from the curves, since (i) no correction has been applied for the varying sensitivity of the photomultiplier at different wavelengths; (ii) the curves have been recorded under different monochromator slit widths and photomultiplier voltages.

However, they show, quite adequately, that for the optimal concentration of solutes, as commonly used, a rather complete shift to the spectrum of the secondary phosphor seems to occur, independently of the particular way the luminescence in the liquid is excited.

I am not prepared, at present, to determine the possible origin of the discrepancies observed in spectral shifts produced by second solutes in liquid scintillators.

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Value of a "Negative" Experiment in Extrasensory Perception

When is a negative result of sufficient value to warrant publication in *Science*? This question is raised by the report of Smith and Canon (*1*) on what they considered to be an experiment in extrasensory perception (ESP). After giving an ESP test to psychology students, they obtained results attributable to chance, results which therefore provided no evidence of ESP. The wording of the report implied an important bearing on earlier positive ESP results. It is an important question for research in general whether such a negative report has any generalization value.

Failure to confirm previous reported results is an important finding when the essential conditions are replicated. Such exact replication is seldom attainable in a field so complicated with uncontrolled variables as psychology. In addition, Smith and Canon did not even pretend to replicate any previous research; there was almost no similarity to any experiment that has yielded positive results on ESP.

ESP is recognizably difficult to demonstrate, and no one claims to know how it can be reliably produced on demand. The phychological conditions essential to its functioning are only slowly emerging from the studies of recent years. Under such circumstances, failure to approximate previous results may have no significance.

The Smith and Canon experiment was, unfortunately, not well designed as a research in ESP. The problem was new to the authors, and when Canon wrote me about his plan I replied with a four-page analysis from which I quote:

> Merely to carry through your experiment as it is designed and get the chance results that I should expect you to get would not prove anything except just to add another confirmation of the wrong way to approach an unfamiliar field.

One of the faults I indicated lay in the curious device of making all the targets (or stimuli) to be identified by the subject *of one kind*—an unnecessary deception that went against all rational expectation on the part of the participating subjects. Another grave error lay in the unpsychological disregard of the elusive character of ESP and of the special need, therefore, to provide the test participant with conditions known to be favorable to the demonstration of the ability.

Naturally, I offered to help Canon to design a better experiment. One of the suggestions made was that he first become acquainted with what had already been learned about how to stimulate subjects to perform most effectively in controlled ESP tests. Emphasis was laid on the need for arousing strong interest or motivation. A warning was also given on a statistical handicap in his design, one that involved the risk of a serious "stacking error" when the same target sheet is used in testing a large number of subjects.

In any case, no single negative experiment could be important today against the vast accumulation of positive evidence for ESP in the 18 volumes of the *Journal of Parapsychology* and elsewhere. Such an experiment proves nothing about such researches as, for example, the well-known Pratt-Woodruff (*2*) or the Soal-Goldney (*3*)ESP series. However, it *is* important for the future of any branch of science that the standards of evidence be as strict in *criticism* of new findings that conflict with old ways of thinking as in the *establishment* of such new findings.

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