

CONCLUSION

It seems reasonable to conclude from this very limited bird's-eye view of European trends that clinical psychology, as a science and profession, is making significant strides abroad. American psychology has no monopoly on progress. It may well behoove us to look beyond our oceans and linguistic frontiers. International congresses, such as those sponsored by the International Union of Scientific Psychology, the growing interest of APA and of its divisions, the support of other affiliated or related groups, and perhaps the realities of the jet and space age augur well for International Psychology.

REFERENCES

CHOYNOWSKI, M. [On the awakening of Polish psychology. *Culture and Society*, 1957, 1, No. 1.] (Transl. by R. B. Zajonc. *Amer. Psychologist*, 1957, 12, 730-733.)

DAVID, H. P. Clinical psychology in other lands. In D. Brower & L. E. Abt, *Progress in clinical psychology*. Vol. III. New York: Grune & Stratton, 1958.

DAVID, H. P., & VON BRACKEN, H. (Eds.) *Perspectives in personality theory*. New York: Basic Books, 1957. London: Tavistock, 1957. Bern: Hans Huber Verlag, 1958.

EYSENCK, H. J. Training in clinical psychology: An English point of view. *Amer. Psychologist*, 1949, 4, 173-176.

MINTZ, A. Recent developments in psychology in the USSR. *Annu. Rev. Psychol.*, 1958, 9, 453-504.

RAZRAN, G. Psychology in communist countries other than the USSR. *Amer. Psychologist*, 1958, 13, 177-178.

SIMON, B. (Ed.) *Psychology in the Soviet Union*. Stanford, Calif.: Stanford Univer. Press, 1957.

STERN, E. (Ed.) *Die Tests in der klinischen Psychologie*. Zurich: Rascher Verlag, 1954. 2 vols.

SUMMERFIELD, A. Clinical psychology in Britain. *Amer. Psychologist*, 1958, 13, 171-176.

ESP AND FLYING SAUCERS: A CHALLENGE TO PARAPSYCHOLOGISTS

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IT has been claimed that the reason it may be impossible to devise a "repeatable" ESP experiment—one that gives results favorable to ESP in at least a majority of repetitions done by qualified skeptical experimenters—is that variables of personality and of attitude toward ESP in experimenters as well as in subjects affect results and that some experimenters are doomed to failure. On occasion, the necessity of this criterion of repeatability has been blandly denied, although it has been a fundamental requirement of proof in all experimental science.

J. B. Rhine claims to have proved the existence of ESP in spite of admitted failure to specify conditions for a repeatable experiment. He has argued that subtle uncontrollable variables make the latter impossible, but that the "positive" experiments have hit upon the correct combination of variables and constitute proof of the phenomenon—that unless we can pick some fault in the published reports of these experiments, we must accept them as proof. This is tantamount to denying the necessity of the criterion of repeatability in establishing scientific proof. As E. G. Boring stated (in correspondence long ago), it is unwise to sponsor an evaluative judgment of whether this or that research is valid purely on the basis of published reports alone. This points to the necessity for repeatability, which has been emphasized by R. A. Fisher (1938): "Very long odds . . . are much less relevant to the establishment

of the facts of nature than would be a demonstration of the reliable reproducibility of the phenomena."

Explanation of "failures" in terms of attitudes of skepticism and of handling of the subjects has caused most reputable experimentalists to lose interest in ESP. Nevertheless, to test the hypothesis that my own results were a function of my "experimental personality," I obtained grants from the Parapsychology Foundation.

In consultation with the Duke Parapsychology Laboratory staff, I designed an experiment in which *both experimenters and subjects* were fractionated on variables of self-confidence versus insecurity and of belief versus disbelief in ESP, using criteria of these traits that were agreed upon in consultation with the Duke staff. My students were carefully screened for these traits, and experimenters and subjects were selected from among the interested ones. The first experiment (completed in 1955) gave marginally significant differences from chance expectation (at the 5% level of confidence), which means that the results were favorable to ESP. Feeling that, if the results were genuine they would stand up under repetition, I performed the same basic experiment again in 1956. The results of both the second experiment and of the two experiments combined (which yielded a total of 1,920 ESP "runs" gathered by 24 carefully selected experimenters upon 24 carefully selected subjects) were negative—that is, they were those to be expected by chance. While these ex-

perimenters and subjects should be equivalent to many times this number of unselected cases, if the small N is a factor in the results, it would become apparent in further repetitions.

