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PROGRESS IN PARAPSYCHOLOGY¹

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IT IS GOOD TO SEE so many faces of old and new friends and collaborators, each constituting something vital: Jan Ehrenwald with the urgently needed depth approach; pioneer experimenters with new methods like George Estabrooks and Gaither Pratt; vigorous young beginners like Stephen Abrams; my treasured research companion from the Harvard of 1942, Gertrude Schmeidler.

I

We are gathered to advance the study of parapsychology. I am grateful indeed that you have honored me with an opportunity to address you on this problem. The only task that seems to me worthy of the occasion is that of asking how we may more wisely, rapidly, effectively improve our research. I say this partly because I feel a serious risk of stagnation, preoccupation with minor problems and a dearth of big ideas in our field. I admit, in relatively honest moments, that my own lack of incisive imagination and an increasing inability to create new perspectives has irked me and made me feel guilty, and in the same breath that I believe this unimaginativeness is characteristic, more or less, of our whole movement. Philosophical breadth is important; but it is not the same as research creativeness. I shall attempt to throw myself bodily at the target of research creativeness or lift myself by those bootstraps which will take me out of the sluggishness of today;

¹ Paper read at the Convention Dinner of the Parapsychological Association on September 5, 1958.

and if I fail I shall at least lunge toward a target which it will be your task to define more clearly.

II

I have six specific suggestions to offer. (1) First, that we study more closely the history of science, and note the peculiar attitudes of the great movements in which science breaks with its past, striving to see how the evolution of parapsychology might benefit from awareness of the nature of those great turning points. (2) Second, I shall ask about the role of scientific models, and ask whether anything is wrong with our models in parapsychology. (3) Third, I shall ask you to think with me about models which utilize our knowledge of biology. (4) Fourth, I shall try to raise the same question about the social-cultural context of our work, conceived in terms of a field-theory which provides the context within which paranormal processes may occur. (5) Fifth, this will lead to the question whether hypothesis-construction and model-building can be undertaken and suitably tested within that larger framework which comprises both the biological and the cultural context of paranormal events, and ask whether the problem of confirmability or repeatability, the genuine experimental control of paranormal processes, is ready for formulation in these same terms. (6) Sixth, I shall speculate on the question whether we are consistently applying what we know as psychologists. Specifically, I shall ask what troubles may be arising from conceptions which may no longer be appropriate in psychology, and I shall stress our need to use models which are appropriate for the half-formed, half-determined, probabilistic nature of cognitive, affective, and conative functions in response to a half-formed world, which throw upon the living organism the need to use whatever paranormal functions it possesses.

III

1. To begin then with the history of science. A moving object continues along a straight line, as Newton showed, until deflected. The same is true of an intellectual movement. To predict, you must extrapolate. Science does more and more of what it is

already doing. But ultimately another trend, dealing with the phenomena in a different way—for example, optically rather than mechanically—interacts with the first; and there is an *emergent*, a new thing in the world. Examples from psychology are the interaction of Darwinism with experimental physiology, as in the work of Walter Cannon, and of psychoanalysis with physiological optics, as in the movement known as the “New Look,” appearing first, perhaps, in Hermann Rorschach’s ink blot test and in Harry Murray’s Thematic Apperception Test. Are we using this principle of emergence, or just coasting in straight lines? As I read the Journals, I see dozens of attempts to proceed by extrapolation from the experiments of Rhine; but almost no cross-fertilization by combining Rhine, let us say, with Tyrrell or Whately Carington, or Freud, or James. What would happen if somebody read James on Mrs. Piper’s trance and saw its relation to experimental precognition?

2. Secondly, almost everybody today is convinced that psychology has done poorly to go on with the hypothesis-construction and hypothesis-testing that were appropriate a half-century ago. Almost never does a single independent variable stand forth, neatly permitting everything else to be held constant while we measure the dependent variables. We deal, ordinarily, with a matter of interdependent components in a complex situation, and we must use the Fisher methods, especially when we deal with interactions. But have we in parapsychology learned this lesson? While biology and psychology have had to admit that they cannot continue to go on with the single-track approach, most of our work in parapsychology is still one-variable work. We need perspective and practice in model-building and model-testing.

