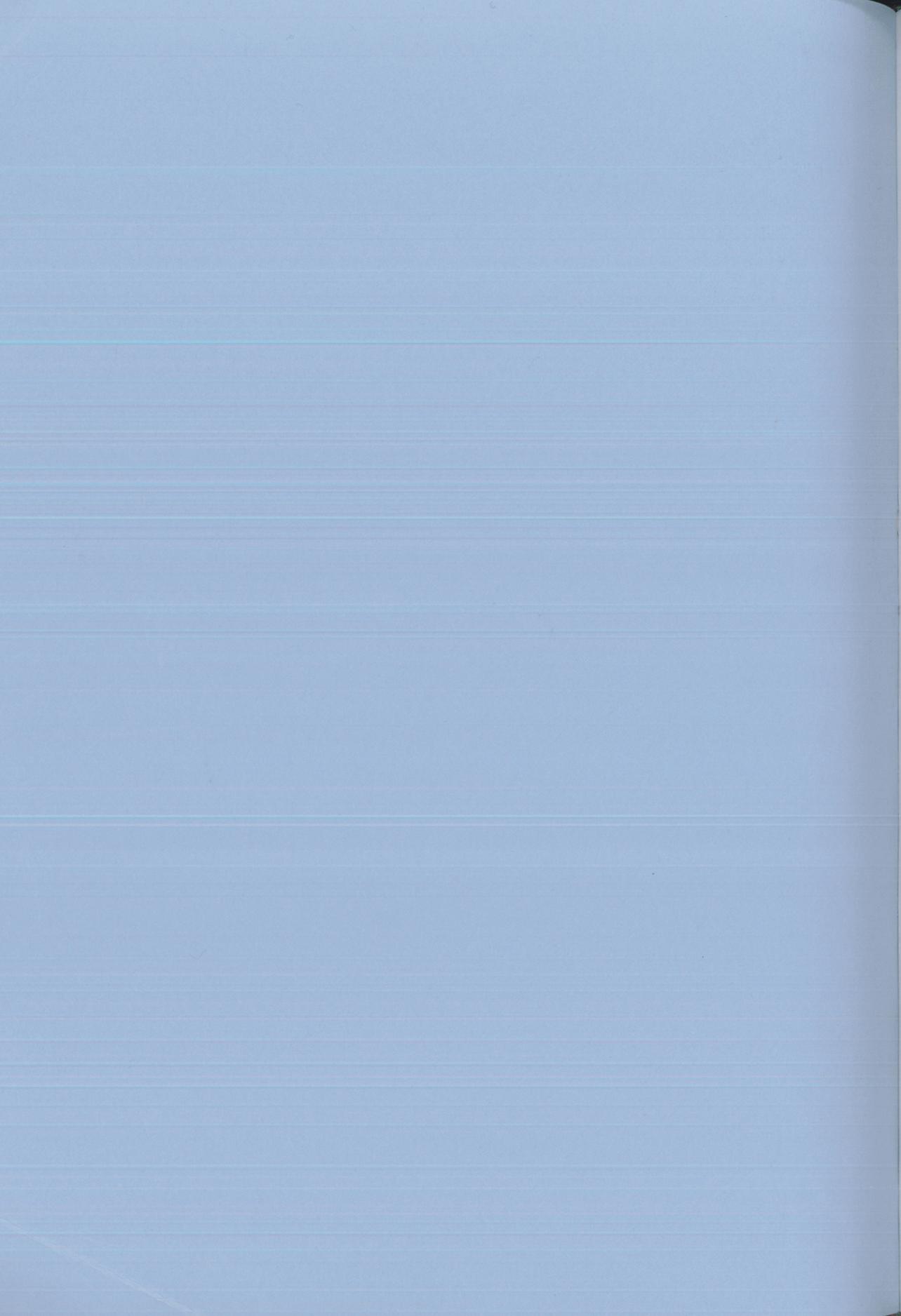


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Message to our Readers

Welcome to the second *European Journal of Parapsychology (EJP)* to be produced from Edinburgh. We have received many favourable comments on the new format of the journal, and we hope this favourable feedback will continue. Following the lapse in publication while operations transferred from Utrecht to Edinburgh, the *EJP* now comes out annually. To cover the gap, last year's journal (volume 8) was dated 1990-1991, and to bring things up to date this year's journal (volume 9) is dated 1992-1993. Next year, volume 10 will be dated 1994. The aim is to publish in the Spring of each year, though delays in receiving and processing articles for the present journal have postponed publication by a couple of months, for which we apologise.

Since last year's issue, the *EJP* has expanded the variety of articles it contains, to include book reviews, notices, and readers' comments. It is *your* journal, and it cannot survive without your submissions, so please continue to support the journal by sending in articles. These need not be full, formal journal articles, debate and comment stimulated by other articles in the journal, and by events in the parapsychological community in general, are also welcomed. We would like to thank all the authors and referees who have contributed to this issue of the *EJP*.

Last year, the *Journal* included French translations of the abstracts of the articles, the translations kindly provided by Michel-Ange Amorim. This idea has appealed to many of our readers and several others have generously given their time and effort to expanding the number of languages covered. We would like to thank Gerd Hövelmann for the German translations, Hans Michels for the Dutch, Massimo Biondi for the Italian, Fátima Regina Machado for the Portuguese, and again Michel-Ange Amorim for the French. It would be good to hear the reactions of readers to the helpfulness, or otherwise, of these abstracts.

The *European Journal of Parapsychology* is a small-scale publication. It is produced at the University of Edinburgh by Caroline Watt (using Microsoft Word™ for Windows), and printed by the University of Edinburgh Press. Richard Wiseman and Robin Taylor helped set out the title page. Robert Morris and Deborah Delanoy help in coordinating the handling of articles, and Julie Milton is responsible for copy editing. We would like to expand the number of subscribers to the *EJP*, and hope that existing readers will recommend the journal to others who might be interested.



An Effect Size Estimator for Parapsychological Research

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Abstract: We describe a standard, easily interpretable effect size estimate for parapsychological research, the proportion index (pi or π). This estimate gives the hit rate on a scale on which the null value is always .50, no matter what the number of equally likely alternatives. The index pi is helpful in the design, analysis, and meta-analysis of parapsychological research. First, it can be used to help select the best choice of the number of response alternatives. Second, significance tests and confidence limits for π can be readily computed within a study. Of particular importance for meta-analytic purposes, tests of heterogeneity of a set of π s and of contrasts among them are described as well as a coefficient of the robustness of replication of a set of π s.

One-Sample and Two-Sample Research

In the behavioural sciences, the vast majority of investigations are based on the two-sample research design or its multi-group extensions, as when we want to compare two treatment conditions, or the performance of female versus male subjects. In parapsychological research in particular, however, the vast majority of investigations are not based on the comparison of two or more groups. Instead, the standard investigation employs a one-sample research design in which the average

performance obtained is compared not to the average performance of another group but to a theoretical level of performance that would be expected if the null hypothesis were true. The null hypothesis asserts that the average obtained performance does not differ from the performance that would be expected if there were no parapsychological ability.

For example, if we wanted to investigate the ability to predict the results of 100 tosses of a fair coin, the null hypothesis would be that we would be correct 50% of the time. A test of the null hypothesis would then require us to compare the obtained performance level with the 50% level expected if the null hypothesis were true.

Author Notes: Preparation of this paper was supported in part by the Spencer Foundation and the National Science Foundation, though the content of the article is solely the responsibility of the authors and is based in large part on some of our earlier work (Rosenthal & Rubin, 1979; 1982; 1989; 1991; and Rosenthal, 1990). The order of authors was determined alphabetically. Correspondence concerning this article should be addressed to Robert Rosenthal, Department of Psychology, Harvard University, 33 Kirkland Street, Cambridge, Massachusetts 02138, USA.

Effect Size Estimation and Significance Testing

For many years behavioural researchers in general tended to equate the results of their research with the level of significance attained. Recently, however, there has been a steadily increasing and healthy emphasis on the importance of the magnitude of the effect as well as its statistical significance

(Cohen, 1977; 1988; Cooper, 1981; Fleiss, 1969; Glass, 1976; Honorton, 1985; Hyman, 1985; Rosenthal, 1986, 1991; Rosenthal & Rubin, 1978; Smith & Glass, 1977).

In fact, there is a quite general relationship between a test of significance and the size of the effect, given by the following generalized relationship, which can be formalized in any particular case:

$$\text{Significance Test} = \text{Effect Size} \times \text{Size of Study.} \quad (1)$$

This informal equation implies that for any fixed non-zero effect size, the test statistic (e.g., t , F , χ^2 , Z) will reach significance with large enough sample sizes, and this fact makes the testing of significance a less scientifically informative procedure than the estimation of effect sizes.

There are many different estimates of effect size, but most of those commonly used in the behavioural sciences fall into one of two families, the r family and the d family. The r -family estimates yield some variant of the product moment correlation between an independent and a dependent variable. The d -family estimates yield some variant of the difference between the experimental and control group means divided by the standard deviation computed usually from both the experimental and control groups. For reasons presented elsewhere (Rosenthal, 1991), it can be argued that r is the most generally useful single effect size estimate for two-sample behavioural research.

There are practical problems, however, in interpreting various effect size estimators such as the Pearson r . We found that both experienced statisticians and experienced behavioural researchers seriously underestimated the practical implications of even quite small r s and even smaller r^2 s. Accordingly we proposed the binomial effect size display (BESD), which represents the effect size r as the amount of improvement in the success rate attributable to a certain treatment procedure. For example, a Pearson r of .20 ($r^2 = .04$) corresponds to an improvement of favourable outcomes from .40 to .60 (Rosenthal & Rubin, 1979; 1982).

Although quite generally applicable to the two-sample type of research design in which two (or more) conditions are compared, the BESD is not directly applicable to one-sample multiple choice type data. For such data the primary effect size indicator has been h , the difference between the obtained proportion and the proportion expected under the null hypothesis after both proportions have been arcsin transformed (Cohen, 1977; 1988). Although such an effect size indicator can be used for power calculations, it has no intuitive interpretation that would allow us to evaluate the practical significance of the effect. Accordingly we proposed a new effect size indicator, the proportion index (pi), symbolized as π , that has the following properties:

1. It makes different stimuli within a given study more directly comparable to one another.
2. It makes the results of different types of studies more directly comparable.
3. It helps investigators optimize the design of their one-sample multiple choice type research.
4. It is directly testable for significance.
5. It is readily placed within confidence limits.
6. The heterogeneity of a set of π s is readily computed.
7. Contrasts among a set of π s are readily computed.

Some of the technical details about the index π have been described elsewhere (Rosenthal & Rubin, 1989; 1991); our purpose here is to introduce this index to the parapsychological research community.

The Proportion Index (pi)

The proportion index (pi) shows the proportion correct, or hit rate, on a scale on which .50 is always the null value. Thus, an effect size =.50 represents 'no effect' on the π scale. In a situation in which there are only two choices, equally likely under the null hypothesis (e.g., as in the predicted toss of a coin), the observed effect size pi or π , is simply the observed proportion of

Table 1
 Values of π for selected values of P and k

Number of Levels (k)											
P	2	3	4	5	6	7	8	9	10	11	12
.010	.01	.02	.03	.04	.05	.06	.07	.07	.08	.09	.10
.050	.05	.10	.14	.17	.21	.24	.27	.30	.32	.34	.37
.100	.10	.18	.25	.31	.36	.40	.44	.47	.50	.53	.55
.111	.11	.20	.27	.33	.38	.43	.47	.50	.53	.56	.58
.125	.12	.22	.30	.36	.42	.46	.50	.53	.56	.59	.61
.143	.14	.25	.33	.40	.45	.50	.54	.57	.60	.63	.65
.167	.17	.29	.38	.45	.50	.55	.58	.62	.64	.67	.69
.200	.20	.33	.43	.50	.56	.60	.64	.67	.69	.71	.73
.250	.25	.40	.50	.57	.62	.67	.70	.73	.75	.77	.79
.300	.30	.46	.56	.63	.68	.72	.75	.77	.79	.81	.83
.333	.33	.50	.60	.67	.72	.75	.78	.80	.82	.83	.85
.400	.40	.57	.67	.73	.77	.80	.82	.84	.86	.87	.88
.500	.50	.67	.75	.80	.83	.86	.88	.89	.90	.91	.92
.600	.60	.75	.82	.86	.88	.90	.91	.92	.93	.94	.94
.700	.70	.82	.88	.90	.92	.93	.94	.95	.95	.96	.96
.800	.80	.89	.92	.94	.95	.96	.97	.97	.97	.98	.98
.900	.90	.95	.96	.97	.98	.98	.98	.99	.99	.99	.99
.950	.95	.97	.98	.99	.99	.99	.99	.99	.99	.99	1.00
.990	.99	.99	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00

Table 2
 Values of P for selected values of π and k .

Number of Levels (k)											
π	2	3	4	5	6	7	8	9	10	11	12
.10	.10	.05	.04	.03	.02	.02	.02	.01	.01	.01	.01
.20	.20	.11	.08	.06	.05	.04	.03	.03	.03	.02	.02
.30	.30	.18	.12	.10	.08	.07	.06	.05	.05	.04	.04
.40	.40	.25	.18	.14	.12	.10	.09	.08	.07	.06	.06
.50	.50	.33	.25	.20	.17	.14	.12	.11	.10	.09	.08
.60	.60	.43	.33	.27	.23	.20	.18	.16	.14	.13	.12
.70	.70	.54	.44	.37	.32	.28	.25	.23	.21	.19	.18
.80	.80	.67	.57	.50	.44	.40	.36	.33	.31	.29	.27
.90	.90	.82	.75	.69	.64	.60	.56	.53	.50	.47	.45
.95	.95	.90	.86	.83	.79	.76	.73	.70	.68	.66	.63
.99	.99	.98	.97	.96	.95	.94	.93	.93	.92	.91	.90

correct responses. When there are more than two equally likely choices, the index π converts the proportion of hits to the proportion of hits made if there had been only two equally likely choices. Thus, if there were three choices, choosing the correct alternative .60 of the time would be equivalent, using π , to choosing the correct alternative .75 of the time given only two choices. The value of π , the proportion correct transformed to a two-choice standard situation, depends simply on k , the number of alternative choices available, and P , the raw proportion of hits:

$$\pi = \frac{P(k-1)}{P(k-1) + (1-P)} = \frac{P(k-1)}{1 + P(k-2)}. \quad (2)$$

Table 1 shows π as a function of selected levels of P and of k (from 2 to 12). For any specified level of k , a larger P yields a larger π and, for any given level of P , π increases as k increases. Table 2 has been prepared to show more directly how the proportion of correct choices or hits required to reach any specific level of π decreases as π increases.

A technical discussion (Rosenthal & Rubin, 1989) provides the motivation for π showing that, under a very natural assumption, it is the maximum likelihood estimator of the probability of correct response given two choices. Also the standard error of π is derived, as is the optimal k , as a function of π (Rosenthal & Rubin, 1989). Although other assumptions can be formulated to derive a different effect size measure (e.g., Shaffer, 1991), in almost all situations that arise in practice, these other measures convey the same message as π (Rosenthal & Rubin, 1991).

Design

Although the primary purpose of π is to provide an effect size estimate, π can also play a valuable role in the design of parapsychological research. The investigator will often have a choice of k , the number of categories, for example, the number of alternatives in a ganzfeld experiment. The

best choice of k (in the sense of the most precisely estimated effect size π) can be obtained from the following equation:

$$k - \text{best} = \frac{1}{1 - \pi} \quad (3)$$

for any value of π greater than .5 and less than 1.00.

Values of π less than .5 are less likely to be of interest but they can be used in equation (3) by substituting $(1-\pi)$ for π .

Table 3 shows that for most levels of π likely to be found in parapsychological research, the best choice of k tends to be small. In general, the smaller the effect size is expected to be, the smaller should be the value of k selected.

Table 3

Best^a choice of k for varying levels of π .

π	Best choice of k
.50	2
.60	2.5
.67	3
.71	3.5
.75	4
.78	4.5
.80	5
.82	5.5
.83	6
.85	6.5
.86	7
.87	7.5
.88	8
.88	8.5
.89	9
.89	9.5
.90	10
.91	11
.92	12.5
.93	15
.94	17
.95	20
.96	25
.97	35
.98	50
.99	75
.99	100

^a In the sense of a smallest confidence interval for π .

Another simple way to decide on the optimal k is to ask the following question: For the anticipated effect, how large a k can be chosen and still be expected to yield a 50% hit rate? The answer to this question is the same as the answer in Table 3, that is, the 'best choice of k ', or k -best.

Analysis

Significance Testing

There are three traditional approaches to significance testing for the one-sample situation: (1) the exact binomial test, (b) the large-sample normal approximation, and (c) the one-sample, or $1 \times k$, χ^2 test. Any of these may be used for significance testing of the raw proportion (P) obtained even before π is computed from equation (2). They are equivalent in large samples, but the exact binomial test is most accurate in small samples. For a discussion of these three procedures and criteria for selecting among them it is useful to consult e.g., Siegel (1956), Siegel and Castellan (1988), and/or Snedecor and Cochran (1989).

A direct significance test of π is also available based on the fact that the (large sample) standard error of π (SE_{π}) is given by the following:

$$SE_{\pi} = \frac{1}{\sqrt{N}} \left[\frac{\pi(1-\pi)}{\sqrt{P(1-P)}} \right] \quad (4)$$

so that the corresponding large-sample test of significance for π is given by¹:

$$Z = \frac{\pi - .50}{SE_{\pi}} = \frac{(\pi - .50)\sqrt{N}}{\pi(1-\pi)/\sqrt{P(1-P)}}. \quad (5)$$

Table 4 can be used to find SE_{π} directly from common values of P and k , by dividing the tabled value by \sqrt{N} .

¹ Notice that equations (4) and (5) involve both the observed population P and the observed value of π calculated from P as found from equation (2) or Table 1. Although these expressions use the large sample standard error, we expect essentially as accurate inferences as when using normal-based procedures on P itself.

Example of significance testing. Suppose a ganzfeld experiment is conducted in which 64 trials are employed with four alternatives available for each trial, and the results show a total of 26 'direct hits' out of 64 trials for a hit rate of $26/64 = .406$. How accurate were these subjects in terms of effect size and what is the associated significance level? From equation (2) we find

$$\pi = \frac{P(k-1)}{1 + P(k-2)} = \frac{.406(4-1)}{1 + .406(4-2)} = .672.$$

Had we used Table 1 instead, the intersection of the row (P) = .400 and the column (k) = 4 would have yielded $\pi = .67$, very close to the more precise value found from equation (2).

Our obtained effect size estimate π means that the obtained accuracy rate of 40.6%, based on four alternatives, is equivalent to about 67% accuracy had there been only two alternatives.

From equation (4) we find the large sample standard error of π :

$$SE_{\pi} = \frac{1}{\sqrt{N}} \left[\frac{\pi(1-\pi)}{\sqrt{P(1-P)}} \right] = \frac{1}{\sqrt{64}} \frac{(.672)(.328)}{\sqrt{(.406)(.594)}} = .0561.$$

Had we used Table 4 instead, the intersection of the row (P) = .400 and the column (k) = 4 would have yielded $SE_{\pi} \times \sqrt{N} = .454$, which when divided by $\sqrt{N} = \sqrt{64}$ would have shown $SE_{\pi} = .0568$, very close to the more precise value we found from equation (4).

For our test of significance, equation (5) gives:

$$Z = \frac{\pi - .50}{SE_{\pi}} = \frac{.672 - .50}{.0561} = 3.07,$$

$p = .0011$, one-tailed.

Table 4

Values of $SE_{\pi} \times \sqrt{N}$ for selected values of P and k.

P	Number of Levels (k)										
	2	3	4	5	6	7	8	9	10	11	12
.010	.099	.195	.287	.375	.460	.541	.620	.695	.768	.837	.905
.050	.218	.395	.540	.695	.757	.837	.903	.957	1.000	1.037	1.066
.100	.300	.496	.625	.710	.765	.800	.820	.830	.833	.831	.825
.111	.314	.509	.631	.707	.753	.779	.792	.795	.793	.786	.776
.125	.331	.523	.635	.700	.735	.751	.756	.753	.744	.732	.719
.143	.350	.536	.635	.686	.708	.714	.710	.699	.685	.670	.653
.167	.373	.548	.629	.663	.671	.665	.652	.635	.616	.596	.576
.200	.400	.556	.612	.625	.617	.600	.579	.556	.553	.510	.489
.250	.433	.554	.577	.566	.541	.513	.485	.458	.433	.410	.389
.300	.458	.542	.537	.508	.473	.440	.409	.381	.357	.335	.315
.333	.471	.530	.509	.471	.433	.398	.367	.339	.316	.295	.276
.400	.490	.500	.454	.405	.362	.327	.297	.271	.250	.232	.216
.500	.500	.444	.375	.320	.278	.245	.219	.198	.180	.165	.153
.600	.490	.383	.304	.250	.212	.184	.162	.145	.131	.120	.110
.700	.458	.317	.239	.191	.159	.136	.119	.105	.095	.086	.079
.800	.400	.247	.178	.138	.113	.096	.083	.073	.066	.058	.054
.900	.300	.166	.115	.088	.071	.060	.051	.045	.040	.036	.033
.950	.218	.115	.078	.059	.047	.040	.034	.030	.027	.024	.022
.990	.099	.050	.034	.025	.020	.017	.014	.013	.011	.010	.009

Note: Standard errors of π may be obtained by dividing the table entries by \sqrt{N} .

Confidence Limits

The standard error of π can also be used to set confidence limits for π . For example, the 95% confidence limits are given by equation (6):

$$\pi \pm 1.96SE_{\pi} \quad (6)$$

For the preceding example, $\pi=.672$ and $SE_{\pi}=.0561$ so the 95% confidence limits are:

$$.672 \pm (1.96)(.0561) = .672 \pm .110,$$

or approximately from .56 to .78.

Had we preferred some other confidence intervals we would simply have replaced the 1.96 of equation (6) by the desired standard normal deviate. For example, had we wanted the 99% confidence interval we would have used:

$$\pi \pm 2.576SE_{\pi} = .672 \pm .1445, \quad (7)$$

or approximately from .53 to .82.

Design implications. In the example we have been discussing, there had been four alternatives available. If we had known beforehand that the effect size in terms of π would be about .67 (that is, about .67 with 2 choices) we might have preferred to use only three rather than four alternatives. That conclusion is supported by Table 3, which shows that for $\pi=.67$, the best choice of k is three. Only after π exceeds .71 would four alternatives be better than three in the sense of yielding the shortest expected confidence interval for π .

Meta-Analysis

When we have two or more studies each yielding its own effect size estimator

π , we may often wish to apply the powerful techniques for comparing and combining results known as meta-analytic procedures (e.g., Cooper, 1989; Glass, McGaw, & Smith, 1981; Hedges & Olkin, 1985; Rosenthal, 1987; 1991). Combining the results is straightforward; we simply average the obtained π s with an appropriate weighting (Rosenthal, 1991). Comparing the results can be by means of either diffuse or focused tests.

Diffuse Tests

Testing heterogeneity. These tests assess the statistical significance of the heterogeneity of the obtained π s using the following χ^2 test on $m-1df$, where m is the number of independent studies:

$$\chi^2(m-1) = \sum \left(\frac{\pi_i - \bar{\pi}}{SE_{\pi_i}} \right)^2 \quad (8)$$

$$\text{where } \bar{\pi} = \frac{\sum w_i \pi_i}{\sum w_i} \quad (9)$$

$$\text{and } w_i = \frac{1}{(SE_{\pi_i})^2} \quad (10)$$

Example of a diffuse test. From a recent meta-analytic investigation of 28 ganzfeld experiments (Harris & Rosenthal, 1988; Honorton, 1985; Hyman, 1985) the results of the first three listed studies are shown in Table 5. For each study we find the number of trials (N), the number of correct guesses (hits), P (hits/trials), and k , the number of stimuli from which the correct one was to be selected. Also shown are π , $(SE_{\pi})^2$, and w as obtained from equations (2), (4), and (10) respectively. From equation (9) we find $\bar{\pi}$ as follows:

$$\bar{\pi} = \frac{(178.6)(.70) + (44.4)(.68) + (61.5)(.09)}{178.6 + 44.4 + 61.5} = .565.$$

To test the heterogeneity of these three studies we use equation (8) to find:

$$\begin{aligned} \chi^2(3-1) &= \frac{(.70-.565)^2}{.0056} + \frac{(.68-.565)^2}{.0225} + \frac{(.09-.565)^2}{.0162} \\ &= 17.77, p=.00014 \end{aligned}$$

Therefore, we can conclude that the results of these three (arbitrarily selected) studies differ significantly among themselves.

Focused Tests

Far more informative than the diffuse or omnibus tests are the focused tests, or contrasts, that address quite specific research questions (Rosenthal & Rosnow, 1985; 1991). Any hypothesis about what features of a study might be significantly related to the obtained effect size, π , can be tested by the following z statistic:

$$Z = \frac{\sum (\lambda_i \pi_i)}{\sqrt{\sum [(\lambda_i)^2 (SE_{\pi_i})^2]}} \quad (11)$$

where the λ_i are contrast weights, which sum to zero (e.g., for four studies ordered by mean age of subjects, $\lambda_1=-3$, $\lambda_2=-1$, $\lambda_3=+1$, $\lambda_4=+3$ represent a linear trend in age, and $\lambda_1=-1$, $\lambda_2=+1$, $\lambda_3=+1$, $\lambda_4=-1$ represent a quadratic trend in age).

Example of a focused test. Suppose we had hypothesised that, in ganzfeld experiments, subjects would perform relatively better when the task was less 'complicated', for example when k , the number of alternatives from which one was to be selected, was smaller rather than larger. Table 5 shows that the three studies associated with k s of 4, 6, and 5. The correct contrast weights or λ s representing the variable of complexity (defined by k) can be obtained by subtracting the mean value of k (5.0) from each of the three k s. The resulting λ s are -1, +1, and 0, respectively. Since these λ s sum to zero they are proper contrast weights.

Applying equation (11) we find:

Table 5
Results of three ganzfeld experiments

Study	N(trials)	Hits	P^a	k	π	$(SE_{\pi})^2$	w
1	32	14	.44	4	.70	.0056	178.6
2	10	3	.30	6	.68	.0225	44.4
3	14	0	.03 ^b	5	.09	.0162	61.5

^a $P = \text{Hits}/\text{trials}$

^b For purposes of computing the standard error, P s of .00 and 1.00 are adjusted by adding the correction $.5/(N+1)$ to .00 or subtracting it from 1.00. Therefore P is adjusted to .03, and π is adjusted to .09.

$$Z = \frac{(-1)(.70) + (+1)(.68) + (0)(.09)}{\sqrt{(1)(.0056) + (1)(.0225) + (0)(.0162)}} \\ = \frac{-.02}{.1676} = 0.12, p = .45, \text{ one-tailed.}$$

This result provides little support for the contrast with weights -1, +1, and 0 for studies 1, 2, and 3, respectively. Examination of the results shown in Table 5 shows that almost all the variation among the three π s is due to the difference between studies 1 and 2 on the one hand and study 3 on the other. The contrast weights for that comparison are +1, +1, and -2 and these yield the following z :

$$Z = \frac{(+1)(.70) + (+1)(.68) + (-2)(.09)}{\sqrt{(1)(.0056) + (1)(.0225) + (4)(.0162)}} \\ = \frac{1.20}{.3048} = 3.94,$$

$p = .000041$, one-tailed.

Since $z^2 = \chi^2(1)$, we find that the $\chi^2(1)$ associated with this contrast = 15.52, which accounts for $15.52/17.77 = .87$ of the overall $2df \chi^2$ for heterogeneity. Most of the variation among the three π s, therefore, is associated with this contrast. Since these three studies were selected arbitrarily, we should not, of course, attach any scientific meaning

to these results, which served only as a numerical illustration. Also, since the contrast weights +1, +1, and -2 were selected after examination of the results, the associated p -value will be too liberal, indicating significance too often.

Coefficient of robustness of replication

In our earlier discussion of the heterogeneity of a set of π s, our evaluation of heterogeneity was in terms of significance testing. Although such tests have some utility, they suffer from the problem that two identical sets of effect sizes may differ dramatically in the significance of their heterogeneity tests if their sample sizes (e.g., N of trials) differ appreciably. It is, therefore, often informative to employ an index of heterogeneity that is independent of sample size, for example, the root mean square (S_{π}) of the obtained π s. The coefficient of robustness of replication is defined simply as the mean $\pi - .50$ divided by the S_{π} or the reciprocal of the coefficient of variation (Rosenthal, 1990), or

$$C_{r.o.r.} = \frac{\pi - .50}{S_{\pi}}$$

This coefficient is particularly useful for comparing the findings from two or more research areas for their robustness, adjust-

ing for differences in the number of studies or trials in each research area.

The utility of this coefficient is based on two ideas - first, that replication success, clarity, or robustness depends on the homogeneity of the obtained effect sizes, and second, that it depends also on the unambiguity or clarity of the directionality of the results. Thus, a set of replications grows in robustness as the variance of the effect sizes decreases and as the distance of the mean effect size from the null value increases. Incidentally, the mean may be weighted, unweighted, or trimmed (e.g., Tukey, 1977). Indeed, it need not be the mean at all but any measure of location or central tendency (e.g., the median).

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Die Abschätzung von Effektgrößen für die parapsychologische Forschung

Zusammenfassung: Wir beschreiben ein standardisiertes, unschwer interpretierbares Abschätzungsverfahren für Effektgrößen in der parapsychologischen Forschung, den Proportional Index (π oder π). Dieser Schätzwert gibt die Trefferrate auf einer Skala an, deren Nullpunkt, unabhängig von der Anzahl gleichwahrscheinlicher Alternativen, stets bei .50 liegt. Der Index π ist hilfreich bei der Anlage, der Auswertung und der Meta-Analyse parapsychologischer Forschungsprojekte. Zunächst kann er bei der Auswahl der geeignetsten Anzahl von Antwortalternativen helfen. Zweitens sind Signifikanztests und Confidence-Limits für π in der Untersuchung einfach zu berechnen. Besonders bedeutsam ist das Verfahren für die Zwecke der Meta-Analyse: Heterogenitätstests für ein Set von π s und für Gegensätze zwischen ihnen werden ebenso beschrieben wie ein Koeffizient für die Robustheit der Replikation eines Sets von π s.

Schatter voor Effectgrootte in Parapsychologisch Onderzoek

Samenvatting: We behandelen een eenvoudig te interpreteren standaardschatter voor de effectgrootte in parapsychologisch onderzoek: de proportie-index (π of π). De schatter geeft het trefferniveau op een schaal waarvan het nulpunt altijd 0,50 is, ongeacht het aantal even waarschijnlijke alternatieven. De π is nuttig bij het ontwerp, de analyse of meta-analyse van parapsychologisch onderzoek. Hij kan helpen bij de bepaling van het beste aantal antwoordalternatieven. Daarnaast kunnen binnen een onderzoek makkelijk significantietests en betrouwbaarheidslimieten voor π worden berekend. Met name voor meta-analyses worden tests voor de heterogeniteit van een set π s en het contrast daartussen behandeld, evenals een coëfficiënt voor de robustheid bij replicatie van een set π s.

Un Indici di Entità dell'Effetto per la Ricerca Parapsicologica

Sommario: Nel lavoro viene descritto un indice standard, di facile interpretazione, di entità dell'effetto, utile per la ricerca parapsicologica: l'indice di proporzione (π , or π). Questo indice consente di valutare il tasso di successi su una scala in cui il valore nullo è sempre 0,5, indipendentemente dal numero di alternative di uguale probabilità. L'indice π è utile nelle fasi di progettazione, analisi e meta-analisi di una ricerca parapsicologica. Innanzi tut-

to può contribuire a compiere la scelta del miglior numero di alternative di risposta. In secondo luogo, consente di calcolare facilmente i test di significatività e i limiti di confidenza per π all'interno di un determinato studio. Di particolare importanza, ai fini della meta-analisi, vengono descritti alcuni test di eterogeneità e di contrasto per un gruppo di π , nonché un coefficiente della robustezza della replicazione di un gruppo di π .

Un Estimateur de la Taille de l'Effet pour la Recherche en Parapsychologie

Résumé: Nous décrivons une estimation standard, facilement interprétable, de la taille de l'effet pour la recherche en parapsychologie, l'indice de proportion (Proportion Index, pi ou π). Cette estimation fournit le taux de réussite sur une échelle sur laquelle la valeur nulle est toujours .50, quel que soit le nombre d'alternatives également probables. L'indice pi est utile dans la conception, l'analyse, et méta-analyse de la recherche en parapsychologie. D'abord, il peut être utilisé afin d'aider à sélectionner le meilleur choix du nombre de réponses alternatives. Ensuite, des tests de significativité et des intervalles de confiance pour π peuvent être facilement calculés dans une étude. D'une importance particulière, pour des propos de méta-analyse, des tests de hétérogénéité d'un ensemble de π et de contrastes parmi eux sont décrits ainsi qu'un coefficient de robustesse de la réplication d'un ensemble de π s.

Um Avaliador do Efeito de Tamanho para a Pesquisa Parapsicológica

Resumo: Descrevemos um modelo facilmente interpretável da estimativa de tamanho para a pesquisa parapsicológica, o índice de proporção (pi ou π). Esta estimativa dá a maior pontuação numa escala em que o valor nulo é sempre .50, não importando qual seja o número de igualdade ou de alternativas. O índice pi é útil no projeto, na análise e na meta-análise da pesquisa parapsicológica. Primeiro, pode ser usado para selecionar a melhor escolha do número de respostas alternativas. Segundo, os testes de significância e limites de confiança pelo π podem ser prontamente computados em um mesmo estudo. De particular importância aos propósitos da meta-análise, testes de heterogeneidade de um conjunto de π s e de contrastes entre eles são descritos assim como um coeficiente de solidez da replicação de um conjunto de π s.

A Further Attempt to Separate the Yins from the Yangs: A Replication of the Rubin-Honorton Experiment with the *I CHING*

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Abstract: This study was an attempt to replicate previous research conducted by Rubin and Honorton (1971, 1972), who set out to see whether the Chinese book the *I Ching* could be used to obtain information whose relevance to a personal question exceeded the level of chance. Subjects were 55 first-year psychology students at the University of Adelaide who varied as regards the degree of their belief in the efficacy of the *I Ching*. They each constructed a personally meaningful question, threw coins to obtain a reading, and, under double-blind conditions, rated this and a control reading for degree of relevance to their question. The difference between these ratings was the dependent variable. Results of the five planned analyses indicated that (1) the mean difference-score was not significantly above chance, and that (2) the difference-scores of sheep (as defined by the Australian Sheep-Goat Scale) were non-significantly higher than those of goats, but that (3) those who believed in the efficacy of the *I Ching* scored significantly higher than those who disbelieved ($r = +.24, p < .05$, one-tailed); also as predicted, (4) sheep tended to have greater belief in the efficacy of the *I Ching* ($r = +.67, p < .005$), and (5) believers in the *I Ching* rated their readings as more relevant ($r = +.32, p < .01$). Some other significant findings, unplanned and unexpected, were noted.

'... experimenting with synchronicity seems to be impossible under ordinary conditions. In the *I Ching*, the only criterion of the validity of synchronicity is the observer's opinion that the text of the hexagram amounts to a true rendering of his psychic condition.' (Jung, 1967, p.xxv)

The *I Ching*, or Book of Changes, is said by some to be the oldest book in the world (Wing, 1982, p.12). Originating in China, it is used principally as a tool for divination, or, as we might say, as a way of eliciting

information by paranormal means. It consists of 64 unique hexagrams, or six-line structures, each line carrying a piece of binary information and known as a yin line or a yang line. Each hexagram has attached to it a distinctly different reading describing an outcome or human situation. The user of the Book asks a question, and employs either yarrow stalks (the traditional method) or coins (the more usual method in modern times) to generate in a random way a particular hexagram, the reading for which, it is claimed, is the answer to the user's question. The information so obtained is considered by many users to be more relevant to their query than chance alone would allow.

A potential problem with such readings is that a favourably disposed user of the *I Ching* may tend to see (especially when the

We wish to thank all the students who took part in this experiment.

This article fulfils the publication policy of this journal, that is, it was submitted and evaluated prior to the experimental data being collected.

readings are lengthy or vague) matches to events in their lives that are really attributable to chance. Convinced users deny this and claim that the readings are more accurate than coincidence would allow. The present study set out to examine whether a sample of mostly first-time users of the *I Ching* could indeed employ it to obtain information that was more relevant than chance would permit. In other words, it set out to find whether such users can paranormally arrange the situation in such a way that the reading of the hexagram chosen does indeed correspond to the characteristics of the event being asked about.

Lawrence Rubin and Charles Honorton published in 1971 a brief report of an attempt to investigate a similar question. Subjects were presented with readings that were meant for them as well as with pseudo-readings intended to act as controls, the hypothesis being that ratings would be assigned that indicated that the real readings were more accurate than the controls. This did not occur. Instead, what they found was a sheep-goat effect, inasmuch as subjects who disbelieved in ESP scored significantly lower than those who believed.

In the experiment to be described in this paper, the method of Rubin and Honorton was to be followed as closely as possible, with some slight deviations in execution of detail, as well as with the addition of a few small extras. For example, a larger *N* was to be used, in order to increase the likelihood of detecting what may be a small effect.

It was expected (firstly) that while the overall difference between ratings of actual and control readings might not be significant, there would (secondly) be a significant correlation between these difference-scores and the belief-system of the subject, more positive difference-scores being obtained by the sheep and more negative difference-scores being obtained by the goats. A third (and new) prediction was that subjects who had a positive attitude towards the *I Ching* would obtain more positive difference-scores than subjects

with a more negative attitude. The fourth prediction was that sheep would have a more positive attitude towards the *I Ching*, a finding reported by Murphy and Lester (1976). And finally, irrespective of the accuracy of their readings, subjects with a positive attitude towards the paranormal or to the *I Ching* were expected to see their readings as containing more relevance to their personal question than would disbelievers.

Method

Subjects

The study began with 55 subjects (25 males, 30 females), all of them first-year psychology students at the University of Adelaide, South Australia. Age ranged from 16 to 46 years, with a mean of 19.8 (s.d.=5.1). All subjects participated anonymously, being identified only by date of birth and tutorial time.

Materials and Procedure

Data collection took place over the two-week period of August 4-13, 1992, M.A.T. being the principal controller of this collection, assisted by J.A.B. and D.M.S.

Week 1. In Week 1, subjects had explained to them by M.A.T. the idea behind the *I Ching* and similar uses of books for divinatory purposes. They then filled in a questionnaire measuring two spheres of attitude:

(1) a visual analogue scale (Hayes & Patterson, 1921) concerning attitude to the *I Ching*, as shown in Figure 1.

(2) the visual analogue scale version of the 18-item Australian Sheep-Goat Scale (Thalbourne, 1991, November; Thalbourne & Delin, submitted for publication).

Subjects were then asked to come up with a question, preferably of great personal relevance¹, about which they would

¹ One reviewer argued that seeking an answer that would not have great personal meaning might be an important ingredient for success with the *I Ching*. Given that not all our subjects felt that they had especially significant events going on in

Figure 1

Visual analogue scale concerning attitude to the *I Ching*

Please read both ends of the following rating-scales, and place an X on the line at the point which best corresponds to your opinion.

I am completely convinced that information cannot be obtained using the *I Ching*, at a level of accuracy over and above what can be attributed to pure coincidence.

I am completely convinced that information can be obtained using the *I Ching*, at a level of accuracy over and above what can be attributed to pure coincidence.

like information (avoiding questions with 'yes/no' answers), and preferably of the form 'What attitude should I take towards...?', 'What advice can you give me regarding...?', 'What will be the effect of...?', 'What will be the outcome of...?', 'What is going on in my relationship with...?', 'What does this day have in store for me...?' They were encouraged to construct questions whose answers should be fairly clear not in the far-distant future but in about a week's time.

Each subject wrote their question on a piece of paper and then placed it inside a small white envelope, which they sealed. (Subjects were assured that the question would not be examined by the experimenters, to preserve their privacy.) Each subject then threw three coins six times, and recorded the total number of heads and tails each time on a special record sheet. The white envelope and the record sheet were then placed into a large brown envelope, so that neither M.A.T. nor J.A.B. nor D.M.S. saw the pattern of heads and tails.

After all these data had been collected, P.S.D. then, in private, converted the coin outcomes into hexagrams (one hexagram if there were no so-called 'changing lines', and two if there were any changing lines). He then produced corresponding control hexagrams (or hexagram pairs), simply by

their lives at that time about which to ask, a self-report measure of such meaningfulness might be useful in future work.

changing all yin lines to yang and vice versa, keeping, however, the changing lines the same for actual and control readings². He next prepared, and kept secure, lists detailing which particular readings were actuals and which controls. The relevant pages from the *I Ching* were then xeroxed, and changing lines indicated by a check-mark.

For the hexagram readings, the version of the *I Ching* used was that published by Wing (1982), whereas the readings that were used for the changing lines were, because of the convenience of their layout, those given by Wing (1979). If the reading involved multiple pages these were stapled together. Actual and control readings were labelled '#1' and '#2' at random, and were placed into each subject's brown envelope.