Both flying saucers and ESP may exist, but few trained experimentalists will accept them as proved until they have met all criteria of controlled experimentation, including repeatability of the observations. To obtain this proof the experimenter must accept the responsibility of specifying the conditions under which consistent observations may be made. Since there is no universal negative, an hypothesis of the nature of ESP cannot be disproved, and the burden of proof rests squarely on the positive claimants. It is not important that my own experiments did not offer evidence for ESP; for, even had they done so, they would have proved no more alone than the presently existing positive experiments. The important thing is that all skeptical experimenters should be able either to reproduce the results in their own laboratories or to observe such results being reproduced in other laboratories. Rhine has rejected observations of an experiment by a skeptical committee or individual as invalid on the grounds that such procedures put the subject on the spot and interfere with the operation of *psi* capacities. Such measures, it would seem, scare the ESP out of him.

As a major step in furnishing the necessary repeatability for experimental proof of ESP, I challenge the parapsychologists to settle upon one experimental design—*any* one that offers adequate control—and to obtain adequate repetitions of it with positive results. I had hoped to obtain interest in repetitions of my own experiment. Rhine, Osis, and other parapsychologists have, however, shown an interest in repetitions of an experiment by Anderson and White (1956) with school children, which gave significant differences in ESP scores between students who liked and those who disliked the teacher-experimenter, and which was repeated by Anderson (1957) and by Anderson and White (1957) with similar results. The Anderson-White technique of gathering data is similar to my own, and their experiment, being a group experiment, is easier to do. If these results are genuine, the experiment should bear repetition by a large number of laboratories. Thus I propose the following:

1. Let the parapsychologists, as represented by the Duke Laboratory, settle upon the Anderson-White experiment—or *any one* specific experimental design—and submit this design to all interested experimenters through the pages of the *Journal of Parapsychology*.

2. Let a committee appointed by a scientific organization act as moderator and call for experimenters to examine this design. The design should include the qualifications for experimenters.

3. Let time be allowed for the criticisms of this design to be returned from prospective experimenters and forwarded to Duke.

4. From the criticisms, let the Duke Laboratory synthesize an exact procedure that appears acceptable to the majority of experimenters. This procedure would then be resubmitted to all interested and qualified experimenters through the *Journal of Parapsychology*.

5. Let the committee make a final selection of experimenters who are willing to repeat this final form as exactly as possible. Here it would be vital that three groups of experimenters be secured: (a) those who have previously obtained positive ESP results, (b) those who have previously obtained negative ESP results, and (c) those who have done no previous ESP experimentation. The number in each group should be odd and equal. A minimum of five experimenters would probably be needed in each category, though more would be preferable. Several months would be needed for completion of the experiments, but a deadline should be set.

6. When completed, let a report of each experiment be submitted to the committee, who would announce the findings of all experiments.

Then comes the evaluation of the over-all results. The experimental design would, of course, specify the statistical procedures and the level of significance acceptable as a "positive result." But it would now be necessary to determine the meaning of the combined results of all experiments. Here several considerations would govern:

1. If the majority of experimenters in each of the three categories obtained significant positive results, the presence of an extra-chance factor (which Rhine calls ESP) would be proven to the satisfaction of most experimentalists. It is important not to conclude proof from the significance of the total pooled data of all experimenters—for a given category or for combined categories—unless the majority of individual experimenters obtained positive results; for spurious extreme results in one improperly controlled experiment could give significance to the total data, even if all other experimenters obtained negative results.

