingly small. Enright (1995) does not find an error in our analysis, but argues that it was customized afterwards and thus not valid; other evaluations would not give significant results. In a rejoinder Betz et al. (1996) showed that the initial analysis remains valid and other statistical procedures yield significance as well. In particular, Ertel (1996) presented a completely different evaluation which reveals very high statistical significance. Although none of these calculations were shown to be incorrect, Enright continues to debate the data.

Ertel's analysis (1996) of the barn-experiment was presented to Enright prior to publication. From January to March 1996 an extensive scientific exchange occurred between Enright and Ertel. In continued e-mails with data attachments Ertel answered numerous questions in great detail. I was kept informed by Ertel, because several aspects of the data had to be clarified, and Enright was aware of this connection. Finally, on May 27 Enright mailed: *the matter is resolved*, indicating that his doubts regarding our data and Ertel's way of handling them were removed. Not the faintest objection had he raised to our study. This is what I forwarded in a talk given at that time (but published much later), now criticized by Enright. We could hardly anticipate that he would tear his own message to pieces.

Incidentally, precisely this critique was put forth in 1998 in the German journal *Skeptiker* (2/1998); my rejoinder and clarification appeared shortly thereafter (3/1998).

Enright presents in his figure 2 the best run (consisting of ten trials) we ever obtained and attributes the effect to chance alone. The probability against chance is 0.2 percent (Betz et al. 1996). More important, at three other sessions this operator performed three additional runs (ten trials each) and reproduced his success. When all forty trials are combined the probability against chance is still below 5 percent. We did not evaluate the fact that this operator operated unchallenged for more than 10 years as a highly successful water dowser (Betz 1995).

Enright notices quite correctly that the best operators produced not only notable hits near the ideal hit-line (x = y in Figure 2, 6a and 6b), but also concentrated data points near the mirrored line x = -y, clearly evident by visual inspection of the graphs. This is precisely the reflection-effect already extracted by Ertel in his analysis (1996) of the total data, statistically highly significant. Enright terms this loosely *anti-dowsing skill*, but did not make clear whether he admits trends towards a nonrandom effect or interprets this skill as a demonstration that dowsers do not even *reach chance results* (note the well-known misinterpretations of psi-missing).

Giving a *simple alternative strategy* Enright repeats his false statistical interpretation which we have shown to arise from his misconception of null hypotheses (Betz et al. 1996). He claims seriously that *dowsers could perform better if they—as successful strategy—simply choose the midpoint in each trial, instead of attempting to dowse.* Even a nonexpert readily recognizes the two conflicting assertions: on the one hand Enright claims all along that dowsers perform according to chance. On the other hand, he suggests that just these operators by using a (necessarily) random strategy will perform better than by chance.

For strange reasons Enright continues to claim that we had selected the forty-three operators from some 500 candidates by performing thousands of preliminary tests, thereby arranging optimal stimuli for each dowser. Nowhere did we say so. It is absolutely clear that such an undertaking would have been impossible to carry out.

Enright claims that operators produce only random results and we have always admitted that the effect tested in the barn is extremely small. It remains Enright's secret how one singles out the best operators when there are (nearly) no effects which can be used as selection criteria. In fact, the forty-three operators were never selected due to preliminary tests; they came along, performed, and went away. Our final result was not obtained until the complete data was analyzed.

References

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The Whole Truth

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I begin here with only a few of the *many statements of Professor Betz's* rejoinder, restricting myself to the three most offensive ones.

1) Despite Betz's assertion to the contrary, I have never, in correspondence of any sort, or even in casual conversation, granted the validity of the analyses undertaken nor the conclusions drawn by Betz and coworkers from the Munich dowsing experiments. Betz has made the same insulting accusation elsewhere (Betz 1997) over a year ago. He must have (Betz and Enright 1998) misunderstood a remark of mine made to Ertel, but I cannot guess which one. More specifically, none of my e-mail messages to Ertel contained either the word "matter" nor the word "resolved," let alone that my last message to Ertel of March 26 (not May 27!) contained both. I know of no e-mail exchanges whatever with Ertel or Betz in May 1996. It is noteworthy that in the past year, Betz has not produced the text that led him to his beliefs. Instead, I stand by my original conclusion: A more misguided application of statistical analysis, leading to unwarranted conclusions, is difficult to imagine. This is a hallmark example of the fact that just about any set of data can be sufficiently tortured so as to support the interpretation of choice. Statisticians sometimes call this "data mining." A clear-cut demonstration of the actual nature of the results from the Munich experiments, independent of statistical manipulation, is instead provided by figure 1 here (figure 3 in Enright 1999). If dowsing were a genuine phenomenon, a cloud resembling the Milky Way should appear in this graph, a dense aggregation of points stretching from lower left to upper right. Such a pattern can only be detected there by wishful thinking.

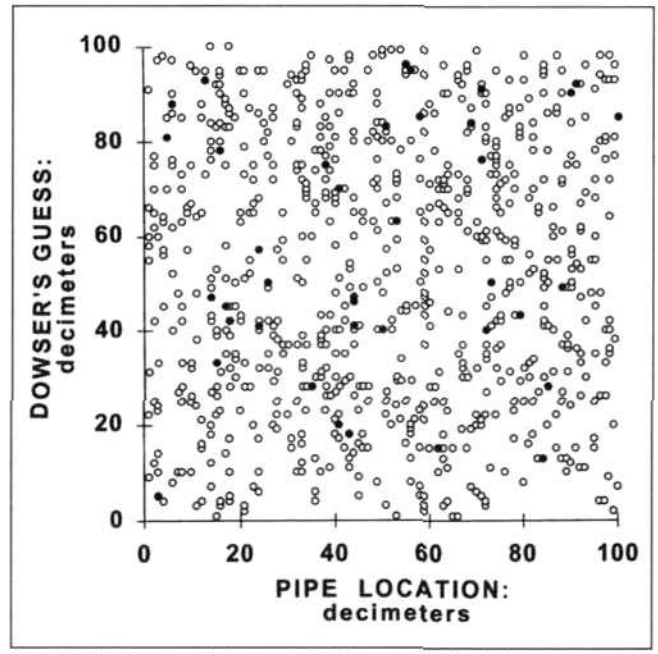

Figure 1. All 843 test results' reported by Wagner et al. (1990) for the 43 dowsers, selected by the researchers from among nearly 500 candidates. (Filled circles: two data points at identical coordinates.)

2) Betz asserts that they nowhere said that data were used from only forty-three dowsers out of about 500 candidates. Not so: The final report, which provided data on only forty-three dowsers, says: "... werden die etwa 400 Versuchspersonen aus der früheren Pilotstudien nicht berücksichtigt" [In translation: "... about 400 candidates from the preliminary experiments were ignored," p. 31 in Wagner, Betz, and König 1990]. I concede, however, that I cannot document my inference that those neglected 400 probably didn't do well in the pilot studies.

3) Betz accuses me of a misunderstanding about null hypotheses, in my demonstration (figure 7 in Enright 1999) that even the six "best" of the dowsers could have done better than