Week 2. Due to the absence of two subjects, Week 2 had a sample size of 53 rather than 55. Thus, all analyses involving this week were for a sample of 53.

² This represented a departure from the method of Rubin and Honorton, who used random number tables to select their controls. We, however, wished to exclude as far as possible the operation of experimenter psi by using a more mechanical method. A reviewer objected to our using opposite-hexagrams on the grounds that readings so produced might be thematically similar to each other, causing them to be awarded similar ratings. We had in fact considered this possibility; we examined a number of paired readings but were unable to discover any systematic similarity or dissimilarity between their themes.

Each subject began the session by filling in a one-page questionnaire that asked four questions, as follows:

(1) 'Before the experiment last week, had you ever heard of the *I Ching*?' (yes/no);

(2) 'Before the experiment last week, approximately how many times had you ever used the *I Ching*?' (never/once or twice/a number of times/regularly);

(3) 'Since the experiment last week, did you look up the *I Ching* to find the reading corresponding to your coin-throws?' (yes/no); and

(4) 'Regarding the question that you put to the *I Ching* last week, how important is it to you to have an accurate answer?' (not at all important/somewhat important/reasonably important/ extremely important).

The first question - ever having heard about the *I Ching* - was asked mainly out of interest. It was thought that the fourth question, however - importance attached to an accurate answer - might be a potential predictor of performance, with subjects obtaining more positive difference-scores (indicating more relevant actual readings) when they attached greater importance to an accurate reply.³

Questions 2 and 3 were asked because of a possible form of sensory leakage in the experiment: subjects well-familiar with the *I Ching* might have memorized or otherwise recorded their coin-throws, looked up the corresponding hexagram during the following week, and thereby been able to pick the actual from the control reading. Having answers to these two questions was thus thought essential should positive results be obtained. In the event, no-one admitted to so looking up their hexagram and (to anticipate the results), the one fairly regular user gave a lower relevance rating to the actual reading. Thus, this form of

leakage appears not to have occurred.

Subjects were then permitted to re-read their personal question if they wished, and to read their two sets of readings - actual and control. They rated each reading on a 1-10 scale for appropriateness to their question, 1 being labelled 'completely lacking in relevance', 10 being labelled 'extremely striking feeling of relevance'. It was emphasized that different ratings should be given to the two sets of hexagrams, to avoid difference-scores of zero.

When all subjects had had a reasonable amount of time to do their ratings, M.A.T. opened a list revealing, for that particular tutorial group, which readings were actual and which control. The relevant ratings were immediately recorded, and difference-scores were calculated, as well as a score indicating the maximum amount of relevance that the subject was prepared to concede to either reading (in effect, the higher rating of the two).

Details of Planned Analyses

There were five planned analyses, for all of which α was set at .05, one-tailed. Two of these were of essentially psychological interest, that is, they did not imply the operation of the paranormal, while three analyses were capable of providing evidence for the existence of parapsychological processes. Thus, in regard to the former two, it was hypothesized that:

(1) scores on the Sheep-Goat Scale would be positively correlated with scores on the scale measuring attitude to the *I Ching*, to be tested using a Pearson correlation coefficient; and that

(2) scores indicating that a subject tended to judge more relevance to be in their readings would tend also to be associated with higher scores on measures of attitude to the *I Ching* and of the sheep-goat variable in general, to be tested using two Pearson correlations. On the other hand, the difference-scores were used to test the three parapsychologically-relevant hypotheses:

³ All four of these questions were devised only after a paper had already been submitted to this Journal, and so it must be emphasized that, strictly speaking, this particular conjecture could not count amongst those hypotheses set up in advance in a formally preplanned way.

(3) the hypothesis that there would be, overall, an above-chance level of scoring, to be examined using a *t* test for a single sample on the difference-scores⁴, $\mu = 0$;

(4) the hypothesis that there would be a general sheep-goat effect, to be examined by calculating a Pearson correlation between the difference-scores and the Sheep-Goat Scale; and

(5) the hypothesis that there would be an *I Ching* sheep-goat effect, to be examined by calculating a Pearson correlation between the difference-scores and attitude to the *I Ching*.

Results

Acquaintance with the I Ching

About one-third of the subjects had heard of the *I Ching* before this experiment, and two-thirds had not. Ninety-four per cent had never used it before, 4% had once or twice, and just one person had used it a number of times. Thus, as a group, the subjects were essentially first-timers. Receiving an accurate answer was most commonly described as being only 'somewhat' important.

Attitude to the I Ching

The visual analogue scale was scored with a ruler, and in such a way that the minimum possible score was 1 and the maximum possible 44. Actual scores for the 55 subjects who began the experiment ranged from 1 to 40 – almost entirely across the possible spectrum of belief. The mean score was 21.58 (s.d.= 10.17), which indicates that, on average, subjects were uncertain about the efficacy of the *I Ching*.

The 18-Item Sheep-Goat Scale

As in a previous study (Thalbourne, 1991, November; Thalbourne & Delin, submitted for publication), this scale had a

high internal reliability coefficient: Cronbach's α was .94. The scale theoretically ranges from 1-36, and here ranged from 2-30, with a mean of 16.36 (s.d. = 6.98). Thus, the average subject was uncertain about psychic phenomena.

Correlation Between the Sheep-Goat Scale and Attitude to the I Ching

It was predicted (Hypothesis 1) that those subjects with a more positive attitude towards the paranormal would show a similarly positive attitude toward the *I Ching*. In fact, the Pearson correlation between these two attitudinal variables was +.67 ($p < .0005$). Thus, those who believed in and thought that they had experienced psychic phenomena tended to believe that 'information can be obtained using the *I Ching*, at a level of accuracy over and above what can be attributed to pure coincidence.' The finding of Murphy and Lester (1976) is thus confirmed.

Correlation Between Degree of Perceived Relevance and (i) the Sheep-Goat Scale and (ii) Attitude to the I Ching

It was predicted (Hypothesis 2) that those subjects with a more positive attitude towards the paranormal and/or to the *I Ching* would tend to attribute more relevance to at least one of their readings. Indeed, this might be seen by some critics of the paranormal as a cognitive bias that distorts accurate evaluation of readings (e.g. French, 1992). Be that as it may, the hypothesis received partial support here: those with a favourable attitude towards the *I Ching* attributed significantly more relevance to their readings ($r = +.32$, $N = 53$, $p < .01$); the correlation with the Sheep-Goat Scale was in the right direction, but not significant ($r = +.20$, $p = .08$). It is to be noted that, in the absence of further information, this difference cannot be interpreted as implying that believers over-attributed; it may be that disbelievers failed to recognize connections that were present, or even that each group had differentially

⁴ This gives results identical to a correlated samples *t* test comparing the mean ratings for actual and control readings.

relevant readings. It is to this question that we now turn.

Overall Degree of Above-Chance Scoring

It was predicted (Hypothesis 3) that the mean difference-score would be significantly above chance. The mean rating for the actual readings was 5.72, and for control readings, 5.51, giving a mean difference-score of +.21 (s.d. = 4.01). A *t* test for a single sample yields a *t* ratio of 0.38, which is not significantly different from the Mean Chance Expectation of 0.00. Rubin and Honorton obtained a very similar *t* ratio for this analysis, namely, 0.33. Both experiments thus suggest that an unselected group of subjects probably cannot be expected to yield overall significant deviations from chance.

A General Sheep-Goat Effect

It was predicted (Hypothesis 4) that the scores on the Sheep-Goat Scale would be positively correlated with the difference-scores. Though the Pearson correlation was in the right direction, it failed to reach significance: $r = +.16$, $p = .13$. Thus, the results of the Rubin-Honorton study were not duplicated.

An I Ching Sheep-Goat Effect

It was predicted (Hypothesis 5) that the scores on the measure of attitude to the *I Ching* would be positively correlated with the difference-scores. This prediction was confirmed: $r = +.24$, $N = 53$, $p < .05$. Those who believed in the efficacy of the *I Ching* did in fact tend to rate their actual reading as more relevant than their control reading, in the absence of any normal information as to which was really the correct one.

Findings Unplanned and Unexpected

The widespread use these days of computer analyses of data brings with it a number of advantages as well as potential pitfalls, not to mention situations which may harbour an element of both. Into this

latter category may fall post hoc analyses, and significant results that are accidentally observed in the course of other analyses. A former colleague of the second author, Bernard Babbington-Smith, used to speak of 'the principle of indirection.' By this he meant the tendency, which he regarded as crucial for the progress of science, for the most worthwhile and interesting findings from research to be unexpected to the researcher. Of course, un-planned analyses must be treated with some degree of caution, and should certainly not be relied upon without replication. Unfortunately, too, parapsychological researchers have often been accused of extending their analyses, in the absence of theory, until something, anything, 'significant' is found. For these reasons we think it necessary to distinguish clearly the planned analyses from the exploratory, or post hoc ones.

In this section, then, are presented, in the order in which they came to light, a number of such post hoc analyses and their findings, which we feel may have some value in relation to future work. The reader is, of course, free to discount any or all of the 'significant' findings.

(1) Subjects were asked what degree of importance they attached to obtaining an accurate answer to the question that they were posing the *I Ching*. Their responses to this item were intercorrelated with their difference-scores. As believers in the *I Ching* might expect, those who attached greater importance to obtaining a correct reading obtained difference scores indicating that they perceived more relevance in their actual reading ($r = .17$), but this correlation was not significant ($p = .225$).

(2) The 34 subjects who had never heard of the *I Ching* prior to the experiment had a mean difference-score (+1.15) which was significantly higher than the mean score for those who had heard about it (-1.47: $t = 2.38$, $df = 51$, $p = .021$, two-tailed). The corresponding Pearson correlation is +.32. This seems a somewhat bizarre finding.

(3) Attitude to the *I Ching* was virtually independent of whether the subjects had heard of the *I Ching*. Given that both variables correlated significantly with difference-score, it seemed reasonable to enter them both into a multiple regression equation. Multiple *R* was .42, $F(2,50) = 5.5$, $p = .0069$. Thus, the subjects who did best were those to whom the *I Ching* was a new idea and yet who adopted a positive attitude towards it. On the other hand, negative-scorers were those who had heard of the *I Ching* and who did not accept its efficacy. This result could be seen as support for a theory of the psychic efficacy of open-mindedness, or of naive acceptance.

(4) Rubin and Honorton might object that their sheep-goat measure was different from the one used in this study, and that therefore the failure to replicate their sheep-goat effect is hardly surprising. Therefore, the item in the Australian Sheep-Goat Scale apparently most similar to theirs was extracted, namely, item 1, which concerns belief in the existence of ESP in the abstract. This correlated very highly with attitude to the *I Ching* ($r = +.72$, $p < .0005$), but the correlation with difference-score, though higher than that for the complete Sheep-Goat Scale, is still not significant: $r = +.20$, $p = .152$.

(5) Perhaps the most interesting post hoc finding was uncovered when attitudes towards the paranormal, towards the *I Ching* and towards the importance of having an accurate answer were intercorrelated

(i) with the relevance ratings that were (in fact) for the actual readings, and (ii) with those relevance ratings that were (in fact) for the control readings. These findings are displayed in Table 1.

Table 1 shows that in regard to the correlations for relevance-of-actual-reading, the degree of perceived relevance was greater for those who believed in the *I Ching*, for those who believed in ESP, for those who scored high on the Sheep-Goat Scale, and for those to whom it was important to have an accurate answer.

Ordinarily, and in the circumstances where a subject is knowingly evaluating the relevance of their actual reading, this would undoubtedly be regarded as a purely psychological phenomenon and possibly as an alternative explanation for reports of the paranormal (French, 1992). Of course! Believers see more relevance because they want to see it! However, if this is so, then, given the circumstances of this present experiment, the same phenomenon should also make itself felt in regard to the reading which (unbeknownst to the subject) was the control one.

However, the corresponding correlations for the control readings are such that, in this case, knowing the attitude of the subject affords no ability whatsoever to predict how relevant they will perceive their readings to be. Why the absence of the 'obvious' psychological phenomenon of believers seeing greater relevance? There should be *no* difference between actual and control readings in this regard. This unusual finding is submitted as an enigma. It

Table 1
Pearson correlations between various beliefs and attitudes and relevance-ratings, for actual and control readings

	Rating of Actual Reading	Rating of Control Reading
Attitude to <i>I Ching</i>	.42**	.02
Belief in ESP	.39**	.06
Sheep-Goat Scale	.28*	.01
Importance of Accuracy	.29*	.00

* $p < .05$ ** $p < .01$, two-tailed

might reasonably be interpreted as suggesting, however, that the subjects in some sense 'knew' which was the actual reading, and found it relevant to the extent that they expected it to be.

Discussion

The preplanned analyses can be seen as having added some detail to our knowledge of the rather complex structure of belief in paranormal phenomena. With regard to the paranormal itself, the negative result on direct above-chance scoring obtained by Rubin and Honorton was replicated, but not their general sheep-goat effect. The current study's positive sheep-goat effect in relation to belief in the *I Ching* itself could, however, be regarded as a weak kind of replication of that finding. This was the only one of the preplanned analyses that might be seen as suggesting a psi effect.

The post hoc analyses, on the other hand, were much more suggestive. Thus, dividing the subjects according to their scores on the acquaintance variable produced the superficially odd result that subjects who had never heard of the *I Ching* appeared better able to discriminate the 'real' reading from the control one. In fact, of course, the idea that a naïve, accepting person is most likely to experience psychic phenomena is not merely unoriginal; it has become part of the folklore of psychical research.

The multiple regression examining the relationship of attitude to and acquaintance with the *I Ching* to difference-scores seemed to deepen and strengthen the specific sheep-goat effect found in the planned analysis. The other relationships between the attitude variables and the relevance scores seemed, however, to be part of another story that the data appeared to be trying to tell; that subjects were responding differently to the control and actual readings (when they should not have been able to tell which was which). Particularly striking in this regard were the differences between the correlations for actual and control readings, as shown in Table 1. It would

be easy, however, to be over-impressed with the consistency of these results. Since the different attitude variables are related, their correlations with other variables are non-independent, and are, therefore, to some extent conveying the same information.

Even taking this into account, it is difficult to escape the impression that subjects were, in effect, treating the actual and control readings differently, and hence, in some sense, discriminating the one from the other, in spite of the effort taken to ensure that there were no normal cues to aid that discrimination. To take this impression seriously, however, is to encounter a theoretical difficulty that cannot be overcome with the current design. If we cannot assume that the subjects' impressions of relevance are veridical - and the correlation of $+0.34$ between *total* relevance scores (the actual *plus* the control scores) and belief in the *I Ching* could be seen as suggesting we cannot - greater relevance scores for the actual readings than for the control ones need not be seen as validating the *I Ching*. Subjects who succeeded in discriminating the actual reading, parapsychically or otherwise, might simply ascribe greater relevance to it. Thus, both the Rubin and Honorton study and the current one envisage the actual reading as being discriminable from the control one (assuming positive results) on the basis of its greater relevance, but what if it is given a higher relevance rating because it has been discriminated by the subjects?

This is, of course, just one example among many of a difficulty in interpreting experimental results in any area, that is an inevitable consequence of the entertainment of psi hypotheses. It is the potential for this kind of problem that fires the determination with which many scientists maintain their scepticism about psi effects. It would be hard enough if we were only prepared to consider one particular variety of psi phenomenon. If telepathy may exist, how can we conduct a double-blind experiment in psychology? If psychokinesis is possible, how can we rule out experimenter effects in physics? But if we cannot rule out telepa-

thy, psychokinesis, precognition or clairvoyance how can we even demonstrate which of them is involved in a particular result?

The present design does not permit us clearly to distinguish the effects of a functional *I Ching* from those of the subjects' paranormal ability to subvert the double-blind technique. All we can do is to look at the apparent shape of the phenomena as revealed in the data, and make judgements. Thus, we may note that the almost total lack of correlation between the belief variables and relevance ratings for the control readings, together with quite high corresponding correlations for the actual readings would seem to imply either that the believers were genuinely recognising greater relevance in the actual readings, or that *all or most* of the subjects, believers or not, were, at some level, correctly identifying the actual readings, and attributing degrees of relevance to them commensurate with their levels of belief. The former feels inherently the more plausible hypothesis, but such an intuition is difficult to justify, given the current level of our knowledge of the phenomena involved. It is difficult to see how the design could be modified so as to avoid this sort of problem, although the comparative results of a group of subjects asked to produce relevance ratings for pairs of readings both of which were control ones might prove helpful.

One more theoretical question was evoked by the post hoc exploration of the data, although any serious relevant analysis would require a much larger study. This question relates to the distribution of *I Ching* readings to be expected if this method of divination actually did work. It seems clear that, if chance alone is operating, we should expect the 64 hexagrams to turn up, in a large sample, with approximately equal frequency. It is not so clear what we should expect if chance were not operating. If, however, there is a tendency, as claims of validity for the *I Ching* would seem to imply, for readings to correspond in some way to the questions asked, one might perhaps expect that the distribution

of hexagrams would be other than an equal-probability one. For example, this might come about if some kinds of questions were asked more commonly than others: this might lead to an excess of some particular hexagrams in the readings. We ought therefore to be able to gather evidence for some paranormal effect associated with the *I Ching* simply by counting how often the different hexagrams were obtained. There were simply not enough data in the current sample for this kind of analysis to be carried out: at least 300 readings would be required before it was really feasible to look at the distribution as a whole. But clearly this is an additional avenue that might be examined in further experimentation.

If, as seems to be the case, we have evidence consistent with view that the *I Ching* may indeed work under experimental conditions, at least for those who believe in its efficacy, then it might be interesting, if slightly unconventional, to ask the Book to have the last word. The first author therefore threw three coins with such a request in mind. The following were indeed the very last words of the first hexagram produced:

'...in teaching others everything depends upon consistency, for it is only through repetition that the pupil makes the material his own.' (Wilhelm, 1967, p.116)

Perhaps this is the *I Ching's* way of stating those tried-and-true last words of so many scientific papers, especially in parapsychology; replication is needed.

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Ein weiterer Versuch, Yins von Yangs zu trennen: eine Replikation des Rubin-Honorton-Experiments mit dem *I Ging*

Zusammenfassung: Diese Untersuchung war dem Versuch gewidmet, frühere Forschungen von Rubin & Honorton (1971, 1972) zu wiederholen, die herausfinden wollten, ob das chinesische Buch des *I Ging* dazu geeignet sei, Informationen zu erhalten, die für ein persönliches Problem überzufällig bedeutungshaltig sind. Als Versuchspersonen dienten 55 Erstsemester-Studenten der Psychologie an der University of Adelaide mit unterschiedlichen Glaubensbereitschaften hinsichtlich der Wirksamkeit des *I Ging*. Jede von ihnen formulierte eine Frage von privater Bedeutung und warf dann Münzen, um eine Aussage des *I Ging* festzulegen. Unter doppelblinden Versuchsbedingungen nahm sie dann, je nach dem Grad der Einschlägigkeit für die gestellte Frage, Einstufungen dieser Aussage und einer Kontrollaussage vor. Der Unterschied zwischen diesen Einstufungen war die abhängige Variable. Die Ergebnisse der fünf geplanten Analysen lassen folgende Schlüsse zu: (1) Der Wert für die mittlere Differenz der Einstufungen lag nicht signifikant über der Zufallserwartung; (2) die Differenz für die Einstufungen der Sheep ('Schafe'), festgelegt nach der Australian Sheep-Goat Scale, waren unsignifikant höher als die der Goats ('Böcke'); dagegen lagen (3) die Ergebnisse derjenigen, die an die Wirksamkeit des *I Ging* glaubten, signifikant höher als die von jenen, die diesen Glauben nicht teilten ($r=+.24$, $p<.05$, einseitig); wie vorausgesagt, neigten (4) die Sheep zu stärkerem Glauben an die Wirksamkeit des *I Ging* als die Goats ($r=+.67$, $p<.005$), und (5) diejenigen, die an das *I Ging* glaubten, hielten die erhaltenen Aussagen für einschlägiger als die Nichtglaubenden ($r=+.32$, $p<.01$). Zudem ergaben sich, ungeplant und unerwartet, einige weitere signifikante Befunde.

Nieuwe Poging de Yins van de Yangs te Scheiden: Replicatie van Rubin en Honortons Experiment met de *I Ching*

Samenvatting: Wij probeerden een replicatie uit te voeren van eerder onderzoek van Rubin en Honorton (1971, 1972), om na te gaan of het Chinese boek *I Ching* informatie biedt waarvan de relevantie voor de beantwoording van een persoonlijke vraag groter is dan op grond van toeval wordt verwacht. De proefpersonen, 55 eerstejaars studenten psychologie aan de universiteit van Adelaide, geloofden in verschillende mate in de effectiviteit van de *I Ching*. Elk van hen noteerde een voor hemzelf belangrijke vraag en gooide dan een munt om een antwoord te ontvangen. Daarna scoorden de proefpersonen, onder dubbel-blind condities, in welke mate een antwoord uit de *I Ching* en een controle-antwoord relevant waren voor de gestelde vraag. Het verschil tussen beide scores was de afhankelijke variabele. Vijf vooraf geplande analyses leverden het volgende op. (1) De gemiddelde verschillscore was niet-significant boven kansniveau. (2) De verschillscores van de sheep (conform de Australian Sheep-Goat Scale) waren niet-significant hoger dan van de goats, maar (3) zij die in de effectiviteit van de *I Ching* geloofden, scoorden significant hoger dan de ongelovigen ($r=+0,24$, $p<0,05$ eenzijdig). (4) Zoals verwacht, hechtten sheep meer geloof aan de effectiviteit van de *I Ching* ($r=+0,67$, $p<0,005$) en (5) scoorden zij die in de *I Ching* geloven de antwoorden als relevanter ($r=+0,32$, $p<0,01$). We melden ook enkele significante resultaten uit ongeplande analyses.

Un Ulteriore Tentativo di Separare gli Individui 'Yin' dagli 'Yang': una Replica dell'Esperimento di Rubin e Honorton con l'*I Ching*

Sommario: Il presente studio mirava a replicare una ricerca effettuata in passato da Rubin e Honorton (1971, 1972), i quali avevano cercato di verificare se con il testo cinese dell'*I Ching* si ottengono responsi a domande di interesse personale che risultino significativi oltre il livello casuale. Soggetti di questo lavoro sono stati 55 studenti del primo anno di Psicologia all'Università di Adelaide, con opinioni diverse riguardo la validità dell'*I Ching*. Ognuno di loro doveva formulare una domanda che fosse soggettivamente significativa e gettare poi le monete per effettuare la selezione del responso. In ultimo, per valutare in termini quantitativi il grado di pregnanza della risposta ottenuta, con una procedura a doppio-cieco doveva attribuire un punteggio di significatività a questo e a un altro responso di controllo. La differenza tra i due punteggi costituiva la variabile dipendente. I risultati delle cinque analisi programmate hanno indicato: (1) che il valore medio della differenza non era significativamente superiore a quello atteso per effetto del caso; (2) che i valori di differenza delle 'pecore' (soggetti così definiti secondo l'Australian Sheep-Goat Scale [scala australiana pecore-capre]) non erano significativamente superiori a quelli delle 'capre'; ma (3) che i punteggi di chi credeva nella validità dell'*I Ching* erano significativamente più alti rispetto a quelli di chi non ci credeva ($r=+.24$, $p<.05$, a una coda); che inoltre, come predetto, (4) le 'pecore' tendevano ad avere una fiducia maggiore nella bontà dell'*I Ching* ($r=+.67$, $p<.005$) e (5) che chi credeva nell'*I Ching* giudicava più rilevanti le risposte alle proprie domande ($r=+.32$, $p<.01$). Sono stati evidenziati anche altri risultati significativi, non pianificati né attesi.

Nouvelle Tentative afin de Séparer les YINs des YANGs: Une Réplication de l'Expérience de Rubin & Honorton avec le *I CHING*

Résumé: Cette étude a tenté de répliquer la recherche antérieure menée par Rubin et Honorton (1971, 1972), qui examina si le livre chinois *I Ching* pouvait être utilisé afin d'obtenir une information dont la pertinence à une question personnelle dépasserait le niveau du hasard. Les sujets furent 55 étudiants en Première Année de Psychologie à l'Université d'Adelaide variant par leur degré de croyance en l'efficacité du *I Ching*. Chacun d'eux a construit une question personnelle signifiante, lancé des pièces afin d'obtenir une lecture, et, en conditions de double-aveugle, a évalué celle-ci ainsi qu'une lecture contrôle pour ce qui est de leur degré de pertinence par rapport à la question. La différence entre ces évaluations était la variable dépendante. Les résultats de cinq analyses planifiées indiquèrent que (1) le score de différence moyen n'était pas statistiquement significatif, (2) les scores de différence des moutons (tel que défini par l'Echelle Mouton-Chèvre Australienne) n'étaient pas significativement plus élevés que ceux des chèvres, mais que (3) ceux qui croyaient en l'efficacité du *I Ching* avaient des scores significativement plus élevés que ceux qui n'y croyaient pas ($r=+.24, p<.05$, unilatéral); aussi comme prédit, (4) les moutons tendent à avoir une croyance plus forte dans l'efficacité du *I Ching* ($r=+.67, p<.005$), et (5) ceux qui croient au *I Ching* ont évalué leurs lectures comme étant plus pertinentes ($r=+.32, p<.01$). D'autres résultats significatifs, non planifiés et inattendus, ont été remarqués.

Mais uma Tentativa em Separar os Yins dos Yans: Uma Réplica do Experimento de Rubin e Honorton com o *I Ching*

Resumo: Este estudo foi uma tentativa de replicar uma pesquisa anterior conduzida por Rubin e Honorton (1971, 1972), que planejaram-na para ver se o livro chinês *I Ching* poderia ser usado para obter informações cuja relevância para um problema pessoal excedia o esperado pelo acaso. Os sujeitos foram 55 alunos do primeiro ano de Psicologia da Universidade de Adelaide que variavam conforme o grau de sua crença na eficácia do *I Ching*. Cada um formulou uma interrogação sobre algo pessoal e significativo, atiraram moedas para obter uma leitura e sob condições duplamente cegas, pontuaram e passaram por uma leitura de controle em relação ao grau de relevância de suas questões. A diferenças entre essas taxas foi a variável dependente. Os resultados das cinco análises planejadas indicaram que (1) a principa diferença de resultado não foi significativamente acima do acaso, (2) a diferença de resultados das ovelhas (como foi definido pela escala cabra-ovelha australiana) não foram significativamente mais altas que as das cabras, mas que (3) aqueles que acreditavam na eficácia do *I Ching* tiveram resultados significativamente mais elevados que aqueles que não acreditavam ($r=+.24, p<.05$, 'one-tailed'); como o previsto, (4) as ovelhas tenderam a ter uma maior crença na eficácia do *I Ching* ($r=+.67, p<.005$) e (5) aqueles que acreditavam no *I Ching* pontuaram suas leituras como mais relevantes ($r=+.32, p<.01$). Alguns outros resultados significativos, não planejados e inesperados, foram notados.

The Effect of Belief in the Paranormal and Prior Set Upon the Observation of a 'Psychic' Demonstration

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Abstract: This study investigated how belief in the paranormal and prior set may affect the observation of a 'psychic' demonstration. Forty subjects were classified as either 'sheep' or 'goats' according to their degree of belief in the paranormal. Subjects were then divided into four groups: two groups of 'sheep' and two groups of 'goats'. Each group was shown a video-taped demonstration of ostensible psychic ability. One group of sheep and one group of goats were told the demonstration was 'authentic' whilst the other two groups were told it contained 'trickery'. Subjects were then asked to complete an observation questionnaire that pertained to the demonstration. The results were analysed by way of a two-factor factorial analysis of variance. It was found that 'sheep' and 'goats' did not differ significantly in their observation of the 'psychic' demonstration. Subjects who were given contrasting prior sets were found to show a difference in observation at the 5% level of significance. Results were tentatively interpreted in terms of differential encoding and storage of information. Implications and suggestions for future research are briefly outlined.

The following study examines ways in which the observation of a 'psychic' demonstration can be influenced by expectations. Research into eyewitness testimony has recognised that information processing on the part of the observer is far from exhaustive. Buckhout (1974), for instance, has proposed that there is often inaccuracy in recall of events in an ambiguous situation because conclusions are reached by evaluation of fragments of information stored in memory that are reconstructed - fragments that can be

poorly representative of what has actually occurred. Research has also shown that prior expectation can influence the perception of events (e.g. Zadny & Gerard, 1974; Massad et al., 1979).

This investigation applies these concepts to events of a paranormal nature, because critical views of parapsychology are often based on the idea that researchers are susceptible to being misled into believing that paranormal phenomena are present when, in fact, they are absent (e.g. Hansel, 1980; Alcock, 1981). Criticisms of this nature have led parapsychologists to spend much time studying the possibilities of deception within parapsychology and 'psychic fraud' (Morris, 1986a, 1986b; Wiseman, 1991, 1992). Throughout this work the aim has been to model sources of deception in a psychological context that can then be applied to specific situations.

This study was carried out as the author's final year undergraduate dissertation at The Liverpool Polytechnic. I would like to thank Dr. Mike Daniels, of Liverpool John Moores University (formerly The Liverpool Polytechnic), for his assistance throughout this project.

An important preliminary attempt to analyse sources of deception by Morris (1986a) examines the need to recognise some possible areas of deception and outlines strategies by which an observer can be deceived by a 'pseudo-psychic', i.e. one who claims psychic ability whilst employing trickery.

Some of the most recent research along these lines has been that carried out by Wiseman (1991) at the Koestler Chair of Parapsychology in Edinburgh, who has examined the influence of belief in the paranormal upon recall of a 'pseudo-psychic' performance. Wiseman found that those subjects designated as sheep rated the performances as more 'paranormal' in content than did the goats, and that goats recalled significantly more information important to the effect's methodology than did sheep, even after being told that the performance contained trickery. He found no differences between sheep and goats in the recall of 'unimportant' aspects of the performance ('important' and 'unimportant' recall questions were rated as such by two independent magicians). Wiseman suggests his results may reflect differences in encoding and storage of information at the time of observation rather than retrieval and reconstruction in response to the questionnaire.

In addition to the expectations that one may have based on beliefs, it is possible to manipulate expectations by presenting the observer with a prior set towards an object or event. For example, studies such as that by Massad et al. (1979) have reported that prior sets introduced by the experimenters influence subjects' perception of a subsequent series of events, and that the resulting biased sample of information that subjects acquire may restrict their "retrospective reinterpretation" of the event as measured through recall.

Massad et al.'s study involved two subject groups witnessing a cartoon in which geometrical characters interacted; the contrasting sets given by the experimenters interpreted one of the figures as either a 'guardian' or a 'bully'. Although the two groups saw the same stimulus tape, they interpreted the sequence of events differently and showed marked differences in subsequent recall performance. A similar study by Zadny & Gerard (1974) compared the effect of interpretive sets presented either before or after the viewing of filmed events, and reported that interpretations were altered only when such information was provided prior to viewing. The researchers interpreted their results as being due to a "selective registration" of information relevant to the anticipated intentions of event participants.

Thus, it would be fair to suggest that the way in which a 'psychic' demonstration is presented may have an effect upon the perception of events. In this respect, observers could either be told that the demonstration they are about to witness is 'real' or told that it is 'fake'.

In a scenario such as this, the most distinct differences in interpretation and perhaps in subsequent recall would be expected between sheep who were told it was 'real' and goats who were told it was 'fake', because in both these cases the interpretive sets are confirmatory of prior belief. Wiseman's findings would lead us to suggest that the goats who were told it was 'fake' would perform better than the sheep who were told it was 'real'. The effect of an interpretive set that is contradictory to prior belief such as would be the case for sheep who were told it was 'fake' and goats who were told it was 'real', will be dependent upon how strongly the individuals hold their beliefs and how far the individuals comply with demand characteristics, i.e. how much importance is placed upon the set provided by the experimenter.

Design

The object of the investigation was to examine differences in observation of details in a video of a psychic surgery demonstration. Comparisons were made between sheep and goats, and between groups given contrasting interpretive sets prior to observation. It was therefore necessary to arrange subjects into four independent groups: A, B, C, and D as summarised below:

	Prior set	
	'authentic'	'trickery'
Sheep	A	B
Goats	C	D

Observation was measured by subjects' responses to a set of questions about the nature of the events on the tape — the psychic surgery questionnaire (PSQ). It was hypothesised that there would be a significant difference between sheep and goat PSQ scores, and PSQ scores of groups given contrasting prior sets.

Method

Subjects

The investigation was carried out with 40 subjects who agreed to take part in the study during their lunch break. Of these, 36 were undergraduates attending the Liverpool Polytechnic, and 4 were lecturers at the Polytechnic. There were 24 males and 16 females. The ages of the subjects ranged from 18 to 52 years, with a mean age of 23 years.

Materials

(a) Sheep-goat questionnaire

This brief questionnaire asked for the subject's on towards phenomena of a para-

normal nature. In total, there were six defined phenomena: telepathy, clairvoyance, precognition, psychic surgery, psychokinesis, and mediumship. Subjects responded by rating each phenomenon in terms of how likely they regarded it to be. This was done on a five-point scale with responses ranging from 'an established fact' to 'an impossibility'.

(b) Stimulus video-tape

The 'psychic' demonstration was presented on a video-tape that contained footage of a psychic surgeon displaying his abilities. The film lasted 3.5 minutes, was in colour, and was of good quality. The first minute showed the 'psychic' being interviewed about his claims, and the remaining 2.5 minutes was devoted to the demonstration. This consisted of the 'psychic' supposedly removing a blood clot from the back of a patient (using a technique known as 'cupping') without any form of anaesthetic. At the end of this demonstration it could be seen that there were no scars left behind and the patient reported that he had felt no pain, i.e. it could be considered a standard demonstration of psychic surgery.

(c) Prior sets

The two contrasting sets used in this study were as follows:

'authentic' set: 'You are about to be shown a film clip in which a psychic surgeon is seen to display his abilities. Since this footage was filmed, investigations by independent researchers have shown him to be authentic. Please watch the clip closely as afterwards you will be questioned on its contents.'

'trickery' set: 'You are about to be shown a film clip in which a psychic surgeon is seen to display his abilities. Since this footage was filmed, investigations by independent researchers have exposed him as using trickery. Please watch the clip

closely, as afterwards you will be questioned on its contents.'

(d) *Psychic surgery questionnaire (PSQ)*
(see appendix)

The PSQ was designed to test the subjects' general observation of the stimulus tape. It consisted of six questions that pertained to the film, but that were not particularly relevant to the accomplishment of trickery. The scoring procedure for the PSQ was simply to award a mark for each correct answer and to total these marks for all six questions.

Procedure

Subjects were informed that they were about to take part in a psychology experiment in which they would be asked for their opinions of the paranormal. They were also notified that there would be a second phase to the study in which they would be required to watch a short video film.

Each subject was given a sheep-goat questionnaire and instructed to read it through carefully before marking their responses accordingly. The subjects were asked to initial their questionnaire for later identification. Subjects were allowed to complete this task at their leisure although it was emphasised that they should give their personal opinions and should not confer.

Once all 40 questionnaires were returned, a sheep-goat score was obtained for each subject. The subjects with the lowest 20 scores were classified as sheep and those with the highest 20 scores were classified as goats. Ten sheep were then randomly allocated to group A, and ten to group B. This was done by shuffling the completed 'sheep' questionnaires and dealing them alternately into two face-down piles. Goats were randomly allocated to

groups C or D by shuffling the 'goat' questionnaires and proceeding in the same way.

The observation task took place approximately one week after this initial classification. This was primarily dependent upon when it was convenient for subjects to arrange a further meeting. Each group was independently taken into an experimental booth (this was a room containing a television set, a video-recorder and approximately a dozen chairs) and shown the stimulus tape with the appropriate prior set, i.e. groups A and C were given the 'authentic' set, and groups B and D were given the 'trickery' set. In each case, the set was read aloud by the experimenter prior to watching the film. Subjects were seated between five and ten feet away from the television.

Immediately after watching the tape, each subject was provided with a copy of the PSQ and asked to read through the instructions before indicating their answers by circling the appropriate letter. There was no set time limit for this task although it was again emphasised that they should not discuss their answers. The subjects were instructed to initial their answer sheets as per the sheep-goat questionnaires, for later identification. Once the questionnaires had been collected, the subjects were debriefed and thanked for their cooperation. PSQ scores were obtained for each subject as outlined earlier.

Results

As can be seen from Table 1, subjects overall had a mean sheep-goat rating of 17.40, slightly toward the sheep end of the scale (scores of 18 would indicate neutrality). Scores in the two set conditions were very similar, indicating the procedure for random allocation was successful.

Table 2 presents the mean PSQ scores for all four groups. As expected, goats

Table 1
Mean sheep-goat scores.

	Prior set		
	'authentic'	'trickery'	
sheep	13.80	14.50	14.15
goats	20.20	21.10	20.65
	17.00	17.80	

Table 2
Mean PSQ scores.

	Prior set		
	'authentic'	'trickery'	
sheep	2.10	3.00	2.55
goats	2.70	3.20	2.95
	2.40	3.10	

produced more correct answers than sheep and those given the 'trickery' set did better than those who were told the demonstration was authentic. Also as predicted, the greatest difference was between the goats in the 'trickery' condition and the sheep in the 'authentic' condition.

The data were subjected to a two-factor factorial analysis of variance as summarised in Table 3, which shows that only the effect of prior set was statistically significant.

Discussion

The findings show that the evidence bearing on the sheep-goat effect is nonsignificant but the results are in the direction

that one might expect, i.e. subjects who are on the lookout for trickery (goats) appear to be marginally more observant than those who are not (sheep).

However, prior set as imposed by the experimenter asserting whether or not the 'psychic' may be engaging in trickery does appear to have a significant effect upon observation. Subjects who were expecting trickery perform significantly better than subjects who expected the demonstration to be authentic. Although the interaction was not significant, reference to Table 1 suggests that the most distinct differences are between sheep given the 'authentic' set and goats given the 'trickery' set. Thus interpretive sets that confirm prior belief appear to strengthen the effect of expectation. This is consistent with the findings of Jones et al. (1976) and Wiseman (1991) suggesting that goats (i.e. those with a tendency to suspect trickery) are consistently more accurate in their observation of 'paranormal' demonstrations.

A simple, yet plausible explanation for this is that observers expecting trickery may selectively attend to features of the proceedings that may bear on the issue of deception, whereas observers with little reason to expect deception will not be so analytical in their observation, showing more interest in the overall effect. Although this may be a reasonable interpretation of what is happening, this explanation is not reflected in the present investigation because the questions on the PSQ did not directly pertain to matters that were relevant to how the 'psychic' might have cheated. Instead it seems that observers who expect trickery may pay more general attention to the proceedings, thus being more vigilant, thereby processing the information in a more efficient way. The differences in performance may therefore

Table 3
ANOVA summary table.

Source	SS	df	ms	F	p
Sheep/goat	1.60	1	1.60	1.33	ns
Prior set	4.90	1	4.90	4.80	0.05
Interaction	0.40	1	0.40	0.39	ns
Within cells	36.60	36	1.02		
Total	43.50	39			

have been due to the effectiveness of the observers' encoding and storing of information, as influenced by their expectations as opposed to differences in retrieval strategies. This is the view supported by Wiseman (1991), who found that there were no differences in recall between sheep and goats as a result of 'response set', measured by asking subjects 'pseudo-important' questions that were designed such that one might assume that they referred to aspects of a trick's methodology, but in fact were not relevant to how the trick was accomplished. If goats had answered in accordance to a response set, one would have expected goats to have assumed that the 'pseudo-important' questions were relevant to the trick's secret and to have responded accordingly. Wiseman found that this was not the case.

Massad et al. (1979) also gave consideration to a selective processing and memory encoding hypothesis in preference to differential retrieval processes, claiming that contrasting prior expectations influenced the initial perception of an event, and that the subsequent recall is based upon biased information gleaned from this initial perception of events.

Commentary on design and methodology

Sheep-goat classification - the problem of definition.

In this investigation, the experimental design required the subjects to be classified into sheep and goats so that half the subjects were labelled as 'sheep' and the other half were labelled as 'goats'. Thus the 20 most 'sheepish' subjects could be separated from the the 20 most 'goat-like' subjects. It will be seen that in any population other than the 40 subjects used here, borderline subjects might be classified differently. This is highlighted by the fact that three subjects who had a sheep-goat rating of 17, and three subjects who had a rating of 18 were classified as goats due to the nature of the experimental design, although a rating of 18 reflects an undecided subject.

This anomaly raises the issue of whether borderline subjects should be classified as either 'sheep' or 'goats'. A solution to this problem could have been achieved by allowing, say, 60 subjects to complete the sheep-goat questionnaire and

involving only subjects with the 20 lowest scores and with the 20 highest scores in the observation task, eliminating the borderline, undecided subjects.

It is also recognised that it may not have been valid to separate the subjects through a questionnaire that dealt with six aspects of the paranormal because the observation task related only to psychic surgery. However, it was felt that it would have been difficult to classify subjects effectively as sheep or goats on the basis of a single question about subjects' opinions on psychic surgery alone. Such a case highlights the problem of definition, although the study would have benefited from the testing of the questionnaire on a pilot population to ensure a moderate correlation between the questions.

Imposition of prior set.

