gerous to assert that there is "no parallel among established mental functions." In the psychophysiological field particularly, there are several candidates. Finally, *even if it had been established and there were no parallel among mental functions,* there would be no essential difficulty in comparing it with one of the many familiar performances that exhibit no learning in adults—for example, reflex behavior.

7) "Different investigators obtain highly different results." This is the most distressingly irresponsible comment of all. ESP is a capacity like any other human capacity such as memory, in that it varies in strength and characteristics from individual to individual and in the one individual from one set of circumstances to another. The sense in which Rhine and Soal (Price's example of "different investigators") have obtained "highly different results" is when they have been dealing with different subjects or markedly different circumstances—for example, different agents; and exactly the same would be true of an investigation of, for example, stenographers' speed in taking dictation or extreme color blindness.

There remains only statistical precognition, which is certainly not susceptible to the types of explanation currently appropriate in physics: but then it is not a phenomenon in physics. Even if it were, it is difficult to see why Price thinks that we properly accommodated our thought to the distressing and counterintuitive idea that the earth is rotating whereas we should not accept precognition. His test for distinguishing new phenomena from magic is hopeless from the start ("The test is to attempt to imagine a detailed mechanistic explanation") because (i) it is of the essence of the scientific method that one should have means for establishing the facts *whether or not* one has already conceived an explanation and (ii) it would have thrown out the Heisenberg uncertainty principle and action across a vacuum—that is, nuclear physics and the whole of electricity and magnetism—along with ESP.

Finally, Price's "ideal experiments" are only Rube Goldberg versions of the standard tests plus a skeptical jury. The mechanical contrivances would be welcome if only parapsychologists could afford them, and the jury is obviously superfluous because, according to Price's own test, we should rather believe that they lie than that the experiments succeed. However, in our experience, skeptics who are prepared to devote some time and hard work to the necessary preliminary study and experimenting are welcome in the laboratories at Duke and London. Without the training, one might as well have (as Price would say) 12 clergymen as judges at a cardsharps' convention.

The allegations of fraud are as helpful or as pointless here as they were when they were made of Freud and Galileo by the academics and others who honestly believed that they *must* be mistaken. They are irresponsible because Price has not made any attempt to verify them (as he admits), despite the unpleasantness they will cause, and because it has been obvious since the origin of science that any experimental results, witnessed by no matter how many people, *may* be fraudulent.

Probability, Logic, and ESP

P. W. Bridgman

The recent article by G. R. Price in *Science* [**122**, 359 (26 Aug. 1955)] entitled "Science and the supernatural" directs renewed attention to a situation that doubtless has given many people, including myself, a feeling of discomfort, to say the least. My own attitude was expressible in a paraphrase of Price's quotation from Hume to the effect that he would be unwilling to accept such phenomena as those claimed for extrasensory perception (ESP) unless he could be convinced that their genuineness would be less miraculous than the occurrence of fraud somewhere.

My own attitude did not seize on the possibility of fraud, although it seems to me that Hume's position is irrefutable; it seized, rather, on the way in which contemporary arguments for ESP depend on considerations of probability. I felt somewhat vaguely that I would rather think that my understanding of probability is faulty than believe in the genuineness of ESP. My scruples against the use of probability arguments had nothing to do with the details of the calculation of the enormous numbers that represent the odds against the scores obtained in ESP tests. I was willing to take the word of the many technically competent persons involved that the grinding of the machinery by which these numbers were obtained had been according to Hoyle. My scruples went much deeper and were concerned with the logic of the application of probability concepts to concrete events.

It has long been apparent that there is something "funny" about the probability situation. Probability rigorously applies to no concrete happening. If we calculate that the chance of throwing a 6 with a die is one-sixth, and throw the die and obtain a 6, there is no method whatever by which it may be shown that the chance "actually" was one-sixth. Yet the phenomena to which the probability calculations justifying ESP are applied are concrete actual happenings, many of them a matter of record in black and white.

My old feeling that the logical situation should be further explored was fortified by a recent occurrence that is the immediate occasion for this note. I was reading in *Science* [**122**, 471 (9 Sept. 1955)] a review of the recently published collection of 1 million random numbers, when it burst on me in a flash of illumination that random numbers cannot be published. For a set of random numbers is a set in which it is impossible to predict any subsequent number from the preceding numbers, or in which there is no connection between the different numbers. But the subsequent numbers may be predicted, if the set is published, merely by reading the published list, and all the numbers of the set are connected by being written together on paper. A list of numbers *obtained* by a random *process* might perhaps be published if we could answer the question, What is it that

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makes any specific process random? The list itself cannot give the answer, because, given only the list, the process by which the list was obtained cannot be reproduced; there are an infinite number of ways of generating any finite list.

Randomness is an ephemeral thing, having meaning only during the activity of generating the numbers, and passing into limbo with the consummation of the process. And its ephemeral meaning was a meaning only in a certain universe of operations—we could see no way of predicting the next number in terms of operations drawn from our repertoire. The repertoire was to a certain extent arbitrary, dictated to a large extent by our purposes of the moment. These purposes might, for example, dictate that we focus our attention on those aspects of the situation that can be expressed in mathematical language of an acceptable degree of simplicity. I suspect that the prospective users of the list of 1 million random numbers have in mind only such mathematical purposes and limitations, and that any remarks made here will not affect in the slightest degree the value of the list for them or diminish its sales.