3. Third, I cannot personally entertain much hope of major breakthroughs without an attempt at definition of the complex multi-dimensional biological contexts in which paranormal events occur. This may be thought premature. But we are already bogged down by the large number of untestable hypotheses and the fact that those few which pay off lead, as a rule, to no experiment confirmable or repeatable by others. Plainly, complexity of models is demanded by complexity of events in science. And there is no

way to discover deficiency in models that is half as good as to construct them in detail and see if they are at least consistent and oriented to such reality as we know. Take a simple biological conception, very good so far as it goes, that certain drugs facilitate ESP. But one of the first things in all drug research is the richness of individual differences. Take a beautiful study like Roger Williams' book on *Biochemical Individuality* and you note that every biochemical event is related to an individual biochemical system. Studies of drugs, toxins, foods, oxidation, remind us that a suitable model for biochemical research today must be based on the best view of the individual living system that we can obtain. The same is true for every biological component in our study of our subjects.

4. The same conception of multiple interacting factors forces itself upon us with a vengeance when we deal with the sociocultural contexts of paranormal events. There is still almost no cross-cultural research in parapsychology, and even within our own culture, the studies of interpersonal factors in the parapsychological laboratory are limited to a few studies of subject-experimenter relationships. But the relationships are certainly dependent upon the family and community background of subject and of experimenter. Of course, if the results we get are consistent, we can for the time being emphasize what we can control, but if they are inconsistent, they presumably arise partly from sociocultural contexts that have not yet been analyzed.

5. As is already happening in general psychology, such an analysis will certainly force us into the development of models in which biological and cultural components are viewed in their interaction. If we can attempt the same in parapsychology, it will profoundly influence our way of thinking about our research methods. I suspect indeed that our deficiency in inventing new methods (for there have been almost no new methods at all since those invented by Rhine twenty or more years ago) may be due to deficiency in models. It is only when baffling conceptual problems loom upon us that we have the courage and the imagination to invent a new method. Yet paradoxically much of the history of science is a simple matter of finding new methods. What happened to astronomy when Newton and Leibnitz invented the differential and integral calculus;

to biology when Leeuwenhoek invented the microscope, to medicine when the Hertzian waves became a method of examining the interior of the body? Yes, something vigorous must be done to launch us on a bender of method-inventing. I know of nothing that can do it but the discovery of the deficiency of our models, as shown in the non-confirmability of so many of our reports.

But why is it, in the meantime, that we do not see with open eyes how inadequate our models are? I think the answer lies mainly in our manner of looking at this problem of confirmability. The fundamental rule in laboratory science is that you truly have captured a phenomenon and begin to understand it only when you can so fully specify the conditions which engender it, that you can yourself make it happen again and again, and other qualified workers can do the same. We do not, for the most part, even attempt this in parapsychology. The few cases such as the Schmeidler, van Busschbach, and Anderson-White studies, that seem promising, are indeed important but they are very far from achieving that standard of confirmability which we expect when we hear the word science, and it is clear from these cases that there are at least a half-dozen variables, personal and interpersonal, that will have to be understood and controlled before we can write a consistent statement of what happens. In the meantime there are literally dozens of new one-variable studies which, even if they succeed, can serve only to dilute the urge to create a science, a discipline, a coherent system of ideas out of parapsychology.

6. But perhaps our persistence in making certain methodological assumptions, to which we cling despite cumulative evidence that they are errors, is due to our habit of not using the psychology that we know. One assumption relates to the supposed basic uniformity of the effects of experimenter attitudes; another, the supposed uniformity of the effects of subject attitudes, particularly college student attitudes. Assumptions about the uniformity of the attitudes of subjects in experimental situations don't work well even in simple tasks like the learning of nonsense syllables, and certainly work very badly in the delicate interpersonal situations involved in paranormal interaction. If psychiatrists are learning that patients' responses in psychiatric hospitals are expressions of very complex field relation-

ships, perhaps subjects in parapsychology experiments will ultimately be understood to require at least this much sophistication. We are dealing with plastic cumulative material in which time trends as well as cross sections have to be kept constantly in mind. Even in the more sophisticated studies the deep-level needs and attitudes of experimenters and subjects are but dimly understood, and of course yield no consistent predictable results. A combination of the anthropologists' and the clinical psychologists' skill might help us to understand who those people really are whom we scoop up in our experimental net, and might deal then with full-bodied people.