It will be appreciated that ensuring that the subjects take notice of the experimenter's instructions and prior description of the stimulus tape is extremely difficult to control, i.e. one cannot be sure that the 'set' has been sufficiently imposed. A step towards measuring the effect of the instructional set in future research would be to ask subjects to rate the content of the tape in terms of how 'paranormal' they believed it to be.

Measurement of observation.

A measurement of subjects' confidence in their answers would have been desirable in addition to the yes/no type responses. The questions were chosen to reflect one's general observation of the stimulus tape as opposed to a strict recall test so as to enable examination of subjects' overall perception of events.

Conclusions

If we are to accept the hypothesis of differences in observation of a 'psychic' demonstration between observers who are told to expect trickery and those who are told not to, then it may have implications for the modification of observer-training techniques with particular relevance to the training of researchers who study 'gifted' subjects. It may be more productive to concentrate observer-training on the initial stages of observation (in this case by asking individuals to assume that the phenomena they are about to see contain trickery of some sort) rather than attempting to train more efficient retrieval strategies after the event has been observed. This may prove to have some relevance for the study of eyewitness testimony outside of parapsychology, for example, in criminal cases in which observers had reason in advance to be suspicious of the crime-related events. However, one must remain extremely cautious when generalising to more natural situations than the present study in which observation tasks took place under controlled conditions.

One direction future research might take would be to examine ways in which observers perceive ongoing behaviour. A number of studies (e.g. Newton, 1973; Newton et al., 1977; Massad et al., 1979) have suggested that actions are encoded by the perceiver's selection of successive 'points of definition', or breakpoints, in the behaviour stream. The researchers suggest that each breakpoint indicates the perceiver's judgement of the point at which an action has occurred and that breakpoints appear to be selected in such a way that they provide a summary of the information gained from observation of the sequence of events. Results from such previous studies

have shown substantial differences in the breakpoints discriminated in the event as a result of differences in prior set, and that differences in segmentation may have caused subsequent differences in recall. Hence, analysis of behaviour segmentation may provide further insight into how prior set can influence an observer's perception of events of a paranormal nature.

In conclusion, it would appear that first-hand eyewitness testimony of a paranormal event may not be as reliable as one might have thought. The present study echoes the findings of the recent research of Wiseman (1991) in that biasing appears to act upon the initial stages of observation rather than at the recall and reconstruction stages.

Appendix

Psychic Surgery Questionnaire (PSQ)

Below is a brief questionnaire about the film-clip you have just witnessed. In order to answer each question, please circle the letter of the answer you consider to be correct. If you find that you are unsure of some answers, please estimate to the best of your ability.

1. In what year did 'Dr. Kahn' first manifest himself?

- a. 1985*
- b. 1986
- c. 1987
- d. 1988

2. During the psychic surgery demonstration was the 'surgeon' wearing glasses?

- a. Yes
- b. No*
- c. Don't know

3. How long did the psychic surgery demonstration last?

- a. Less than 1 minute
- b. 1 - 2 minutes
- c. 2 - 3 minutes*
- d. 3 - 4 minutes

4. Was the 'surgeon' wearing a wrist-watch?

- a. Yes
- b. No*
- c. Don't know

5. During the psychic surgery demonstration, how many times did the camera angle change as a result of editing?
- 6
 - 8
 - 10*
 - 12
6. How long did the entire film-clip last?
- 1 - 2 minutes
 - 2 - 3 minutes
 - 3 - 4 minutes*
 - 4 - 5 minutes

*denotes correct answers

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O Efeito da Crença no Paranormal e da Tendência Prévia Sobre a Observação da Demonstração de um 'Paranormal'

Resumo: Este estudo investigou como a crença no paranormal e a tendência prévia podem afetar a observação da demonstração de um agente 'paranormal'. 40 sujeitos foram classificados como 'ovelhas' ou 'cabras' de acordo com seu grau de crença no paranormal. Os sujeitos foram então divididos em 4 grupos: dois grupos de 'ovelhas' e dois grupos de 'cabras' e a cada um foi exibida uma demonstração em vídeo-tape de habilidades aparentemente paranormais. Disseram a um grupo de ovelhas e um grupo de cabras que a demonstração era 'autêntica' enquanto que disseram aos outros grupos que havia 'trucagem'. Pediram aos sujeitos para completar uma questionário de observação que dizia respeito à demonstração. Os resultados foram analisados por meio de uma análise fatorial de tipo bi-fatorial de variação. Descobriu-se que as ovelhas e as cabras não diferiam significativamente em sua observação da demonstração do agente 'paranormal'. Descobriu-se que os sujeitos a quem foram propostas inclinações prévias diferentes demonstraram uma diferença na observação em 5% do nível de importância. Os resultados foram experimentalmente interpretados em termos da codificação diferencial e do armazenamento de informações. Implicações e sugestões para o uma pesquisa futura estão brevemente destacados.

Effetto della Credenza nel Paranormale e delle Informazioni Preliminari sull'Osservazione di una Prova di 'Sensitività'

Sommario: Questo studio ha esaminato in che modo il credere nell'esistenza del paranormale e il ricevere informazioni preliminari possono influenzare l'osservazione di una prova di 'sensitività'. Quaranta persone sono state classificate in 'pecore' e 'capre', a seconda del grado della loro credenza nel paranormale; poi sono state divise in quattro gruppi: due di 'pecore' e due di 'capre'. Ad ogni gruppo è stata mostrata la registrazione video di un'apparente prestazione paranormale. A un gruppo di pecore e ad uno di capre è stato detto che la prestazione era autentica, mentre gli altri due gruppi sono stati avvisati che era stata compiuta con un 'trucco'. Tutti i soggetti dovevano poi rispondere a un questionario riguardante ciò che avevano visto. I dati sono stati assoggettati a un'analisi della varianza a due fattori. E' risultato che le 'pecore' e le 'capre' non differivano in maniera significativa nell'osservazione della prestazione 'paranormale'. Fra i soggetti che avevano ricevuto informazioni preliminari diverse è stata registrata una differenza nell'osservazione a un livello di significatività del 5%. I risultati sono stati interpretati in termini di diversità nell'elaborazione e memorizzazione dell'informazione. Vengono discusse brevemente le implicazioni di questo lavoro e avanzati suggerimenti per ricerche future.

Der Einfluß von Glaubensbereitschaften und Voreinstellungen auf die Beobachtung einer 'paranormalen' Demonstration

Zusammenfassung: Diese Studie untersucht, wie der Glaube an das Paranormale und vorherige Einstellungen die Beobachtung einer vermeintlich 'paranormalen' Demonstration beeinflussen können. Vierzig Versuchspersonen wurden, je nach der Stärke ihres Glaubens an das Paranormale, als 'Sheep' bzw. als 'Goats' eingestuft. Die Probanden wurden dann in vier Gruppen aufgeteilt: zwei Gruppen mit 'Sheep' und zwei Gruppen mit 'Goats'. Jeder Gruppe wurde ein Videoband mit der Aufnahme der Demonstration mutmaßlich paranormalen Fähigkeiten vorgeführt. Je einer Gruppe von 'Sheep' und 'Goats' wurde mitgeteilt, die Demonstration sei 'authentisch', während den beiden anderen Gruppen versichert

wurde, die Aufnahmen zeigten 'Tricks'. Jede Versuchsperson hatte dann einen Beobachtungsfragebogen mit bezug auf die Demonstration auszufüllen. Die Ergebnisse wurden einer bifaktoriellen Varianzanalyse unterzogen. Es zeigte sich, daß 'Sheep' und 'Goats' sich hinsichtlich ihrer Beobachtung der 'paranormalen' Demonstration nicht signifikant unterschieden. Versuchspersonen, die gegensätzliche Vorinformationen erhalten hatten, zeigten auf dem 5-Prozent-Niveau signifikante Beobachtungsunterschiede. Die Ergebnisse wurden zurückhaltend im Sinne differentieller Informations- und Speicherkodierungen ausgelegt. Implikationen und Anregungen für künftige Forschungen werden kurz skizziert.

Effect van Geloof in het Paranormale van Voorinformatie op het Waarnemen van een 'Paranormaal' Verschijnsel

Samenvatting: Dit experiment onderzocht hoe geloof in het paranormale en bepaalde voorinformatie het waarnemen van een zogenaamd paranormaal verschijnsel beïnvloeden. Op basis van hun gemeten geloof in het paranormale werden 40 proefpersonen verdeeld in 'sheep' en 'goats' (resp. gelovigen en ongelovigen). Beide groepen werden daarna gesplitst in twee subgroepen, die elk bepaalde voorinformatie kregen voordat zij gingen kijken naar een videoband met een demonstratie van verondersteld paranormale vermogens. Eén groep sheep en één groep goats werd verteld dat de verschijnselen echt paranormaal waren, terwijl de twee andere werd gezegd dat de band trucages toonde. Daarna vulden alle toeschouwers een vragenlijst in. De resultaten werden genalyseerd via een variantieanalyse met 2 factoren. Er was tussen sheep en goats geen significant verschil in de interpretatie van de getoonde verschijnselen. De waarneming door proefpersonen die vooraf tegengestelde informatie hadden gekregen week op 5% significant af. De resultaten worden voorlopig beschouwd als verschillen in het coderen en opslaan van waargenomen informatie. Implicaties en suggesties voor nieuw onderzoek worden kort besproken.

L'Effet de la Croyance au Paranormal et de l'Etat d'Esprit a priori sur l'Observation d'une Démonstration'Psychique'

Résumé: Cette étude a examiné comment la croyance au paranormal et l'état d'esprit a priori peuvent affecter l'observation d'une démonstration 'psychique'. Quarante sujets ont été classés soit 'moutons' soit 'chèvres' selon leur degré de croyance au paranormal. Les sujets ont ensuite été divisés en quatre groupes: deux groupes de 'moutons' et deux groupes de 'chèvres'. Chaque groupe a visionné un enregistrement vidéo de la démonstration d'une aptitude prétendument psychique. On a dit à un groupe de moutons et un groupe de chèvres que la démonstration était 'authentique' alors qu'on a dit aux deux autres groupes qu'il y avait une 'supercherie'. On a ensuite demandé aux sujets de remplir un questionnaire d'observation concernant la démonstration. Les résultats ont été analysés à l'aide d'une analyse de variance bi-factorielle. On a trouvé que les 'moutons' et les 'chèvres' ne différaient pas significativement dans leur observation de la démonstration 'psychique'. Les sujets conduits à adopter un état d'esprit contrastant avec leur croyance ont montré une différence significative au seuil de 5% dans leur observation. On interprète provisoirement les résultats en terme d'encodage et de stockage différentiel de l'information. On esquisse brièvement les implications et suggestions pour une recherche future.

Psychic Healing and Complementary Medicine

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Abstract: An overview of research on the effectiveness of 'psychic' healing on human subjects indicates that psychic healing can be effective, especially on subjectively experienced state of health. Objectively measured effects are much less pronounced. However, the strength of the effect of psychic healing seems strongly dependent on the patient's knowledge that treatment is attempted and appears to be mainly related to psychological variables associated with the patient and with the healer-patient interaction. Negative effects of psychic healing and of complementary medicine in general happen mainly when the patient terminates conventional medical treatment or does not consult a conventional medical practitioner concurrently with complementary treatment.

From an overview of mainly sociological studies of complementary medicine it can be concluded that the main characteristics of the various types of complementary medicine do not differ. Their effectiveness is on average about 68% and the main reason for the patient trying complementary treatment is that conventional medicine no longer seems to help them. Only their methods and the theoretical interpretations on which the various types of complementary medicine are based differ strongly. For all types of complementary medicine it is found that the effect of the method itself on the state of health of the patient is small or non-existent, compared to the effect of psychological variables associated with the patient and the healer. Therefore it is assumed that the effectiveness of the various complementary methods is to a large extent based on the same process. Complementary medicine and the placebo effect have much in common. However, from the data it appears that complementary medicine seems somewhat more effective than placebo treatment.

Although experimental evidence suggests the existence of a paranormal anomalous effect, the size of the effect is small compared to the effects observed in psychic healing. In the case of many complaints, it is also unclear how a possible paranormal effect could result in an improvement of health. Hence, although the possible contribution of a paranormal process cannot be excluded, it is unlikely to account for the observed effects of psychic healing on patients. Analogous reasoning can be applied to the explanations offered by the proponents of other types of complementary medicine. In general, the scientific validity and experimental basis of those explanations are weaker than the evidence for a paranormal effect. Therefore it seems likely that some important psychological processes are the basis of the main part of the observed healing effects in complementary medicine and in placebo treatments. A number of such psychological variables are discussed.

State of health can be measured objectively in terms of physiological or medical conditions and subjectively, in terms of how well the patient feels and is able to function. Objective measurements allow scientific medical treatment and if such treatment exists contributes to effective treatment. However, when objective measurements are not possible, or patients suffer from ailments and no objectively measurable deviation can be found, the subjectively measured state of health becomes the main criterion for measuring the effect of a treatment. In such cases a conventional scientific medical approach might not be possible and complementary medicine is perhaps one of the most effective treatments the patient can be offered.

Most research in parapsychology is aimed at understanding and controlling paranormal experiences such as telepathy, precognition and psychokinesis. Experimental research, for example, Ganzfeld and micro-PK studies, is reasonably successful (at least judged by the standards that are applied in the social sciences) and demonstrates that we might be on the track of an anomalous phenomenon. The origins of parapsychology lie in spontaneous paranormal experiences and in the activities of psychics and healers. In everyday life practical applications of the paranormal, such as the activities of psychics and healers, still play a more prominent role than parapsychology as a science. People are generally more interested in the question of how effective these applications are than in the results of laboratory studies or in scientific explanations. They want to know whether they should consult a psychic for a specific problem or what to expect from treatment by a paranormal healer. It is an important aspect of scientific activity to provide society with answers to such questions; answers not based on belief or disbelief in the paranormal but on factual and available research data.

Research with psychics dates back to the last century. A previous study (Schouten, 1991) presented an overview of all quantitatively evaluated studies with psychics and mediums. Based on these data conclusions could be drawn about the value of the assumed paranormal impressions of psychics and about the processes that play a role in psychic-client interactions.

Compared to psychics, a much larger number of paranormal healers are active. It seems somewhat surprising that despite the general success of conventional scientific medicine, and in view of the unproven effects of psychic healing and the weak and strange explanation its practitioners offer for their treatment, the method still attracts numerous patients. There must be strong reasons for the continuation of the existence of psychic healing. But despite the larger number of psychic healers, the amount of research carried out on psychic

healing is substantially less than that done on psychics. Fortunately, there are a number of studies of related fields of alternative healing that have yielded data also relevant for the understanding of psychic healing. Together they provide a sufficient basis to allow some conclusions with regard to the questions of the effectiveness of psychic healing and of the factors that contribute to this effectiveness.

Psychic healing is used here as a common term for a healing practice whose other names include mesmerism or animal magnetism (especially in use on the continent), spirit healing, ritual healing, paranormal healing, mental healing, psionic medicine, non-contact therapeutic touch and, the latest term I noticed, therapeutic intent. No doubt more terms exist. Although the practice is centuries old, its modern version is strongly influenced by the Austrian Mesmer who sought to explain healing effects by his theory of animal magnetism. Mesmer introduced making 'passes' (movements of the hands along the body without touching the body) based on the idea of 'restoring the balance of the patient's animal magnetism'. The idea that healing is basically the restoration of an 'imbalance' in the patient is still popular with many healers. Another idea that many healers believe in, is that the healing effect is due to the transfer of some 'energy' from them to the healee, or that they act as a transfer point for the transmission of such energies, which originate from spirit entities.

The very term 'psychic healing' suggests that healers who apply psychic healing do so by applying some psychic or paranormal influence on the patient's disease or affliction, thereby healing the patient. Thus, for instance, Benor defines psychic healing as 'the deliberate influence of a person or persons on another living thing or things (animal or plant) by mechanisms which are beyond those recognized and accepted by conventional medicine. The term 'paranormal' has been commonly applied to such influences' (Benor, 1986, p.96). This paranormal element plays a central role and indeed, judging from the

available literature on psychic healing, the psychic aspect seems much more prominent to the authors than the second part of the term: the healing.

Even a cursory glance at the reference list of this overview makes clear that this study deviated strongly from usual publications and overviews on psychic healing (see for instance Attevelt, 1988, Benor, 1990, Edge et al., 1986, p.248-251). These overviews centre on the possible paranormal aspects of healing and consist to a large extent of a discussion of research in parapsychology on the effects of psychokinesis on plant growth, bacteria, enzymes, or another person's physiology. Such studies might be relevant for the explanation of psychic healing, but also might not be relevant.

In science one should make a strict distinction between what people do or experience and the verbal explanation they offer for it. A case in point is astrology. When astrologers make statements about or predictions for clients, such statements might well be more often correct than expected by chance expectation. However, instead of finding out by what process the astrologer arrives at these statements, which normally involves much more than just drawing a horoscope and applying some rules, nearly all research to date has been focused on the explanation that the astrologer offers, i.e. that the constellation of celestial bodies at the moment of birth (or conception?) is of relevance. The only suggestive evidence that this explanation would merit some attention from scientists is the recent research into the Gauquelin effect. The Gauquelin effect, however, is rather weak and specific and it would at best explain only one statement from an astrologer, i.e. about the client's profession. This tendency to focus on the explanation provided by the practitioners, and even to accept or reject a priori the phenomena involved based on the likelihood of acceptance of such explanations, instead of studying the phenomena proper, is quite peculiar and in my opinion does not make much sense.

Spontaneous paranormal experiences offer another example. In this case the

experiences people report are undoubtedly real and can be studied. However, the explanation the people themselves believe in, i.e. that these experiences were brought about by some unknown paranormal process, is just one of the possible hypotheses. The characteristics of the experiences themselves should provide us with explanatory hypotheses for testing in research rather than the beliefs of the experients. Elsewhere I have called this the 'pragmatic' approach (Schouten, 1986).

Applied to psychic healing, this implied that I should start with making a distinction between what healers do, that is, healing people, and the explanations they offer for what they do, that is, that any healing effect is due to some paranormal process. I think it should be first decided whether psychic healing does have a healing effect at all. This is often assumed in discussions on psychic healing, but only rarely is some evidence offered for that assumption. Because healers work almost exclusively with human beings (a recent study by Attevelt (1992) indicates that only a few percent of the 300 or so Dutch healers polled treat animals as well), I limited this question to research with human beings. Thus I have only included studies that involved attempts by healers to improve some health problem from which the subjects or patients suffered; for instance, attempts to lower blood pressure level in hypertension patients or to speed up healing of wounds.

The basic question is, of course, why many people still turn to psychic healers for treatment. Is this because they are gullible, badly educated and ignorant about the possibilities of conventional scientific medicine? Or is it because psychic healing is effective where other therapies have failed? The latter assumption is not unlikely because one should expect that ineffective therapies do not withstand the test of time and would gradually vanish. If psychic healing is effective, then it is important to find out what the relationship is between effectiveness and a number of variables, for instance the type of complaint patients have. Only after collecting data on

characteristics of patients, complaints and effectiveness, does it become possible to derive from the characteristics observed ideas and hypotheses as regards possible explanations. Psychokinesis might play a role in explaining psychic healing, but is by no means the only explanation one can think of. And even if one accepts psychokinesis as a real and established phenomenon, there is still a wide gap to fill between a possible PK influence of a healer on the patient and the realization of a healing process.

Psychic healing cannot be considered as an isolated phenomenon; it is a method of healing among a wide variety of healing methods. To gain an impression about the value of psychic healing, for instance as regards its effectiveness, one needs criteria with which to compare it. Therefore I have included quite a number of studies on other types of complementary medicine in order to see in what respects they show similar or different characteristics. Such data also might yield indications about the mechanisms involved in these types of healing.

Throughout this paper I have used the terms 'complementary' medicine, 'non-conventional healing', or 'alternative' healing for treatments aimed at healing people that are not part of conventional scientific medicine as taught in universities. From the data that are discussed below, however, it becomes clear that the term 'complementary' medicine is the most appropriate. It appears that patients nearly always first turn to conventional allopathic practitioners and if later on they turn to a non-conventional practitioner the non-conventional treatment is taken mostly concurrently with and in addition to conventional allopathic treatment. Hence opponents of unconventional therapies must realize that patients would not seek unconventional remedies if conventional medicine had healed them.

Subjectively Experienced and Objectively Measured Effects of Healing

Two criteria for effects of treatment are commonly applied in studies on healing: changes in health according to objectively measured variables, for instance blood pressure values, and state of health as experienced by the patient and measured by questionnaire or scales. In conventional medicine a tendency exists to consider subjectively experienced state of health as a consequence of some objectively measurable illness condition and hence to concentrate only on objective measurements. However, although frequently the two are related that does not imply that the two always strongly correlate. There are instances in which patients suffer from complaints for which no objective deviation is found and which therefore do not lend themselves well for treatment by conventional medicine. On the other hand it can happen that people with serious medical deviations function quite well and are not even very bothered by it.

Because subjectively experienced state of health and objectively measured state of health do not always fully correlate it seems to me that both are essential criteria to measure the healing process. This applies especially to the subject of the present paper because one gets the impression that patients who turn to complementary medicine often suffer from ailments for which a low correlation exists between objectively measured and subjectively experienced state of health. A patient with a broken arm will experience a nearly perfect correlation between the state of the arm and the pain he suffers. But such patients will be rarely found in the practice of the unconventional practitioners; they stick to conventional treatment.

For patients, the subjectively experienced state of health might often be more important than the objectively measured condition. Treatment should be aimed at curing the disease and making the patient feel well. If curing the disease results in the patient feeling well that is to be the preferred solution. But unfortunately that ideal solution is often not attainable. In such cases, changes in subjectively experienced state of health also constitute an important criterion in evaluating the success of treatments. In many cases it is not even possible to detect objectively measurable medical reasons for the complaints. Then the subjectively experienced state of health is the only criterion that can be applied for measuring the effect of a treatment. Hence in the following overview, data on both objectively measured changes in state of health and data that relate to subjectively experienced health are discussed. Apart from the first study, presented mostly for historical reasons, the studies are discussed in the alphabetical order of the authors' names.

In addition there is often confusion as regards what is meant by the effect of the treatment. If a study employs a proper experimental design, in which two matched groups are compared, one receiving the treatment and the other not, then the 'effect of the treatment' is often operationalized as the difference observed between the experimental and the control group. However, for patients the effect of the treatment is more often associated with the difference between the state of health after treatment is completed compared with the state of health before the treatment. I have distinguished between the two by calling the effect operationalized as a difference between experimental and control group as the 'effect of the method', whereas the effect of the treatment as the difference between state of health after and before treatment will be called the 'effect on the patient'.

Studies on the Effect of Psychic Healing on Animals

That the problem of alternative practitioners is not only one of our day is illustrated by an investigation by a Dutch State Committee in 1917 (Rapport Staatscommissie, 1917). The committee consisted of 12 medical specialists who invited alternative practitioners (after guaranteeing that they would not be legally prosecuted for their cooperation) to treat patients who were under their care. The committee was forced to ask their own patients to cooperate in this investigation because the healers found few of their patients willing to participate. Hence the participating patients could hardly be considered representative for the group of people who made use of complementary medicine in those days, and some of the doctors' patients who had agreed to participate refused to continue the complementary treatment after one or a few sessions.

All patients were treated by alternative healers under supervision of the physicians, if possible even in the presence of the doctors. The study involved 96 patients, among them some who were quite seriously ill. This is illustrated by the fact that seven of them died during or shortly after the period of the investigation. All cases were medically evaluated on degree of improvement with the exception of 11 cases in which the patient never returned to the supervising committee member (the report does not state whether perhaps these patients were so satisfied with the alternative treatment that they gave up on the conventional treatment). Although no figures are presented, the committee stated that some patients were satisfied with the effects of the complementary treatment. In a small number of cases objective improvements were also observed. It was also observed that the patients appeared better able to cope with their complaints

even when medically they appeared unchanged or even to have deteriorated. The committee attributed all improvements observed to 'suggestion' and advised the government to maintain the then existing situation which gave only conventionally trained doctors the right to treat patients.

Surprisingly few experimental studies of the effectiveness of psychic healing were found that fulfil basic requirements such as proper controls and a double-blind design. One of the most productive experimenters in this respect is J.T.M. Attevelt from The Netherlands. After two extensive survey studies which are discussed below (Attevelt, 1981, 1982, 1988) he carried out two experiments to study the effectiveness of psychic healing using both objective and subjective estimates for measuring changes in the patients' state of health. The first study involved patients suffering from asthma (Attevelt, 1988), and the second one, patients suffering from hypertension (Attevelt et al., 1987; also reported in Beutler et al., 1987, and Beutler et al., 1988).

In his survey studies, Attevelt found that over 80% of patients suffering from chronic aspecific respiratory afflictions (cara or asthma) and treated by psychic healers reported improvement in their condition due to the treatment. These results, however, were based on a subjective assessment by the patients. The first Attevelt study was aimed at the question of how far these subjectively experienced improvements could also be demonstrated when objective measures were applied. In cooperation with the Lung Function Section of the Medical Department, spirometer and peak flow meter readings were chosen to establish an objective evaluation of the patients' state. In addition patients were requested to keep records of various details of asthmatic attacks they suffered during the period of the experiment and data were collected on the patients' general state of health and functioning.

Cara patients could express their interest in participating in the experiment following an announcement in several national newspapers. After registration, diagnostic data were obtained from each

patient and from their doctor. Based on these data, three groups of patients were formed which matched each other as much as possible as regards the medical condition. As an extra precaution their lung function was again determined at the Academic Hospital, after which, according to a randomization procedure, the patients were distributed over the three groups, each of 32 patients. The groups were also comparable as regards age and gender.

Each group of patients was randomly assigned to one of three experimental conditions. The patients in the 'Optimal' condition were treated by psychic healers who used the same methods as they did in their everyday practice. Patients in the 'Distance' condition were treated by healers from behind a one-way screen whereas patients in the 'Control' condition were not treated at all. Patients in the 'Distance' and 'Control' conditions did not know to which condition they had been allocated and hence did not know whether they had been treated or not. The design was based on the assumption that if an effect could be demonstrated, it could be attributed to a psychological effect associated with the patient (in the case that the 'Control' group improved), to a psychological effect associated with the healer (in the case that the 'Optimal' group performed better than the 'Distance' group) or to a paranormal effect (in the case that the 'Distance' group improved more than the 'Control' group).

Based on statistical data it was known that improvement could be expected within eight treatments. The Federation for Paranormal Healers (NFPN) provided six qualified healers, who all felt confident that they could heal effectively under the conditions of the experiment. A double-blind design was employed. The assistants who took the peak flow measurements were also unaware of the association between names and conditions.

The patients in the 'Optimal' condition improved significantly on peak flow measurement and non-significantly on the spirometer values. Improvement in subjectively experienced state of health was reported by half of these patients; the oth-

ers reported no change or, in the case of two patients, deterioration. Similar results, however, were observed for the patients in the other two conditions. The absence of a difference between the 'Distance' and 'Control' conditions indicated that no paranormal factor was involved. According to an analysis of variance the groups did not differ in degree of improvement, except for a trend that in terms of subjective experience, the patients in the 'Optimal' condition felt more improved than patients in the other groups. This difference became significant when 'Optimal' and 'Control' groups were directly compared. Correlations between peak flow measurements after sessions and subjectively estimated state of health was positive: $r=+.32$. Although this correlation was statistically significant it is clear that there still remained quite a discrepancy between objective measures of health and subjectively experienced health. Hence it appears that in this experiment psychic healing had an effect but that the effect was mainly due to psychological factors associated with the patients. However, the stronger subjective improvements in the 'Optimal' condition also suggest some psychological effect associated with the healers.

In a second study by Attevelt, 1200 patients suffering from hypertension who showed an interest in participating in an experimental study on psychic healing were first sent a questionnaire to collect data on their medical history. After receiving patients' permission a similar questionnaire was sent to their doctors. Based on the data received, 200 patients were selected who, according to WHO criteria, suffered from hypertension and who had no additional illnesses or complications. These patients were invited for medical screening. Based on this screening 120 patients were finally selected for participation. These 120 were grouped in triplets based on comparable blood pressure values and whether they took anti-hypertensive drugs. Then from each triplet patients were randomly allocated to one of the three conditions of the experiment.

The design of the study was similar to the design of the asthma study described above and involved the same three conditions: 'Optimal', 'Distance' and 'Control'. The experiment involved 15 sessions for each patient, one per week. Treatment in each session lasted 20 minutes. Prior to and after each session paramedical assistants took blood pressure values. Patients were regularly checked by a physician for possible changes in medication, diet, and other non-experimental changes that might have influenced their blood pressure. All proceedings during the experiment were recorded on video. At the end of the study all patients filled out a questionnaire on well-being and on their subjectively assessed degree of improvement or deterioration during the period of the experiment. Twelve well-known healers from different Dutch healing associations who all had confidence in their effectiveness in both direct treatment as well as treatment from behind one-way screens treated the 80 patients from the 'Optimal' and 'Distance' conditions.

The experiment can be characterized as being of a triple-blind nature. Not only were patients (except those of the 'Optimal' group), staff, medical assistants and physician blind as regards the conditions the patients were assigned to, but also the statistical analyses were performed without the statisticians knowing the relationship between sets of data and conditions. Between screening and the start of treatment all three groups showed on average an equal reduction in blood pressure. During the 15 weeks of treatment all three groups showed a further strong reduction in blood pressure. No difference in degree of improvement was observed between patients taking or not taking anti-hypertensive drugs. Mean blood pressure for the patients in the 'Distance' condition was systematically lower than for the other conditions. However, a multivariate analysis showed only a weak significant difference ($p<.05$) between groups for diastolic blood pressures. A direct comparison between blood pressure values of the 'Distance' and

'Control' conditions yielded a non-significant difference.

According to the subjective estimates of the patients themselves, 83% of the patients in the 'Optimal' condition felt improved. For the 'Distance' condition this percentage appeared to be 43% and for the 'Control' condition, 41%. Thus the personal treatment by the healer clearly contributed to the feelings of well-being of patients. No significant correlation was observed between subjective estimates of improvement and blood pressure reduction.

If the effect of lower blood pressures in the 'Distance' condition is attributed to a paranormal factor, it is clear that that factor contributed only little as regards strength of effect compared to the objectively measured improvement observed for all conditions. Hence in this study it was found again that psychic treatment had an effect on health, even objectively measured, but that the effect was mainly due to psychological factors associated with the patients. Subjectively measured in terms of well-being, psychic healing was relatively strongly effective; over 80% of the patients in the 'Optimal' group felt improved. From the difference between 'Optimal' and 'Distance' groups it can be concluded that this effect was also partly due to psychological factors associated with the healers.

It is somewhat surprising that the taking of medication did not have any influence on the degree of blood pressure reduction during the 15 weeks of observation. In this study the correlation between objectively measured and subjectively experienced state of health was even lower than observed in the asthma study. Despite a nearly equal drop in objectively measured blood pressure in the three conditions, the subjective improvements varied from 41% to 80%. However, hypertension as such is not felt by the patients; rather, certain symptoms become manifest. This might also be the explanation of why in this experiment the percentage of patients from the 'Optimal' group reporting subjectively experienced improvement is larger than the 'Optimal' group in the asthma study.

The results of both studies indicate that psychic healing can result in objective improvement and, to a larger extent, subjective improvement in the patients, but that these improvements are mainly related to psychological variables associated with the patients themselves. The effect on the patients appears substantially larger than the effect of the method.

Patricia Heidt (1981) of the USA studied the effect of 'therapeutic touch' (the term coined by D. Krieger, see below) on the anxiety of hospitalized patients. Anxiety was measured before and after the experiment by means of a 20-item questionnaire for which normative data were available. Ninety volunteer patients took part, allocated (it is not described how) to one of three conditions: 'Therapeutic Touch' (TT), 'Casual Touch' (CT) and 'No Touch' (NT). In the TT condition healing was explicitly intended, in CT the patient was touched by taking pulse rates, and in NT the healer sat beside the patient and talked with the patient without touching. In all conditions the author acted as the healer. The difference between TT and the other conditions are described by her as a difference in state of the healer. In applying TT she entered a meditative state of consciousness, in contrast to the 'ordinary state of awareness' that applied to the other conditions.

A significant difference between pre- and post-test anxiety scores was observed in the TT condition but in neither control condition. The author attributed this difference to the effect of therapeutic touch. However, the three conditions are not really comparable. In the TT condition the author meditated and was silent. In the other conditions the author either took pulse rates, by methods including using a stethoscope, or discussed how the patient had responded to the medical treatment. Both activities might be rather upsetting to patients hospitalized in a cardiovascular unit of a hospital. In addition all interventions were administered by the author herself who might not have been entirely neutral as to her own capacities as a healer. Thus the results might have been due to

factors other than the application of therapeutic touch.

A double-blind clinical trial to test the efficacy of prayer was reported by Joyce and Weldon (1965). Although healing by prayer is, at least as regards method, different from psychic healing the authors apparently felt it to belong to the 'spectrum of so-called paranormal healing' (ibid., p.367). Forty-eight patients suffering from chronic stationary or progressively deteriorating psychological or rheumatic diseases were selected to take part in the study. It is not clear how many belonged to each group of complaints. Patients were matched in pairs as closely as possible for sex, age and clinical diagnosis. More than half of the pairs could also be matched for marital status and religious faith. Patients from each pair were randomly allocated to the experimental and control groups. Patients did not know they were subjects in an experiment and the physician examining the patients was kept blind as to which group each patient belonged to.

The trial lasted six months during which period six prayer groups prayed daily for the patients of the experimental group. No direct contact existed between prayer groups and patients. At the end of the treatment period a physician examined the patients again. Changes (according to unspecified criteria observed in the patient's clinical condition and in his or her attitude towards the illness were taken as the main dependent variables. The net change in the treated patient was compared with that of his or her paired partner and greater improvement or less deterioration was given a positive sign. Although the patients in the experimental group fared somewhat better, the difference between the two groups was far from significant.

Another study generated by the concept of therapeutic touch was reported by Keller and Bzdek (1986) of the USA. Thirty patients suffering from tension headache were treated by TT healers and headache was measured before, straight after, and four hours after treatment by administering pain questionnaires. Tension headache is of course a subjectively experienced state of

health. The authors created a control group of 30 patients by treating patients in the control group exactly as those in the experimental group except that the healer, instead of trying to heal, mentally counted backwards. It is not clear to what extent the experimental and control groups were comparable as to other variables that might have influenced their headaches. Tension headache reduced significantly in the two groups but to a significantly stronger degree in the experimental group. No difference between the conditions was observed in the four hours post-test. However, when all subjects were removed from the two groups who had taken other treatment to relieve their headache (20 of the 60 subjects) during the four hours after the treatment was administered, the significant difference showed again. Hence this experiment showed a pattern similar to that observed in the Attevelt studies. The largest effect appeared to occur in all groups, but the experimental group apparently received some extra benefit from the method itself, resulting in significantly lower pain scores.

F.W. Knowles (1954) of the UK had the opportunity while serving in India to study some ancient traditional methods of psychic healing of that country. The most easily demonstrable effect of these methods appeared to be the relief of pain. Often such relief was temporary but tended to become complete and permanent after repeated treatments. Knowles became highly interested and practised the method himself with much success. Later he started experimentation. He applied his psychic healing to artificially induced injuries that enabled standardized testing of the healing rate of those injuries. At first he applied acid burns. Two (sometimes four) wounds were produced on the skins of healthy volunteers. Healing was applied to one of these wounds, the other served as a control. The aim of the project was to compare the effects of autosuggestion and of psychic healing. Forty-two experiments were performed but Knowles felt that nothing conclusive had been demonstrated, partly

because the measurement technique appeared not to be optimal.

He improved his method by applying a much more accurate technique for inducing heat burns. Thirty-seven experiments followed, in which autosuggestion, hypnotic suggestion and psychic healing were compared. He expected especially positive results from hypnotic suggestion. However, no effects with any of these methods were found. He tried several more experiments but gave up because of the consistent negative results. So, he was left with the question of how to explain the discrepancy between these experiments and his very positive clinical experience. According to Knowles, the majority of patients responded with positive effects to psychic healing, regardless of whether the patients approached the treatment with faith or with scepticism. But attempts to relieve artificial pain in healthy subjects, even in credulous ones, nearly always failed. Hence it is possible that psychic healing can only affect truly sick patients.

In a later paper Knowles (1956) sums up his experiences with psychic healing. He felt that his healing was only effective when he made a conscious effort of mind to heal the patient but he found also that this was not enough. As he states it, 'when I treated a patient without his knowledge of my intention to help him, the treatment nearly always failed' (Knowles, 1956, p.113). In view of the results of the studies discussed above, Knowles' lack of results is not surprising. If the main effect of treatment is due to psychological variables associated with the patient then those would have affected both wounds simultaneously and hence no difference should be expected. However, for the same reason Knowles' experimental design seems a very good one to demonstrate a possible paranormal effect in healing, because the influence of all psychological variables must be the same for the two wounds.

Stimulated by research by Grad (1965, 1967, 1970), Grad et al. (1961), and Smith (1972), and based on her background in Eastern religions and her experiences when working as a nurse for the healer Estebany,

Dolores Krieger (USA) became strongly interested in and convinced of the potential healing effects of the 'laying-on of hands'. Eastern literature taught her that a healthy person has an overabundance of *prana* and that the ill person has a deficit. The deficit is actually the illness. *Prana* can be transferred to another person if one has the intention to do so. Thus healing depends on the healer being healthy and having an intent to heal. The literature also states that *prana* is intrinsic to what we call the oxygen molecule. Hence she got the idea that haemoglobin (the oxygen-carrying pigment of red blood cells) might be the connecting factor in healing. She coined a new term for the laying-on of hands: 'therapeutic touch'.

In Krieger (1975) an overview is presented of a number of studies in which she investigated the effects of healing on haemoglobin levels. In three studies, none of them blind, Estebany acted as the healer treating patients in the experimental condition. In all studies the experimental group showed significantly higher mean haemoglobin levels. However, the conditions did differ in more respects than just treatment versus non-treatment, and variables that might have affected haemoglobin levels were apparently not always controlled; hence the increase in mean haemoglobin level in the experimental groups might also have been due to other reasons. In a follow-up study the experimental and control conditions were made more comparable. In the experimental condition 16 nurses trained in TT each treated at least two patients, whereas in the control condition 16 other nurses only touched patients as required in routine nursing procedures. Various other aspects of the experiment were also improved, as for instance the introduction of a check on the comparability of the two groups of patients as regards pre-test haemoglobin levels and distribution of age and sex, which have an important effect on haemoglobin levels. Despite the increased rigour of the experiment the effect was again demonstrated. In the experimental group a significant difference between pre- and post-test haemoglobin

values was observed but not in the control group.

Another study in which patients suffering from hypertension were involved was carried out by Miller (1982). Eight healers tried to heal over distance 24 patients in the experimental group. Healing was applied in addition to orthodox medical treatment. The application of distance healing made it easy for Millar to apply a double-blind design. Because psychic healing is normally done in the presence of the patient it is difficult in these studies to ensure a blind design. No significant changes in dependent variables were observed except for an improvement, significant at the .01 level, in systolic pressure in the experimental group. However, in view of the fact that this result was a selection out of a number of analyses and considering that the experimental and control groups might have differed in other respects such as differences in medication, the results are not really convincing.

The Dutch neurologist Musaph (1949a 1949b) studied seven patients of the Dutch psychic and healer Croiset. All these patients had consulted the healer for neurological complaints, mainly palsies of the limbs. The patients reported on various sensations they experienced during the treatment, including sensations of warmth, contractions of the muscles, drowsiness, tiredness, a feeble electrical current and pain in the muscles. Musaph carried out several small experiments: patients were treated from another room without them knowing it; patients were treated from a different room and they knew it, but they did not know at what time the treatment would start; and narcotized patients were treated. In all cases it was clearly demonstrated that the reactive contractions in the paralysed limbs that the patients normally showed during treatment did not coincide with the periods of treatment but with the expectations the patients had as to the moment of onset of treatment. Musaph was able to induce similar effects, both subjective sensations as well as reactive contractions, in patients by applying different methods. He therefore concluded that the

hypothesis of 'magnetism' is unnecessary and misleading and he considered psychic healing to be a form of psychotherapy. He attributed the sensations described to an increased supply of blood to the treated limb.

A small but quite revealing study on psychic healing was reported by Rehder (1955), head of a department of a hospital in Hamburg, Germany. Rehder described the cases of three patients confined to their beds and who were medically given up. So Rehder, who apparently really cared about his patients and was willing to try everything, took the unusual step of contacting a famous psychic healer. This healer, who lived in Munich, was willing to try to 'heal' these patients by healing at a distance. The 'sendings' would take place at specific times in the mornings. However, Rehder did not tell anyone, including the patients, about these sendings. No effects at all on the patients were observed. Then Rehder decided to do a little experiment himself. He told the patients about this famous healer, gave them a book from this healer to read, and did everything to convince these patients that healing at a distance would work (not telling them of course that the healer had already tried to heal them). After the 'belief' preparation was completed he informed the patients that the 'sendings' would take place during a certain period in the day, a period he knew that the healer normally did not work at all. This time the healer was not informed.