The paradox inherent in the application of a probability calculation to any concrete situation is well brought out by a comment of Bertrand Russell, who remarked that we encounter a miracle every time we read the license number of a passing automobile. For if we had calculated the chance that we would see that particular number, the chances would have been millions to one against it. In what respect is the situation here different from that presented by a better-than-chance score in an ESP experiment? a score for which we may suppose that our preliminary calculation of the chances gave the same expectation value as the automobile number. The occurrence of the automobile number does not jar us, and we continue to put it down to chance despite the odds against it, whereas the ESP result does jar us, and we say that it could not have been chance.

There are several features here that demand comment. In the first place, we have to justify ourselves in not regarding the automobile number as a miracle. This justification we offer rather easily, although it might be difficult to give a logically rigorous defense of our justification. We might, for example, offer in justification a consideration of the distribution of the chance of occurrence of all possible numbers. Within limits, the chance for the occurrence of any one number is the same as that for any other —there is no heaping up of probabilities in favor of any one number or range of numbers. We could not have *expected* the particular number to turn up, but on the other hand, we are not *surprised* when it does. We reflect that *some* number *had* to occur, and we let it go at that and think no more of it unless we are prodded. If we are prodded to tell exactly what we mean when we say "this past event was chance," we admit that there is no property inherent in the event by which we can verify that it actually was chance, and we seek for the meaning elsewhere.

We may seek the meaning of "was chance" in what we do about it. Now the paradoxical thing is that when we say "was chance" we do nothing about it—we have come to the end. The reason that we have come to the end is that consistency with our position forbids that we attempt to go further. If we went ahead and sought for an explanation or any sort of rational involvement, we would be stultifying our conclusion that the result was chance. For instance, if after seeing the automobile number and noting the state of issue, we begin to reflect on the relative number of registrations in the different states, we have abandoned our position that the event was a chance event. As long as we remain consistent and do nothing, we are safe, despite the fact that we have effectively changed our definition of chance when we pass from anticipation of an event to viewing its occurrence in the past. In fact, the operational meaning of "this *was* chance" involves our resolution to handle the situation just by doing nothing. And it is our resolution to do nothing that protects us from the logical punishment to which we would normally be subject for changing our definition.

These considerations, I think, make it particularly clear that the locus of chance is in ourselves, with strong involvements of "expectation" and "surprise," and that there is little that is "objective" about it.

Consider now the situation presented by the ESP scores. Unlike the automobile situation, there is here an enormous heaping up of probability in the neighborhood of a particular score (5 out of 25 for the conventional testing cards). We could not expect a score for which the adverse odds were millions to one, and we *are* surprised when it turns up. We cannot now say with the same cogency as before "there *had* to be *some* score," but instead we draw the conclusion that the result could not have been chance.

We have to ask what we mean when we say "this event was *not* chance." Since we have already made an attempt to tell what we mean when we say "this event *was* chance," we might be tempted to think that our new question is trivial and that its answer is implied in the answer we have already given. I think, however, and this is perhaps the crux of this note, that this is by no means the case. "Was chance" and "was not chance" are not simply related to each other as two terms in traditional Aristotelian logic, subject to the rule of the excluded middle. Because what we do to give meaning to "was not chance" is not simply or obviously determined by what we do to give meaning to "was chance." Whether there is any necessary connection in logic between the meaning of these two expressions is by no means apparent. That they are connected in use is another matter. If the advocates of ESP were content to say "All I mean when I say 'the event was not chance' is that the event was not expected and surprised me," I think we could have no quarrel. But it would be almost humanly impossible to stop with such a simple statement, and the advocates of ESP have shown their humanity by not stopping, but have gone ahead and envisaged all sorts of consequences, consequences that would usually be implied in an Aristotelian, excluded middle, system. Thus we can imagine them saying "Chance events are subject to no formulatable regularity—the events of ESP are nonchance; therefore they are subject to some regularity," with the usual additional implication that we are in the presence of a new unknown faculty of the mind. It seems to me that the only justification for drawing such a conclusion in a non-Aristotelian system is to be found in the actual exhibition of some sort of pertinent regularity, and this, as far as I know, has not been done.

There is a deep-seated difference between the way in which positive and negative probability arguments are fruitfully applied in practice to concrete situations, to which we have seen that the concept of probability does not rigorously apply at all. If the situation is a positive one, which we can characterize by saying "here we have the play of chance," then we can draw fruitful conclusions from the mere statement, without going further. This is shown by countless examples, as in the tables of a life insurance company, or the kinetic theory of gases, or the theory of the atomic nucleus with its calculation of the best construction for a hydrogen bomb by the Monte Carlo method. But if the situation is a negative one, characterized by saying "here we do *not* have the play of chance," we have something radically different. Here we *are* compelled to go further, and fruitful application is not achieved until we succeed in exhibiting the regularity that we suspect. The detective who says "It was not chance that five murders were committed by the same technique" has said nothing until he exhibits the man who committed the five murders. Wanting the ratification of exhibition, the statement of nonchance is merely an invitation, 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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