vitation, or an incentive, if you feel that way, to further investigation. ESP, with its statement of nonchance, but with its utter failure to exhibit any regularities or to perform a single repeatable experiment, is the only instance of which I am aware in which a serious claim has been made that nonchance should be capitalized simply because it is nonchance.

The situation covered by the word *probability* is a desperately complex situation, mostly of our own making and in our own minds, with a fragile and fleeting dependence on time, and never coherently connected with concrete "objective" events. I personally can now see so much here that needs to be thrashed out and clarified that I am unwilling to accept the genuineness of any phenomenon that leans as heavily as does ESP on probability arguments.

Where Is the Definitive Experiment?

George R. Price

Since I have already stated at some length my views on psychic phenomena (*1*), I am reluctant to engage in continued arguments that can in no way settle the basic issue. As I wrote in the concluding paragraph of my paper, "the only answer that will impress me is an adequate experiment." Nevertheless, some brief comments on the statements by Soal, Rhine, Meehl and Scriven, and Bridgman are in order.

The Basic Issue

The most important portion of "Science and the supernatural" was the section suggesting new experiments. My two colleagues at the University of Minnesota, Meehl and Scriven, are incorrect in stating that my argument "stands or falls on two hypotheses . . . (i) that extrasensory perception (ESP) is incompatible with modern science and (ii) that modern science is complete and correct." My argument stands or falls on the two hypotheses that (i) previous demonstrations of psi phenomena have not been convincing to most scientists and (ii) that it is possible to perform convincing experiments meeting all objections that parapsychologists have made to previous suggestions for public demonstrations.

The most significant points that the reader should notice about the present correspondence are (i) that neither Rhine nor Soal has in any way criticized my proposed tests as unfair or technically faulty, and yet (ii) both of them reject these suggestions. Why?

Soal rejects the suggestions on the grounds that the results would be only temporarily convincing. However, if skeptics were even temporarily convinced, then numerous additional experimenters would begin investigating parapsychology and evidence could continue to accumulate.

Rhine rejects the suggestions on the grounds that a similar challenge issued by seven psychologists (*2*) was successfully met in the past, yet the results convinced none of the seven. But this is not correct. Angier *et al.* wrote as follows: "It must be emphasized that in no program is it possible, in advance . . . to cover all precautions. . . . It is necessary, therefore, that there be the most competent possible supervision, as indicated in Section IX below." Section IX read:

"The experiment should, throughout, be under the direction and control of two or more psychologists who are regarded by members of the profession generally as competent in the experimental field. One of these superintendents must be on duty during every work period, and have actual oversight of the conduct of the tests.

"In view of the present situation, and the need of a definitive experiment, it is highly desirable that the experiment be set up under the superintendence of three psychologists, each from a different university."

The Pratt and Woodruff experiment (*3*) did not meet the conditions of Section IX.

Meehl and Scriven criticize the proposed tests on the grounds that "the jury is obviously superfluous because, according to Price's own test, we should rather believe that they lie than that the experiments succeed." I cannot follow this argument at all. If people would believe the entire jury of twelve to be dishonest in preference to believing in psi phenomena, then logically Meehl and Scriven should recommend a much larger jury, instead of calling the jury superfluous.

Meehl and Scriven also state, "The mechanical contrivances would be welcome if only the parapsychologists could afford them. . . ." I cannot agree with this. The fact is that mechanical contrivances do not seem to be welcome to most parapsychologists. For example, in 1948, while Soal was still successfully experimenting with Mrs. Stewart, B. F. Skinner suggested that he use simple recording devices and other mechanical aids (*4*). Far from following these excellent suggestions, Soal contented himself with writing—as he describes it—"a calm, but perfectly devastating reply" (*5*). Secondly, I am quite sure that money can be raised for the sort of demonstrations that I suggested. If parapsychologists have special difficulty in raising money for their ordinary research, it is probably because of the peculiar rules of their game. It would similarly be difficult to raise funds for development of a uranium mine that never shipped any ore and that could be seen only by a special group of initiates.

Are there any crucial defects in my proposed tests? I can see possibilities for minor improvements—for example, using an inaccurate rather than an accurate timing circuit in the random number generator and letting the examining committee consist of seven parapsychologists and eight skeptics since a seven to eight ratio would appear fairer than the one to two ratio I previously proposed. But nobody has yet pointed out to me any important defect. To be sure, Rhine calls my proposals "fantastic" and Meehl and Scriven use the expression "Rube Goldberg." But what do such terms mean? If any of these men or anyone else has specific criticisms or sugges-

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tions for improvements, it would be kind of them to make the suggestions known.

In addition, I hope that some properly qualified person will volunteer to take charge of planning and arranging a definitive test, in the event that the parapsychologists change their minds and offer to participate in one. I would think that, primarily, such a person should be a scientist of high reputation; and it would be desirable (though it is not essential) that he be one who has in the past taken a public stand against parapsychology.

Miscellaneous Points

For Bridgman's views I have the utmost respect, especially because his writings have played a major role in the shaping of my own scientific philosophy. Nevertheless, I do not feel that his present probability arguments provide an escape from the dilemma of believing in extrasensory perception or in fraud. In a great deal of this work, Soal had his subjects alternate between telepathy and clairvoyance, and he found "extra-chance" results for the former only. In one run the Agent would know the identities of the 5 code cards (telepathy); in the next run he would not know their identities (clairvoyance). The clairvoyance runs consistently gave results in accordance with what standard probability theory predicts, and the telepathy runs gave quite different results. I do not see how this can possibly be explained on the grounds that there may be some basic flaw in our concept of probability.