The results were quite astonishing. In all three cases, in addition to strong feelings of increased well-being, various objective improvements in the conditions of the patients were observed. Within a few months all three of the patients were well enough to leave the hospital. A few more similar cases are described that also resulted in drastic improvements in the patients' conditions. According to Rehder these results were not unexpected. It is the belief that counts, not the method that the healer applies. If nothing works any more, then often belief still does. Belief can become very strong especially in the case of patients who are desperate and have suf-

ferred already for a long period of time, partly also because due to the illness patients tend to become less critical.

Richmond (1946) of the UK reported a small study in which an observer attended treatments provided by a healer (F. Knowles). In each case notes were taken upon a printed form on which the patient had first recorded details about his or her complaint and about medical or hospital treatment. A record was kept of the duration of each treatment and of its effects as reported by the patient.

Direct observations were available from 43 patients. Of these patients, 35 reported either that a painful condition was relieved at the time of treatment or that a chronically recurrent pain ceased to recur after a number of treatments. Thirteen of these cases resulted in a complete recovery; at least, patients themselves appeared satisfied that a cure had been effected. The author noted that it was difficult to find suitable cases because he only included patients who were suffering pain from an identifiable physical cause and who were not under some kind of medical treatment. Otherwise improvement could have been due to the other treatment. This was the reason that the study involved a fairly high proportion of obstinate ailments of long standing for which medicine had done what it could and apparently could do no more. However, from many studies we know that this is not an uncommon situation for patients of complementary medicine.

In order to obtain an impression of the nature and results of psychic healing, Rose (1955) of the UK approached patients who claimed to have benefited from healing and asked them to provide medical records. He obtained 90 cases that were all claimed (by a healer, the patient, or the press) to have derived considerably benefit after orthodox medical treatment had failed. However, in 58 cases it was not possible to obtain medical or other records. Of the remaining cases he found that in 22 the records were so much at variance with the claims that further investigation was useless. In 14 cases improvement occurred, although some-

times temporarily, and in one case no benefit was gained and the patients continued to deteriorate. This study clearly demonstrates that claims made by healers or patients have to be treated carefully. It is, however unclear from the study who had made the claims in the 22 cases in which such large discrepancies between claim and medical record were observed.

One of the better known earlier studies of psychic healing was carried out in Hans Bender's Freiburger Institute (Germany) by Inge Strauch (1958, 1963). During seven months in 1955 a well-known German healer treated patients on four successive days each month, in one of the institute's rooms. Two staff members of the Medical Department of the University of Freiburg took care of medical supervision and measurements. In total, 650 patients participated. Before being treated each patient provided a biographical sketch and a description of the history of the illness. If the patient was willing they also completed a number of psychological tests. The physicians checked the subjective account of the patient by a short medical examination. Patients who returned for more treatment sessions (38%) were each time questioned again each time. A group of 160 patients were selected for more detailed research. They were given a clinical examination in the medical clinic before and after treatment. These patients served to establish an objective evaluation of the healer's degree of success. Sociological data were obtained from all patients.

Women constituted two thirds of the patients. Eighty per cent of the patients were older than 40 years of age and one third were between 45 and 55. Diseases of the heart, circulation and blood vessels accounted for 25% of the complaints, followed by inflammations, diseases of the bone and rheumatism. About 75% of the patients had suffered from their complaints for more than five years and hence could be considered chronic cases. About 60% of the patients who first saw the healer were at the same time under conventional treatment. This percentage increased during the period of the study, partly because the

healer explicitly recommended the patients also to consult a conventional practitioner.

Strauch included some interesting and often neglected questions. One question was how the patients rated the results of the conventional medical treatment. This gives, of course, a biased picture because these patients had a reason to turn to complementary medicine. More than a third characterized their health as 'temporarily improved' as a result of the medical treatment and 18% as 'distinctly improved'. A further 30% reported no change. Hence about 54% could be classified as belonging to the 'benefit' group as far as conventional medicine was concerned.

The majority of the patients had heard of the healer via acquaintances. Of these acquaintances 80% had been a patient of the healer. Fifty percent gave as their motivation for consulting the healer that they wanted to try everything. Twenty-six percent did so because they felt that medicine was unable to help them. Strauch also investigated the expectations the patients had of the treatment. Only 3% were convinced they would be cured and only 18% were convinced improvement would occur. The majority merely hoped for improvement. About 20% indicated a 'wait and see' attitude and 5% were downright critical. Interestingly these figures hardly changed after the first treatment by the healer.

During treatment the majority of the patients reported subjective sensations. Most frequently reported were sensations of warmth. More than two thirds of the patients reported a sudden diminution or intensification of their symptoms. Data on subjectively experienced effects of the treatment were obtained from 538 patients. Improvement was reported by 39% and temporary improvement by 22% of the patients. Thus 61% of the patients had somehow benefited from the treatment. Only 10% felt deteriorated and the condition of 29% remained unchanged. Patients who concurrently were also under conventional treatment reported less improvement.

It proved possible to evaluate the extent of objective changes in 247 patients. However, when changes in condition cor-

responded to the expected course of the disease the doctors allocated that patient to the 'no change' group. It is not clear how and when exactly the doctors arrived at their expectations of the development of the illnesses and whether these predictions were recorded. By this criterion, objective improvement was observed in 11%, no change in 75% and deterioration in 14% of the cases. It is clear that the subjective assessments by the patients strongly deviated from the doctors' classifications. Thus of the group of patients whom the doctors had classified as 'unchanged', 61% felt they had improved, and of the group of patients whom the physicians had rated as having deteriorated, 50% had the opposite opinion and declared that they felt better.

Patients were also administered psychological tests. In order to study a possible relationship between personality variables and reaction to psychic treatment Strauch constructed two opposite groups of patients. The 'positive' group included 63 patients who all reported improvement and felt from the onset onward very positive about the possibility of being healed, whereas the 'negative' group included 43 patients who were critical and reported no change or deterioration. However, with most test scores quantitative analyses failed to discriminate between the two groups. Differences were observed in imaginative capacities and degree of tension; the negative group was found to be more imaginative but also to be more tense and critical.

According to Strauch the lack of objective improvement confirms her general impression that the healer's influence produces, in the main, subjective changes. This subjectively experienced improvement is produced chiefly by the attitude of the patient, by his or her expectations and readiness to respond. A more critical attitude might block a favourable response as, for instance, indicated by these patients also having had less subjective sensations during treatment. It should be noted, however, that the way the objective data were processed was highly 'subjective' in itself. As we know, most of these patients had been suffering for more than five years

from their affliction so one wonders how these doctors arrived at their prediction that apparently a number of them could be expected to improve objectively and therefore ended up in the 'no change' or 'deteriorated' group.

Tenhaeff (1960) of The Netherlands reported on some small experiments carried out with a psychic healer who called himself a 'radiaesthetist', meaning a person sensitive to radiation. The aim of these tests was to study the supposed ability of the healer to diagnose 'paranormally' the illness the patients suffered from. Although as such outside the scope of the present overview, the results are of interest because in the test the healer was presented with blood samples of both sick and healthy subjects and his task was to discriminate between the two types of samples. In total 40 trials were carried out, on average three to five a day. Tenhaeff presents no figures but states that the results were occasionally interesting from a parapsychological point of view, but medically of doubtful value. That implies that apparently this healer, who enjoyed quite a reputation for being successful and who therefore had attracted Tenhaeff's interest, was not able to distinguish between blood from healthy and from ill people.

Frequently patients of psychic healers report feelings of warmth during treatment. Van Wijk et al. (1984) of Holland measured these sensations objectively by measuring skin temperature on the nose. One patient with a long history of psychic treatment and who reported strong subjective sensations due to treatment acted as the subject. Measurements were taken in a laboratory while the healer remained in her own practice. At pre-arranged moments of time the healer applied 'healing at a distance'. All parties involved were informed about the times healing was supposed to take place. Temperature increased during the period of treatment by about three degrees.

A follow-up study consisted of 15 trials with the same patient. In eight trials the patient was informed about the times of treatment, but not in the remaining seven.

The trials in which the patient was not informed about the time of treatment did not result in any increase in temperature. Hence it can be concluded from these data not only that subjectively experienced sensations of warmth during treatment can be due to real increases in temperature in the part of the body involved (not really a new finding), but also that such sensations are not caused by any healing process emanating from the healer but depend on the healee's expectations. This conclusion confirms earlier findings such as those by Musaph and Rehder.

In another study by van Wijk et al. (1985) skin conductance values were measured according to the 'method of Voll' in patients who were treated by and in the presence of a psychic healer. The authors concluded that differences between skin conductance values measured at various places on the body became less after treatment. They interpreted this in terms of an increasing harmonization in the patient's vegetative functions which in turn is supposed to have a beneficial effect on health.

In a third study skin conductance was again measured before and after the experimental session at seven different locations on the body. Patients consulting a healer's practice volunteered to take part in the study. Patients were randomly assigned to the 'treatment' group ($n=41$) or a 'control' group ($n=39$). Patients in the control group waited for the period that a treatment would normally take; 'post'-treatment measurements were taken, and then these patients received the psychic treatment they had come for. All patients were fully informed about the procedures. Many analyses were carried out but only a few weak significant differences were observed between the treated and control groups. The authors concluded that the paranormal treatment had resulted in a more equal distribution of conductivity values across the body, which they interpreted as a 'normalization' of the activity of the sympathetic nervous system on the different locations of the body. However, if corrections for multiple analyses had been applied to the observed significant probability values

it is doubtful whether any factual basis would have remained for that conclusion. In addition, even assuming the conclusion to be correct it is not clear what a reduction in differences of conductivity values has to do with an improved state of health.

Recently Wirth (1990) of the USA published a study in the Knowles tradition, on the effect of healing on artificially induced wounds, but in a more sophisticated way and with more success. For the healing he uses the term 'noncontact therapeutic touch'. By using a skin-punch biopsy instrument, full-thickness dermal wounds were inflicted on the arms of healthy subjects who were then randomly assigned to experimental and control conditions. Subjects were kept blind as to the nature of the experiment. All subjects took part in 16 daily 5-minute sessions in which they each put their arm with the wound through a hole in a closed door into a room not visible to them. They were told that during this period an automated device would record the 'energy' flowing from the wound. All sessions with experimental subjects took place in the morning when an experienced healer (TT practitioner) performed noncontact therapeutic touch on the subject's wound. Subjects in the control group followed the same procedure except that in their case the room in which they put their arm remained empty.

On days eight and 16 the physician who had applied the skin biopsy procedure traced the perimeters of the wounds and cleansed and redressed the wounds. The perimeter tracings were sent to a laboratory assistant who calculated the surface area. The physician and laboratory assistant were also kept blind as to the purpose of the experiment and group assignments. The measurements of wound surface were used as the dependent variable in the statistical analyses.

After completion of the sessions the subjects were informed about the true nature of the experiment and were asked, among other things, whether they had heard anything from the other room or had felt anything touch their arm during the session. All answered in the negative. The

average wound sizes on day zero (the day the wounds were inflicted) was equal for both groups, around 59 square millimeters. On day eight the average for the treatment group was only four mm², and on day 16 less than one mm². For the control group the comparable data were 19 and six mm². On day eight the groups differed significantly according to a *t*-test. On day 16 the proportions of healed versus non-healed wounds for the two groups were significantly different. Hence the authors conclude that NCTT is a highly effective treatment for dermal wounds in healthy subjects. This result is at variance with Knowles' findings.

In a second study, Wirth et al. (1992) followed the same basic design but with some modifications. It is presented as a replication and extension of the first study. This study did not use surface measurements of wound size but utilized a photography method because that method is an established and accurate measurement system for cutaneous wounds. Twenty-four subjects participated, and again they were randomly assigned to the treatment or control group. It is only mentioned that groups did not differ as regards distribution of age. In this study wound size was not measured in square millimeters but a physician, blind to the procedure, gave estimates on wound closure and re-epithelialization on a five-point scale (0-25%, 25-50%, 50-75%, 75-99%, 100%). Measurements were not taken on days eight and 16 but on days five and ten. Wounds were also judged on infection, scar formation and pigmentation of scar and cosmetic appearance. In addition to the physician's measurements, three independent physicians who were also kept blind judged all 48 photographs of the wounds on the criteria 'fully healed' or 'not fully healed'. In this study subjects did not put their arms through a hole into an adjacent room but instead were treated (or not treated) from behind a one-way screen.

The data for re-epithelialization and wound closure proved identical. Combining categories, a significant difference was observed between the treated and control groups for day five, and a marginally sig-

nificant difference for day ten. The judgments of the three independent physicians correlated fairly highly with the assessments of the first physician and confirmed the existence of the difference. The other categories did not lend themselves well to analysis, due to the still large number of unhealed wounds on days five and ten in the control group. It is concluded that NCTT therapy combined with traditional medical care (such as dressing) is a safe and effective nursing intervention.

There are a number of studies into the efficacy of therapeutic touch that are discussed by Benor (1990) in his overview on psychic healing research but that are unpublished and not available to me. Hence I just mention them briefly based on what Benor wrote. Meehan (1985) applied TT to alleviate post-operative pain. No significant difference between experimental and control conditions was observed. Pain medication proved clearly superior for pain relief to TT treatment. Goodrich (1974, unpublished), in a study on psychic healing, asked three judges to rate subjective reports that healees wrote after a real or supposed treatment period in order to see whether the judges could identify when a healing treatment had occurred. Healers and healees were given a schedule for distance treatment and were led to believe that they had received the same schedule. However, in reality half of the treatments were scheduled nonsynchronously, at least an hour after the healee expected them. The judges were given the healers' and healees' self-rating forms completed by the patients after the scheduled treatments and asked to identify the synchronous and non-synchronous treatments. They were able to do so with a significant degree of success.

Another unpublished study was done by Quinn (1984). Her study resembled the Heidt investigation and also involved cardiovascular patients and measurement of anxiety. Her experimental group of 30 patients received TT and demonstrated a significantly greater reduction in post-treatment anxiety scores than did the

patients of the control group who did not receive TT treatment. Ferguson (1986, unpublished) apparently compared trained versus inexperienced healers and found that the difference between healing pre- and post-treatment was significant for both groups but significantly better for the experienced healers. Fedoruk (1984, unpublished) studied the effect of TT on premature infants but with little success. Collins (1983, unpublished) studied the effect of TT on leukaemic children but according to Benor (1990, p.25) there are so many flaws in the study that no conclusions can be drawn from them. Randolph (1979, unpublished) presented healthy subjects with stressful stimuli and then applied TT or casual touch. Dependent variables were skin conductance, skin temperature and muscle tension. No difference was observed between the conditions. Parkes (1985, unpublished) like Heidt and Quinn also studied the effect of TT and mock-TT on the anxiety levels of hospitalized patients. No doubt contrary to expectation anxiety scores in the two groups increased to the same extent.

From the data presented above it can be concluded that treatment by psychic healing did have an effect on the health of patients. The effects were stronger for subjectively improved state of health than for objectively measured indicators of health. It appeared important that the patient knew that treatment was being attempted; in only a few studies (for instance those by Wirth) were effects observed when patients did not know that healing was being attempted. The objectively observed effects of the method, due to the nature of the treatment itself, were much weaker or non-existent compared to the effects on the patients, due to psychological variables associated with the patients. Psychological factors associated with the healer-healee interaction also contributed to the observed subjectively experienced improvement. In none of the studies were strong indications of possible negative effects of the treatment found.

Some Data on Studies of Effectiveness for Other Types of Complementary Medicine

In order to interpret the implications of the findings described above it seems useful to compare the data described above effectiveness with the data on effectiveness of other types of complementary medicine. Fortunately a number of recent publications have appeared that provide such data.

Jacobs et al. (1991) have reviewed the literature on studies in which the effectiveness of complementary remedies on rheumatic diseases was studied. They found 60 publications relevant to the issue. However, many of them concerned remedies such as special diets that are typical only for this disease. Studies were dichotomized according to methodological qualities into 'convincing trials' and 'trials that seemed to be less valid and/or difficult to interpret'. Most studies found of the better-known types of complementary medicine concerned acupuncture. Of the 'acceptable' studies, nine did not and two did result in the acupuncture treatment being more effective than the control (placebo) treatment. For homoeopathy only one acceptable study was found that resulted in a positive effect of the treatment, and for manual therapy four acceptable studies were found with two resulting in a positive effect and two not. The authors conclude that most types of complementary remedies are not more effective than control or placebo treatment. On the other hand, they caution against the assumption that the effectiveness of all conventional treatment has been proven and cite an example of an effective remedy applied in conventional medicine for which no satisfactory explanation exists.

Ter Riet et al. (1990) from the Medical Department of the University of Maastricht, The Netherlands, carried out a similar literature survey concerning acupuncture and chronic pain. A literature search yielded 51 controlled clinical studies on the effectiveness of acupuncture on chronic pain. These studies were classified on a 100-point scale according to 18 pre-defined methodological

criteria. This enabled the study of a possible relationship between methodological shortcomings and positive outcome (acupuncture proved more effective than control or placebo treatment). The quality of even the better studies appeared rather mediocre. No study obtained more than 62% of the maximum 'methodology' score.

The results of the 11 better studies (those with a score > 50), are according to the authors, contradictory. Five of these indicated a significant effect of the treatment, six did not. Eight of the 24 studies with a positive outcome scored 40 points or more and 13 of the 27 negative studies scored in this range. But because no studies of really acceptable quality were available they thought it wise not to draw any conclusion about the efficacy of acupuncture in the treatment of chronic pain.

The same authors also studied the effectiveness of acupuncture on asthma (Kleijnen, ter Riet, & Knipschild, 1991). In assessing asthma conditions, lung function and severity of symptoms are very important. The authors stress that when subjective symptoms are the main outcome measure, substantial improvements in patients in the control group can be expected and so large groups with a similar prognosis at the baseline are needed for valid assessment of the efficacy of the method of treatment applied. Again studies were classified according to methodological criteria. Thirteen studies were found but the quality of even the eight better studies (those obtaining more than 50% of the maximum methodology score) proved to be mediocre. The best study obtained 72% of the maximum score. According to the authors of the papers, eight studies reported a positive effect of acupuncture on asthma, implying that the acupuncture treatment produced better results than a sham treatment. However, only three of these studies obtained a methodology score of over 50%. Hence it appears that 'success' decreased with improved quality of the studies. It is concluded that the claim that acupuncture is effective in the treatment of asthma is not based on the results of adequately performed experimental studies.

The existing evidence remains contradictory. A similar result was obtained in an overview of studies in which acupuncture was applied as an aid to quit smoking (ter Riet et al., 1990). Here again the results became more negative as the methodological quality of the studies increased.

The effectiveness of homoeopathy was studied by Kleijnen, Knipschild, and ter Riet (1991) from the same university department. An exhaustive literature survey resulted in 107 controlled trials in 96 published reports. As in the previous overview study experiments were scored according to a list of criteria for methodological quality. The authors attached much importance to the number of participants in the studies. They stated that in most indications for homoeopathic treatment subjective symptoms are the main outcome and that therefore substantial improvement in the control group can be expected. Hence, because of possible differences in baseline values between groups due to the randomization procedure, fairly large groups are needed for valid assessment of efficacy.

Many experiments seemed to be of low quality but fortunately there were a number of exceptions. Of the 105 trials, 22 obtained a score of 55 or higher and 16 a score of 60 or more. Based on the interpretation of the data as given by the experimenters themselves, a positive result was observed in 81 experiments and in 24 trials no effect of homoeopathy could be demonstrated. However, in 42 of these trials the reviewers felt that insufficient data were given to check the claimed results. Nevertheless the evidence remained largely positive. If only the best 22 studies are considered 15 trials showed a positive effect as opposed to seven experiments that did not. Although a trend existed that the proportion of successful studies was lower for the better-quality experiments, the difference in proportions between high- and low-quality studies was not significant.

In the discussion the authors signal the unavoidable possibility of publication bias, especially with a controversial topic such as homoeopathy. Conventional and 'alternative' journals might tend to accept

or reject submissions based on outcome rather than on methodological criteria. But they noted no relationship between result of the study and place of publication. They did, however, identify six studies that were never submitted for publication, possibly because the results were negative. Probably because of the unproven and somewhat peculiar theory behind homoeopathy the result of the review came as a surprise to the reviewers. However, in their view if a treatment works knowledge of the mechanism of action is not necessary, and numerous examples from conventional medicine can be cited in which the mechanisms of the treatment are hardly understood or not at all understood. They also note that the influence of publication bias, data massage, bad methodology, and so on, which might have influenced their database, also happen in conventional medicine and that the financial interests of pharmaceutical industries are considerable and probably not a less strong motivation in conventional medical research than the homoeopaths might have in proving their belief to be right. However, they conclude that although the evidence is positive the low methodological quality of most studies prevents us from drawing a definite conclusion. They rightly argue that additional evidence is needed, not in the form of more low-quality studies but from running a few well performed controlled trials with large numbers of participants under rigorous double-blind conditions.

The latter comment in particular should be kept in mind in evaluating these results. It seems to me that sometimes in overviews there is a noticeable tendency to add up the results of a number of weak studies and then to attach the same value to the average outcome as to the outcome of a few high quality studies. Unless one is certain that the weak spots cannot have influenced the results, weak studies cannot be added up. In my opinion the results of many weak studies are more comparable to the results of a few weak studies than to the results of a few high quality studies.

It appears that the data from other types of complementary treatment as dis-

cussed above resemble more or less the pattern observed in the studies of paranormal healing. The average quality of the research is mediocre. In some studies an effect of the method could be demonstrated, in others not. Apparently the effectiveness of all these unorthodox treatments, expressed as a difference between experimental and control groups, is not very strong. In two of the studies discussed above the authors stated explicitly that substantial improvements in the control groups could be expected. Hence it seems safe to assume that also in these treatments the psychological effects associated with the patients are in general much stronger than the effect of the particular method itself. No indications were observed of the experimental groups doing worse than the control groups. Thus in these studies again no evidence was found for negative effects of the treatments on the health of patients.

Further Studies on Psychic Healing and on Other Types of Complementary Medicine

There are quite a number of (mainly sociological) studies that have contributed data relevant to the issue of the assessment of the value of psychic healing but that have not dealt exclusively with psychic healing; they also include data on other types of complementary medicine. Some of these studies do not even specify the types of complementary medicine but concern the use of complementary methods in general. I have included these studies because they yield data that enable the assessment of the possible agreements and differences between psychic healing, other types of non-orthodox medicine, and conventional medicine. Such data are of importance because they might offer indications of how to explain the effects of psychic healing.

I have tried to cover all studies that mention data on psychic healing. As regards studies on other types of complementary medicine I have included all those I was able to trace but probably more studies that are relevant can be found. How-

ever, the data that all the studies discussed offer seem to me to yield a fairly consistent picture. Therefore I do not consider it likely that additional studies would drastically change the conclusions derived from the material presented. All studies on traditional medicine from non-Western cultures have been excluded; not because they would be irrelevant but because of the limits to the size of this paper and because I do not think that conclusions from this study, which apply to Western societies, can be generalized to other cultures.

In a discussion of the problems associated with the scientific evaluation of unorthodox practitioners Aakster (1977) from The Netherlands mentions briefly a small study carried out under his supervision with patients suffering from chronic headaches. No figures are given. Two groups were to be compared, one following conventional treatment and one consulting complementary practitioners. It appeared, however, practically impossible to find two groups of comparable patients. Patients turning to alternative medicine were in many respects quite different from the patients consulting conventional practitioners. According to the author, the patients turning to complementary medicine appeared to suffer from more serious types of chronic headache than the others. Despite that, the complementary treatment seemed to result in distinctly better improvements, not only as regards the subjective experiences of the disease but also in a statistically significant reduction of suffering from various physical complaints. However, because the two groups were not really comparable the author considered the results as at best of suggestive value.

A survey of the use of unproven cancer remedies among patients visiting a Finnish university hospital was reported by Arkko et al. (1980). The group comprised 61 male and 90 female cancer patients. About 40% of the patients reported having confidence in the potential benefits of unorthodox remedies. That percentage appeared not to differ for males and females. An even higher percentage of patients (45%) had actually tried such

remedies. But here a large difference between males and females was observed: twice as many females tried unorthodox remedies as compared to males. The most frequently given reason (40%) for trying unorthodox remedies was that the user had heard or read about its beneficial effects. Ten per cent reported that they did so based on personal experience of the remedy having proven to be beneficial earlier.

Only 9% of the respondents reported positive effects of the unorthodox treatment on the cancer itself. However, 52% claimed that the treatment had a positive effect on their general state of health. Within the whole group, 66% of the patients had heard of cures effected by unproven cancer remedies, while 37% reported that they had heard of their use being unsuccessful. The majority of the patients felt that the attitude of the medical staff towards these remedies was not favourable and had not discussed such remedies with the medical staff. The use of these unproven therapies mostly took place simultaneously with conventional cancer treatment. Conventional cancer treatment can be a rather traumatic experience with no cure guaranteed and hence, according to the authors, it is not surprising that patients were tempted to use complementary remedies, most often advised by well-meaning friends or relatives, which are safe and non-toxic.

In Attevelt's PhD thesis on psychic healing, obtained in 1988 from the University of Utrecht, he reports, among other things, on two survey studies conducted at the beginning of the eighties (Attevelt, 1981; 1982; 1983). The two studies involved 65 Dutch practitioners of paranormal healing and 4,379 of their patients. Older and younger age groups were equally strongly represented in the patients with an average age of 47 years. Two thirds of the patients were female. The average duration of their complaints was seven years. The complaints were of a mixture of a chronic and non-chronic nature, 47% of the patients reporting that they had suffered for less than three years from their complaint. The most frequently reported complaints were musculoskeletal (45%) and about 50% of

the complaints concerned pain. On average, patients needed 18 treatments. Eighty per cent of the patients received 35 or fewer treatments. Forty-two per cent of the patients appeared to be strongly improved, 44% felt improved, and 14% of the patients indicated no change. Only about 1% of the patients felt that they had deteriorated during the period of receiving psychic treatment. In a follow-up study carried out half a year after treatment was terminated it was found that the reported improvement remained relatively stable over that period. Interestingly Attevelt found no differences in success/no-change proportions between the different types of complaints. Thus complaints of a supposed psychosomatic nature do not distinguish themselves by having a higher likelihood of being successfully treated compared to complaints of a different nature.

Bakker (1976) of The Netherlands collected data from 250 patients from ten different psychic healers. Two thirds of the patients were female. In 90% of the cases the general practitioner (GP) or specialist knew about the patient's complaints. Psychological disturbances were the most cited reason (35%) for consulting a healer, followed by musculoskeletal problems (22%). Most complaints were chronic; 42% of the patients had already suffered for more than five years from their complaint. Ten per cent of the patients no longer had contact with conventional medicine. Dissatisfaction with how they had been treated by conventional practitioners had not been the reason for turning to alternative healers because about 75% of the patients were of the opinion that the GP or specialist had spent sufficient time on their problems.

Forty-one per cent of the patients felt much improved due to the psychic treatment, 50% somewhat improved and 9% had not experienced any change. Only one patient reported deterioration. If all patients with less than seven treatments are excluded then 46% appeared to be much improved, 47% somewhat improved and 7% unchanged. Hence the degree of satisfaction did not change drastically with increased number of treatments. Also no

relationship was observed between 'benefit' and the patient's belief as regards the mechanisms of healing (a 'psychological' versus a 'paranormal' explanation). No difference in healing 'capacities' were observed between healers. A comparison of attitude towards GP and healer revealed that patients felt slightly more positive about the healer. However, in this respect these patients constituted a biased sample because they must have been not entirely satisfied with the conventional treatment, otherwise they would have not tried alternative treatment.

This study was repeated a few years later (Bakker, 1979). This time 281 patients from 22 healers were involved. The results were similar to those observed in the previous study. However, for unknown reasons in this study no benefit figures were presented. New data were that 32% of the patients had been hospitalized for the complaint for which the healer was consulted, that over 50% did not allow the investigator to contact the specialist (who of course did not know the patient was seeing a healer) and that three patients were rejected by a healer for treatment because the healer felt that healing would not work for them.

Bostrom and Rossner (1990) asked 233 Swedish medical clinics to send in reports on cases in which, according to the staff, alternative treatment had resulted in a delayed diagnosis and/or deterioration of the clinical condition. Alternative treatments also included diets and a stay in health resorts. The cases covered five years from 1984 to 1988. In total, 123 cases were received, an average per clinic of one case in ten years. In six cases death was attributed to the use of complementary medicine. The most serious consequences were noticed for cases in which the patient terminated medication and replaced it by some sort of alternative treatment. In several cases patients lost weight due to diets and, as a result, increased activity of the disease was noticed. No figures were presented on the total number of cases treated in these clinics in the five years covered by the study for comparison. The authors concluded that, although few, these cases rep-

resent avoidable outcomes of alternative treatment methods. According to them, alternative therapies can be harmful by preventing the application of scientifically proven medical regimes. The results of other studies support this view.

In the sixties in The Netherlands non-orthodox practitioners were still occasionally prosecuted. A rather famous legal case against a Dutch herbalist was the reason for Cassee (1970a, b) to study the question of who those people were who kept these illegal practices alive by letting themselves be treated by unqualified healers. He was not the first one. Ten years before, Van de Vall (1956, see below) did a similar study for the same reason.

Cassee studied a representative sample of 300 inhabitants of Utrecht, a middle-sized city in the centre of the country. Twelve per cent of the respondents answered that they had at least once in their life consulted either a psychic healer or a herbalist. The majority of the respondents had a positive attitude towards such healers. Therefore Cassee concludes that apparently in this respect the law is not in agreement with what the majority of the people feel.

In order to study why people turn to psychic healers Cassee obtained a sample of 290 patient registration cards from psychic healers associated with one of the foremost healers' associations. These cards contained information on the background of patients, the complaints of the patient and their diagnosis by a medical doctor as reported by the patient.

As regards the complaints, diseases of the locomotor apparatus (mainly rheumatism) were the most frequent complaint (30%) mentioned by users of complementary medicine, followed by psychological problems (20%). Cassee has the impression that the use of complementary medicine also often concerns complaints that are difficult to relate to some demonstrable affliction. From a comparison with national data on the use of conventional medicine it appears that mainly patients with chronic complaints turn to alternative healers. For patients, non-qualified healers fulfil a real

complementary function, the main reason for consulting non-qualified healers being that conventional medicine no longer seemed effective. Contrary to general expectation he also observed a positive correlation between patients consulting conventional medicine and consulting complementary practitioners. Most people appear to select doctors as well as complementary practitioners by a sort of 'lay-referral system'. Hence a positive correlation also exists between knowing someone who made use of complementary medicine and using it oneself. In the majority of the cases the patient is the one who decides to terminate the complementary treatment.

According to the healers themselves, 73% of these patients had improved and 15% were unchanged or still under treatment (the remaining 12% were classified as 'unknown'). From Cassee's survey of 300 inhabitants of Utrecht it turned out that about 75% of the respondents reported knowing someone who had made use of complementary medicine. Twenty-three per cent of these acquaintances reported a positive experience, 14% a negative experience and for 63% the experience with complementary medicine was a mixed bag, with both positive and negative aspects. Although the two studies are not directly comparable, the differences in percentages suggest that the healers are inclined to present too rosy a picture of the results of their activities.

Cassee concluded that it is especially the group of patients who feel that conventional medicine is not able to help them who keep the system of unorthodox medicine alive. As he formulated it, 'Without patients there would not be any healers' (Cassee, 1970a, p.408; 1970b). Therefore he expected the system of complementary medicine to continue to exist. Also in view of the finding that the majority of the people did not support the enforcement of the law that guarantees the monopoly of conventional medicine he argues that it would make better sense to give complementary medicine legal status.

Cassileth et al. (1991) reported a study comparing survival and quality of life

between two matched groups of 78 patients each receiving conventional and unconventional cancer therapy. Although of interest the study is not directly relevant to this overview, both because very few cancer patients make use of the main types of alternative treatment (such as homoeopathy, acupuncture and psychic healing) and because the non-orthodox treatment discussed is a rather specific one and especially aimed at treating cancer. Nevertheless I mention it because one surprising finding was that there were no statistically significant differences in survival either within or between the two groups of patients. It should be added though, that the patients turning to unconventional treatment methods often use these in addition to conventional therapies. However, quality-of-life scores were consistently better among conventionally treated patients. But this has clearly nothing to do with the type of treatment itself. From the data it appears that the difference between the two groups already existed at the moment the first data were collected and the size of the difference remains the same during the period of the study. This indicates that patients who on average feel worse as regards their state of health are more inclined to turn to non-conventional medicine.

Based on interviews with 150 patients who had been treated by alternative healers, Christie (1991) of Norway reported that nearly all the patients had consulted one or more conventional practitioners first. Alternative methods were mainly used by these patients as a supplement to conventional medicine. Many patients kept in touch with both alternative practitioners and conventional doctors. Research showed that between one fourth and one fifth of the population in Scandinavia have consulted an alternative practitioner at least once. However, these percentages are much higher for patients who have been seriously ill.

In the 'Medical News' section in the *British Medical Journal* of October 1986 (Editorial, *BMJ*, 1986), it is stated that in a survey conducted by *Which?* (a publication

of the British Consumers' Association) 82% of those who had tried complementary medicine claimed to have been cured or to have improved. Most people went to complementary practitioners for help with pain or a joint problem, 81% having sought advice from their general practitioner first.

In 1980 a Dutch magazine *Maatschappij en Gezondheidszorg (MGZ)* polled 200 district nurses on what they had learned from their clients about the clients' experiences with complementary medicine (Editorial *MGZ*, 1980). Nearly all reported that clients had asked for information about complementary medicine and had talked about their experiences with alternative practitioners. The majority of the nurses felt that it would be useful if more information on complementary medicine had been provided in their training. In 62% of the cases the client had judged favourably the contact with complementary medicine; in 12% the experience was a negative one.

The 'diplomarbeit' (MSc thesis) by Egeler and Wällisch (1987), students at the University of Freiburg, Germany, involved a pilot study of psychological variables associated with healing. Three healers and 62 patients, of whom two thirds were female, participated. Patients were investigated before, immediately after and four weeks after completion of treatment. Over 70% of the patients were older than 40 years. Complaints of a musculoskeletal nature were most frequently mentioned (32%), followed by psychological problems (20%). Immediately after completion of the treatment 70% reported improvement. Four weeks after termination of the treatment 60% still felt improved, 35% considered their condition basically unchanged and 5% felt further deteriorated.

The patients' mood increased significantly positively due to treatment and the patients reported an improvement in symptoms of the illness, which remained unchanged during the four weeks after the treatment ended. A multiple regression analysis yielded a number of psychological variables that correlated significantly positively with reported improvement. These included: experiencing sensations during

treatment; having expressed a less positive mood before the start of the treatment; having expectations of becoming healed; and rating oneself as being receptive to this type of treatment and number of treatments. The same analysis repeated for the data collected four weeks after termination of treatment revealed that expectation and the experience of subjective sensations still correlated significantly. Patients were not very good in their prediction of how successful the treatment would be; however, the healers appeared to do even worse and achieved a zero correlation. No correlations were observed between healer characteristics and improvement of patients. The results confirm to a large extent the findings of the Strauch study, also from the Freiburg area, despite the thirty years that lie between them.

A European Workshop on non-orthodox medicine was held in the summer of 1989 in Utrecht, The Netherlands. Contributions came from representatives of various European countries. A few of these papers are discussed here. Sermeus (1989) presented data from various studies carried out in Belgium. From a 1984 study in which a representative sample of families from the Flemish population was investigated it appeared that 39% of the families had used one or more forms of non-orthodox medicine. Homoeopathy was mentioned most often. Psychic healing accounted for 7% of the use of non-orthodox medicine, a clearly smaller percentage than that found with their neighbours, the Dutch. In most studies it was found that more females, middle-aged and better educated people used non-orthodox methods. However, a tendency was noted for patients from psychic healers to have a lower socio-economic background. In a study by van Hecke (1978) based on data from 100 patients from non-orthodox healers it was found that problems with the limbs, muscles and joints dominated, but problems of the stomach, abdomen and digestive system predominated with paranormal therapies. However, there was a marked absence of acute symptoms, infectious diseases, malignant tumors and confinements among the

patients studied. Approximately 73% of the patients had been suffering for more than one year from their complaints before they decided to try non-orthodox remedies. Eighty-five per cent had first consulted a GP and 56% a specialist first. According to the patients' subjective experiences the GPs had the lowest success rate, specialists scored slightly higher and the alternative practitioners scored highest. It should be noted, though, that these patients also constituted a very biased sample. They are the ones who turned to some form of unconventional medicine because conventional therapy did not help them as well as they would have preferred. Patients who are cured by conventional methods do not end up in the practice of alternative healers.

From another study it appeared that patients of alternative practitioners had on average more complaints than patients of specialists and a more negative image of their general state of health. They also appeared to suffer longer from their complaints than patients from specialists and had a longer history of treatment. As regards subjective satisfaction, GPs scored highest on the extreme positive score when complaints are considered that can be cured in one consultation. Specialists scored highest on extreme positive scores for symptoms requiring more than one session to cure. However, in both cases the non-orthodox practitioners scored best if the less extreme positive judgments are also included. Dissatisfaction with the results of the conventional treatment was the main reason for patients to try non-orthodox treatment. It is noteworthy that as regards homoeopathy and acupuncture the majority of the treatments were carried out by licensed physicians.

N.K. Rasmussen reported on the Danish situation. Based on a national interview survey with a sample size of 4,753 carried out in 1987, it turned out that 23% of the population had tried an alternative remedy at least once and 10% had done so in the year before the survey was held. However, the distribution over the various types of alternative remedies was quite different from those found in Belgium and

Holland. The use of 'zone therapy' was highest (42%), followed by 'natural medicine' including homoeopathy with 36%. Acupuncture was used by 15%. Psychic healing was not mentioned and hence probably included in the 'other' category, which contained all remedies with low frequencies of occurrence. About twice as many females used alternative remedies, and most patients suffered from either migraine or back disorders. No major social differences were observed between users and non-users.

Long-term illness had a significant influence on frequency of physician contact and use of medicines and also a significant influence, but to a lesser degree, on use of alternative medicine. If people who had and who did not have contact with conventional medicine in the previous year were compared on use of alternative medicine it appeared that people who had contact with physicians included a significantly higher proportion of users of alternative medicine. Alternative treatment clearly served a supplementary function. A specific illness was most frequently mentioned as the reason for using non-conventional remedies, rather than dissatisfaction with or criticism of conventional medicine. The author concluded that the Danish population considers complementary medicine as a group of natural and legitimate treatment methods and that it is mainly the established professional groups, especially the physicians, who perceive non-orthodox medicine as a real alternative that they have to compete with.

According to Tuula Vaskilampi in Finland, a distinction can be made between complementary medicine in Finnish traditional medicine and in more modern forms of alternative healing. Of the latter, homoeopathy and psychic healing are mentioned. From a 1982 survey ($n=1,613$) it appeared that 27% of the population had used a form of modern alternative healing and 14% a traditional Finnish folk remedy. The author assumes, however, that these figures represent an underestimate. No figures are presented on psychic healing specifically, but it is stated that particularly spiritual healing

and different other 'mixture' forms of therapies were on the increase. The majority of the population (60%) had used only conventional medicine and only 1% had used only an alternative remedy. Hence nearly all users of non-orthodox treatments had also made use of conventional methods. Women used alternative therapies more often than men. Traditional folk healing was prevalent in the rural areas of the country whereas modern alternative methods attracted relatively younger urban people. The author stated that, according to different surveys, 60% to 80% expressed satisfaction with alternative remedies. However, most of these patients expressed satisfaction with conventional medicine as well.

Francoise Bouchayer from France reported that, according to a 1987 survey among GPs, 46% of the GPs declared that they applied one or several alternative remedies. The main source of information about the use of alternative medicine is a survey carried out in 1985 of a representative sample of 1,000 persons. About 50% of them responded that they had used alternative methods at least once. The most widely used method was homoeopathy (32%), followed by acupuncture (21%). Psychic healing was not mentioned. People using alternative methods appeared to have a higher socio-economic status. A preponderance of female users was observed. As regards effectiveness, 70% of the users considered alternative medicine relatively effective for minor ailments (such as general fatigue or headaches), 65% for chronic complaints and only 9% for serious complaints such as cancer or cardiac problems. Conversely, 11% considered alternative remedies as not effective for minor complaints, 15% as not effective for chronic symptoms and 38% as not effective for serious complaints.

The other reports from this workshop are not discussed here because they either did not contain data or reported on studies which are already discussed separately in the present paper.

The results of a survey on satisfaction with treatment by complementary healers was published by Fleuren et al. of The

Netherlands in 1990. The aim of the study was not only to assess satisfaction but also to determine which factors contribute to experienced satisfaction. Data were obtained from 227 patients who had consulted an alternative healer within the previous five years. In addition to background data various variables were measured, including general state of health and external health locus of control. Satisfaction was measured by means of a six-point scale, derived from a 19-item scale that was subjected to factor analysis. Satisfaction was measured for 20 aspects of complementary treatment, for instance, information given by the healer, the way the patient was treated, and the results of the treatment.

A preponderance of females (167) was observed. The educational level of the respondents was above average. The most often-mentioned complaints were psychological distress, pain in the joints and digestive troubles, mainly of a chronic nature. The majority of the respondents scored low on external health locus of control. This implies that they felt themselves to be responsible for their own health. On the whole the respondents had high expectations of the complementary treatment and they still had a relatively high degree of appreciation for it. The appreciation for conventional medicine appeared significantly lower. Three quarters of the respondents had turned to complementary medicine because they felt that conventional treatment had not resulted in any progress.