Another assumption which we would make less frequently if we functioned as psychologists is the assumption that every paranormal cognitive process is inevitably a form of perception rather than a form of recall, recognition, phantasy, conceptual thought, or what not. Even the use of the term paranormal cognition, awkward as it is, might remind us that we are constantly dealing with more than perception. A humble but important illustration of this came up in connection with Charles Stuart's reminder that two things can be paranormally related because they are alike or because they are associated, rather than because both constitute an expression of the same event. If you are trying to transmit to me the word *hat*, my response *head* may, under certain specific conditions, be the expected paranormal response; or to the word *tennis* the appropriate response may be *golf* or *ball* or *love* or *set*, depending upon a variety of contexts ordered around the word *tennis*. We know this in the study of aphasia. Are we sufficiently aware of the principles of general psychology to recognize that a paranormal process may be very far from a carbon copy of the material indicated in the target? This principle was, of course, stated at the Duke University laboratory many years ago, and was a cardinal feature of one phase of the work of the late Whately Carington, but I look in vain as I read the Journals for evidence that this way of thinking has gotten into our blood. I fear in fact that the use of the term psi, useful as it is in some connections, is making us forget the difference between extra-sensory perception and other paranormal cognitive functions.

Another issue that makes me wonder whether we are really working as psychologists is our tendency to set up for our subject

tasks which are essentially mechanical in nature, in which there is no initiative, no creative exploration. This can at times become a violation of the reality of the paranormal process. The paranormal process is evidently a weak biological function, making fleeting evanescent contact with reality in its own way, almost never like the sharp, clear, exteroceptive function of seeing and hearing. The paranormal processes may apparently in some cases be stand-ins or substitutes for exteroceptive function, and in some cases very possibly represent "potentiations" in which the exteroceptive function at a low level is eked out and abetted by the paranormal process. In order to set up what we consider a proper experiment, we ignore this possibility and assume that regardless of the situation, we must completely exclude sensory cues. Now for purposes of certain kinds of elementary statistical analysis, the exclusion of sensory cues is a fundamental and essential first step. But if there should be very rich interactions, in R. A. Fisher's sense, between ordinary sense perception and extrasensory functions of various sorts, we should mutilate or exclude these altogether by our usual methods. We have talked now for many years about the potentiation in which a weak paranormal process is grafted upon a weak normal perceptual process, in which we compare the perceptual process working without ESP with what occurs when the ESP factor is added. Yet, as far as I know, two hesitating and tentative little studies are the only ones in which this principle is being expressly formulated.

Still another example of our lack of contact with psychology: One psychological principle which has spread through the psychological world like wildfire in the last ten or a dozen years is the conception of game theory, emphasizing the hazard element or aleatory element involved in all transactions with the environment. This idea is related of course to information theory on the one hand and to Brunswik's conception of probabilistic functionalism on the other hand. The organism wanders through this tangle or undergrowth which we call the world of the senses, piecing together fragments of usually unrepeatable evidence in endlessly varying patterns, and though bungling a good deal, still making some sort of contact with reality. One of the basic functions of the organism is to shift the P-values constantly, relying sometimes on evidence

in which the P-value is one-half, that is, there is an equal likelihood of a door being opened or closed, and in other cases where P-values are of the order of one in a thousand, where for example a cry for help or the sound of a siren is just one out of literally a thousand different stimuli that might hit us, so that the significance for the individual of the siren or the cry for help is enormously greater than it could be if P-value were one-half. The constant utilization of different P-values in empirical attunement with cultural and individual idiosyncracies is one of the major things which should preoccupy the parapsychologist. One reason for this lies in the basic trueness to life of the shifting P-value model. Another equally important reason is that for the most part, with the faint extra-sensory powers of today, a large proportion of our experiments reach only marginal significance and on repetition do not again achieve this marginal significance. Perhaps the use of a lower P-value would alert us to those relatively rare but important things which indicate a true function, standing out crisply against a background and moving with a swifter replication. First impressions could later yield the solid outlines of a true paranormal contact with reality. Empirical manipulation of P-values by using different kinds of target materials is one of the most fundamental ways of getting into parapsychology the true spirit of our openness in relation to a vague and challenging environment. This happens to be a useful statistical principle; but more important, it is a useful psychological principle.

IV

So you see, I am pleading here for more self-criticism, more sophistication, more biological-mindedness, more cultural-mindedness, more psychological-mindedness. The fact that there are only a few of us, with no great funds coming our way, is indeed too bad. But the initial work of Rhine, Carington, and Soal did not call for large numbers, nor for much money. What was called for was ideas. To the younger people especially I would say: older people can do a little to help you, but today and in the future you are the fabricators and the processors of ideas; it is your ideas that will catalyze the growth of parapsychology. It is your turn now.

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