2. If the majority of the experimenters who have previously obtained positive results now obtained positive results while the majority of those who have previously obtained negative results now obtained negative results, this would indicate that the results are a function of the experimenters; and the final answer could then be given only by the third category of experimenters, those who have previously done no ESP experimentation, since we would not know which of the other categories was in "error."

3. If the majority of results in this third category were consistent with the majority in either of the other

categories, it would go far toward giving the final answer; for it would offer a very strong indication that the "error" was in the opposite type of experimenters.

4. The committee, augmented by neutral experimental scientists (none of whom has taken part in the experiments), should render a final report evaluating the total findings.

The problems represented by ESP are too complex to be easily solved in any pat fashion, and the results of these repetitions might leave questions to be answered by further study. But they would go a long

way toward clarifying the issues and pointing up the next necessary steps.

REFERENCES

- ANDERSON, M. Clairvoyance and teacher-pupil attitudes in fifth and sixth grades. *J. Parapsychol.*, 1957, 21, 1-11.
- ANDERSON, M., & WHITE, R. Teacher-pupil attitudes and clairvoyance test results. *J. Parapsychol.*, 1956, 20, 141-157.
- ANDERSON, M., & WHITE, R. A further investigation of teacher-pupil attitudes and clairvoyance test results. *J. Parapsychol.*, 1957, 21, 81-97.
- FISHER, R. A. *J. Parapsychol.*, 1938, 2, 267.

HOW DOES ONE DECIDE ABOUT ESP?

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I SHOULD like to acknowledge two things at the outset: one is the fairness of the Editor in allowing this simultaneous publication, and the other is Crumbaugh's gift as a title maker. Over the years I have watched with sympathy the latter's efforts at mastering the interpersonal relations involved in successful ESP testing. I could wish now only that in his frustration he had chosen some more useful course than this impatient challenge. Nevertheless, I shall try to clarify the two *major* points on which I think he is somewhat confused. The first has to do with his proposal as a scientific procedure, the second with his conception of where parapsychology stands today. But first there are two *minor* factual errors that should be corrected.

In one lesser slip Crumbaugh refers to "most reputable experimentalists" being led to "lose interest in ESP." As a matter of fact, there has never been any such wholesale interest to be lost, either on the part of experimentalists in psychology or out of it, and whether reputable or not. However, such interest as has been taken in ESP among the scientific professions is, according to available indications, apparently growing rather than declining. I would even dare to say the interest is actually growing more rapidly than the advances in the research itself.

The other rather small correction deals with Crumbaugh's remark that "Rhine has rejected the observations of an experiment by a skeptical committee or individual . . ." I have at no time rejected any such observation; moreover there are, as Crumbaugh himself doubtless knows, significant experimental results of ESP experiments conducted with observers present,

some of them known to be skeptical. S. G. Soal and his associates have rather specialized in this feature (1943, 1954, 1959). Personally I *had* rather rely on good experimental design such as the two-experimenter plan and, of course, on repeated and independent confirmation. As in any delicate psychological relation (for example, psychotherapy) supernumerary observers during ESP testing are likely to be a hazard to the subject's effectiveness.

THE TWO MAIN ISSUES

I will first consider Crumbaugh's proposed "majority vote" method dealing with the question of ESP, regarding it from the point of view of scientific method. It is, so far as I know, the first time such a standard of evidence or canon of acceptability has been proposed in dealing with a scientific question. The usefulness of the majority vote in many social institutions is, of course, quite a different matter. It is difficult to see (and Crumbaugh has offered no help on the point) just what this particular quantitative result, a majority of positive researches, would contribute to the finality or acceptability of the ESP hypothesis. Each individual must, of course, decide for himself what his personal criteria of acceptability are for a new finding: how many and just what type of confirmatory researches he requires. Crumbaugh has formulated his requirements; but, since it is one that has never had any effective use as a method of science, it seems likely to remain a purely personal one.

On the second major point of difference, I think Crumbaugh merely misunderstands just what it is parapsychologists claim they have established. His pro-