Respondents showed a fairly high degree of satisfaction on all 20 aspects. The percentages of positive appraisal varied from 74% to 99%. Eighty-one per cent indicated satisfaction with the results of the treatment. On the whole, little discrepancy was observed between respondents' expectations and the results of the contact with complementary medicine. Three variables in particular appeared to influence the degree of satisfaction: the discrepancy between expectations and results, general state of health and the appreciation of complementary medicine in general. These variables accounted for no less than 72% of the variance in the variable 'satisfaction'.

The use of non-conventional medicine in the UK was investigated by Fulder and Munro (1985). Practitioners of complementary medicine from different areas in the UK were sent a questionnaire, which was returned by 136 (a response rate of about 50%). The findings did not differ much from comparable figures from The Netherlands. On average there were about 12 practitioners per 100,000 of population, which is about 27% of the number of GPs. About 2.5% of the population received treatment by non-orthodox practitioners each year. The average number of treatments was 9.7 for each patient. The number of patients an alternative healer saw in a week was less than half the number of patients for general practitioners. Not surprisingly it appeared also that the consultation time for non-orthodox practitioners was considerably longer than those of GPs. Two thirds of the patients were women, and patients were drawn largely from the upper social classes. According to the authors, these results are not compatible with the picture of gullible patients unable to understand the medical possibilities or to make discriminating choices.

A study aimed at investigating different beliefs about health and illness in patients visiting a GP and patients visiting a homoeopath was carried out by Furnham and Smith (1988). Two relatively small groups of patients (45 GP patients and 42 patients of a homoeopath) who did not significantly differ in terms of sex, age, education, marital status, religion and income, were asked to complete a questionnaire concerning perceived susceptibility to disease and illness; beliefs concerning their own control over their health; their mental health; preventive measures that they took for staying healthy; and perceived efficacy of traditional versus alternative medicine. The two groups appeared not to differ much on any of the dependent variables measured. The main difference observed was that the homoeopathy patients seemed more critical and sceptical of conventional medicine and medical care and services in general. In fact, 81% replied that the reason they had turned to homoe-

opathy was dissatisfaction with conventional medicine.

The homoeopathy patients also seemed to be psychologically more disturbed but according to the authors that might well be explained by the fact that they were on the whole sicker and had suffered longer from their complaints. And contrary to general expectation, it was rather the patients of the general practitioner who seemed to have 'blind faith' in the efficacy of the treatment and the (GP) healer. The homoeopathy patients also appeared much more sceptical in this respect towards the therapy they preferred. The authors consider this an indication that homoeopathic treatment cannot be considered simply a placebo treatment.

Glik (1986) of the USA studied psychosocial wellness among participants of spiritual healing groups. Data from two such groups comprising in total 176 people were compared with 137 other utilizers of conventional medicine. These data are not typical for the usual methods of spiritual healers because patients were primarily involved in group activities and not individually treated. However, it is of interest to note that these activities as such appeared to contribute to a higher degree of well-being. Although the participants of healer groups and medical patients did not differ in physical health problems, and despite contrary indications such as a greater proportion of unmarried persons, lower average income, lower average education and more elderly members, the healing group participants had consistently higher scores on measures of psychosocial wellness. The authors attribute this partly to the assumption that the healing group activities both reinforced a belief system enhancing the individual's feeling of self-worth and suggested the possibility of 'healing'. Such activities are reminiscent of a practice frequently found in spiritual and psychic healing, that is, that patients are treated in the presence of a group of other patients.

Based on follow-up interviews (Glik, 1990) with the participants of these spiritual healing groups, Glik discovered a

peculiar process. For the first study respondents had described their health problems, often more than one. The most common types of somatic complaint were backache, chronic pain, arthritis, hypertension and headache. Next to somatic problems came psychological problems including symptoms of depression, anxiety, fear or anger. Respondents were asked at second and third interviews about the overall effect of healing. Only 8% indicated 'not healed at all'; the others all claimed a certain degree of improvement with 20% indicating 'complete recovery'. However, based on the answer to the question of what problems were cured or not cured some strong discrepancies were noted between the problems listed in the first interview and the problems that had supposedly been cured. Based on further analyses it was found that actually no associations were found between changes in health status and the degree of healing claimed. In fact, there was little change in the health status of respondents generally.

From content analyses of the answers it became apparent that healing was often described as a process of symptom alleviation, relief from distress, or an acceptance of some health or life situation. Glik concludes that there existed a marked trend for symptoms and problems to become 'redefined' over time to provide a better 'fit' with outcome descriptions of healing than the original problem formulations would allow. Redefined health problems were often less serious, less medical, more chronic and more 'emergent' than those initially defined, and respondents who redefined problems were significantly more likely to claim a healing experience. Thus according to Glik, in the context of spiritual healing groups of belief systems, healing events can be considered social productions or socially constructed events.

At the request of the Cystic Fibrosis Foundation of The Netherlands a study was carried out by Groen (1982) among families with children who had cystic fibrosis into the use of non-orthodox medicine. Of the 540 mailed questionnaires, 278 were returned. The average age of the 365

cystic fibrosis children of these families was 12 years. Homoeopathy and psychic healing were the best known types of complementary medicine in these families. Over half of the respondents had considered unconventional treatment for their children but only half of these (26%) had actually taken the step and had taken their child to an alternative healer. In 29% of the families one or both of the parents had once consulted an alternative healer for themselves. This percentage is higher than found in comparable studies. Therefore it is concluded that families with cystic fibrosis patients consult non-orthodox practitioners comparatively more often.

Psychic healing accounted for 23% of all non-orthodox treatment tried by the families. The average number of non-orthodox treatments was seven. Nearly all of the children also remained under mostly specialist treatment or supervision within conventional medicine during the period of non-conventional treatment. The fact that non-orthodox treatment took place did not affect the frequency of visits to the specialist. Hence the respondents clearly considered the non-orthodox treatment as complementary, and not as an alternative to conventional medicine. Groen also investigated the motivation of the respondents to consult or not to consult a non-orthodox healer. The non-users stated as their main reasons that they had confidence in conventional medicine and had no faith in or did not know about non-orthodox methods. The users of complementary medicine stated as main reasons in 52% of cases a pragmatic reason (such as: 'if it does not help it does not hurt either') and in 41% of cases the reason of conventional treatment not working. A significant finding was that those families who turned to complementary medicine were not less satisfied with the way the specialist handled their case than those who did not.

If the degree of satisfaction with conventional and unorthodox treatment is compared for those families who used both methods concurrently it turns out that respondents were significantly more satisfied with conventional treatment (77%)

than with unorthodox treatment (50%). Twenty per cent were dissatisfied with unorthodox treatment, as compared with 8% who were dissatisfied with conventional treatment. This dissatisfaction with unorthodox treatment seemed partly due to parents having expectations of it that were too high. The degree of satisfaction appeared higher when the alternative healer gave him- or herself a more supportive role. Also, parents with, on average, older children were significantly less satisfied with the effects of the alternative treatment. The author concluded that alternative treatment can offer a contribution to cystic fibrosis but of limited significance and mainly of a complementary and not of an alternative nature.

In 1958 an investigation was carried out in the practice of a Dutch healer (Haas, 1960; Tuyter, 1960). All patients who registered for consultation were first examined by a doctor before being admitted to the healer. The main aim of the study was to check on the healer's accuracy in making a diagnosis. Those data will not be discussed here (but apparently the physicians were occasionally rather impressed; in a number of cases the healer correctly diagnosed problems that had escaped the attention of the examining physician). Two thirds of the patients were female. In 98% of the cases the patient had first consulted or was still consulting a conventional practitioner. The main reason the patients gave for consulting the healer was that conventional medicine appeared unhelpful. According to the medical examination, 14.5% of the patients would have been endangered if they had not also consulted conventional medicine and accepted conventional treatment. It is unclear whether this group included any of the (2%) of the patients who went directly to this healer without consulting a general practitioner or specialist. Also the physicians noted that they had the impression that a number of patients (no figure given) had turned to the healer to escape a conventional treatment prescribed to them.

In his book *The Power to Heal* David Harvey (1985) writes that despite the numerous accounts written about healers

there has been little systematic examination of the phenomenon. Therefore he conducted a survey in Britain with patients of healers. Respondents were asked to give their own evaluation of their state of health before and after treatment. The aims of the study were to ascertain whether people could confirm any benefit from treatment and to examine the circumstances under which such improvement occurred. Nine healers were asked to supply the names of about 20 patients whom they believed had shown a positive response to the treatment. These patients were sent questionnaires and 151 usable ones (a response rate of 81%) were returned. The majority of these people had nothing but praise for the healers they had seen, but that is not really surprising in view of the fact that the healers themselves provided the names.

Although the sample is clearly biased the results are worth discussing because the data can in many respects be considered as yielding upper values. Thus the 30% found for the number of patients claiming complete recovery in this study can be considered an upper limit in view of the way the sample was chosen. Only four of the patients declared that they had not experienced any improvement. About 75% of the respondents consulted a healer because of failure of medical or other therapies to produce improvement. The others came for pragmatic reasons ('nothing to lose by trying') or because the healer was recommended by someone who had been successfully treated.

Ninety-six per cent of the respondents said that they were aware of some response during the healing session. The most commonly reported sensation was warmth (74%) followed by a feeling of relaxation and calmness. Respondents listed 41 kinds of health problem that led them to consult an alternative healer. The most frequently mentioned were back pain and arthritis, followed by migraines and headaches. Healing was apparently a slow process because 41% needed more than 20 treatment sessions. Forty-five per cent indicated that they believed from the beginning onwards that healing would help them; the

rest were either uncertain or sceptical. Not surprisingly two thirds of the respondents declared that their experience had strengthened their belief in the reality of paranormal healing. Women constituted two thirds of the sample. Also about two thirds of the respondents had not informed their doctor that they had consulted an alternative healer.

Heye et al. (1980) from Belgium interviewed 70 patients, suffering from serious arthritis and who were either hospitalized or under the supervision of a university hospital, about their use of complementary medicine. In contrast to the Harvey study described above this study can be considered as yielding lower limit values. These patients were still in a serious condition and most had a progressively deteriorating illness history. Two thirds of this group had already suffered for more than five years from their affliction, and one third for even more than 15 years. Hence this sample was strongly biased in that these patients had not reacted well to any previous treatment, conventional or complementary. It should be added that according to the authors, who specialize in this illness, medication can delay the illness, but recovery is unlikely.

Seventy-five per cent appeared to have tried methods other than conventional ones, but most of these were some remedy of their own. If only the familiar types of complementary medicine are considered, psychic healing ranked first with 32%. Most patients (55%) had turned to complementary medicine in the first three years of their illness. The use of complementary methods seemed to decrease with the increase of the duration of the illness. As to the benefit of the complementary methods, the descriptions are somewhat unclear. Only for acupuncture, homoeopathy and psychic healing were results mentioned. For acupuncture three of the 14 patients were described as being satisfied. In the case of homoeopathy one patient out of seven was positive, and as regards psychic healing it is stated that nearly all 27 patients experience no effect or at the beginning only a temporarily positive effect. Only

four patients reported a more lasting improvement. These data suggest a lower limit for the benefit rate of complementary medicine of eight out of 48, or about 17%.

Non-prescribed treatments in rheumatic diseases were also studied by Higham et al. (1983). A pharmacist interviewed 158 patients receiving treatment in a hospital rheumatological clinic. The 49 male and 109 female patients were aged 20 to 83 years and had a mean duration of the disease of 11 years. All except one had at least once tried non-prescribed remedies. However, most of these were self-prescribed remedies such as liniments, heat treatments, etc. Only 13% involved consulting alternative practitioners. Of the 26 patients who consulted an alternative practitioner, 13 (50%) rated the treatment as either very or somewhat helpful, but some patients also complained that they felt worse after treatment from practitioners of alternative medicine. A sobering thought, though, is that the simple hot-water bottle was rated by far the most effective self-prescribed remedy; not less than 40% of the patients classified that remedy as 'very helpful'. Patients rarely reported negative side effects from their self-prescribed treatments.

In a small survey study Hoekstra (1988) investigated possible negative effects of alternative medicine by questioning GPs in the north of Holland. Of the 262 GPs who received a questionnaire, 120 responded. Of these, 80 GPs confirmed that in the past one or more of their patients had turned to non-orthodox medical treatment. However, most of the GPs believed that in less than half of such cases would the patient inform them about it. About half of the GPs reported that they had had one or more experiences in which the alternative healer had in some way not behaved properly. The most frequently cited complaint was that the alternative healer or patient did not inform the GP. Hoekstra described the 36 most negative experiences with alternative healers that the GPs reported. From these descriptions the most serious cases nearly all involved advice or activities by the healer that turned out to be

damaging because they resulted in the patient not consulting or not informing his or her physician.

The use of complementary remedies in rheumatology patients was also studied by Kronenfeld and Wasner (1982) in Alabama, USA. Of 98 patients visiting the rheumatology group of a university medical centre nearly all, 94%, reported that they had at least once used an unorthodox treatment. However, disease duration appeared to be on average about 11 years, with a very large range from one to 50 years. Most patients had tried more than one remedy, with an average of 3.7. Over 80% of the respondents had been seeing a physician for more than a year before they tried a non-orthodox remedy. A detailed analysis of the data did not show a relationship between age, sex, race, duration of the disease, education and income and the frequency of use of unorthodox treatments. Unfortunately, despite the fact that all respondents were individually interviewed apparently no data on benefit were collected. The authors concluded that usage of unconventional remedies was much more widespread than was believed and that the notion that mainly people with low income, low education and from a different cultural background use unproven remedies is inaccurate.

Nicolauda Louwerens (1978) studied the characteristics of the patients of a well-known Dutch healer. She took a random sample of 221 people from 2,310 who had consulted the healer in the previous two years. All respondents who agreed to participate, 180 in all, were individually interviewed. In all cases at least one year had passed between the first meeting of the patient with the healer and the time of the interview. In addition data from a further 732 patients were obtained by mailing them a questionnaire.

Two thirds of the patients were female and four fifths in the age bracket of 40-59 years. A remarkable finding was that about 60% of the patients, who all had to pay themselves for their treatment, were covered by the National Health Service and hence could obtain conventional medical

service free of charge. Musculoskeletal and psychological complaints were most frequently mentioned. Forty per cent of the patients stated that they had improved due to the treatment, the condition of 56% did not change and about 4% reported deterioration. According to Louwerens a significant correlation was observed between improvement due to treatment and complaints of a psychological nature. A similar positive correlation was observed between duration of the treatment and success. About half of the patients reported that they had felt subjective sensations during treatment, most frequently one of becoming warm. Apparently most patients were so favourably impressed by the healer that 54% told the interviewer that they had spontaneously recommended this healer to acquaintances. Fifty per cent of the patients said that they started out having faith in the healer's powers, but the other half was either rather pragmatic ('if it does not help, it does not hurt') or downright sceptical. Although no figures are presented, it is stated that the latter attitude did not prevent successful treatment.

Only 5% of the patients had turned directly to the alternative healer without first consulting a conventional general practitioner or specialist. Nearly all patients appeared to have consulted the healer concurrently with taking conventional treatment. It is noteworthy that quite a number of patients who were seeing a specialist turned to this healer, who was also famous for his abilities to diagnose illnesses 'paranormally', in order to find out what was wrong with them; a clear indicator of the occasional lack of communication between specialists and patients in those days (the study was carried out around 1960). However, that situation seems clearly improved. From more recent studies we know that dissatisfaction with the way conventional medicine treats its patients is not a reason to turn to complementary medicine. About half of the patients felt that a conventional physician would be more effective in treatment than the psychic healer; the others felt either that they were equally effective or that the two

(conventional and complementary) were so different that they were not really comparable. Two thirds of the patients had suffered from their complaints for more than five years.

Funded by a TV company preparing a production on complementary medicine, Limberger et al. (1981) conducted a survey study and obtained data from 3,782 respondents, a sample representative of the Dutch population. It turned out that 6.9% of the sample had consulted an alternative practitioner in the year before the investigation and 436 (11.5%) respondents indicated that they had done so within the last five years. Of these 436 respondents, 26% had consulted a psychic healer. The average amount of money spent on consultations was lowest for psychic healing, despite the fact that the average number of consultations per year (8.1) for psychic healing was above the average (7.1) for all types of complementary medicine.

Based on these data a follow-up study was carried out in which 216 respondents who in the previous year had used complementary medicine were compared with respondents who in the same period had consulted a specialist, with respondents who in the same period had consulted a GP, and with respondents who had not consulted any practitioner. It appeared that especially as regards age (which was correlated with state of health) the users of complementary medicine were most comparable to the group of respondents who had consulted a specialist.

Of special interest for the present study was that the authors looked for differences between the various types of complementary medicine and found none, except that patients from chiropractors had, not surprisingly, a higher score on being physically handicapped. But as regards type of complaint, degree of improvement (on average 77% indicated that they had been somewhat or substantially improved), and relationship between type of complaint and degree of improvement, no differences at all were observed. Also no difference was observed between practitioners of complementary medicine with (55%) and without

a conventional medical degree. About half of the respondents who had used complementary medicine indicated that frustration with conventional medicine had played a role in their motivation to do so. But the most often cited reason was a purely pragmatic one; they wanted to be healed. Seventy per cent of these patients had suffered for more than a year from their complaints, and 40% for even more than five years.

Most studies on non-orthodox medicine obtain their data from patients. Many non-orthodox healers are affiliated to a professional organization in the field in which they practice. Maassen van den Brink and Vorst (1988) polled all such organizations in The Netherlands (at that time 35 in total) to obtain data on various aspects of the use of alternative healing. Six main types of non-orthodox medicine could be distinguished: natural health, psychic healing, homoeopathy, anthroposophical treatment, acupuncture and manual healing. The latter, however, involved mainly physiotherapists.

This study is of interest because it provides some data on how psychic healing in Holland compares with the other main types of non-orthodox medicine. Of the healers applying non-orthodox methods, 36% appeared to have had medical university training, 43% (including the physiotherapists) had had another type of conventional medical training, and 36% were healers with no conventional medical training. Hence the popular picture of the alternative healer as an untrained, uneducated primitive person does not fit the data. It should be added, though, that only in the case of psychic healing did all healers appear to have no conventional medical training. Clearly psychic healing has a low social status compared with the other main types of non-orthodox healing. Of all practitioners without conventional medical training, 22% apply psychic healing.

Moore et al. (1985) carried out a study to evaluate the characteristics of patients seeking treatment by complementary medicine. A questionnaire was given to 65 patients who visited a centre for alternative

therapy for the first time. Eight weeks after the first interview a follow-up questionnaire was issued. The largest group of patients was female, married and in the age group of 26 to 50 years. Patients came primarily for chronic complaints with a mean of duration of symptoms of nine years. Pain accounted for 45% of the complaints. Only three patients had bypassed conventional medicine; the others had seen either a general practitioner or a specialist.

Fifty-four patients stated that the failure of conventional medicine was their reason for turning to alternative medicine. Most of these people had a good relationship with their general practitioner and thought that they had received satisfactory treatment from conventional doctors. Two thirds of the patients believed that alternative methods worked. After eight weeks, 59% of the patients felt improved and a significantly lower mean depression score and significant decrease in pain were measured. Positive expectations appeared to be correlated with the degree of the effect of the treatment.

In 1979 the NWP, a Dutch society for complementary practitioners that also included psychic healers among its members, asked a bureau of attitude research to conduct an investigation into the attitudes of people towards complementary medicine (NWP, 1980). In a representative sample of 1,030 people, each person was orally interviewed at home. The study presents many details, for instance on political background of users of complementary medicine, which I will not discuss here. Only the data of interest for the present overview are mentioned.

Sixty per cent of the respondents had a positive attitude towards complementary medicine, 30% were neutral and only 10% were against complementary medicine. Better-educated respondents had on average a more positive attitude. Twenty per cent of the respondents had at least once consulted an alternative practitioner, either for themselves or on behalf of their children. About 46% of the respondents who had turned to complementary medicine did so because they felt conventional medicine could not

help them. Eighty per cent had consulted a physician either before or concurrently, but 20% had turned directly to complementary medicine. (It should be noted that the NWP required their members to ensure that their patients also consulted conventional medical practitioners concurrently). Of those who had turned to complementary medicine 68% reported having benefitted from it, 25% reported 'no change' and 4% could not yet give a judgement. The 'benefit' percentage did not differ between the group of respondents who felt conventional medicine could no longer help them and the respondents who gave other reasons for their decision to turn to complementary medicine. Over 80% of the respondents felt that people should have the right to decide for themselves what type of medical care they wanted.

In Poland, Pawlicki et al. (1991), staff of a cancer clinic in Krakow, investigated the negative effects on treatment for cancer due to possible delay in onset of conventional treatment when patients seek complementary, unconventional treatment. They stated that according to their own data, in general more than 60% of cancer patients were delayed as regards the commencement of treatment. The delay due to the patient was on average 13 months, but delay was also caused by physicians. They caused a delay of seven months on average. Pawlicki et al. became interested in the question of to what degree the increase of use of unconventional medicine contributed to such delays.

They asked 433 patients who were treated in their clinic to fill out a questionnaire on causes for delay, duration of delay, and type of complementary medicine the patient might have tried. About 25% of the patients (110) indicated that they had seen an alternative practitioner. According to the authors, of these 110 patients 52 had a delay in conventional treatment. The average duration of the delay for these patients turned out to be about 5.5 months. They concluded that about 25% of the patients had a reduced chance of healing from conventional medicine.

The authors are certainly justified in pointing out the risks when patients who turn to alternative practitioners do not consult conventional medicine concurrently. But the way they use their data to support this view seems somewhat artificial. They apparently only asked the respondents for details of delay in treatment and for use of alternative methods. Then all delay observed was attributed to the use of alternative medicine. Because, according to their own data, over 60% of the patients had delays for on average much longer periods it is clear that many other causes contributed to these delays, possibly also in the cases of the 52 patients who turned to alternative practitioners. Secondly, no delay data are provided for the 223 patients who did not deviate from conventional treatment. This group would have been a more appropriate group for comparison than using 'no delay' as the criterion. As has been found in other studies, often alternative healers actually encourage patients to seek conventional treatment. If that also happens in Poland the patients using alternative medicine might even have lower delay periods than the others. Thirdly, the final conclusion, that 25% of the patients had been put at risk, is also somewhat misleading. At best, assuming that all other patients had had no delays, this could have been 52 of the 433 patients, i.e. about 12%.

Seventy-eight women patients suffering from rheumatoid arthritis and treated in a British Regional Rheumatic Diseases Unit were interviewed by Pullar et al. (1982) about their expenditure on, and benefit derived from, alternative medicine. These figures were compared with expenditures on aids and on conventional treatment. This study is especially of interest because it is one of the few in which figures are reported for the same group of patients for experienced benefit of both conventional and non-orthodox treatments. Forty-seven patients reported having paid for some form of alternative medicine in the year before the interview. In that year most money was spent on aids for the home, not surprising considering the type of complaint, and all patients reported that they

had benefited a lot from those investments. The second largest amount was spent on conventional medicine; 23% of the patients reported no benefit from this, 42% derived a little and 36% a lot of benefit from that money spent.

The authors were surprised at how little money (12% of total expenditure) had been spent on alternative medicine. Of these treatments (many patients had tried various forms of non-orthodox medicine) in about half of the cases (53%) no benefit was derived; in the other half the patients reported a little (36%) or a lot (11%) of improvement. Thus in this study not surprisingly conventional medicine proved to be the most effective, but perhaps not such a big winner as most would expect (a 78% benefit rate for conventional medicine versus 47% for non-orthodox treatment). Only in one case did non-orthodox treatment result in deterioration. The authors concluded that expenditure on alternative medicine was low and that only a small proportion of the patients found it helpful. However, the latter part of the conclusion seems somewhat strongly expressed in view of the figures they reported.

The Dutch Bureau of Statistics, a government agency, provides annually all sorts of statistical data on the Dutch population, including on health service consumption. In some of their investigations they included questions on the use of complementary medicine. These data are valuable because they are based on fairly large samples of the population and generate a lot of background information on the respondents which allows a direct comparison between various aspects of the use of conventional and alternative medicine. Van Sonsbeek (1983) presented the data from two such nationwide investigations in which questions on the use of complementary medicine were included, one from 1979 (7,247 respondents) and one from 1981 (7,239 respondents). In 1979 2.3% of the population consulted one or more alternative healers (not including physicians who apply alternative methods); in 1981 that figure had increased to 3.8%. From the 1981 data it appears that if physicians who

applied alternative methods are included, the percentage rises to 6.4%. This implies that in 1981 about 60% of the consumption of complementary medicine was provided by practitioners without a conventional medical degree; in 40% it was mostly a GP who applied complementary methods. For over 70% the alternative practitioners applied homoeopathy, acupuncture, natural methods or psychic healing. In 1979 and 1981, psychic healing accounted for 17% and 18% respectively of total alternative consumption. The average number of consultations per year was about 5.7 for alternative practitioners and 4.6 for GPs. Fourteen per cent and 15%, in 1979 and 1981 respectively, or respondents who turned to complementary medicine consulted more than one alternative practitioner in the year of investigation. Patients consulting alternative practitioners were 60% female, middle-aged people (with a strong decrease in the > 60 years group), comparatively well-educated, and suffering from chronic complaints of generally bad health.

However, the value of this investigation is the chance it provides to compare users of unconventional medicine with patients who did not turn to complementary medicine. Thus van Sonsbeek ran a multivariate analysis to study the correlation between the characteristics of respondents' backgrounds and whether they had consulted alternative practitioners. From these analyses it appears that of the above mentioned variables (age, gender, education, whether patients used the National Health Service or were privately insured, general state of health and chronic affliction) nearly all correlations disappear. Only 'general state of health' correlates somewhat with the tendency to turn to complementary medicine. Van Sonsbeek concluded that on the basis of background characteristics it is hardly possible to predict which patients will use complementary medicine.

From the raw data it appears that patients who consulted alternative practitioners also had on average more contact with practitioners of conventional medicine. However, this correlation can also be

fully explained by the worse average state of health that these patients had. The finding is important, though, because it tells us again that on the whole patients consider complementary medicine as really complementary, and not as a replacement for conventional medicine. This implies that the argument sometimes offered for accepting complementary medicine, that is, that it would create a cheaper health care system, is probably incorrect.

From similar survey data obtained in 1985, 1986 and 1987 (van Sonsbeek, 1988) it appears that the use of complementary medicine is on the increase. Sample sizes were about 9,000 each. The percentage of the population consulting a complementary healer increased to 4.5% in 1985, and further increased to 5.2% in 1987. For unknown reasons the contribution from psychic healing also increased, from 18% in 1981 to 27% in 1985, after which year the percentage does not change. Hence psychic healing accounts for over a quarter of the total use of complementary medicine in the Netherlands. Other findings also confirm those from the earlier surveys. In the three survey years the main complaints for which patients consulted complementary medicine concerned back-pains and other musculoskeletal problems (32%), followed by psychological problems (13%) and headaches (9%). The most cited reasons for turning to an un-orthodox practitioner was that conventional medicine did not work, followed by distrust of conventional medication. Again a positive correlation was observed between conventional and complementary medical consumption. No indications were found that users of complementary medicine turned away from conventional medicine. As before, a preponderance of patients who were female, more highly educated, mainly between 30 and 60 years of age and with chronic complaints appeared to turn to unconventional medicine. However, as seen from the results of the van Sonsbeek studies, these characteristics are not typical for the use of complementary treatments but are associated with the general medical consumption of these groups.

A large postal survey of non-orthodox health care practitioners and their patients was undertaken by Thomas et al. (1991) in Great Britain. Here again the objective of the study was to describe the characteristics of patients seeking alternative treatment. Over 2,000 qualified practitioners (that is, practitioners who belonged to a national professional organization) of complementary medicine (including acupuncture, naturopathy, and homoeopathy) and a random sample of 2,473 of their patients were the subject of this investigation. The majority of the patients, 78%, had musculoskeletal complaints. Two thirds of the patients were women, but that figure did not deviate from the known percentage of women consulting general practitioners. Both the lowest (16-24) and the highest (> 65) age groups were under-represented in this group of patients. Two thirds of the new patients of unconventional healers claimed never to have used any non-orthodox treatment previously.

The majority of the patients reported having received conventional treatment for their main problem, but a fairly large proportion (36%) had not. However, this did not preclude their having sought the advice of their general practitioner before seeking non-orthodox care, because 18% of this group had seen their general practitioner less than two weeks before they turned to a non-orthodox practitioner. A strong association was observed between the type of problem and the use of the two systems of health care. In particular, patients reporting atopic conditions, headaches or arthritis were likely to combine non-orthodox treatment with previous and concurrent conventional treatment.

In 1957 in The Netherlands, complementary medicine was illegal and practitioners of non-orthodox medicine were occasionally prosecuted. Of course, this 'evil' continued to exist because it still attracted clients. Hence Van de Vall (1956) became interested in the question of what kind of people turned to alternative healers and why they did so. He studied particularly interest in psychic healers and herbal medicine. A representative sample of 340

families in the city of Utrecht was drawn and all families were orally interviewed. Over 60% of the respondents appeared familiar with the concept of psychic healing. Not surprisingly the better educated respondents also appeared to be better informed about this subject. About 40% of the respondents knew at least one person who had ever consulted a psychic healer and 24% even knew one or more addresses of psychic healers (again, this was illegal at the time). However, it also appeared that about half of the respondents were not aware of the fact that psychic healing was illegal. Only 19% agreed that psychic healing was illegal.

Fourteen per cent of the respondents admitted that someone in their family had consulted a psychic healer. Except for one respondent all declared that they would first consult a conventional physician before turning to complementary medicine. Based on further questions the author concluded that the chronic duration and nature of the complaints were the main reasons for turning to complementary medicine. As to the efficacy of the treatment by psychic healing, 54% of the those who had consulted a healer said that they had benefited from the treatment and 39% indicated 'no change'. The remaining 7% were not able to answer this question. No clear characteristics were found that would discriminate between the 'users' and 'non-users' of complementary medicine.

Visser (1988) of The Netherlands concluded from a survey of 360 general practitioners as respondents that 47% of the general practitioners occasionally applied one or more unorthodox treatments. This consisted most often of prescribing homoeopathic medication. But contacts with complementary practitioners without a medical degree were practically non-existent and the general practitioners clearly viewed these remedies as purely complementary.

In addition 8287 patients from 14 GPs were approached (Visser, 1989) and asked whether they ever visited an alternative healer. Of the 4,642 who responded 11.7% confirmed this and had done so in the previous year. Approximately twice as many

women as males went to a complementary practitioner. Psychic healing accounted for 23% of these visits when the two year period is considered and for 12% when only the last year is taken.

Next a group of 357 patients who had tried complementary remedies (the 'users') were compared with a group of 414 patients who had not (the 'non-users'). According to the patients' estimates, general state of health was slightly better for the non-users. Users much more frequently reported suffering from a chronic ailment (79% versus 51%). Little difference between groups was observed as regards satisfaction with their GP. Differences were observed between the percentages of users who judged positively the complementary remedy they had tried. Homoeopathy and acupuncture scored highest (78% and 72% respectively), psychic healing lowest (43%). This study is the only one in which an apparent difference in satisfaction was observed between different types of complementary medicine. Almost 40% of the complaints concerned the locomotor apparatus, followed by neurological problems (mainly migraine) with 14% and psychological problems with 10%. Although some differences between the various types of complementary medicine are observed, it appears that in the case of homoeopathy, acupuncture and psychic healing patients came with quite a variety of complaints. The most often cited reason for trying complementary medicine was that the patient had heard about it and wanted to try it also. Only a minority (about 20%) went because they were true believers in the complementary method of their choice.

It is of interest to note that the physicians (Visser, 1988) made a clear distinction between the various types of complementary medicine: manual therapy, homoeopathy and acupuncture scored highest in their esteem (60% or more felt positive about these remedies), psychic healing lowest (only 10% positive). No doubt these attitudes are based more on the 'image' these methods have than on effectiveness rates. However, Visser notes that another reason might be that in the case of acupuncture

and homoeopathy it is easy to apply the methods without 'believing' in the theory (especially in the case of homoeopathy, where the method might involve only prescribing a homoeopathic medicine), whereas in the case of psychic healing the theory (the belief in being able to exert a paranormal influence) is much more closely connected with the method. Hence psychic healing lends itself much less to application in the conventional medical health system. As regards the knowledge of the GPs about the various alternative remedies a similar pattern was observed; psychic healing also scored by far the lowest.

The view that the general practitioners based their judgement of complementary methods on the 'image' they had of these methods finds support in the results of another study in which general practitioners were asked to express their belief in the efficacy of the various alternative procedures (Knipschild et al., 1990). Half of them believed acupuncture to be effective for chronic pain, and 25% thought acupuncture could help with asthma. Forty-five per cent thought that homoeopathy was effective in the treatment of upper respiratory tract infections or hay fever and 30% that it would work for chronic joint problems. The other types of complementary medicine, except manual therapy, inspired much less confidence. It is of interest to note that these general practitioners tended to associate the effectiveness of the various types of complementary medicine with certain types of complaints. These associations appear to me to be based more on the supposed 'explanations' for these methods than on experience or factual data. At least, from most studies I know, one gets the impression that the various types of complementary medicine do not differ much as regards effectiveness rates (subjectively measured), nor as regards the types of complaints they treat. That general practitioners are relatively open-minded as regards complementary medicine appears also from a British study reported by Wharton and Lewith (1986). They found that of the 145 general practitioners who

participated, 45% had some training in complementary methods and applied those occasionally, a figure nearly equal to the Dutch percentage of 47%.

Visser et al. (1990) investigated the use of complementary medicine by patients suffering from arthritis. Cooperation was obtained from 71 medical specialists in this disease and from 1,179 of their patients. Of these patients 43% had consulted a complementary practitioner at least once. Psychic healing accounted for 25% of the total complementary consumption. The majority of the patients went to an alternative healer because conventional medicine did not work for them or because they considered themselves desperate and willing to try anything. As observed in other studies on arthritis, patients most often turned to a complementary practitioner when they had had the illness less than five years. Patients who used complementary medicine were only slightly less satisfied with their conventional practitioner. As regards the effectiveness of the treatment, for which patients could assign scores (on a scale of one to ten, ten being 'most effective'), the patients who used complementary medicine assigned on average 7.3 to the effectiveness of the conventional treatment against 5.8 for the effectiveness of complementary treatment. Hence these patients still considered conventional medicine more effective, but apparently not sufficiently effective. Not surprisingly those patients who did not use complementary medicine assigned a higher effectiveness value, 8.2, to conventional treatment. On other 'satisfaction' measures conventional practitioners all scored higher than the non-orthodox practitioners. This confirms that dissatisfaction with how they are treated by conventional practitioners is not the reason why people decide to see other healers.

An Overview of the Data of These Studies

The main data from the studies presented above are summarized below. If average values are presented the range of the values and the number of studies on

which the average is based are given in brackets.

1) *Number of people consulting complementary practitioners*

On average 22% (range 4%-49%; $n=15$, n being number of studies) of the population has at least once tried a complementary remedy. If patients only are considered this percentage is considerably higher (mean=65%; range 38%-99%; $n=7$). Duration of the complaint probably has a strong effect on these percentages: the longer a patient suffered the greater the likelihood that complementary remedies were tried. Between 90% and 98% reported having consulted a conventional practitioner first or in addition but this figure might have been somewhat inflated by social desirability. On average 12% (range 2%-20%, $n=8$) reported having directly consulted a complementary practitioner. These patients might have been considered to belong to a 'risk group' because they might have included patients for whom conventional treatment was needed but delayed. Over 90% of the patients of complementary medicine appeared also to consult conventional practitioners and in a number of studies a positive correlation was observed between the use of conventional and unconventional medicine. Patients consulting complementary practitioners were above-average users of the health system but that was related to the fact that they also had long illness histories.

2) *Psychic healing as part of complementary medicine*

The proportion of psychic healing in the total consumption of complementary medicine varied between less than 1% and 38%. It appears that strong differences between countries were observed in the frequency of use of the various types of complementary medicine. Hence it seems not very useful to present an average value for the proportion of psychic healing. Psychic healing seemed most strongly represented in Belgium, Germany, The Nether-

lands and the UK (mean=23%, range 6%-38%; $n=13$).

3) *Characteristics of users of complementary medicine*

On average, two thirds of the patients of complementary practitioners were female. This percentage varied remarkably little over the studies (mean=64%; range 53%-74%; $n=16$). Most patients were between 40 and 60 years of age with a drop in use of complementary methods for the older age groups. The socio-economic level of the patients was above average but one study suggested that patients of psychic healers may come from lower socio-economic backgrounds. Users of complementary medicine had long illness histories and mainly suffered from chronic complaints.

4) *Nature of the complaints*

Patients of complementary healers mainly reported complaints of a chronic nature. Complementary treatment was sought for all sorts of problems, from simple colds to cancer, but nearly all patients were still ambulant and physically able to take the initiative to try complementary methods. Complaints of musculoskeletal (mean=32%, $n=10$) and psychic disorders (mean=23%, $n=8$) were most often mentioned. About half of the complaints concerned some sort of chronic pain. In general, complementary methods were applied to all sorts of complaints, but because of the nature of the methods, certain complaints tended to be associated with certain methods. Thus hay fever and homoeopathy were related, and the same applied to chronic pain and acupuncture. But these correlations seemed not so much based on effectiveness rates but rather on the 'image' the methods had.

5) *Motivation for using complementary medicine*

However, from various survey studies in which patients were compared who did

and who did not use complementary methods, multivariate analyses revealed that none of the above-described characteristics correlated significantly with the use of complementary methods. A tendency was observed that patients who used complementary medicine did have a worse general state of health and that this variable contributed to the decision to try alternative methods. Also, relatively few patients used a specific complementary method for ideological reasons, that is, because they strongly believed in the ideas on which the method was based. There appeared also no difference between users and non-users as regards their satisfaction with the way the conventional practitioners treated them. The main reason for turning to complementary methods was clearly a pragmatic one, the fact that conventional medicine did not work for these patients and that they were willing to try anything to solve their problem.

6) *Improvement due to complementary treatment*

On average 65% (median=70; range 17%-91%; $n=25$) of the patients reported a subjectively experienced degree of improvement. For serious illnesses as cancer, or in the case of patients with a very long illness history of ten to 20 years (often in the case of rheumatism) the effectiveness rates were relatively low. But even in those cases 17% to 50% of the patients reported improvement. On average 8% of the patients reported deterioration (range 1%-21%; $n=10$), again with the most unfavourable figure (21%) for cancer. The subjectively experienced improvement appeared mainly linked to either a reduction in seriousness of symptoms (for instance pain) related to the disease or to an increased capability for coping with such symptoms.

When complementary treatment was compared with conventional treatment the results were a mixed bag. A few studies reported a more positive attitude towards conventional treatment, but in others the opposite effect was observed. The sample studies were in general not unbiased as

regards this question. However, in all studies the differences in appreciation observed were not impressive. Except for serious illnesses (such as cancer or advanced rheumatism) no relationship appeared between degree of improvement and type of complaint. In addition not much difference in effectiveness seemed to exist between the major types of complementary medicine such as homoeopathy, acupuncture and spiritual healing. For all these treatments approximately the same percentages for improvement were reported. In the few studies where effectiveness for different types of alternative treatment were directly compared, no significant difference was observed. Only the Visser (1988) study indicated a lower effectiveness for psychic healing. Also no difference was observed in the few studies in which effectiveness was compared for practitioners of unconventional treatments with and without a medical degree.

7) Negative effects of complementary treatment

Despite some efforts from conventional medicine to prove otherwise, few data were suggestive of negative effects from complementary treatment on the health of the patients. In nearly all controlled studies the treatment group improved more or to the same extent as the control group. Negative effects would have resulted in the treatment group showing less improvement or even deterioration. Negative effects from placebos are well known and are called 'nocebos'. In addition, it turns out that nearly all serious negative effects have been due to the patient either terminating conventional treatment or medication or simply because they did not bother to consult a conventional practitioner. Hence potential harmful effects from complementary medicine can be largely avoided if complementary practitioners take care that their patients also stay under the control of conventional practitioners.

Discussion of the Data

Although the material has been presented without much comment, that does not imply that the conclusions drawn by the authors are all acceptable. A number of these studies, especially among those that attempted to measure objective healing effects, seem of poor quality and are of limited value.

There are a number of basic conditions to which any experimental study aimed at measuring the effectiveness of a certain intervention should adhere. Without the inclusion of a control group very little can be concluded when some change is observed in the treated group. Unless large samples are compared, which is rarely the case because of the enormous costs involved, control and treated groups must be really comparable on the dependent variables before intervention takes place and on those variables (except the intervention) to exert an influence on the dependent variable. Particularly when small groups are employed it is a rather dangerous procedure just to assign subjects randomly to experimental and control groups. Random fluctuations can easily create differences between groups not attributable to the experimental variable. Hence random assignment should only be applied after the groups have been matched on the variables that might influence the healing process under study.