It is interesting that most of Rhine's communication is devoted to stressing the incompatibilities between science and psi phenomena, while the Meehl and Scriven letter is largely devoted to arguing that there is no incompatibility. Of course I am on Rhine's side in this matter and must resist the temptation to reply to Meehl and Scriven. It does not seem proper for me to use up more space in *Science* arguing a matter in which I am strongly in agreement with Rhine and Soal.

Soal has made a number of errors in describing my suggested procedures for cheating. For example, it is not correct that my procedures "all depend on the Agent being in collusion with the chief Experimenter or with the Percipient." The Agent was not necessarily "in the trick" in my Procedures 2, 5, and 6. For imitating the Stewart series, with its 15 successful Agents, I would employ mainly Procedures 5 and 2. To imitate the experiments in the Shackleton series in which Wassermann prepared the random number lists, I would employ Procedure 4 where possible, and a variation of Procedure 5 if the Agent was being watched too closely to permit use of Procedure 4. Therefore, most of Soal's discussion of the honesty of this or that person is irrelevant. If Soal did cheat, it probably was not by procedures requiring intentional cooperation from Rozelaar, Wassermann, or the four Agents from Queen Mary College.

Soal submitted a virtually identical statement to the *Newsletter* of the Parapsychology Foundation, and this statement, together with a more detailed reply from me, has already been published (*6*).

Rhine is in error in thinking that I "believe that all parapsychologists are liars and montebanks" or that I charged that "a hundred or more research scientists . . . [had indulged] in a gigantic hoax involving the hiring of confederates and such" (*7*). Outside of Soal's work, I do not believe that we are confronted with many experiments so excellent that we are forced to choose between ESP and fraud. But there are a few such cases.

Rhine, Soal, and Meehl and Scriven all complain that it was improper of me to discuss the possibility of fraud. Naturally I did this with considerable reluctance, but it was absolutely essential that this question be treated frankly in order to settle things one way or the other.

Rhine complains that I did not dig up "some tangible evidence [of fraud] concerning at least one parapsychologist." Now, of course, when it comes to phenomena so gross as to be apparent without statistical tests, there is available all sorts of evidence of fraud. For example, according to the March–April, 1955 *Newsletter* of the Parapsychology Foundation, the Society for Psychical Research, London, will shortly publish a 70,000-word report showing that the late Harry Price, one-time honorary secretary of the University of London Council for Psychical Investigation and author of *The Most Haunted House in England*, himself faked some of the evidence for the haunting of Borley Rectory. But in connection with phenomena so subtle as to be detectable only by statistical tests, my feeling was that it would be quite difficult to prove in 1955 that *A* had whispered something to *B* in 1945.

Soal complains that I wrote "a diatribe of unsupported conjecture." But I did not. My conjectures that parapsychologists might be capable of fraud were supported by the eminent authority Soal himself (*8*):

"There is unfortunately among American investigators an atmosphere of showmanship which has created in the minds of British scholars a deep distrust. British scientists for instance are not favourably impressed by Rhine's discovery of a telepathic horse (or was it a precognitive clairvoyant pony?), by the sudden vanishing of Dr Reiss' phantom percipient into the blue of the Middle West, by the perfect scores of 25 cards correct in 25 successive guesses alleged to have been made by Pearce and the child Lilian, by the card-guessing feats of Pearce while sitting in a motor car and similar marvels.

"Such things simply do not happen in England, or if occasionally they appear to happen they are quickly exposed as frauds or conjuring tricks. In America they are not exposed; they are proclaimed genuine with a blare of trumpets."

Conclusion

Rhine has stated that publication of my paper is "on the whole, a good event for parapsychology." It would be wiser for him to see it not as a good event but as a good opportunity. This challenge has presented him with the opportunity to achieve at one stroke the scientific recognition for which he has been struggling for almost 30 years. But if he and Soal continue to evade the challenge, then publication of the paper will prove to have been a very bad event indeed for parapsychology.

References and Notes

1. G. R. Price, *Science* 122. 359 (1955).
2. R. P. Angier *et al. J. Parapsychol.* 3. 29 (1939).
3. J. G. Pratt and J. L. Woodruff, *ibid.* 3, 121 (1939).
4. B. F. Skinner, *Am. Scientist* 36. 456. 482 (1948).
5. S. G. Soal, *Proc. Soc. Psychical Research* 50, 67 (1953).
6. The September–October 1955 issue (vol. 2. No. 5) of the *Newsletter of the Parapsychology Foundation* (500 Fifth Ave., New York 36, N.Y.) contains statements by J. B. Rhine, S. G. Soal. G. R. Price, and D. Wolfle.
7. Statement by J. B. Rhine. quoted by R. K. Plumb. *New York Times*. 27 August 1955. p. 1.
8. S. G. Soal. *The Experimental Situation in Psychical Research* (Society for Psychical Research, London, 1947), p. 26.

I remember, one day, saying how uphill the work was, and he [Thomson] answered, "Yes, that is why there is so much credit in doing anything."—RAYLEIGH, *The Life of J. J. Thomson.*