I will not discuss the other experimental pitfalls, such as non-blind studies, except one: the so-called 'regression to the mean' problem. If we take a group of patients in an extreme condition then, knowing that there are always fluctuations, the likelihood is pretty high that after some time, or intervention, a change in the direction of less extreme values will be observed. This is an important factor especially for those studies in which it is not possible to ensure properly matched groups. Hence it is easy to 'prove' the value of any treatment by taking some patients in an extreme condition and showing that

they 'improved'. Because these patients are in an extreme condition the supposed 'healing' is the more impressive. I suspect that many stories about impressive healing effects are based on such cases.

The application of proper control data also applies to the more sociological type of study discussed. To study only treatment groups without comparing them with other samples can yield very misleading conclusions. For instance, in all studies it was observed that about two thirds of the healers' patients were females. This suggests that women were more inclined to turn to alternative healers than males. In fact, from the van Sonsbeek and Limberger studies in which users of complementary medicine were compared with non-users it appeared that such was not the case. (The only indication that supported the opposite conclusion was the Akkro study: in that study a larger percentage of the women tried complementary remedies compared to males).

Despite the often sub-optimal quality, the studies in which changes in health were objectively measured seem to yield some consistent data. It apparently strongly contributed to the effectiveness of healing when the patient knew or expected that treatment was taking place. In most studies changes in objectively measured variables did occur but they seemed unimpressive in size. From the van Wijk studies it is clear that the sensation of warmth patients reported during treatment could be based on a real increase in body temperature. The Attevelt study with hypertension patients yielded a relatively strong objective healing effect, but according to the medical staff its strength was comparable to the effect of a mild anti-hypertension medicine. The Rehder study is the only one in which rather drastic improvement was observed, but that study involved only a few patients.

When objective changes did occur they most often also occurred in control groups. The degree of common change in treatment and control group always seemed much larger than the differences in effect of treatment, if any, between these groups. That implies that the effect of the psychic treatment itself was either not present or rather

small compared to the effect of other variables that influenced all groups equally. Although some of the published studies that applied blind procedures and proper controls provided some suggestive evidence for a possible paranormal effect, the nature of these effects and the small number of studies do not warrant the conclusion that such an effect can be considered proven. But some evidence does suggest that a paranormal effect might be possible. My point is, though, that even if a specific healing effect of the psychic treatment is assumed, then the influence of that effect on the healing process seems much smaller than the contribution of other variables. It is of interest to note that in none of these studies were clear indications found that the treatment had a significant deteriorating effect on the patients.

In the Attevelt studies both objective and subjective changes in state of health were measured. But in both studies an interesting phenomenon was observed. In the 'Optimal' conditions about 70% to 80% of the patients reported a subjectively experienced improvement, percentages that were only slightly higher compared to what is normally found for the effects of complementary treatments. In the two conditions in which patients were not treated the changes in blood pressure or asthmatic conditions were on average comparable with those found for the optimally treated groups, but the subjectively experienced improvement reduced to about 40%. Hence comparable changes in health according to objective measures resulted in quite different changes in experienced subjective improvement. This finding appears to have two consequences. It confirms what was stated above that the correlation between objectively measured health and subjectively experienced health can be quite low and it suggests that the same objective improvement can occur despite the fact that subjectively, patients do not feel strongly improved. Hence the physical presence of the healer or the certainty that healing is attempted apparently contributes substantially to the feelings of subjective improvement.

From the overview studies on homoeopathy and acupuncture it appears that for these treatments similar results are observed. There exists a tendency for the method itself to have some effect, but that effect cannot be very strong because in quite a number of studies it could not be demonstrated at all. On the other hand, although no data on effect sizes for control groups were presented, in the Kleijnen et al. (Kleijnene, ter Riet, & Knipschild, 1991; Kleijnen, Knipschild, & ter Riet, 1991) studies it is explicitly mentioned that when subjective symptoms are the main outcome measure (which is the case for most studies on acupuncture and homoeopathy) substantial improvements in the control group can be expected. We know from the various sociological studies discussed above that the effectiveness rates, subjectively measured, for the various types of complementary medicine do not differ much. Hence it also seems likely that in the case of the other types of complementary medicine it holds that the effect of the treatment on all participating groups is much larger than the between-group differences.

Does the Paranormal Offer an Explanation for the Effects of Paranormal Healing?

One of the conclusions of the overview presented above is that actually as to the pragmatic aspects of the healing situation little difference was observed between the various types of unconventional treatments. All had approximately the same degree of effect on mainly subjectively experienced state of health independent of the type of complaint. Only the seriousness of the illness seemed to influence effectiveness. Thus it would make sense to assume that a common complex of variables was responsible for the success of all these methods. This seems a much more logical approach than to assume that the comparable results of all these various methods were based on all sorts of different processes. From various studies on psychic healing it appeared that psychological variables associated with the patient and healer

had the strongest influence. In view of the similarities in characteristics of the different types of unconventional treatment there is no reason to assume that psychological variables do not also play a major role in all types of unconventional treatment. Most of the complementary remedies are also characterized by the fact that their theoretical validations appear very deviant from a scientific point of view. At least, one can say that for all of them the scientific evidence for the validity of the assumed processes is very scanty. In this respect psychic healing might find itself in a rather favourable position. The amount of scientific research on parapsychology seems much larger than that on the mechanisms of acupuncture or homoeopathy. However, the fact that all these different theories are so weakly founded in science strengthens the view that probably one common mechanism, based on the effect of psychological processes, is mainly responsible for the healing effect of unconventional remedies.

From various overview studies in parapsychology, including a number of recently published meta-analyses, it appears that some anomalous or paranormal interaction between people can take place (Braud & Schlitz, 1992; Honorton et al., 1990). Could such interactions explain the healing effect in paranormal healing? Braud's research seems of the most direct relevance to this question. He demonstrated in a number of experiments that people can have a direct paranormal effect on the skin conductance levels of another person. He interprets this effect in terms of relaxation. However, the strength of the effects he observed is rather small and comparable to an effect of, for instance, a relaxation exercise. Hence this phenomenon seems hardly able to explain the relatively much stronger effects observed in healing. The same applies to the data observed in psychokinesis research. The strength of these effects is, despite its significant nature, very small and usually only observable in large numbers of trials.

In addition there are other reasons why I think it unlikely that a paranormal process plays an important role in psychic

healing. Assuming that some psychokinetic effect is exerted during the healing sessions, there still remains the question of what process this effect is exerted. Psychic healing deals with a number of varying complaints and if the psychokinetic effect is responsible for the success, it has to be applied to different conditions in the patient, depending on the complaint, in order to be effective. The experimental evidence from research in psychokinesis is not suggestive of the existence of strong, consistent and selective PK effects that can be changed at will from one target to the other, maintaining their strength and direction. If such was the case, one could also expect that healing attempts would rather often result in deterioration of health, because from the parapsychological literature it appears that 'psi-missing' and 'displacement' effects are common phenomena.

There is also no reason to believe that healers are more capable of exerting a psychokinetic influence than other people are. None of the few studies in which healers were tested on psychokinetic abilities have been very successful, at least not to the extent that the results observed deviated strongly from the results of studies with non-healers. That healers themselves don't believe in this explanation is indicated by the results of the recent investigation by Attevelt (1992) mentioned earlier. He asked healers whether they felt that they possessed psychokinetic abilities. Fewer than 4% answered 'yes' to this question.

It is also questionable whether psychokinesis is really a matter of exerting influence on another system. Psychokinesis is traditionally understood as 'mind over matter', the idea that the human 'will' influences other systems. However, psychokinesis can also be understood in terms of a correlation between two sets of events. One of the main theories in parapsychology, von Lucadou's Model of Pragmatic Information, views all paranormal interactions as non-local correlations that do not allow signal or energy transfer, thus explicitly denying the possibility that psychokinesis,

in terms of an external and invisible force, can cause changes in a system.

The only further evidence that psychokinesis might play a role in healing is provided by some studies, traditionally cited in overview studies on psychic healing, that have studied the effect of treatment by a healer on artificially induced wounds in mice or on the growth of plants. Benor (1990) presents an extensive overview of all these studies. But the above-mentioned arguments apply here too. The effects are generally weak and replication studies often fail. Benor applies a sort of vote-counting technique to argue that these studies indicate a healing effect by paranormal means. However, as has been stated often, most recently by Utts (1991), the vote-counting technique is almost useless for assessing the magnitude of an effect. The phenomena studied are also far from typical of the practice of psychic healing. Healers rarely attempt to influence wounds or plant growth. Even if these studies are considered as having proven that wound healing and plant growth can be influenced by healers, it seems premature to generalize these effects to all the types of problem handled in the healers' practice with human subjects.

In all studies in which the patient's knowledge of the time of healing was manipulated it was found that the patient's expectations were a major factor in the healing process. Often no healing effect was observed when the patient did not know that healing was attempted. This also strengthens the hypothesis that psychokinesis has little influence. Hence although I do not exclude the possibility that occasionally a paranormal effect might contribute to the healing process, as for instance was suggested by the results of one of the Attevelt studies, it seems to play a minor role compared to other variables.

Although I have restricted this discussion to the theoretical validation of psychic healing, I suspect that similar arguments can be raised when discussing other types of complementary medicine such as homoeopathy or acupuncture. For instance, if it could be demonstrated that the effects

of homoeopathy are largely independent of the exact nature of the homoeopathic 'medicine' that is administered to the patients, then it would be safe to assume that the effect of the homoeopathic treatment was also largely due to psychological variables associated with the patient. It seems rather unlikely that the explanations on which the various other methods of complementary medicine are based are scientifically valid. These explanations are nearly all at variance with known scientific or physiological processes and experimental support for their validity hardly exists. In fact, psychic healing is the only case of complementary treatment for which it holds that at least a large body of experimental evidence is available that supports the claim that a paranormal effect on human beings (such as in cases of ESP) might be possible. In the case of most of the other types of complementary medicine the main evidential argument in favour of the theory behind the method seems to be that the method works. But that argument, of course, does not prove anything about the validity of the explanation.

Complementary Healing and the Placebo Effect

The concept of the placebo is well known in the health sciences. A placebo is any treatment that does not have a specific effect on the treated disease but that affects the patient's general state of health. Placebos mainly affect subjectively experienced state of health but objective changes in health following placebo treatments have also been found. Placebo effects can result in objective improvement in the treated condition, perhaps as a result of improved feelings of well-being or because the placebo reinforces the inbuilt ability for self-healing. In general placebos are associated with positive effects on health, but negative effects from placebo treatments (nocebos) are also known.

According to Fields and Levine, the placebo response probably accounts for more subjective improvement than any single category of 'active' drug (Fields &

Levine, 1981, p.745). But the placebo response is a rather complex phenomenon. Replication is infrequent even when seemingly identical procedures are used on highly similar populations (Shapiro & Morris, 1978). Not all patients respond to placebo treatment and this has been the reason for suspecting that responsiveness to placebos could be a personality characteristic. This has turned out not to be the case. Although most people seem to have the ability to respond to a placebo, it appears that a previous positive response to a placebo does not enhance the probability that the same patient will again show a positive response in a different situation. However, there are indications for a relationship between the efficacy of the response and degree of discomfort, concern and stress (Wormsley, 1985). In addition, the placebo effect is dependent on various situational variables (even to the extent that the colour of a placebo pill might influence its effects) and on variables associated with healer-healee interactions.

The placebo response is inherent in all healing situations, conventional or otherwise. Benson and McCallie (1979) in their discussion on the relationship between angina pectoris and the placebo effect presented striking examples of the influence of placebo effects when new drugs are introduced. They state: 'A recurrent pattern is present in the history of various treatments for angina pectoris. When a drug or surgical procedure is first introduced, there is general enthusiasm and hope for its effectiveness. In early studies and in anecdotal evidence, proponents... report remarkable benefits, with therapeutic effectiveness seen in the vast majority of the patients. Only later do more adequately controlled studies appear... (that) find the therapy 'no better than' inert, control placebo pills... Quantitatively, the pattern is consistent: the initial 70 to 90 per cent "base-line" placebo effectiveness in the enthusiasts' reports decreases to 30 to 40 per cent "base-line" effectiveness in the skeptics' reports' (ibid., p.1,424). As a result, the therapy is abandoned, replaced by a new one, and disappears from the medical literature.

Whether the effectiveness of complementary medicine is based on the same mechanisms that cause the placebo response cannot be answered as yet for the simple reason that in both cases we do not know what the mechanisms are. In view of their characteristics it looks as if the placebo response and complementary treatment have much in common. Placebo responses are part of all healing situations, hence placebo effects must contribute to complementary treatments as well. But the question remains whether the placebo response is the only effect of complementary treatment or whether the treatment adds some effect of its own. In view of the experimental data presented above it seems that an effect of the method itself might not be present, and if so the strength of that effect seems much smaller than the contribution from the psychological variables involved. The latter variables are also relevant to the placebo response. Thus it seems likely that the effects of complementary and of placebo treatment are largely due to the same variables.

Beecher (1955) estimated the effect of the placebo response as on average 35%. Dunning (1979) reports effectiveness figures, based on a German study, ranging from 16% to 62%. According to this study, placebo treatment appears most effective for headache (62%), seasickness (58%) and digestive problems (58%), and least effective for hayfever (22%), diseases of the skin (21%), Parkinson's disease (19%), angina pectoris (18%) and hypertension (17%). The Benson and McCallie study discussed earlier reports a 'normal' 30% to 40% placebo effect as opposed to 70% to 80% when belief in the method is still strong. The 70% to 80% under favourable conditions is not entirely comparable to the 'normal' complementary practice because we know that most patients turn to complementary methods for purely pragmatic reasons and not because they are enthusiastic believers. These figures suggest that the effectiveness of complementary methods (on average 65%) is somewhat higher than the effectiveness of the 'normal' placebo response (on average around 30% to 40%).

Another well-known phenomenon, probably related to the placebo effect in the case of less serious illnesses, is that of spontaneous remission. Fortunately in most of the less serious illnesses the immune system is strong enough to turn the tide and to start a healing process. Much more rare are spontaneous remissions in serious diseases such as cancer which, without intervention, normally progress in only one direction. Nevertheless, even in the case of cancer, spontaneous tumor regression is a well-authenticated phenomenon. One of the first reviews of the medical literature (Everson & Cole, 1956; Everson & Cole, 1966) yielded about 170 cases that the authors judged as real spontaneous remissions of cancer. Kent et al. (1989) reported that they had collected over 3,000 such reports from more than 800 medical journals, covering all illnesses, not just cancer. Although the causes for spontaneous remission in serious diseases are still unknown, the authors quote Weinstock who stated that 'In every single case of 'spontaneous' regression where the psychosocial situation is described, a favourable change in (or the favourable psychological change occasionally experienced on facing death) has invariably just preceded the tumor shrinkage' (Kent et al. 1989, p.68). Such psychological changes might not be uncommon in complementary practice. On the other hand, if spontaneous remission in serious illness happens it will easily create the impression of a miracle. In such a case one can expect people to look for special explanations, even if objectively speaking there is none, to match the extraordinary character of the event. If the patient did try an unorthodox healing method at the time it is likely that the remission will be attributed to that method.

An Interpretation of the Results of Complementary Medicine

It is without dispute that conventional scientific medicine provides by far the most successful treatments. Patients are generally well aware of this and therefore first turn to conventional medicine with their

complaints. However, this probably applies mainly to those illnesses for which a scientific explanation is available and hence for which a treatment exists based on this scientific insight. This also implies that for those problems some form of objective measurement is available.

Complementary medicine seems especially effective in improving the general state of well-being and the subjectively experienced state of health in those situations in which conventional medicine is unable to solve the problems. The complaints treated by unconventional methods are often based on problems that cannot be measured objectively. Also, the symptoms and the experienced state of health in itself cannot be measured as precisely as methods in conventional medicine often allow. In such situations conventional medicine is rather powerless, because the ability to measure is a basic condition for medical science.

In contrast to conventional medicine, in which for each complaint a specific treatment exists, complementary medicine is more generally applicable. Basically the same method is applied to a variety of complaints. The differences, in terms of effectiveness, between the various types of complementary medicine are not great. Only a small proportion of the effect of complementary medicine appears associated with the method itself. Psychological factors associated with the patient and with the healer contribute much more strongly to the results. Negative effects of complementary medicine are mainly due to neglecting to consult conventional practitioners or to unjustifiably terminating conventional treatment. However, none of the complementary treatments themselves appear to cause serious negative effects. This also supports the view that the methods themselves do not have much effect on the state of health of the patients.

Thus, as has been argued before, the results of the various types of complementary medicine are probably based on the same processes. The same seems to apply to placebo treatment. Complementary medicine appears effective, especially for

subjectively experienced state of health, but for other reasons than those that their adherents believe in. The methods themselves (homoeopathy, acupuncture, psychic healing, to name only a few among thousands of such rituals), contribute only little or nothing to the effects. If that conclusion is correct, then why do they work? For the answers it makes sense to look first for the common elements in all these treatments. The following factors might contribute to an understanding of this phenomenon.

- Often the treatment, especially in the case of spiritual healing, concentrates on the ailing part of the body. This seems to result in some physiological reactions. For instance a feeling of warmth, probably as the result of increased blood circulation and muscle contractions in the case of paralysed patients. From various studies we know that the patient exerts this influence because it is dependent on the patient believing that he or she is being treated. Such repeated concentration on the ailing part of the body might well result in objectively demonstrable improvement. In the case of other complaints complementary medicine might have some objective effects for different reasons. For instance, the relaxing effect of the treatment might result in lower blood pressure values in hypertension patients.

- Conventional medicine is almost entirely based on the model that subjective experience is a consequence of some objective condition. Hence conventional medicine cannot deal with situations when the opposite holds, that is, when bodily functioning or illnesses are influenced by psychological aspects. But even assuming that the latter does not happen, then still we know from experience that it is often not the illness itself that makes life miserable but the associated subjective and unmeasurable experiences such as pain or nausea. But the ways people cope with these experiences are very much influenced by psychological factors. Thus part of the effect of complementary treatment on subjectively

experienced illness might well be due to an increased ability to cope with the illness.

- Although patients might have high expectations of complementary treatment, what they expect is nevertheless probably different from what they expect from conventional medicine. Conventional medicine often presents the image of being incomparably better and knowing it all compared to the quacks, but therefore not surprisingly patients' expectations of conventional medicine are much higher: they expect to be cured. As regards complementary medicine, expectations seem much more modest, and patients expect to become improved rather than to become cured. Hence the discrepancy between expectation and outcome, which largely defines satisfaction, might be easier to bridge for a complementary practitioner than for a conventional medical practitioner. It is a weakness of nearly all studies discussed that they did measure satisfaction but did not measure expectation.

- Complementary medicine seems especially to work for chronic patients. Chronic patients with a long history of conventional treatment are likely to feel rather desperate and hence might become less critical and less rational as regards unconventional treatment. This will increase confidence and belief which in their turn appear to correlate positively with subjectively experienced improvement.

- Only a minority of the patients turn to a particular type of unconventional treatment because they believe in the ideas on which the treatment is supposed to be based. From the data we know that a substantial percentage of patients try different types of unconventional treatment. But from my own experience with psychic healers I know that patients also consult different healers until they find one who seems really able to help them. I think that this 'search' process is very important and strongly enhances the probability of success.

One attractive aspect of the diversity of complementary medicine is that it provides the patient with many different opportunities to try new ways to do something about his or her problems. In conventional medicine these opportunities are much more restricted because the medical service is so highly organized. Once a patient is treated by a specialist it is very difficult to change either specialist or treatment. But it is well known within conventional medicine that new treatments are initially often very successful, after which a decline effect sets in until effectiveness finally more or less entirely disappears. New treatments probably raise hopes and expectations and might therefore be more effective.

Another important aspect of the 'search' process is that it allows the patient to search until a healer is found who is psychologically an optimal match for the patient. That is probably the reason why most healers have a circle of patients who are really devoted to him or her. This implies that the interaction between healer and patient is probably more effective than what can be expected from a conventional practice.

A third reason for the importance of the 'search' process is that it 'allows' the healer to make use of the natural variation in subjectively experienced state of health. Normally even without treatment patients feel occasionally worse or better. When as with conventional treatment the medication or treatment remains the same, these ups and downs will be attributed to factors outside the treatment. However, if an 'up' period coincides with the beginning of a new (unconventional) treatment the likelihood is large that the patient will attribute the reason for the improvement to the new treatment. Thus the healer and the treatment will inspire a lot of confidence which in its turn might stimulate further improvement.

The role of the 'search' process will probably be larger in countries with a well organized public health system which might restrict the chances of changing doctors. However, even in countries such as the USA many people might feel it wiser

not to maintain good relationships with doctors living nearby. In addition, changing from one complementary practitioner to another often also means a profound change in method of treatment, much more than would be the case when changing from one conventional practitioner to the other.

- Most complementary treatments stress a holistic approach. A central part of this approach is to adopt a more healthy life-style. Of course, conventional physicians will also tell the patient for instance to drink and to smoke less, but such advice is often supplementary. The main treatment consists of other elements, most often medication, and that's what the patient expects and what is supposed to generate the healing effects. But in complementary medicine a more healthy life-style is often central to the treatment. Patients expect that and so they might be more willing to make a serious effort to change life-style once they decide to seek help from a complementary practitioner.

- Part of the success of complementary treatment might be due to the very fact that the method itself does not have an effect. It means, often, that patients stop taking all sorts of medication and change their life-style, for instance by changing diet, which is not replaced by some other intervening mechanism. The absence of possible negative effects of conventional treatment or of habitual use of drugs might create more favourable circumstances for the start of a self-healing process.

- One factor that occasionally will suggest a healing effect to a patient but which actually has little to do with it is 'healing' based on a wrong diagnosis. Some healers might have a tendency to 'diagnose' illnesses that are either non-existent or much less serious than the healer suggests. Such illnesses are of course easy to heal and might give the patient a somewhat exaggerated opinion of the healer's capacities. This situation might also lead to erroneous

stories about miraculous healing performances.

From the interpretation presented above, one can conclude that complementary treatment deserves a place in the health care system. The rejection of complementary treatments seem partly based on the fact that many physicians apply only one definition for healing, i.e. that healing is associated with changes in objectively measured variables. If healing was seen as a multidimensional concept in which subjectively experienced state of health also gets a place then the difference between conventional and complementary medicine largely disappears. From a purely pragmatic point of view there seems nothing amiss with a treatment that makes people feel and function better and which in itself does not have negative side-effects.

However, complementary treatment can have negative effects but these are mainly due to the risks patients run when they don't inform their physician about their medical situation. In my opinion this is not the fault of complementary medicine alone, but rather of the unwillingness of participants in the health care system to cooperate in the interest of the patient. Complementary practitioners should insist and ensure that their patients see a conventional practitioner. The more professionally oriented healer organizations require this from their members. Unfortunately a number of complementary practitioners do not adhere to this basic rule. On the other hand, there is quite a difference between advising and to ensuring. Many patients might not inform the complementary practitioner correctly about their relationship with a conventional practitioner, and might even terminate conventional treatment without informing either the conventional or complementary practitioner. The optimal way would be for the complementary practitioner to make sure that any ongoing conventional treatment is not terminated and that the patient does consult a conventional practitioner. However, the latter solution is most often not possible because of the negative attitude conventional medicine

has towards complementary medicine. Doctors don't want to discuss patients with 'quacks' and often also make it clear to patients that they don't want to hear about contacts with complementary practitioners. Hence it might not be easy for the responsible complementary practitioner to know whether the patient is really seeing a conventional physician.

Apart from the practical aspects involved in integrating complementary medicine in the health-care system, the study of why complementary methods have such positive effects on patients seems highly relevant for the medical sciences in general. Methods that contribute to the well-being of the patient by strengthening self-healing capacities without apparent negative side-effects could be a valuable tool for any physician. In view of the size of the effects, pragmatically oriented studies of the mechanisms of complementary methods could be very rewarding; more rewarding than research aimed at proving or disproving the belief systems that generate these methods.

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Guarigione Psichica e Medicina Alternativa

Sommario: Un'analisi delle ricerche finora condotte sulla validità della guarigione psichica in soggetti umani indica che questa pratica può dimostrarsi efficace in particolare sulla percezione soggettiva dello stato di salute. Le misurazioni oggettive, però, indicano effetti assai meno pronunciati. Ad ogni modo, l'entità dell'effetto della guarigione psichica appare correlarsi strettamente alla consapevolezza del paziente che è in atto un tentativo di guarirlo e sembra vada rapportato soprattutto a variabili psicologiche relative al paziente e all'interazione terapeuta-paziente. Conseguenze negative della guarigione psichica e della medicina alternativa in generale intervengono per lo più quando il paziente interrompe la terapia medica convenzionale o non consulta un medico ortodosso mentre si sottopone al trattamento alternativo.

Una rassegna degli studi, soprattutto sociologici, sulla medicina alternativa consente di concludere che le caratteristiche principali delle varie discipline alternative sono sostanzialmente analoghe: l'efficacia si attesta in media sul valore di circa il 68%, mentre il motivo principale per cui il paziente decide di tentare un trattamento alternativo consiste nel fatto che la medicina ortodossa non sembra più essergli di giovamento. A differire sensibilmente sono soltanto i metodi e le interpretazioni teoriche su cui si basano le diverse tecniche terapeutiche. Per tutte le branche della medicina alternativa si riscontra che l'azione esercitata dalla pratica terapeutica stessa sullo stato di salute del paziente è scarsa o del tutto assente, a confronto dell'effetto dovuto invece alle variabili psicologiche associate al paziente o al guaritore. Si assume perciò che l'efficacia dei vari metodi alternativi sia in larga misura dovuta a questo processo. La medicina alternativa e l'effetto placebo hanno molto in comune, ma dai dati disponibili emerge che la prima è in certa misura più efficace del secondo.

Sebbene l'evidenza sperimentale suggerisca l'esistenza di un effetto anomalo di natura paranormale, le sue dimensioni sono troppo esigue rispetto a quanto si constata davvero nella guarigione psichica. In riferimento alle situazioni più complesse, è altresì poco chiaro in che modo un possibile effetto paranormale possa risultare in un miglioramento dello stato di salute dell'individuo. È poco verosimile, perciò, che un eventuale processo paranormale possa render conto dell'effetto di guarigione psichica riscontrato davvero nei pazienti, per quanto non si possa escludere che entri in ballo anch'esso. Una considerazione analoga si può riproporre in relazione alle spiegazioni offerte dai sostenitori di altri tipi di medicina alternativa: in generale, la validità scientifica e la base sperimentale di quelle spiegazioni sono ancora più deboli degli effetti paranormali riscontrati. Sembra probabile dunque che alla base della maggior parte degli effetti terapeutici osservati nella medicina

alternativa e nei trattamenti con il placebo ci siano processi psicologici importanti: alcuni dei quali vengono qui discussi e analizzati.

Lo stato di salute può essere misurato oggettivamente, in termini di condizioni fisiologiche o mediche, e soggettivamente, in termini di quanto bene il paziente si sente ed è in grado di essere. Le misurazioni oggettive consentono l'intervento medico-scientifico che, qualora esista, contribuisce a realizzare un trattamento efficace. Quando invece le misure oggettive non sono possibili o i pazienti accusano disturbi senza il riscontro di un'alterazione misurabile oggettivamente, criterio principe per misurare l'efficacia di un trattamento diventa la valutazione soggettiva dello stato di salute. In questi casi potrebbe non essere possibile realizzare un approccio tradizionale medico-scientifico e la medicina alternativa diventa allora uno dei trattamenti più efficaci che si possono offrire al paziente.

Psychisches Heilen und komplementäre Medizin

Zusammenfassung: Eine Forschungsübersicht zur Wirksamkeit psychischen Heilens bei menschlichen Probanden deutet darauf hin, daß psychisches Heilen Wirkungen - insbesondere hinsichtlich des subjektiv empfundenen Gesundheitszustandes - zeitigen kann. Objektiv gemessene Wirkungen sind weit weniger ausgeprägt. Die Stärke des Effekts scheint indessen nachhaltig von dem Wissen des Patienten abzuhängen, daß ein Behandlungsversuch unternommen wird. Sie dürfte im wesentlichen mit psychologischen Variablen seitens des Patienten wie auch hinsichtlich der Interaktion zwischen Patient und Heiler in Zusammenhang stehen. Negative Wirkungen des psychischen Heilens und komplementärer Medizin im allgemeinen treten hauptsächlich dann auf, wenn der Patient eine konventionelle medizinische Behandlung abbricht oder wenn er während der komplementären Behandlung nicht zugleich einen konventionellen medizinischen Therapeuten konsultiert.

Aus einer Übersicht über hauptsächlich soziologische Studien zur komplementären Medizin läßt sich der Schluß ziehen, daß sich die Hauptmerkmale der verschiedenen Arten komplementärer Medizin nicht unterscheiden. Die Wirksamkeit der Behandlung liegt im Durchschnitt bei etwa 68%, und der Hauptgrund, warum der Patient eine komplementäre Behandlung ausprobiert, besteht darin, daß die konventionelle Medizin ihm keine Hilfe mehr zu bieten scheint. Lediglich die Methoden und die theoretischen Interpretationen, auf welchen die verschiedenen Arten komplementärer Medizin gründen, unterscheiden sich beträchtlich voneinander. Bei allen Arten komplementärer Medizin zeigt sich, daß die Wirkung, die die jeweilige Methode selbst auf den Gesundheitsstand des Patienten ausübt, gering oder gar nicht vorhanden ist, verglichen mit dem Effekt patienten- und heilerbedingter psychologischer Variablen. Deshalb wird unterstellt, daß die Wirksamkeit unterschiedlicher komplementärer Methoden bis zu einem hohen Grad jeweils dieselbe Grundlage hat. Komplementäre Medizin und der Placebo-Effekt haben vieles gemeinsam. Die empirischen Daten deuten jedoch darauf hin, daß komplementäre Medizin ein wenig wirksamer zu sein scheint als eine Behandlung mit Placebos.

Wenngleich experimentelle Hinweise auf die Existenz eines paranormalen anomalen Effekts schließen lassen, ist die Größe dieses Effekts im Vergleich mit der Wirkung psychischen Heilens relativ gering. Bei vielen Beschwerden ist außerdem unklar, wie ein möglicher paranormaler Effekt in einer Gesundheitsverbesserung resultieren sollte. Somit ist es unwahrscheinlich, daß ein vermuteter paranormaler Prozeß für die beobachteten Wirkungen des psychischen Heilens auf den Patienten verantwortlich ist, wenngleich eine mögliche Beteiligung nicht ausgeschlossen werden kann. In analoger Weise lassen sich die

Erklarungen beurteilen, die von Befurwortern anderer Arten komplementarer Medizin angeboten werden. Im algemeinen sind die wissenschaftliche Validitat und die experimentelle Basis jener Erklarungen schwacher als die Hinweisen, die fur die Existenz eines paranormalen Effekts spreken. Wahrscheinlich durften daher einige bedeutsame psychologische Vorgange die Grundlage eines Grosteils der beobachteten Heilungseffekte durch komplementare Medizin und Plazebo-Behandlungen bilden. Eine Anzahl solcher psychologische Variablen wird erortert.

Der Gesundheitszustand kann sowohl objektiv im Sinne physiologische oder mediziner Bedingungszusammenhange als auch subjektiv anhand der Befindlichkeiten des Patienten und seiner Fahigkeit zu normalem Lebensvollzug gemessen werden. Objektive Memethoden ermoglichen wissenschaftlich-medizinerische Behandlungen, und - sofern solche Behandlungsmethoden bestehen - tragen diese Memethoden zu wirksamer Behandlung bei. Wenn jedoch objektive Messungen nicht moglich sind, oder wenn Patienten Leiden haben, ohne da objektiv mebare Abweichungen feststellbar sind, dann wird der subjektiv ermessene Gesundheitszustand zum wichtigsten kriterium fur die Messung der Wirksamkeit einer Behandlung. In derartigen Fallen mag ein konventionelles, wissenschaftlich-medizinerisches Herangehen unmoglich sein, und komplementare Medizin ist dann moglicherweise eine der wirksamsten Behandlungsmethoden, die dem Patienten angeboten werden konnen.

Paranormale en Alternatieve Geneeswijzen

Samenvatting: Onderzoek naar de effectiviteit van paranormale genezing met menselijke proefpersonen toont aan dat die geneeswijze met name de subjectief ervaren gezondheidstoestand effectief kan beinvloeden. Objectief gemeten zijn de effecten echter veel minder uitgesproken. Het effect lijkt sterk afhankelijk van de voorwaarde dat de patient weet dat hij wordt behandeld en het lijkt vooral gebaseerd op psychologische kenmerken van de patient en van de interactie tussen die patient en de genezer. Negatieve effecten van alternatieve en paranormale geneeswijzen in het algemeen treden vooral op als de patient verzuimt tegelijk met de alternatieve behandeling een gewone medicus te raadplegen of als hij de behandeling door die medicus staakt.

Het vooral sociologisch gerichte onderzoek naar alternatieve geneeswijzen laat zien dat alle verschillende typen op hoofdpunten overeenkomen. Ze zijn in gemiddeld 68% werkzaam en patienten doen er vooral een beroep op omdat ze geen baat meer vinden bij de conventionele medische wetenschap. Sterke verschillen vinden we in de methoden en in de theoretische achtergronden waarop die verschillende vormen berusten. Bij alle alternatieve geneeswijzen heeft de methode op zich nauwelijks of geen effect op de gezondheidstoestand van de patient. Dat effect is te verwaarlozen ten opzichte van de invloed van psychologische variabelen die samenhangen met de relatie tussen genezer en patient. Daarom wordt aangenomen dat de effectiviteit van de diverse alternatieve geneeswijzen voornamelijk via dezelfde variabelen kan worden verklaard. Er is een sterke overeenkomst tussen alternatief genezen en het placebo-effect, maar uit onderzoeksgegevens lijkt alternatief genezen iets beter te werken dan een placebo.

Hoewel experimenteel bewijs het bestaan van een onverklaarbaar paranormaal effect lijkt aan te tonen, is dat effect klein in vergelijking met de effecten van paranormaal genezen. Bij veel klachten is het bovendien onduidelijk hoe een verondersteld paranormaal effect de gezondheidstoestand zou kunnen verbeteren. We kunnen weliswaar niet uitsluiten dat een

paranormaal effect een bijdrage levert, maar het is onwaarschijnlijk dat die bijdrage groot genoeg is om de bij paranormaal genezen optrende effecten te verklaren. Een soortgelijke opvatting past bij de verklaringen die aanhangers van andere alternatieve geneeswijzen geven. In het algemeen zijn de wetenschappelijke validiteit en de experimentele basis van die verklaringen zwakker dan het bewijsmateriaal voor een paranormaal effect. Het lijkt daarom waarschijnlijk dat het leeuwedeel van de effecten van alternatieve geneeswijzen en placebo-behandelingen berust op bepaalde belangrijke psychologische processen. Een aantal van die psychologische variabelen wordt nader besproken.

De gezondheidstoestand kan objectief worden gemeten via fysiologische of medische variabelen en subjectief door na te gaan hoe goed de patiënt functioneert en zich voelt. Via objectieve metingen zijn wetenschappelijke medische behandelingen mogelijk en kan men nagaan hoe effectief die zijn. Is objectieve meting onmogelijk of hebben patiënten klachten waarvoor geen objectief meetbare oorzaak is vast te stellen, dan wordt de subjectief ervaren gezondheidstoestand het belangrijkste criterium bij het meten van het effect van een behandeling. In dergelijke gevallen kan wetenschappelijk medisch handelen onmogelijk zijn en zou alternatieve geneezing misschien een van de meest succesvolle behandelingen kunnen zijn.

Guérison Psychique et Médecine Complémentaire

Résumé: Une vue d'ensemble de la recherche sur l'efficacité de la guérison psychique chez les sujets humains indique que la guérison psychique peut être efficace, particulièrement sur l'état de santé vécu subjectivement. Toutefois, la taille de l'effet de la guérison psychique semble fortement dépendante de la connaissance du patient qu'un traitement est tenté et semble être surtout associée aux variables psychologiques associées au patient et à l'interaction guérisseur-patient. Les effets négatifs de la guérison psychique et de la médecine complémentaire surviennent en général surtout quand le patient met fin à un traitement médical conventionnel ou ne consulte pas un praticien médical conventionnel avec un traitement complémentaire suivi.

D'après une revue d'ensemble des études surtout sociologiques sur la médecine complémentaire on peut conclure que les caractéristiques principales des différents types de médecine complémentaire ne diffèrent pas. L'efficacité est en moyenne d'environ 68% et la raison principale du patient pour essayer un traitement complémentaire est que la médecine conventionnelle ne semble plus les aider. Seules les méthodes et implications théoriques sur lesquelles sont basées les différents types de médecine complémentaire diffèrent fortement. Pour tous les types de médecine complémentaire on trouve que l'effet de la méthode par elle-même sur l'état de santé du patient est faible voire inexistant, comparé à l'effet des variables psychologiques associées au patient et au guérisseur. On suppose donc que l'efficacité des diverses méthodes complémentaires est pour une large part basée sur le même processus. La médecine complémentaire et l'effet placebo ont beaucoup en commun. Toutefois, d'après les données il apparaît que la médecine complémentaire semble être quelque peu plus efficace que le traitement placebo.

Bien que l'évidence expérimentale suggère l'existence d'un effet (anomalie) paranormal, la taille de l'effet est petite comparée aux effets observés dans la guérison psychique. Dans le cas de nombreuses maladies, il n'est pas non plus clair comment un effet paranormal possible pourrait résulter en une amélioration de l'état de santé. Il s'ensuit qu'un processus supposé paranormal, quoiqu'une contribution possible ne puisse pas être exclue, ne peut prob-

ablement pas rendre compte des effets de guérison psychique observés chez des patients. Un raisonnement analogue peut être appliqué aux explications offertes par ceux qui proposent d'autres types de médecine complémentaire. En général, la validité scientifique et la base expérimentale de ces explications est plus faible que l'évidence d'un effet paranormal. Il semble donc probable que certains processus psychologiques importants soient à la base de la plupart des effets de guérison observés en médecine complémentaire et dans les effets placebo. On discute un certain nombre de ces variables psychologiques.

L'état de santé peut être mesuré objectivement, en terme de conditions physiologiques ou médicales, et subjectivement, en terme de comment le patient se sent et est capable de fonctionner. Les mesures objectives permettent un traitement médical scientifique et si un tel traitement existe cela contribue à l'efficacité du traitement. Toutefois, quand les mesures objectives ne sont pas possibles, ou bien si les patients souffrent de malaises où aucun écart objectif mesurable ne peut être trouvé, l'état de santé subjectif mesuré devient le critère principal afin de mesurer l'effet du traitement. Dans de tels cas une approche médicale conventionnelle peut ne pas être possible et la médecine complémentaire est peut-être un des traitements les plus efficaces que le patient peut se voir offrir.

Cura Psíquica e Medicina Complementar

Resumo: Uma revisão dos trabalhos sobre a eficácia da cura psíquica em sujeitos humanos indica que a cura psíquica pode ser eficaz, especialmente nos estados de saúde vivenciados subjetivamente. Os efeitos mensurados objetivamente são muito menos definidos. No entanto, a força do efeito da cura psíquica parece muito dependente do conhecimento do paciente em que se tenta o tratamento e parece estar associada principalmente a variáveis psicológicas que por sua vez estão associadas ao paciente e à interação curador/paciente. Os efeitos negativos da cura psíquica e da medicina alternativa em geral acontecem principalmente quando o paciente termina um tratamento médico convencional ou não consulta um médico convencional, ao mesmo tempo que segue tratamentos alternativos.

A partir de uma revisão de estudos principalmente sociológicos sobre a medicina alternativa pode-se concluir que as principais características dos vários tipos de medicina alternativa não divergem. A eficácia está, em média, por volta de 68% e a principal razão para o paciente tentar um tratamento alternativo é que a medicina convencional parece não ajudá-los mais. Apenas os médicos e as interpretações teóricas em que a medicina alternativa está baseada diferem fortemente. Para todos os tipos de medicina alternativa descobre-se que o efeito do próprio método no estado de saúde do paciente é pequeno ou nem existe, comparado ao efeito das variáveis psicológicas associadas ao paciente e ao curador. Portanto, presume-se que a eficácia de vários métodos alternativo estão, em grande parte, baseadas no mesmo processo. A medicina alternativa e o efeito placebo têm muito em comum. No entanto, pelos dados parece que a medicina alternativa é, de alguma forma, mais eficaz que o tratamento placebo.

Embora a evidência experimental sugira a existência de um efeito paranormal anômalo, o tamanho do efeito é pequeno comparado aos efeitos observados na cura psíquica. No caso de muitas doenças, também não está claro como um possível efeito paranormal poderia resultar em uma melhora da saúde. Assim, é improvável que um suposto processo paranormal, embora uma contribuição possível não possa ser excluída, dê a razão dos efeitos observáveis de cura psíquica nos pacientes. Um raciocínio análogo pode ser aplicado às explicações oferecidas pelos proponentes de outros tipos de medicina alternativa. Em geral, a

validade científica e base experimental daquelas explicações são mais fracas do que a evidencia de um efeito paranormal. Portanto, parece provável que alguns processos psicológicos sejam a base da parte principal dos efeitos de cura observados na medicina alternativa e em tratamentos placebo. Varias dessas variáveis psicológicas são discutidas.

O estado de saúde pode ser medido objetivamente em termos de condições fisiológicas ou médicas e subjetivamente em relação ao quanto o paciente se sente bem e é capaz de funcionar. As mensurações objetivas permitem um tratamento médico científico e se esse tratamento existe contribui para um tratamento eficaz. No entanto, quando não é possível fazer mensurações objetivas, ou o paciente sofre de doenças e nenhum desvio objetivo mensurável pode ser encontrado, o estado de saúde subjetivo medido torna-se o principal critério para medir o efeito de um tratamento. Nesses casos, uma abordagem médica científica tradicional pode não ser possível e a medicina alternativa é, talvez, um dos tratamentos mais eficazes que se pode oferecer ao paciente.

Essay Review: *Spontaneous Psi, Depth Psychology and Parapsychology*

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An essay review of *Spontaneous Psi, Depth Psychology and Parapsychology, Proceedings of an International Conference held in Berkeley, California, October 31 - November 1, 1987*, edited by B. Shapin and L. Coly, published in 1992 by the Parapsychology Foundation, New York, NY. Hbk., xvi + 287 pp. \$20. ISBN 0-912328-42-8.

In reviewing the proceedings of this conference that was held nearly five and a half years ago I find myself in the invidious position of having to criticise the hand that feeds (some of) us. No one who knows the history of funding difficulties for research in parapsychology could underestimate the enormous contribution, in terms of finance, time and effort, that the Parapsychology Foundation has made to this field over the years. Since its inception in 1951 the PF has provided a unique and absolutely invaluable underpinning to research in this field. It is regrettable, therefore, that our expectations for reasonably rapid dissemination of information and opinion from such conferences as I review here, are disappointed.

The initial concept in the title of this conference, spontaneous psi, is open to several interpretations. Charles Tart, in a comment on Jurgen Keil's paper, draws attention to the many ways that the word 'spontaneous' may be used when discussing spontaneous psi. He mentions three: 'appropriate to current situation', 'unpremeditated', and 'unreliable', analyzing the implications of the last interpretation in detail. Alternatively Keith Harary suggested a definition that envisages spontaneity as a function of observer inattention. He implies that the precursors or 'causal factors' of the psi event are available to consciousness and presumably measurable, at least in principle, but that those who later describe it as a spontaneous psi

event were not attending to these factors at the time.

Whether 'depth psychology', the second concept in the conference title, can illuminate these precursors or 'causal factors' depends on the degree to which the elements deemed to be important to such an interpretation of human behaviour/ motivation can ever be reliably measured, even in principle. At this point I had better declare an interest or bias.

I am a clinical psychologist who, in eleven years of education, research and clinical training in psychology, and then fourteen years of clinical practice, has not found the tenets of 'depth psychology', the psychoanalytic interpretations of human experience, to be as effective a therapeutic approach as the cognitive/behavioural ones. As a means to self development the themes of depth psychology, appealing as they do to explanations of behaviour supposed to be found at the level of the unconscious, may have value. However, whereas I am attracted by the music, the lyrics are unconvincing and, I believe, misleading when applied to clinical situations.

It is my view that certain concepts from depth psychology, such as some of the defence mechanisms, have merit as descriptions of behaviour in clinical practice, but the nature of the 'power' behind the mechanism, as described in psychoanalytic interpretations, has doubtful predictive validity. As a very junior post-doctoral

fellow in the University Department of Psychiatry in Edinburgh in the mid-seventies, I took part in discussions about setting up a controlled study of the efficacy of depth psychology compared with cognitive/behavioural intervention in a range of human problems characterised by anxiety and/or depression. The discussion centred on the degree to which elements of each approach could be operationally defined to permit prediction and empirical test. The proponents of the depth psychology approaches could not agree on a research paradigm that would make useful comparisons between the two broad approaches and the discussions faded out. This experience led me to take a less than sanguine view of the value of depth psychology concepts in clinical research.

Spontaneous Psi and Experimental Parapsychology. Jurgen Keil

In this, the first paper of the conference, Jurgen Keil develops Gaither Pratt's speculation - that all psi events may be spontaneous - to make an appeal for widening the research techniques used in parapsychology beyond the laboratory based experimental methodologies. He suggests ways that laboratory experiments may perhaps cope more successfully with spontaneous psi events, principally by recording a larger range of variables such as experimenter and subject needs and expectations. To do this he discusses the relevance of techniques used in field research, particularly ethology.

While this seems a very reasonable suggestion it raises the question of whether the needs and expectations are to be inferred simply from verbal, or at least measurable, behaviour or whether they are to be interpreted from models of 'deep' or unconscious control of human motivation.

As mentioned earlier, in the discussion following Keil's presentation, Charles Tart raised the point that the word 'spontaneous' could be used in several different ways depending on the theoretical model of psi being considered by the speaker. In his paper Keil defines a sponta-

neous case as a psi event associated with a person or persons who experience these events but consider that they have no volitional control over them. Tart points out that this indicates that Keil's model for psi is not one that suggests that psi is somehow inherently unlawful but that there are psychologically unconscious factors operating that override conscious volition by the individual.

Whether or not this is a correct interpretation of Keil's position (see below) Tart makes the interesting general point that in unconscious dynamics models the relevant factors are frequently seen as being 'perverse'. He wonders whether it is possible to derive a model based on a 'friendly' unconscious that might be able to produce reliable psi results. Tart's point introduces a theme that runs through many of the papers of this conference, a theme based on the proposition that it is possible to train and develop in both subjects and experimenters benign states that make more likely the observation of psi. However, I repeat my earlier point, can any model based on unconscious processes have 'rules of transfer' that permit reliable prediction and therefore assessment of the model?

In recommending the development of research techniques, many available already within other disciplines and that will take account of unpredictable (but not unrecordable) psychological factors, Keil does not address the problem of recording unconscious (in the depth psychology sense) variables. It may be that Tart's observations on Keil's definition of 'spontaneous' misinterprets Keil's position. Like some later speakers Keil is particularly concerned with macro-PK events such as those observed in Batchelder's sitter groups. As an example of psychological factors that appear to have been important in both Batchelder's original groups and in Keil's own, he mentions the effects of small movements of the table (as he says 'probably entirely due to unconscious muscular movement'), leading to increasing movements of the table by smaller, more 'believable steps' involving psi. Keil is definitely using the term 'unconscious' here in

the 'not attended to' sense, so he might want to revise his definition of 'spontaneous'.

Clinical Issues in the Parapsychology Laboratory. Julian Isaacs.

In the second paper Julian Isaacs further develops the theme introduced by Keil: that factors related to the psychological state of both experimenter and subject are not only important to the occurrence or otherwise of psi events, but may be manipulable.

Isaacs' interest in PK training, which has been his specialisation for many years, derived out of an interest in observing, in a laboratory, non-statistical PK effects which he has termed directly detectable PK (DDPK). He makes the statement that 'my experiences in PK training have increasingly convinced me that the problem of obtaining 'big psi', certainly of the PK variety, has an important clinical dimension' (p.28). Before looking at this dimension in more detail, Isaacs makes an eloquent appeal for an increased role for qualitative as opposed to quantitative data collection in the laboratory. He reinforces and supplements Keil's view that ethnological techniques have relevance to parapsychology by suggesting that the ethnographic field observation methods used in anthropology could be used as data collection methods to replace the current 'method' of informal parapsychological 'lore'.

As a clinical psychologist I believe that there may be some hazards attached to using the term 'clinical' as Isaacs does. In talking about six factors relevant to PK training that he believes have 'clinical' implications, he runs the risk of both confusing, and possibly creating resistance in, those with whom he interacts, by implying that their behaviour requires a clinical approach. It is true that in talking about performance anxiety (on its own, and in relation to belief), the effect of life events, trainee/trainer relationships, motivation and emotional resistance to PK, he *might* be talking about clinically relevant variables, but only if the individual or individuals

concerned were in distress and sought help. What he appears to be talking about are those elements of interpersonal behaviour that are normal but that in a more extreme form have led to the development of clinical techniques.

He examines the idea that one of the releasers from performance anxiety, which he believes is an inhibitory factor in PK in the laboratory, is the belief that events are already occurring. Isaacs finds it strange that parapsychologists have not mounted a major investigation of the effects of performance anxiety in inhibiting psi performance under test conditions.

Disregarding the fact that the evidence for the effects of anxiety in general on psi performance is equivocal (Palmer, 1977), Isaacs' statement is tautologous. If indeed performance anxiety does inhibit psi performance one possible reason why that phenomenon has not been studied is that its existence or otherwise is inferred from the absence or reduced frequency of the dependent variable, psi. It may be that improvements in psychophysical measurement, allowing non-intrusive measurements of anxiety, may permit this type of experimental investigation. Certainly, were such experiments possible, it would be extremely interesting to investigate the relationship between belief and performance anxiety (Batchelor's 'artifact induction hypothesis') which suggests that the artifactual signals of PK success that Keil mentions in the previous paper as being due to unconscious muscular activities, do in fact act as releasers for PK to occur (Batchelor, 1984).

Psychic Unity, a Meeting Ground for Parapsychology and Anthropology. Marilyn J. Schlitz

The title of this third paper raises hopes that it will be a development of some of the ideas raised by both Keil and Isaacs particularly with regard to the applicability of anthropological research techniques to parapsychology. However, Marilyn Schlitz is specifically interested in the term

'psychic unity'. This term, in its original usage in anthropological literature, particularly the school of unilinear evolution, meant a common psychological structure in terms of emotion, perception, logic of cause and effect, and psychomotor skill.

My reading of Schlitz's paper suggests that she wishes to introduce a psi component in the term 'psychic unity', and in doing so to draw within its meaning terms such as collective unconscious, as used by Jung and other psychoanalysts, and Rupert Sheldrake's morphogenetic fields, to account for cultural parallels - the anthropological starting point of her discussions. I cannot see how the ideas in this paper could be translated into useful hypotheses and Schlitz herself does not suggest substantial ways in which techniques and anthropological inquiry could be used to further her speculation. I tend to agree with Isaacs' criticism in the discussion, which I will leave to the reader to pursue.

The Element of Spontaneity and Precognition Telepathy and Channelling. Ruth Inge Heinze

This paper is a phenomenological description of spontaneous experiences that the author has had and that she believes illustrate precognition, telepathy and channelling. I am concerned about my intolerance for such reports which are clearly honest attempts to discuss experiences that were significant for the author. I suspect it is my disappointment that, however well described, these numinous and personally significant experiences are not available to me and therefore the inferences which are drawn seem arbitrary. In the discussion that followed this paper Charles Tart asks the pertinent question, 'what kind of questions would psychics find interesting for experimental investigation from 'outside' their practice to focus on? What kind of questions might be answered through scientific research that would not come through just doing one's practice more and more deeply?' (p.98)

Psi and the Dynamics of Motivation. Arthur Hastings

In this, the last paper given on the first day of the conference, Arthur Hastings raises questions about the effect of conflicting emotions on the individual's (particularly the child's) decision to acknowledge experiences that might be psi-based. His plea for tolerance from those close to the child, but who do not experience these phenomena, is understandable, but, for reasons that Keith Harary addresses in his paper, appealing for tolerance in the present climate of media coverage of psi may be idealistic.

He makes the statement that 'psychic abilities may emerge for some children to help their physical and emotional survival' (p.100). His examples of family situations that have been experienced by specific psychics are in general terms very similar to those situations that have been claimed by some researchers to be associated with the development of schizo-affective disorders - namely environments with very low expressed emotion by 'significant others' who have rigid but conflicting needs and where tolerance in general is likely to be limited. The ability to opt out of stark reality may be both 'comforting and nourishing' but the child may require early professional help to place such experience in the context of the rest of the child's experience. Without concerned intervention to encourage limits on such 'reality slippage' one might find oneself (as I have on three occasions in my clinical experience) observing an adolescent develop very disturbed psychotic behaviour that includes ideas based on early childhood experience that may have involved psi.

In a discussion of motivation and psi in relation to gambling and 'commodity' experiments, Hastings makes the point that love of money, greed and acquisitiveness are all negatively valued in our culture. As such there may be superstitious constraints upon using psi to such ends.

Keith Harary comments on his involvement in the original Silver study and makes forceful comments on the atti-

tudes of some of the people who were involved. I was intrigued by his critical description of what happens when people 'start seeing honest to God really powerful ongoing results ... some people just freak out - which may be why they also feel unable to handle psi in their own lives' (p.113). Surely the point is that by definition such 'honest to God really powerful ongoing results' are highly unusual and tend to produce equivocal emotions even in the most laid-back individual. Frankly if they didn't there would be something aberrant in their view of the world.

Unfortunately as with the previous two speakers, Hastings, in a discussion of meta-values and meta-motives, particularly with regard to psychotherapeutic research, goes beyond the evidence. I am quite sure that all experienced psychotherapists, of whatever theoretical persuasion, experience sessions or parts of sessions when the normal pedestrian rate of interaction between therapist and client accelerates. At the end of such sessions there is usually a mutual feeling of success, useful distance having been travelled, and a closer emotional bond than had been there previously. With the present limited state of knowledge, based on research, about the nature of effective psychotherapy (Barker, 1985; Prioleau, Murdock, & Brody, 1983), speculation about any factor necessary for effective psychotherapy, without thought-through ideas about the ways in which such speculation could be verified, seems rather pointless.

Spontaneous Synchronistic Events as Seen Through a Simple Communication Model. Robert Morris

This first paper on the second day sees Bob Morris dissecting in an extremely clear manner the various possible elements, psi-related and otherwise, that may be connected with a striking coincidence that has meaning for the observer. He describes the 'source/receiver exchanges, involving the organisms amongst the various real time components of the coincidence, or their contributory causal antecedents, or the

observers that contribute to the final decisions, which can be applied to all circumstances in which one or more observers conclude that a meaningful coincidence has taken place' (p.148).

Unfortunately the detailed dissection that allows Morris to use the term 'to all circumstances', depends, I suggest, on an earlier premise, namely, 'if we allow for unconscious psi effects'. I am not suggesting that unconscious psi is a factor that cannot exist, but merely that if it is included in an otherwise internally consistent model, there is no way that I can see that the model could fail. By internally consistent I mean that the other 'exchanges' all depend on a transfer of information (with no explanation of how this is done) but with the proviso that the source is in principle measurable, whereas the unconscious sources do not have the same quality.

At the end of his introduction to this paper, Morris notes that the model 'may even be able to generate some testable predictions'. This struck me as an unfortunate choice of words because I have difficulty seeing the value of any model, particularly one as detailed as this one, that does not generate testable predictions.

The second part of the paper entitled 'Third factor co-ordination of coincident events' examines the possibility that intelligent, volitionally active factors such as human or nonhuman individuals or groups interested in the induction of pattern or an intelligent coordinating principal such as Gaia or the strong anthropic principle, can be viewed as extensions of the communication model. As Morris points out, for the purposes of completeness, such entities may be included in the organism part of the model but that in doing so there become no limits to what may be included in that part of the model. Finally in this section he examines the possibility that there are non-intelligent passive factors such as a universal tendency towards pattern that encourage coincidence.

Morris argues that, in principle, such factors could serve as third factors (between sources and receivers) responsible for coordinating anomalous coincidences of

the sort he is addressing. He maintains that one of these that has been extensively explored is the concept of the archetype along with the associated idea of synchronicity. Morris maintains that Jung did not use the term synchronicity to refer to all meaningful coincidences, only to those for which an archetype could be identified. No archetype, no synchronicity. To examine this position an extended quote from Jung is necessary:

The meaningful coincidence or equivalence of a psychic and a physical state that have no causal relationship to one another means, in general terms, that it is a modality without a cause, an 'acausal orderedness'. The question now arises whether our definition of synchronicity with reference to equivalence of psychic and physical processes is capable of expansion, or rather *requires* expansion (my emphasis). This requirement seems to force itself on us when we consider the above, wider conception of synchronicity as an 'acausal orderedness'. Into this category come all acts of creation, *a priori* factors such as the properties of natural numbers, the discontinuities of modern physics, etc. Consequently we would have to include constant and experimentally reproducible phenomena within the scope of our expanded concept, though this does not seem to accord with the nature of the phenomena included in synchronicity narrowly understood. The latter are mostly individual cases which cannot be repeated experimentally. This is not of course altogether true, as Rhine's experiments show and numerous other experiences with clairvoyant individuals. These facts prove that even in individual cases which have no common measure there are certain regularities and therefore constant factors, from which we must conclude that our narrow conception of synchronicity is probably too narrow and really needs expanding. I incline in fact to the view that synchronicity in the narrower sense is only a particular instance of general acausal orderedness - that, namely, of the equivalence of psychic and physical

processes where the observer is in the fortunate position of being able to recognise the *tertium comparationis*. But as soon as he perceives the archetypal background he is tempted to trace the mutual assimilation of independent psychic and physical processes back to a (causal) effect of the archetype, and thus to overlook the fact that they are merely contingent. This danger is avoided if one regards synchronicity as a special instance of general acausal orderedness. In this way we also avoid multiplying our principles of explanation illegitimately, the archetype is the introspective recognisable form of *a priori* psychic orderedness. (Jung 1955, pp.138-140).

I hope the extended quote above casts doubt on the interpretation of 'no archetype - no synchronicity'. I interpret Jung as saying that the archetype is one class of acausal orderedness associated with introspective human behaviour but that other ordering properties of the physical world may be associated with a synchronicity without an archetype being necessary. If one insists that there has to be an observer to acknowledge the meaning in the acausal connection and that acausal orderedness with physical origin (such as natural number) will be interpreted by the observer as having archetypal qualities then there is no limit to this explanation of spontaneous psi.

Both in the paper and in the subsequent discussion Morris is rightly insistent that both the communication model and his subsequent discussion on archetypes are presented in an attempt to evaluate such concepts and their potential relevance to parapsychology and that included in this is the 'kind of conceptual sharpening that will allow testing and evaluation'. As with the earlier appeal to unconscious psi I do not believe that the concept of archetypes, as I understand it, can be sufficiently defined in operational terms for research purposes.

In re-reading some of Jung's writings on synchronicity for this review, I have become curious about the effect of recent changes in human experience and their effect on archetypes. In the 70 years that have passed since Jung originally put for-

ward the concept, enormous changes have taken place, particularly in the West, in the way that most individuals experience the events in life that presumably led to the formation of archetypes. Because of demographic changes, changes in health care and social organisation, the trivialisation by the media (and in particular film and television) of death, it could be argued that the 'power' of archetypes has been reduced. Presumably archetypes are representations of the core experiences of humanity but their emotional impact and power, their numinous quality must be susceptible to change. If, in a sense, they are learned aspects of human experience then presumably they can be modified. Whether 70 years of rapid change are sufficient to make a discernible difference is a question that, while interesting, we would have no way of answering because to detect such a change we would need access to a baseline measure.

A particularly interesting dialogue at the end of the discussion on this paper between Tart and Morris recognises that if archetypes have an ordering function and are not just passive, and if one was in a sense to manipulate archetypes in a research setting, then one might be 'opening a Pandora's Box'. The fear or resistance that this thought might engender could affect the conduct of research in this area.

Anomalous Experience in Psychopathology. Vernon Neppe

Vernon Neppe begins his paper with a clear introduction on normality and abnormality in psychiatry and with a useful introduction to the difficulties created for psychiatric diagnosis by what he has termed 'subjective paranormal experience psychosis' where hallucinatory or delusional behaviour has as its core a paranormal experience that may affect the subject's view of reality. He makes the important point that for the psychiatrist it is the form and not the content of an experience that is diagnostically important and that such

experiences must be examined in the wider context of the subject's experience.

Neppe believes that in order to analyze normative experiences there is a need for a classification system. He has suggested a very complex multi-axial schema for evaluating psi experiences (MASEPE). There are 16 axes running from A to P. Each axis letter is the initial letter of the variable under consideration, i.e. axis A is for anomaly level, B - base level, C - content, D - dimensions, E - ego consciousness level, F - form, G - Gestalt factors, H - heuristic perspective, I - intention, J - judgement, K - prior knowledge, L - localisation of focus, M - mental status examination, N - neurophysiological correlates, O - organising environment, and P - psychiatric diagnosis. Within most of the axes there are further levels of complexity; on average each axis has ten associated terms.

While standing in awe of the ingenuity and sheer amount of work that must have gone into this paper, I cannot see how an analysis based on a classification system of this complexity could clarify the situation. In the discussion Morris asked Neppe 'to comment further as to whether you feel that by developing these descriptives you may be able ... to clarify what we might mean by abnormal, psychotic, ill?' (p.179) Neppe's answer, if put concisely, is equivalent to 'no'.

The Psychic Factor in Neurotic Style. Helen Palmer

Helen Palmer's presentation uses the vehicle of an extended phenomenological report to explore the possibility that neurotic style, e.g. paranoid, histrionic, obsessive etc., interacts with psi abilities to give access to information using projection or intuitive impressions that might not be available otherwise.

The psychological perspective that this is viewed from is an psychoanalytic, depth psychology one. She summarises her aim in writing the paper by suggesting that 'such an analysis would support the clinical client in the task of observing and detaching from unconscious defences and would

allow the psychically inclined client to recognise ways in which paranormal perceptions may be operating to intensify neurotic concerns' (p.192). This is a laudable aim.

However, I had difficulty with the term projection as used by Palmer. She quotes David Shapiro's (1965) comments on neurotic styles: 'projection occurs in the act of cognition with keen attention to the external world ... projection is generally faithful to and does not distort apparent reality, nor does it usually include perceptual distortions (hallucinations)'. She then goes on to say '(my) experience of projection is very different from the one that a cognitive explanation would suggest. One can know that one is projecting yet be unable to change the false perception of the environment' (p.190). She appears to wish to use the term projection in the same way as one would project an image with a slide projector. Her description of 'seeing' an individual interacting with people whom she knew before the individual physically showed up in her room is indistinguishable in my mind from an hallucination. This experience may represent some form of psi experience but I do not believe she has made her case that neurotic styles influence the form of these.

Nevertheless, I feel that there is an interesting line of enquiry in her suggestion. I have frequently noticed that individuals with anankastic or obsessional personality who are very concerned about maintaining control seem to be rather apprehensive about the potential that might lie in psi ability. They fear that the detailed cognitions that they have about maintaining a safe environment by rehearsing worst possible outcomes in an attempt to diffuse the power of such a possibility might lead to the occurrence of the worst possible situation through their psi ability. If there is any consistent view about psi in people with such personality characteristics it is one of apprehension and fear. Might this translate to reliable results in the laboratory, such as significant psi missing?

Spontaneous Psi in Mass Mythology, Media and Western Culture. Keith Harary

Keith Harary addresses the very real problem associated with the tendency of film and television fiction to characterise people who have apparent psi experiences in a negative, stereotypic, way that is potentially harmful both to the development of parapsychology and more specifically may inhibit individuals in a clinical setting from discussing aspects of their experience that have caused them concern.

The paper is a comprehensive and thoughtful discussion of the problems posed by the negative stereotypes generated by the media, concluding that psi researchers should be aware of the effect of these stereotypes on their own thinking, and reasons for conducting research in this field, and of the responsibility of researchers to encourage a more positive mass media perspective towards apparent spontaneous psi. I suggest that, unfortunately, until there are authoritative explanations for these phenomena that in a sense normalise them, there is little hope of individuals or indeed the parapsychological community as a whole changing the media's view of this topic to any significant degree.

Case Studies, Folklore and Investigators: Their Role in Experimental Research. Rex G. Stanford

In this, the last of the presented papers, Rex Stanford evaluates studies of ostensibly spontaneous psi, the paranormal in folklore and unexplained interactions in the laboratory. He suggests that case studies of anomalous interaction fulfil several roles. He discusses examples that reflect the belief among many of the early psychical researchers that spontaneous cases, properly studied and evaluated, could constitute important evidence for anomalous interaction.

More recently Ian Stevenson has advocated the careful study of such cases not, as Stanford says, so that terms such as 'conclusive' or 'proof' will be attached to such evidence, but to provide evidence on conceptual or process issues that he feels cannot be addressed, or only with difficulty, in the parapsychological laboratories. Stevenson wishes to establish the authenticity of each report by means of a clear record. It is this latter point that Stanford takes issue with because he feels that such evidence always has considerable ambiguity. However these approaches to reports of spontaneous psi are categorised by Stanford as being 'proof oriented'.

Continuing the view that such cases provide evidence of process related questions, Stanford then discusses Louisa E. Rhine's belief that the role of spontaneous case studies was to provide a unique source of hypotheses that could broaden the basis of research. She felt that the justification for such work was 'the derivation of hypotheses' which necessarily required experimental investigation.

Yet a third role, which Stanford terms 'the pragmatic approach', permits quantitative testing of competing hypotheses using spontaneous case collections with no a priori assumption that psi is present. Here Stanford looks at the extensive work of Sybo A. Schouten using a scoring formula to assess each case. His approach examines a wide array of hypotheses that might have implications for data within the case collections that he has examined. Among these is the potentially important one that Rhine's unauthenticated cases exhibit the same patterns as the carefully authenticated cases collected in *Phantasms of the Living* (Gurney, Myers, & Podmore, 1886). Stanford points out that this brings into question Stevenson's claim that lack of authentication by investigators in the Rhinean approach is very dangerous and can lead to serious error.

Lastly Stanford looks at a class of spontaneous case involving subtle non-intentional or unconscious use of extrasensory information, which Louisa Rhine did not

include in her range of cases, but which constitute the core material for Stanford's psi mediated instrumental response or PMIR model. He delineates the assumption that he sees in virtually all parapsychological literature that the implicit objective of extrasensory functioning is always to communicate to consciousness information about the target situation. He suggests this is an arbitrary and unreasonable assumption and that, contrary to that assumption, it seems obvious that the maximally adaptive response, in a sense the most efficient one, is one that allows one to do what is important in relation to the circumstances, and whether one consciously is aware of it is of lesser importance. This is not to say that given time and attention the reasons for taking one action rather than another would not be recognised by the individual but that there is no need to attend to achieve a useful outcome. Again Stanford does not seem to be using the term unconscious in the formal dynamic sense of depth psychology but more in the inattention sense.

The section of Stanford's paper that I found most challenging was his discussion of the concept of faith. In discussing the hypotheses that may be derived from this concept, particularly the possibility that when a person gives a command with no inward doubts about it being efficacious, but believes that it is happening, it will in truth happen, and that whatever will increase that belief or conviction should aid the process, he is raising again the point made by Keil that 'when we try to increase our movements by smaller more 'believable' steps, progress was more clearly noticeable.' (p.16) In addition to faith, Stanford speculates about expressiveness (such as levitation during religious ecstasy) and vicarious suffering, intended mainly to show that a closer linkage of empirical research with some prevalent ideas in the folklore of the paranormal has the potential to yield insights that would challenge 'our present rather laboratory bound and restrictive theories'.

Conclusions

The reader will have noted that there is a theme to my criticism of many of the papers presented at this conference. Put broadly it is similar to B.F. Skinner's view about the utility of research into cognition in the 1950s and 60s. One would not wish to deny the part that unconscious motives etc. may play in human functioning (including psi), but given our present level of knowledge it is not the most efficient way of researching the phenomena.

The proponents of a depth psychology interpretation of psi experience, most notably Eisenbud (1970), Ehrenwald (1977), and Ullman (1975), have mainly used such theoretical interpretations in post-hoc analyses of possible psi events, most frequently in those involving dream material. While Ehrenwald maintains that psychoanalysis lends itself to rigorous scientific research this has not been my experience nor that of many psychologists in the last 30 years.

Harary's concern about the subject being mistreated by the media will only be less justified when the man in the street believes that there is a reliable phenomenon to be examined. As yet, we cannot reliably elicit, or have an explanation for, the most evidential psi such as macro-PK. I contend that it is incumbent upon us not to move to more complex levels of explanation involving depth psychology and unconscious processes until the less complex levels of explanation are exhausted. In the papers presented at the conference Keil, Isaacs and Stanford allude to factors in human behaviour (such as belief and performance anxiety) that appear both to be relevant and offer some scope for operational definition.

Several of the papers in this volume seem to me to represent an degree of theoretical self indulgence. Stanford makes the case when he says that if the laboratory is to be the testing ground for hypotheses developed from case studies, then considerable care must be exercised to actually

develop true hypotheses: 'in short the investigator must not simply try to generalise a finding from case studies to the laboratory but must create a true tentative explanation of that finding and then deduce predictions from it that can be tested in the laboratory' (p.234). Later on he continues: 'in an area that supplies as little in the way of unambiguous data as do case studies it is tempting to find ways to dismiss alternative interpretations even when they should not be dismissed, the very uncertainty present in the data invites premature closure ... The moral of this story is that experimentalists wishing to test hypotheses built around case studies already in the literature would do well to consider, in searching for possibilities, not only the conclusions and interpretations favored by the case study authors, but to go back to the authors' data and methods in order to examine the soundness of their conclusions and to unearth alternative hypotheses that might have been prematurely dismissed' (p.236).

This urge to premature closure and appeal to explanations that are virtually untestable (in that they involve elements that are so tenuous and ambiguous as to actually prevent the failure of a model) is, I suggest, a style of enquiry that parapsychologists should avoid.

The resources available to parapsychological research are few. It therefore is incumbent on us to use these resources, including experimental hypotheses, in the most parsimonious manner, assuming no more causes or forces than are necessary to account for the facts. Research using techniques such as macro-PK induction using Batchelder sitter groups, or the Ganzfeld, introduced into parapsychology by the late, and much missed, Chuck Honorton and by William Braud, suggest that psi research paradigms exist that allow manipulation of operationally defined independent variables without, at this stage of knowledge, involving variables that are notoriously difficult to define in operational terms.

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Book Review:
Schulwissenschaft, Parawissenschaft, Pseudowissenschaft
[Mainstream Science, Parascience, Pseudoscience]

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A review of *Schulwissenschaft, Parawissenschaft, Pseudowissenschaft [Mainstream Science, Parascience, Pseudoscience]*, edited by Gerald L. Eberlein. S. Hirzel/Wissenschaftliche Verlagsgesellschaft, Stuttgart, Germany, 1991. 186pp. DM 29. ISBN 3-8047-1168-5.

This is the first German language reader dealing with the thorny problem of how to describe similarities or dissimilarities between mainstream sciences, parasciences, and pseudosciences. Edited by Gerald L. Eberlein, a Munich sociologist, this collection comprises four areas of investigation: (1) dowsing; (2) homoeopathy; (3) parapsychology; and (4) astrology. The volume contains 13 articles. Six of them are written by 'critics' (posing as representatives of 'Schulwissenschaft'), five are authored by 'proponents' (posing as representatives of 'Parawissenschaft'), and two contributors take a somewhat 'neutral' stance regarding the basic issue of the volume.

In his introduction (pp. 7-21), the editor makes a rather superficial distinction between 'mainstream sciences' (comprising 'problems, theories, methods and results accepted and approved by academic research communities'), 'parasciences' which, viewed from the sociological perspective, exhibit social structures and processes comparable to those of orthodox sciences although their claim to be 'real' science ('Wissenschaftsanspruch') is still under dispute, and, finally, 'pseudosciences', which, viewed from the epistemological perspective, cannot be accepted (or accepted any longer) as 'established' (in German: 'anerkannte') statements, theories, methods or research programmes. Eberlein's intention as editor of this volume is not just to 'debunk' para-

scientific claims in a CSICOPian manner, but instead to facilitate more 'zetetic' dialogues between 'orthodox science' and 'parascience'. That means, in his own words:

...listening to the para-scientist, having a close look at his/her activities in order to figure out to which criteria, that is norms of scientific behaviour, he/she [the parascientist] is adhering to. (p. 9)

Eberlein then summarises the debates to follow and gives his personal evaluation regarding the problem if dowsing (or radiaesthesia), homoeopathy, parapsychology or astrology are really dealing with genuine anomalies that cannot readily be explained by 'orthodox' or 'conventional' models.

The first chapter on 'dowsing' comprises three articles that deal mainly with a research project on dowsing carried out by a Munich group of experimental physicists directed by Hans-Dieter Betz and Herbert L. König. The project, sponsored by the German ministry for research, achieved a high degree of public visibility and was chosen as a primary target by a newly founded group of German sceptics called GWUP (Gesellschaft zur wissenschaftlichen Untersuchung von Parawissenschaften [Society for the Scientific Investigations of Parasciences]). The contribution of Horst

Löb (pp. 23-44), a physicist from Gießen University and a member of GWUP, presents the usual sceptical outlook of a natural scientist when confronted with claims such as 'earth rays', 'radiaesthesia' or 'geopathy'. He gives a useful summary of what is known in physics on electromagnetic fields and makes some critical remarks on the validity of the Munich study. Its authors, Betz and König (pp. 53-70), give an outline of their study and take the German sceptics group to task for their unfair behaviour. For Betz and König, there is no doubt that some dowzers show reproducible bodily reactions that are both mainly location-dependent and inexplicable by recourse to known normal sensory perception of stimuli. Nevertheless, in their opinion, future attempts at explanation of the dowsing reaction should build from a basically conventional biophysical stimulus-response model. A further step in that direction is a careful documentation of the dowsing results with one selected subject, an engineer, who is apparently able to detect underground water with an astonishing success rate (for details see Betz, 1991). There is also a shorter contribution by Raimund Kaufmann (pp. 45-51), a clinical physiologist from Düsseldorf University, who analyses the rhetoric of the controversy surrounding the Betz-König project (emotional dichotomy between 'believers' and 'doubters') and criticises some of the implicit assumptions of the 'Tugendwächter' ['guardians of virtue'] of the medical scientific establishment in Germany.

The next chapter is on homoeopathy and comprises three contributions. Karl-Heinz Gebhardt, a specialist in internal medicine and practising homoeopathic doctor, discusses in a lucid way the merits and shortcomings of homoeopathic therapy within the hierarchy of therapeutic approaches (pp. 71-82); for him, there exist clear-cut experimental proofs for the clinical efficacy of homoeopathy, a conclusion that is disputed by the following article written by Evarudus J. Ariens, a Dutch professor of pharmacology and toxicology (pp. 83-95). He argues that homoeopathy can be

compared with 'paranormal' healing: what works if it works is just a placebo effect. In his opinion, there are no 'scientifically acceptable controlled clinical studies'. Nevertheless, Günther Harisch and Michael Kretschmer, two physiologists working in the field of veterinary medicine at Hannover University, tested the enzymatic reactions of rats by administering a homoeopathic substance; in their rather technically written article (pp. 97-107) they present some preliminary biochemical data pointing to the fact that homoeopathic treatment of animals can indeed cause some objectively measurable effects. In principle, their article is a plea that basic research into homoeopathy should follow the usual rigorous approach of the natural sciences.

The third chapter is devoted to parapsychology proper and consists of three articles. The editor, sociologist Gerald L. Eberlein, proposes in his article (pp. 109-117) ten hypotheses that should characterise 'mainstream sciences' in contrast to 'parasciences' (for an English version of his approach see Eberlein, 1993). In general, his evaluation of parapsychology is rather negative. He maintains that parapsychologists believe in 'psi phenomena' in a way comparable to cultists. In my own contribution (pp. 131-146), written as a direct rejoinder to Eberlein's theses (when applied to psi research), I tried to give a balanced picture of the current status of parapsychology and to cite some promising research directions. My main point is that for doing empirical work in parapsychology with established methods you need not adopt a 'new' paradigm. Eberlein, in his position as editor of this volume, adds a short postscript (pp. 147-148) where he maintains that I failed to understand his 'science-sociological research hypotheses' by adopting the position of the Parapsychological Association (which mirrors the orthodox norms of a research community) and he ends by citing Hyman's credo that parapsychology (despite its 130 years of history) has failed so far to present convincing evidence to the scientific community. In this reviewer's opinion, Eberlein exhibits a

remarkable unwillingness to come to grips with the technical literature of experimental parapsychology. There is also a somewhat incoherent and sketchy article (pp. 119-129) on the history and subject-matter of parasciences written by the Austrian clergyman Andreas Resch, Chairholder for paranormology and clinical psychology at Lateran University in Rome (Resch prefers to use the term 'paranormology' instead of 'parapsychology').

The final chapter deals with astrology. Bernulf Kanitscheider, a philosopher of science from Gießen University, has an easy task to show (pp. 149-162) that there are a lot of empirical and theoretical arguments undermining the claims of astrology when taken at face value (e.g., that stars could 'influence' our personality or character in a causal way); astrologers, at best, can have a modest function as 'good amateur psychologists' counselling their clients but they should give up their claim to be 'scientific'. There is a rejoinder written by the psychologist and practising astrologer Peter Niehenke (pp. 163-177). In his opinion, Kanitscheider failed to show a deep understanding of the essentials of astrological thought. Astrology, in fact, is no science, but some of its statements are nevertheless 'true' in an empirical way. Astrology should rather be compared with 'altes Menschheitswissen' ('ancient knowledge of mankind'). It does not come as a surprise when Kanitscheider, in his final comment (pp. 179-186), seems unimpressed by Niehenke's arguments. For him, astrology remains a pseudoscience: it offers solutions only to such problems that do not exist without astrology itself. There is no gap in our knowledge that could be closed only by astrology.

On the whole, the book is a good illustration of what Daryl Bem observed when he remarked in his review of *Advances in*

Parapsychological Research, Vol. 5: 'As with many other controversial areas of research... one's evaluation of the data depends heavily on one's a priori beliefs' (Bem, 1989, p.649). So, one possible benefit for the uncommitted reader could be that he or she could become more conscious of his or her own 'belief systems' when confronted with 'unorthodox' scientific claims. Although the Eberlein anthology offers no in-depth treatment of the demarcation issue comparable, for example, with Patrick Grim's *Philosophy of Science and the Occult* (Grim, 1990), it is, at least for the German reader, a welcome addition to the serious literature dealing with the epistemological intricacies that beset our field.

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Book Review:

Jenseits von Licht und Schatten: Die Rolle der Photographie bei bestimmten paranormalen Phaenomenen - ein historischer Abriss [Beyond Light and Shadow: The Role of Photography in Certain Paranormal Phenomena - A Historical Extract]

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A review of *Jenseits von Licht und Schatten: Die Rolle der Photographie bei bestimmten paranormalen Phaenomenen - ein historischer Abriss [Beyond Light and Shadow: The Role of Photography in Certain Paranormal Phenomena - A Historical Extract]* by Rolf H. Krauss. Jonas Verlag, Marburg/Lahn, Germany, 1992. 200pp., 78 illus., DM48. ISBN 3-89445-122-x.

This excellently produced book - it received the KODAK-Fotobuchpreis 1992 (a prize, sponsored by KODAK, for outstanding photographic books) - is the first and rather comprehensive history of paranormal photography in the German language. Its author, an expert in the history of photography, displays a remarkable scholarship in mastering a heterogeneous bulk of material published in both well-known and obscure German, French and English journals, books and pamphlets, often going back to the late 19th century. Dr Krauss acknowledges in his foreword the support of the Freiburg Institut für Grenzgebiete der Psychologie which made available its excellent library holdings with regard to the early history of spiritism and occultism in European countries.

In his introductory remarks the author observes that there were marked parallels between the historical development of photography and of psychical research, and then refers to the difficulty of organising the contents of his book. He discusses the possibility of differentiating between 'anomalous' effects that can only be demonstrated by using a photographic plate (in the sense of Reichenbach's 'odid' force,

Fukurai's 'thoughtography' or Kirlian's technique) and the application of photography to prove the existence of a certain phenomenon (for example Mumler's 'spirit photography'). In the latter case, only photography can 'create' the phenomenon in question. In the course of his studies, however, Dr Krauss decided to organise his material under two headings: 'Photographs of radiation phenomena' and 'mediumistic phenomena'. The author's personal position vis-à-vis the unusual topic of his book is strictly agnostic. He has deliberately chosen the neutral attitude of a historian. In his own words: 'My intention was to present the material as objectively and dispassionately as possible by taking into account the circumstances that argue in favour as well against the authenticity of a certain phenomenon or group of phenomena' (p.12).

The book is divided into three main parts: 'Photography and the Paranormal' (pp. 14-20); 'Photography of Radiation Phenomena [*Strahlenphänomene*]' (pp. 21-98); and 'Mediumistic Photography' (pp. 99-193). There is a useful index and, of course 78 mostly rare pictures carrying the flavour of sometimes bizarre, sometimes astonishing, and sometimes amusing

examples of paranormal photography. Two of my favourite pictures are on pages 33 and 34: 'Electrography of two apparently befriended living larvae of the cockchafer', and: 'Electrography of two hostile Wieners' (sic!).

The chapter on *Strahlenphänomene* deals with Mesmer's animal magnetism and Reichenbach's odic force and its strange applications such as the *Odoskop* [Odometer]; there are paragraphs on 'effluviographic' or 'electrographic' effects on plates', followed by Kirlian photography, Darget's V-rays (V stands for 'vital'), Baraduc's 'iconography' and Ochorowicz's 'ideoplastic' photography (especially his experiments with Stanislaw Tomczyk); there is a rather detailed discussion of Fukurai's 'nengraphy' and, its modern counterpart, Jule Eisenbud's work with Ted Serios including the *Popular Photography* controversy from 1967.

The last chapter is on 'mediumistic photography'. There are paragraphs on the beginnings of '*Geisterphotographie*' (photographs of spirits) citing the work of William H. Mumler and John Beattie; the never-ending story of Katie King and Sir William Crookes is told; there are amusing lessons on how to fake 'extras' and on the gullibility of the public (Hudson, Buguet); Albert v. Schrenck-Notzing and his work on '*Materialisationsphänomene*' are duly cited, and there is, finally, a discussion of the case of the Cottingley fairies.

It is, of course, impossible to summarise such diverse and controversial materials within the space of a review. But for the interested reader who wants to dig deeper

the author has provided more than 460 footnotes citing mostly early German and French sources; they reflect also the amount of archival work necessary to write such a book. It is a pity that Dr Krauss decided to finish his book a little bit abruptly. I had hoped for something like an epilogue reflecting the historical role of photography against a broader intellectual or cultural background especially when dealing with anomalous or fringe claims. For it is an interesting lesson to learn that, apparently, the controversy surrounding the paranormal couldn't be solved - against the hopes of Schrenck-Notzing and other pioneers of psychical research - by using 'objective' techniques.

In summary, then, I would recommend this book as an excellent guide to the primary source material pertaining to the often curious history of paranormal photography. It is clearly written, sensibly argued and makes entertaining reading both for the general reader as well as for the professional parapsychologist interested in that particular field of historical scholarship. For our Anglo-American colleagues surely one more good reason to learn German!

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Book Review: *Parapsychology: The Controversial Science*

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A review of *Parapsychology: The Controversial Science*, by Richard Broughton. London: Rider, 1992. Pp. vii + 408. £10.99, pbk. ISBN 0-7126-5374-0. (Published in 1991 in New York by Ballantine Books, \$22.00 hbk, ISBN 0-345-35638-1).

Relatively few parapsychology books have been written by active experimental researchers. We may publish in journals, but tend not to find time to prepare readable yet responsible surveys of modern parapsychology. Fortunately, Richard Broughton has become one of the exceptions, being granted sufficient time off from his usual duties as Director of Research for the Institute for Parapsychology to write what is surely one of the best introductions to serious parapsychology for non-professionals in many a year. Although primarily aimed at a general audience, it offers new information and fresh insights for those already familiar with the field as well, especially those less familiar with American research. It concentrates mainly on experimental approaches, which is more than excusable given the paucity of information on actual experimental procedures and findings in most popular books.

In a brief introductory section, Broughton attempts to set the tone for the rest of the book by differentiating between experiences and the explanations of those experiences. Parapsychology is described as studying a set of anomalous experiences and events, anomalous in the sense that science at present seems unable to explain them. The task of parapsychology is to explore those experiences and the evidence bearing upon the ways they can be interpreted. Broughton takes pains at the start to paint a picture of parapsychologists ideally not as believers but as researchers,

approaching their work with a scepticism that is healthy rather than corrosive. Given the range of approaches taken on these issues within the parapsychological community, the novice reader may acquire a false sense of the degree to which this approach is truly held unanimously by all who appear in public labelled as parapsychologists.

Chapter 1, 'What is a Psychic Experience?', surveys the diversity of experiences that have been regarded as psychic, emphasising their wide distribution in the public as a whole rather than focusing on the more popular notion of psychic ability as something present only in a small number of 'gifted' people.

In surveying the range of psychic experiences, Broughton essentially relies exclusively on the writings of Louisa Rhine. During a 30-year period, 1948 to 1978, she studied the cases reported by correspondents to the Duke/FRNM laboratory, numbering almost 14,000 cases in all. Her approach was to rely on the good faith of large numbers of respondents rather than selecting a smaller number of initially impressive cases and following them up with more systematic investigation. She reasoned (a) that problems with individual cases would even out given the large numbers such that any patterns in the cases would still have validity, and (b) the whole purpose of the case collections and analyses was to uncover patterns that could then be evaluated by experimental studies, so the

patterns mattered rather than the (inevitably imperfect) evidential nature of any individual case. Broughton introduces us to the main categories of Rhine's cases and presents examples of each.

Broughton notes that Rhine did do some screening of her cases, rejecting almost half of them as having insufficient face validity for one reason or another, but his discussion of 'separating the normal from paranormal' is brief. He also acknowledges that Rhine's general approach has been criticised but gives the reader little feeling for the issues involved or for alternative strategies of more intensive spontaneous case investigation such as those of the European psychical research societies, the Utrecht work of Schouten and colleagues or the American work of Stevenson on reincarnation cases and Roll on poltergeist investigation, for example.

The reliance on patterns is problematic in its own right, as certain flaws may covary with other case features, there may be systematic biases in who reports and what they report, and so on. Given that the collection is composed of material from people who were motivated to write to the Rhines, generally by exposure to their literature or consequent media coverage, then any selective public coverage of one sort of case more than another may well lead to a flood of cases that resemble those mentioned publicly. If I read an article in a national magazine about ESP in dreams, I'm more likely to write to you if I've had that sort of experience rather than others; if I've had several kinds of experiences I'll emphasise those that occurred in dreams because I know you're interested and because your article may have selectively prompted me to remember them.

Finally, given that Broughton is interested in patterns rather than simply the evidential aspect of ostensibly strong individual cases, it is doubly unfortunate that he has not discussed some of the case collections that have been investigated in considerable depth, because that very descriptive depth itself often uncovers rich informative patterns.

Broughton next introduces one of the most important topics for a popular book, the role of mental disorder in ostensibly psychic experiences. Although some therapists are supportive, many seem to assume that frequent reports of psychic experiences indicate mental dysfunction, and regard parapsychologists as reinforcing many delusional systems. The author notes that most people who report psychic experiences show no real sign of mental disturbance and that the delusional systems of the florid schizophrenic are fairly obvious. Unfortunately there is a considerable middle ground, and this general topic needs to be covered in much more detail, especially in a book aimed at a general audience.

The final topic is psychic experiences in other cultures, which is covered in a bit more detail, including some concrete examples. But once again the lay reader would like to know much more, especially one who has done reading in some of the more speculative popular books or who has some familiarity with cultural systems that are supportive of psychic experiences. It is a question of more interest, perhaps, to a non-American readership, as is evidenced by, for instance, the different approaches to psychical research in various European countries.

Chapter 2, 'Mapping the Territory', builds on the previous chapter by presenting in more detail what modern parapsychology is, what topics it does and does not study, where the main research centres are and how parapsychologists go about their work. The approach concentrates on ESP and PK as the core phenomena, while noting that various classes of evidence for survival, including transpersonal experiences, can also be included. Astrology, UFOlogy and cryptozoology are clearly excluded, but the reader is given little basis for understanding why parapsychologists tend to draw the line in certain places and not others. Coverage of the major research centres is written from the perspective of the original Duke Laboratory, and one gets the impression that research is conducted only in the US and Europe.

In describing the methods of parapsychology, case studies and field investigations are presented with a bit more discussion of their methods and the debates about their limits, but most emphasis is placed on experimental methodology as the source of real scientific progress. The introduction of experimental methods is almost solely via Rhine's early card guessing work at Duke, with mention of some earlier British restricted choice work in one sentence, no note of the early experimental work on continental Europe, and no coverage of other procedures.

Chapter 3, 'Origins of the Science', attempts to set modern parapsychology in conceptual context. It begins by describing the presence of claims for special powers or miracles within religious systems, and includes a section on the lives of saints to illustrate the often ambivalent attitude of religious hierarchies towards claimants of special powers. There follows a description of secular psychical research with brief but reasonable coverage of mesmerism, spiritualism (the Fox sisters), mediumship (physical, specifically D.D. Home) and the early days of the British SPR. Early 20th century work in Holland, France and the US is mentioned in one sentence and Germany goes unmentioned. The last third of the chapter concerns the early days of the Rhines at Duke, from 1927 to 1940.

Chapter 4, 'Why Does the Controversy Continue?', is a short chapter on the nature of criticism of parapsychology, both conceptual and methodological, focusing on the rise of organised 'sceptical' groups, specifically CSICOP in the US. Broughton provides a useful summary of some of the main kinds of criticism and takes such groups to task for serving too much as advocacy groups rather than concentrating on bona fide objective scientific assessment of claims. He scores good points, but occasionally seems to adopt a somewhat rhetorical tone himself. This chapter would probably have been more useful much later in the text, when the reader has sufficient familiarity with the nature of the research itself to appreciate the appropriateness (or

lack of same) of various sceptical arguments.

Chapters 5 and 6 are the longest in the book, longer together than all the material that preceded them. Chapter 5, 'Contemporary Extrasensory Perception Research', describes several areas of research that have gained some prominence during the last 25 years. The areas covered are dream research, specifically the Maimonides work; research on the Ganzfeld and personality patterns, particularly the work of Charles Honorton's group, the Institute for Parapsychology, and Carl Sargent (the last being the only non-American work described); remote viewing, primarily work at the Stanford Research Institute but also the transcontinental study of Marilyn Schlitz and Elmar Gruber; precognition work with random number generators, primarily Helmut Schmidt's at the Institute for Parapsychology and elsewhere, plus one study by Charles Honorton with Malcolm Bessent, who had also performed well in a precognitive dream study reported earlier in this chapter; a study of free response ESP within a therapy group context by James Carpenter, a clinical psychologist associated with the Institute for Parapsychology; and a study by Edwin May and Dean Radin at SRI International (as the Stanford Research Institute is now known), to look for precognition of RNG decisions, under conditions designed to prevent interpretation of the results in terms of psychokinesis.

This chapter is thus not so much a survey of ESP findings as a sampler of some of the major lines of research plus a handful of special studies used to illustrate some of the conceptual issues in modern ESP research. Much of the focus is on research carried out at the Institute for Parapsychology. The Ganzfeld findings on personality patterns, a main focus of the Institute's work, are covered nicely, emphasising that there is considerable consistency between labs in the patterns observed, and noting that this line of research provides further evidence that psychic ability appears to be widespread.

However, Ganzfeld findings of equal value by others, such as the difference between dynamic and static targets or the relationships between sender and receiver, go unmentioned. In introducing the different kinds of experiments, Broughton initially presents a vignette of a typical experimental session, complete with external and internal dialogue, much in the style of a fiction writer. I found this a mixed blessing, because it gave the reader a better feel for what an actual session might be like, yet the dialogues often sounded artificial, too much like a fictional re-creation.

Chapter 6, 'Contemporary Psychokinesis Research', is presented in much the same way. We start with research on macro-PK with talented individuals, specifically Felicia Parise and Nina Kulagina. The possibility of fraud by Kulagina is discussed but rather summarily dismissed without giving the reader a very full picture of the possibilities and how one might evaluate evidence for them.

Next come group efforts such as the attempts by George and Iris Owen in Toronto to construct the imaginary ghost Philip; the work of Colin Brookes-Smith and Kenneth Batchelder in exploration of group psychology in reducing resistance to the production of macro-PK phenomena; and the PK metal-bending groups that flourished in the late 70s and early-mid 80s. The last topic leads to a discussion of the work with Uri Geller and the 'Geller clones' or 'mini-Gellers', generally children, who mimicked his apparent metal bending abilities. The final section on macro-PK deals with some of the remarkable claims coming from China, drawing primarily from translated publications giving only partial details.

Broughton then covers micro-PK, particularly Schmidt's PK work with RNGs as it evolved from his precognition work, including the tricky topic of retroactive PK procedures and how they can be used to bring sceptical observers into the experimental situation.

Observational theory is described and, although his coverage will be regarded by cognoscenti as fairly superficial, Broughton

in my opinion does a good job of introducing OT in terms understandable by lay people. The Princeton Engineering Anomalies Research (PEAR) research on micro-PK under the direction of Robert Jahn is introduced, and the final study covered is Broughton's own work with James Perlstrom on micro-PK in a simulated competitive situation. It is extended to resemble a daily life circumstance in which people have come to regard themselves as lucky or unlucky, and is referred to as the Applied-Luck Project.

Broughton is clearly more comfortable when dealing with micro-PK procedures, which can be more tightly controlled against trickery and can readily lead to systematic research, varying both physical and psychological conditions. He describes micro-PK as the most robust line of research in parapsychology today and suggests that it may have much to offer theoretical physicists as they wrestle with the problems of the role of the observer in quantum systems. Yet like most of us, he is clearly fascinated by the macro-PK evidence, with its double or nothing aspect. Either it's the most dramatic form of evidence for psi, bypassing all the vagaries of statistical inference and allowing a host of tidy projects studying its physical properties; or it's a collection of clever tricks, malobservation and non-obvious application of known physical principles. Broughton covers some of the alternative interpretations, including fraud, but seems occasionally to slide over them a bit too quickly. His summing up is balanced and he takes us to task collectively for not pursuing the various forms of macro-PK with more vigour.

Chapter 7, 'Real Ghostbusting', discusses field investigations of hauntings and poltergeists. As with the experimental work, Broughton presents dramatisations of actual cases. First comes a recent (if unspectacular) case investigated by SPR investigators Alan Gauld and Tony Cornell, followed by a detailed description of an early, more dramatic SPR investigation, the Cheltenham case. The scene then shifts to the US for a description of the studies by

Gertrude Schmeidler and Michaelen Maher of sensitives versus control groups taken through purportedly haunted houses to record their impressions, plus investigations of the infamous 'Amityville Horror' case, now generally regarded as completely fraudulent.

Having introduced us by example to various aspects of field investigation methods, Broughton introduces in lucid fashion the complex issue of 'super psi' versus survivalist interpretations. He distinguishes between hauntings and poltergeists, focusing on RSPK (recurrent spontaneous psychokinesis) interpretations of such relatively modern cases as Hans Bender's Rosenheim case and William Roll's Miami investigation. The latter is covered in some detail as an example of Roll's systematic methodology for detecting patterns in object movements. He suggests that although earlier poltergeist investigations appeared to favour survival interpretations, later ones were more suggestive of RSPK, perhaps as a result of the ways the investigations were conducted. Finally, he shares with us some details from a quite recent Polish poltergeist case not yet reported in the parapsychological literature. In his summary, Broughton is able to conclude that such experiences and events are quite unlikely to be explained conventionally, but at present no specific parapsychological interpretation yet has firm evidence behind it.

This is the third longest chapter in the book and the coverage is fairly good considering that the author's own work has been purely with experimental methods. Those familiar with field methods will have wanted more coverage and greater description of the patterns that have emerged in some of the research, and lay readers may have been looking for comments on some of the more popularly known field investigations. But the reader has been given a feeling for some of the styles of investigation plus the main methodological and conceptual issues.

Chapter 8 is 'Life After Death' and considers the remaining major forms of evidence bearing on whether we survive bod-

ily death. Little coverage is given of mediumship, mental or physical, perhaps because the focus is on relatively recent research and Broughton states that current serious research with mediums is virtually nonexistent. More attention is given to research on out-of-body and near death experiences (OBEs and NDEs) both acknowledged as legitimate areas of study outside the parapsychological community as well. OBE experimental research at the Psychical Research Foundation and American Society for Psychical Research are discussed in some detail, including the strategies for comparing super-psi versus survivalist interpretations. For NDEs the survey studies of Kenneth Ring and Raymond Moody receive the most specific coverage, along with various alternative interpretations.

The last form of evidence considered is reincarnation, concentrating on Ian Stevenson's extensive case collection. One Indian investigation is presented in some detail to illustrate the alternative interpretations and how they can be dealt with. Broughton concludes that the evidence for survival is positive and impressive, but not yet adequate.

In general, this chapter like the last one provides the lay reader with a feeling for many of the issues involved in collecting and interpreting material from spontaneous cases and field investigations. But it focuses more on the evidence for survival from various kinds of transpersonal experiences, with too little attention on certain other important areas such as mediumship and apparitions.

Chapter 9, 'Adding It All Up', shifts us back to the experimental data, looking at meta-analyses of groups of similar experimental studies. Formal techniques for combining the outcomes of several studies, or meta-analyses, have been known in the social sciences in one form or another for quite some time. They were introduced to parapsychology in the mid-1980s by Charles Honorton and Ray Hyman in their debate over the Ganzfeld studies. Although still controversial in some respects, they now form a major part of the modern

parapsychological scene and the way researchers regard their data. Meta-analyses emphasise the size of effect in each study, taking into account both the statistical significance of an outcome and the number of trials that contributed to it.

Such techniques can help us estimate the average size of effect present in a large set of studies, such as all dice-throwing studies, or a smaller set, such as only Ganzfeld studies. We can assess the overall level of significance for the available data base and estimate how many unpublished studies would need to be added to the data base to render it no longer significant. Given an overall effect size, we can estimate how many trials we should include in any replication attempt in order to have an acceptable likelihood of obtaining statistical significance. It is also possible to assess how homogeneous the results of several different studies are. If different experimenters produce effect sizes that are quite homogeneous, then that effect size can be said to be replicable.

By looking at other variables from study to study, we can assess which factors appear to correlate with more successful results. This includes analyses of which methodological flaws, if any, correlate with effect size. If the presence or absence of a given flaw appears not to affect results, then that flaw may not be so important after all. Any flaw that does correlate with effect size assumes much more importance.

By looking at what experimental characteristics are most related to effect size, we come to have a far clearer picture of what our results have been telling us than has been the case in the past, when we merely tallied the number of significant and non-significant studies. Most importantly, perhaps, we can build upon this information to design and conduct better studies in the future, thus rendering parapsychology truly a cumulative science.

After introducing us to the intricacies of meta-analysis, Broughton goes on to present the findings of several recent examples, including the Honorton and Hyman Ganzfeld meta-analyses, their differences and similarities; the Honorton automated

Ganzfeld follow-up, designed to address the questions raised earlier; the Radin and Nelson analysis of micro-PK studies; the Honorton and Ferrari survey of forced-choice precognition studies; the Radin and Ferrari assessment of dice PK studies; and the evaluation of the relationship between extraversion and ESP performance by Honorton, Ferrari and Bem.

In many ways, this chapter is Broughton's best, in that he takes a complex but important area, somewhat novel even now within the parapsychological community, and presents it carefully and clearly in a manner suitable for the lay reader as well as the academic. He covers many of the basic issues involved, although more could have been said about some of the problems involved in applying and interpreting meta-analysis; it isn't quite yet the definitive 'controversy-killer' that Broughton declares it to be.

When describing the recent Honorton automated Ganzfeld series, Broughton emphasises the real overall results but does not comment on some of the additional internal findings such as the predicted efficacy of dynamic over static targets. Such findings are even more troublesome for the critics and make the results additionally informative.

Although I see the sense in having a separate chapter for the results produced by meta-analyses, in many respects it might have been better to integrate these results with those of other studies discussed earlier. As it is, the reader is given relatively little sense of how the various groups of findings integrate with each other, of what meaningful patterns are emerging from our data. To what extent, for instance, are the kind of noise reduction models proposed by Braud or Honorton, or the various observational theories, actually supported by the data? In short, are we learning anything *about* psi?

Chapter 10 is rather coyly titled, 'Are We Ready for Applied Psi?' In it we are introduced to various projects in which researchers have attempted to explore various potential practical applications of psi. If psychic functioning is real, as is increas-

ingly confirmed by the data, then it presumably has uses. Many spontaneous cases involve considerable benefit to the experimenters; as Broughton reminds us, the daily life concepts of intuition and luck both potentially have psi components.

Although little controlled research has been done in direct applied areas, Broughton does cover several areas where beginnings have been made. Research on psychics in detective and police work has been conducted with varying sophistication in many countries, as reviewed by Arthur Lyons and Marcello Truzzi in *The Blue Sense*. Various attempts to relate psychic functioning to intelligence and military applications are described, including work at SRI International such as Project SCANATE, and the controversial survey of parapsychology conducted by the National Research Council for the US Army.

Research on healing is given relatively brief coverage, focussing on William Braud's work on remote influence over biological systems, work by Hans Attevelt on non-psi factors in alternative healing practices in Holland, Bernard Grad's work on 'laying on of hands' effects upon biological systems, Chinese exploration of qigong techniques, plus some mention of the research of Douglas Dean and Graham and Anita Watkins.

The chapter ends with a discussion of data compression techniques to yield more consistent results, Stephan Schwartz's use of psychics in an archaeological context and the now highly controversial Delphi project on predicting silver futures by Russell Targ and Keith Harary. Broughton concludes the chapter on a note of cautious optimism, suggesting that success in an applied context, especially the marketplace, would likely come to overshadow academic endeavours emphasising more basic research.

This chapter is a reasonable sampler of various applied efforts and covers many of the issues that make such work doubly controversial.

Chapter 11, the final chapter, is appropriately titled 'Glimpsing the Future'. In this chapter, Broughton helps us look into

the future, assuming that psi exists and can be harnessed to some extent. What are the likely directions that future research will take and what are the pragmatic consequences of success?

Drawing from his own Parapsychological Association presidential address, 'If You Want to Know How It Works, First Find Out What It's For', he covers several perspectives: the technologist, the evolutionary biologist, the social scientist and the theoretical physicist. We are treated to some speculative scenarios as well as cautionary notes, with an emphasis on the theoretical and practical implications of RNG-PK research.

It is suggested rather strongly that future research, in various societies, will be dominated not by publicly inspectable academic research but rather by private research and development teams within commercial and government institutions. Some areas, such as healing, are seen as necessarily linked with basic research aimed at understanding the nature of psi. Other areas such as survival research are regarded as exciting and worthwhile, but unlikely to lead to a resolution within the foreseeable future.

Broughton's overall tone throughout is unabashedly optimistic, noting that although there have been recent closings of psi research facilities there appear to be others opening up in various countries, including several that do not routinely communicate through mainstream parapsychological journals.

Throughout this chapter Broughton provides considerable food for thought, mixing speculation with down to earth reality testing. I think he underestimates the complexities that lie ahead for us if and as we continue to succeed. There are likely to be strong sources of liberal as well as conservative opposition as society comes to grips with the idea that psychic functioning may actually exist and be capable of influencing daily affairs. There will undoubtedly be exploiters, both charlatans and earnest commercial competitors, and the cast of characters will doubtless change. As has happened in the evolution of many analo-

gous areas, the clever amateur, the diligent dilettante among us will be replaced by the well-schooled, sophisticated professional, for better and for worse. Small units will likely be replaced by larger institutions, as Broughton suggests.

It is interesting that he occasionally cites science fiction passages, then backs away from them. I recall (but cannot reference) a statement by a leading science fiction writer that psychic phenomena are among the hardest topics for such writers to cover, because they can never figure out just what the world would actually be like if people had access to psychic functioning. Time and again the story ends with the psychic powers either melting away and illusory or else becoming so strong that the hero just blows away all the villains, *deus ex machina*. Neither is satisfactory. At one level, Broughton knows this dilemma all too well, and tries to avoid either extreme. Nevertheless the malaise shows through; in truth we haven't the foggiest idea of what the properties of psychic functioning will turn out to be, and Broughton succeeds more in showing us the uncertainties than the likelihoods.

As mentioned at the start, my overall impressions of this book are very positive, despite the occasional problems cited above. It is a clearly written sampler of current parapsychology, primarily intended for the lay reader but of interest to the research community as well. Its perspective is generally American and specifically from the vantage point of Rhine's Foundation for Research on the Nature of Man, where Broughton has spent most of his parapsy-

chological career. This is not necessarily a bad thing, as the perspective is a major one and the research covered very important. It is low on theory but high on results, representing more of a bottom-up than a top-down approach.

Having said this, I should add that Broughton's time spent in Scotland has left him with more awareness of Europe than many of his American colleagues. Out of over 200 references, 41 are British and 16 are to non-English sources, 10 from continental Europe and 6 from China. However his recommended reading list of 60 references lists only works in English, 49 of them American.

In summary, Richard Broughton has written a very readable book that gives the lay public a fair representation of the strengths and weaknesses of modern parapsychology. It is somewhat selective, and could have provided more information to help the lay reader discriminate between legitimate and false evidence. At times the author seems to be doing a bit of cheerleading, but this can serve as a useful complement to the dry complexities and ambiguities of standard academic prose, without resorting at the other extreme to the sensationalism preferred by most popular writers.

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Comment:

Rémy Chauvin comments on the article "'Anglo-Saxon' vs. 'Latin' Parapsychology', by Mario Varvoglis

This very interesting article¹ should perhaps be complemented by the reflections of one of the few French researchers who has conducted parapsychological research uninterrupted over the past 40 years.

1. Varvoglis is right in stating that language is not the only issue at hand. One must note that all French researchers understand written English, but very few can follow spoken English, particularly when it is spoken quickly or with a heavy accent (as, say, in the southern states of the US). There is no such thing as a universally spoken scientific language. Anyhow, how can you hope to understand an English presentation when its contents are difficult to understand even in your own language? The few French who go to American congresses do so mainly because the CNRS obliges them.

As I have already argued, it would be possible to remedy this situation by changing the organization of international scientific meetings. In fact, the solution is quite simple. Papers should not be presented orally, but rather studied prior to meetings; during the meetings themselves the author should respond only to questions that can be readily translated or repeated if not understood. The Americans' response to this idea was that it would be too complicated to implement.

As long as international meetings continue in the same spirit, there's little hope that Europeans will come to them.

2. There's another, more serious problem: experimental psychology, the model

for American parapsychology, is a fundamentally flawed approach. Obsessed by numbers and computation, it has brought few real contributions to our understanding of the human psyche. Where are the discoveries, where are the applications? Where are the breakthroughs, the equivalents of the transistor, or of molecular biology?

In fact, the primary focus should not be upon whether or not a fact has been accurately measured, but rather upon whether or not it is an interesting fact in the first place. The incessant concern about precision has led us to forget that it's of little use to measure what is uninteresting. Unfortunately, American parapsychology has followed in the tradition of experimental psychology. Reading many American parapsychological articles is fatally boring. Too often - not always, fortunately - we come to the conclusion that nothing new has been said since Rhine. What major discoveries or applications have we found? Why have we not succeeded in amplifying psi phenomena? Why have we convinced no one about their reality, in the last 40 years? For the same reasons that experimental psychology interests no one but experimental psychologists. I'm for Lorenz, not for Skinner.

This is why the few psi researchers here in France seek out alternative approaches.

3. Americans' rigid publication standards, and their excessive attention to certain norms, leads us to consider that an American-speaking association is not, in fact, an international association. And this, not only because it is opposed to linguistic pluralism, but also because of its assumptions concerning editorial 'good form' - assumptions we find absurd and dictatorial.

¹ Varvoglis, M. (1990-1991) 'Anglo-Saxon' vs. 'Latin' parapsychology: Behind the communication barrier. *European Journal of Parapsychology*, 8, 41-50.

COMMENT

4. Finally, I'd like to end on a positive note concerning French parapsychology. A major private European foundation has recently succeeded in obtaining funds for

psi research. I've been waiting for this moment for over 40 years.

Le Château
18380 Ivoy Le Pré
FRANCE

Notice:
**Two Swiss Foundations Dedicate their Prizes for
Parapsychology**

Dr. T. Locher, Brugg/Biel
Honorary President of Swiss Association for Parapsychology

As it has been a tradition for many years, the 'Swiss Foundation for Parapsychology', Biel, and the 'Dr. A. Hedri-Foundation', Zurich, dedicated their awards in the University of Berne on 24 February 1993. The 'Dr. A. Hedri-Prize 1993 for Epipsychology' (science for research on consciousness after death) was handed to four German researchers" Dr. Ralf Determeyer, Jochem Fornoff, Hildegard Schäfer and Luise Fuchs.

They deserved this honour for their experiments, technical constructions, publications and other merits in the field of Instrumental Transcommunication. The Prize that was donated was 16,000 Swiss francs. All these people were present in Berne to receive their documents handed by Dr. Theo Locher from Biel, President of both Foundations.

The 'Swiss Prize 1993' of the Swiss Foundation for Parapsychology, amounting to SFr. 10,000, went to the two Italian researchers Dr. Paola Giovetti and Dir. Silvio Ravaldini. They were honoured for their merits in making parapsychology known to the public by their books and articles, by issuing the Italian periodical *Luce e Ombra* and for directing the great documentation centre in Bologna.

After the dedication of the Prizes, Dr. Giovetti delivered a speech on 'Comforting and Healing by Paranormal Means'. The whole ceremony was organized by the Swiss Association for Parapsychology, Berne.

Notice:
1993 Parapsychological Association Convention

The 36th Annual Convention of the Parapsychological Association will be held from Sunday 15th August to Thursday 19th August 1993 in Toronto, Canada. The convention will be held preceding the Annual Meeting of the American Psychological Association in Toronto. For information regarding registration and hotel accommodation, contact Mrs Laura F. Knipe, P.O. Box 797, Fairhaven, MA 02719, USA.

Notice:
**17th International Conference of the Society for Psychical
Research - Call for Papers**

The 17th International Conference of the Society for Psychical Research is to be held at the Ingram Hotel in Glasgow between Friday 10th September and Sunday 12th September 1993. Accommodation and meals will be provided at special concessionary rates. There will be a reduced registration fee for Members and Associates of the SPR.

Any person may submit a paper for possible presentation at the Conference and papers may be on any aspect of psychical research. Because of its proximity to Edinburgh, a major international centre for parapsychological research, the Programme Committee is keen to encourage more experimental and technical contributions than usual this year. However, these may be presented at specialised workshops rather than during the general sessions.

The usual time allocated for the presentation of a paper, including discussion, will be 30 minutes. Speakers should aim to make papers readily understandable to non-specialists, unless they are intended for a workshop, and they should not simply read aloud an article intended for publication. There will be no simultaneous sessions. Because the Conference is a residential one, there will be ample opportunity for informal contact between participants. The Program Committee would welcome suggestions for symposia or round-table discussions that could take place outside the time reserved for presentation of papers.

Summaries of proposed papers must reach the Program Committee before 18th June. These summaries should be single-spaced and have a length of 500-1000 words (at most two pages). They should be neatly typed because, if accepted, they will be photocopied and distributed to the participants at the Conference. Authors whose contributions are accepted will be informed by the end of June.

Summaries of proposed papers should be sent to: SPR Conference, Society for Psychical Research, 49 Marloes Road, Kensington, London W8 6LA, England (telephone or fax: 071 937 8984). Overseas submissions should be sent by air mail. The Conference programme will be enclosed in the July issue of the SPR's *Journal*, or it can be obtained on provision of a stamped, addressed envelope to the SPR after mid-July.

Notice:
5th International Euro-PA Conference

The 5th International Conference of the Euro-PA (European members of the Parapsychological Association) will take place from Friday September 24th to Sunday September 26th, 1993. The venue will be the Clos Notre-Dame, Livry-sur-Seine, Melun, France (about 40 miles south of Paris). Please contact the conference organiser, Mario Varvoglis, if you would like to know further details: Mario Varvoglis, 51 bis, Av. P.V. Couturier, 91390 Morsang S/Orge, France.

Notice:
Revista Argentina de Psicología Paranormal

The Director of the *Revista Argentina de Psicología Paranormal* (Argentine Journal of Paranormal Psychology) announces that *RAPP* has moved to a new headquarters:

Revista Argentina de Psicología Paranormal
Salta 2015
(1137) Capital Federal
Buenos Aires
ARGENTINA.

This move results in more ample office space and better access to library and archives as well as to computer and printing facilities. All correspondence should be addressed to Mr Alejandro Parra (Director) at the above address. As a result of this move, both issues of the *RAPP* (Vol. 3, No. 4, October 1992, and Vol. 4, No. 1, January 1993) were due to come out together in January 1993.

Obituary

Charles Honorton

When Charles Honorton died suddenly, of a heart attack at the age of 46, the science of parapsychology lost one of its most insightful, creative, and productive researchers. Like the great scientists who provided much of his inspiration - men such as Sir John Eccles and C.D. Broad - Honorton was motivated, first, by his determination to keep alive in science fundamental questions that are far from being answered and, second, by his belief, counter to that of many scientists, that empirical methods for approaching some of these questions can and have been developed.

In his writings Honorton identified parapsychology as the field that is attempting to make the problem of psychophysical interaction - commonly called the mind-body problem - an empirical rather than solely a philosophical problem. The fundamental question behind this problem, he said, was whether the brain is the *generator* of mind or the *transmitter* of mind. Parapsychology has developed two approaches to this question: first, to ask whether all information or knowledge is sensory in origin, and second, to ask whether mental events cause, as well as result from, physical events. The first approach examines the phenomena of extrasensory perception (ESP), the second examines the phenomena of psychokinesis (or PK).

The major theme that ran throughout all of Honorton's research and writing was that the findings of parapsychology are not isolated, meaningless data or studies, but instead provide indications of a systematic process underlying ESP and PK (together called "psi"), a process that scientists must

discern if they are to understand psi. His own work, therefore, took two major forms. First, in numerous papers throughout his career, he undertook reviews of all research on a given topic or area, to show both how widespread and how cumulative past research in parapsychology has been (in spite of the discouragingly small amount of support for it). Specifically, beginning in the mid-1980s, he introduced into parapsychology the powerful statistical technique of meta-analysis for conducting such reviews of data. All his reviews led him to argue that much research in parapsychology is as replicable, if not more so, as that in many other areas of the behavioural sciences.

Second, and most importantly, Honorton himself produced hypotheses, research and experimental data that were built systematically on previous work - both his own and that of others. Parapsychological data, spontaneous and experimental, suggest that the activities of mind are more extensive than our everyday sensory-motor functioning indicates. Honorton's goal, therefore, was to develop a method that could more readily produce, access or detect this extended mental functioning. On the hypothesis that normal parapsychological functioning is the result of the brain filtering and thus reducing, through the senses, a wider potential range of functioning, Honorton centred his work on the study of states of consciousness in which the normal filtering process has been relaxed, altered or otherwise bypassed. He thus conducted important experimental work on ESP in hypnosis and in dreams. A major contribution, however, was his development for parapsychology of the Ganzfeld technique, a mild sensory deprivation technique that refocuses a person's attention on his or her internal mental processes. For nearly 20 years, Honorton

produced experimental evidence for ESP with this technique, and this work has been immensely important not only methodologically (in eliciting ESP) but also theoretically (in suggesting future research and explanatory models for psi).

The culmination of this research came only recently. A 1985 debate between Honorton and a critic of parapsychology, about the adequacy of the findings of the Ganzfeld research, resulted in a mutually agreed-upon protocol for conducting and reporting future research. Honorton and his colleagues then undertook a long series of studies that satisfied the new guidelines, and the series not only replicated earlier successful studies but also tested and supported hypotheses suggested by earlier studies. The importance of this research, which took on and met the challenges of a responsible critic, is immense.

Among the hypotheses and findings addressed in this last series of experiments were some predicting who might do well in a Ganzfeld ESP experiment, and what kind of targets they might most successfully detect by ESP. Honorton had planned to continue these lines of research, and he had also developed plans for beginning to examine the psychological interaction of the person attempting to "send" the target by ESP and the person attempting to "receive" it.

Honorton understood that science is above all a public process. As a result, he actively encouraged and engaged in debate with those offering constructive criticism. He provided historical reviews showing that parapsychologists have used responsible criticism to improve their research, but have also continued to be plagued by irrational, uninformed, polemical attacks that neglect the scientific issues. He thus urged critics to test their criticism with research of their own.

Honorton said that science, "in its better moments", is a process prompting new research, not "posthumous attacks" on old research. His work, like any other, does not stand alone, unchanging. It will continue to contribute to future science as it gets woven further into what came before and will come after him. The extent to which it does, however, will depend on whether future scientists recognise the importance of the problems he addressed. He believed that parapsychology should more properly be called "psychophysics", since it is attempting to unite revolutionary new findings about both matter and mind; and he believed that "for the first time in history, we have begun to forge an empirical approach to one of the most profound and ancient of mysteries, the nature of mind and its relationship to the physical world".

We can describe Honorton the parapsychologists: it is much more difficult to describe the person and what that loss means to the many who knew and cared for him. Only those who felt the impact of Chuck's personality can appreciate it.

A few days before his death, he told me that, even if he died now, he knew that he had accomplished something. How many of us can say that at the end of our lives, much less at the end of 46 short years?

Emily Williams Cook

Charles Henry Honorton, parapsychologist, born Deer River Minnesota 5 February 1946, Chester Carlson Research Fellow Institute for Parapsychology Durham North Carolina 1966-67; Research Associate/Senior Research Associate/Director of research Division of Parapsychology and Psychophysics Maimonides Medical Center Brooklyn New York 1967-79, Director Psychophysical Research Laboratories Princeton New Jersey 1979-89, researcher Edinburgh University 1991-92, died Edinburgh 4 November 1992.

Erratum

Tony Lawrence wishes to bring the reader's attention to some calculational errors that he found in his paper in the previous issue of *EJP* (vol. 8, 1990-1991, pp. 131-144).

On p.137, the comparison of the ESP scores of sheep and goats in the 'prove ESP' condition should have given $t(17)=0.788$ (the result remains non-significant, $p=.441$). The comparison of the ESP scores of sheep and goats in the 'disprove ESP' condition should have given $t(17)=1.99$ (the result remains non-significant, $p=.063$).

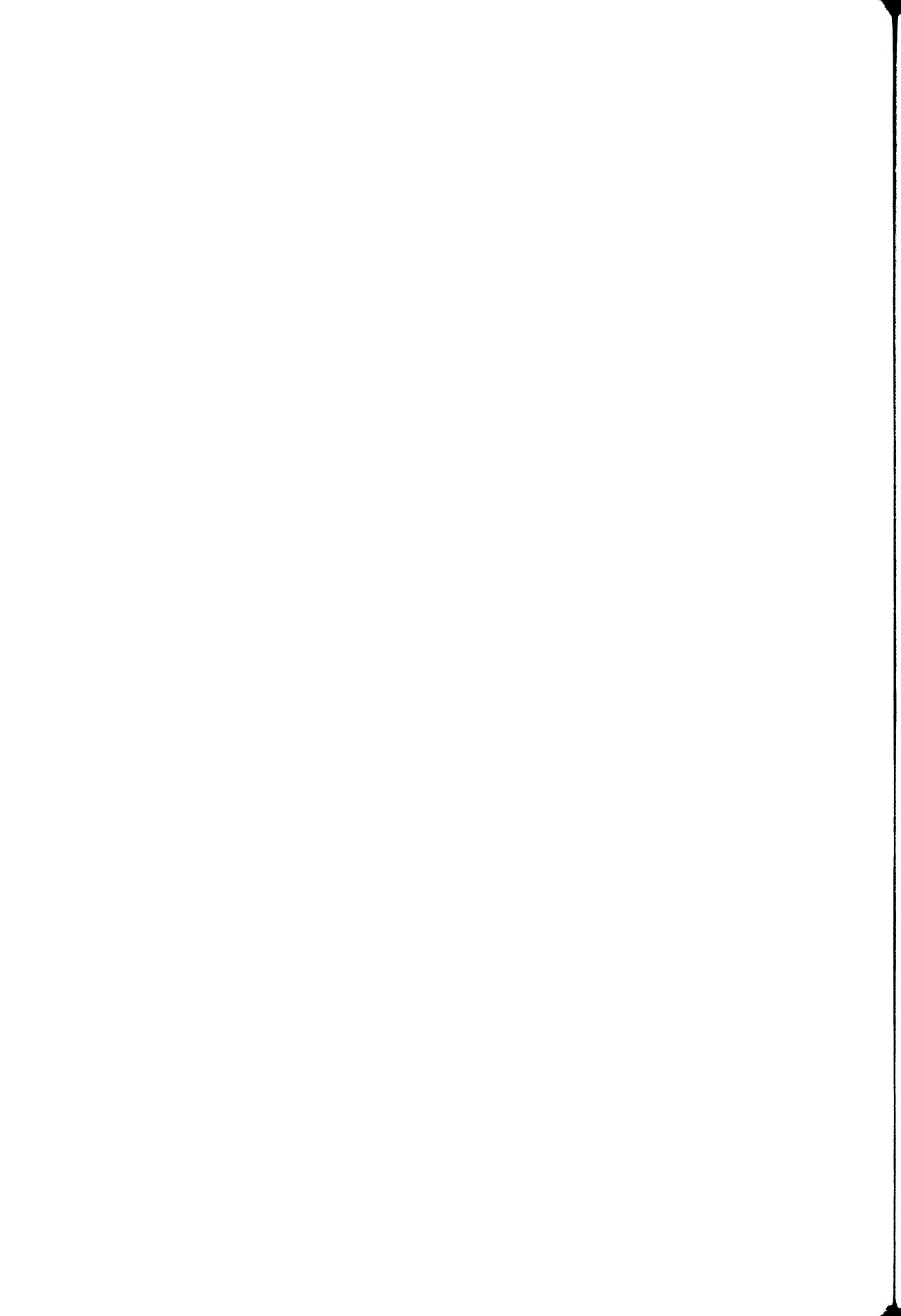
On p.140, the comparison of the number of doubles called by sheep and by goats in the 'prove ESP' condition should have yielded $t(17)=0.677$ (the result remains

non-significant, $p=.507$). The comparison of the number of doubles called by sheep and by goats in the 'disprove ESP' condition should have given $t(17)=1.433$, n.s., $p=.167$ (in the original paper, $p<0.025$).

The errors in these t values were due to unwittingly using the standard deviation instead of the variance in the calculation of t by hand on a scientific calculator.

In addition, in Table 2a on p. 137, the value of z for the sheep ESP scores in the 'disprove ESP' condition should of course be -0.84 , not -0.88 .

Please note that the arguments presented, along with the conclusions, are not at all affected by the changes in these results.





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