

The Problem of Repeatability in Psychical Research¹

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There are many "ways of knowing," but there are more systematic ways and less systematic ways. The more systematic ways involve the confident expectation that in well-defined situations well-defined observations can be made. The belief that Nature is orderly has grown upon us since Chaldean shepherds watched the stars and Stonehenge architects pointed their shafts at the returning sun. Among the family of the ways of knowing—the poetic, the prophetic, the mystic, the heuristic—the ways of science have grown upon us since Galileo and Harvey, since Newton and Darwin, Planck and Einstein. And in every sphere of knowledge called scientific the generalized realities, the scientific laws to which a uniform and explicit method has given rise, become the firm foundation for a society ever more insistent regarding the ultimate regularity of the events among which man must thread his way to an inscrutable future. He may indeed create chaos, but the laws of Nature thus misapplied are nevertheless laws before which he must tremble. If there is to be a benign application of science, it must depend upon discovery and utilization of the orderliness of Nature.

Now repetition is not the *only* hallmark of science. There are unique events in the history of the stars and the history of life on this planet and, of course, in the history of each of us as individual human beings. The uniqueness must be honored and its internal structure and external contextual relations fully understood. This, however, is largely because the systematic methods of observation which go with science permit comparison of events across different portions of the face of the earth and across various expressions of life and across the contemporary successive events which permit a recognizable lawfulness in human life in general and in the sequences which make up the individual life. Among the life sciences laws are at least as fundamental as they are in the physical sciences. In fact, the more probabilistic conceptions of nature—those which stress uncertainty and indeterminacy—belong to the hard core of science much more than they belong to the everyday reality of biochemistry, embryology, physiology. Not by any means would we insist that order or lawfulness is universal in the data marked and

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hailed as science; but as a very strong trend in the development of science as *science*, a conception of uniformity, of process, has had to be built into the very structure of methods of investigation. There are, of course, artifacts, unclassifiable events, "strays" we may call them, observations which we cannot subsume under any category. There are the "flying saucers," so to speak, of physics and chemistry as well as of the mountaintop, the Loch Ness monsters, events which have neither a home nor a rationale.

Years ago I read a charming article in the *Scientific Monthly* with the title: "Do fishes fall from the clouds in rain?" Solemnly indeed, the author traced Greco-Roman observations shown on coins, Medieval and Renaissance fantasies, right up until he showed records of hurricanes which had lifted fishes by the hundreds from the Caribbean and dropped them two hundred miles inland from the Atlantic coast, to continue their splashing in water still salty enough to let them live. The problem here is only in a small measure a problem of replication. You will notice I am not at all pleading that replication is the one inescapable tag by which a scientific kind of reality can be affirmed. I am saying that if the event is unclassifiable, then it is doubly important that it have a rational interpretation, that is, one that fits with the thought patterns of the contemporary human mind. If it has no clear rationality, its only chance of demanding scientific attention is replication. This is my first point then: the joint necessity of replication and of rationality, and the weaker the one leg on which to stand, the more important it is that the other can bear the weight to be borne. One might go as far as J. B. Conant (4) in saying that no event is ever accepted as science until it falls into an ordered and accepted system. Or we might follow T. S. Kuhn's position (10) that scientific discoveries are never accepted until an old system of ideas has long been overripe and ready to fall, and until new observations are not only intrinsically strongly and clearly heralded, but plainly offer the rational possibility of bringing in a new and orderly system. Those of you who know the field of experimental parapsychology know how very far we are from a coherent system which is ready to replace the Newtonian and the nineteenth-century types of physics. We may rail at them and say that in terms of Planck, Einstein, and Heisenberg they are all rotten ripe, ready for decay. But it will take both honest experimental replication up to the hilt and with it an ordered system of ideas before the new science has any chance of establishing itself.

The problem of replication in its broad contours relates primarily to the consistency with which certain described conditions offer observable evidence of well-defined events. And secondly, there is the matter of the consistency of individuals in maintaining their

own type of performance under constant or closely comparable conditions. The former problem, the problem of consistency of results under specific conditions, is the challenge with which I think we must now deal.

Do we have in parapsychology laws comprising generalizations about the conditions under which extrasensory perception occurs—laws like the conservation of energy, or the laws of Charles and Boyle regarding pressures of gases, or laws like Einstein's $E = mc^2$? Let us take three widely quoted parapsychological laws. The first relates to motivation. It indicates, as in much of the early work of Rhine at the Duke University Parapsychology Laboratory (16), the superiority of motivated over unmotivated conditions. Here we might include the instances in which very great motivation pushed some of the early subjects to fantastically high scoring levels, or under adverse conditions drove them to extreme sub-chance scoring. These are dramatic instances of lawfulness—or would be if anybody could, in fact, show that uniform application of motivation produces uniform enhancement of results. Notorious indeed are the decline curves within continuous card-calling and from one session to another, or the so-called loss of parapsychological powers evident even in big-time results like those of Martin and Stribic (12,13). But no one can say whether increasing motivation through rewards and punishments will really work or not. It is convenient to resort to the concept of satiation or a sheer confusion in the maintenance of a favorable working attitude. Motivation is one of those things that works when it works.

The second factor, closely related to this, is that of the intensity of the affect involved in the target material, as in the experiments of Moss (14); or another closely related matter, the intensity of the affect related to the experiment or the experimenter or the experimental partner. From Moss's data we would conclude that the affective tone of the target material itself is important; but from a comparison of different experimenters working with comparable subjects, and from the studies of subjects emotionally close to one another, as in one of Stuart's series (20) and as in the work of Soal (19) with Gloria Stewart, we become confused as to the precise role which the affect is playing. No replication experiment, so far as I know, has been attempted to see just where the affect plays its part.

From this we pass to a third factor, the factor of general attitude toward the task, so intensively explored for a quarter of a century by Schneider (18) with a rather striking series of successful replications, showing that those who believe in the possibility of success in clairvoyance under the experimental conditions have scores that run above chance expectation, while those contrarily

disposed tend to run below chance. In an analysis by Mangan (11) it appears that not only has this factor of attitude been rather well replicated in the majority of studies by Schneider himself, but also in the majority of those by other experimenters. Schneider has herself drawn attention to the fact that attitude toward the experimenter and the experimental setting may complicate the procedure, and we are far indeed from the possibility of setting up a multi-variate design in which the various attitudinal and affective factors can be combined with some confidence that a particular experiment will succeed.

One factor quite evident here when you look at the phenomena from a psychometric point of view, a point of view based on familiarity with psychological tests of all sorts, is the fact that most of the scoring levels are only slightly above chance expectation. Consequently, only a very small fraction of the variance which appears in the above-chance scores is attributable to ESP; in other words, ninety-nine per cent or more of the observed variance is due to factors other than ESP. What are the possibilities for getting a decent replication under these conditions? Only, of course, by securing very large masses of material and with heroic patience and devotion continuing year after year. Where would chemistry be if the replication of its familiar daily demonstrable effect depended upon phenomena of this sort? We may think of the Curies extracting a milligram of radium from many tons of pitchblende, but here it was possible to use a sort of distillation to increase the concentration of radium, week after week. So far attempts to build up ESP in this way have failed. Of course, Tart (21) has told us that our model is wrong and that if we reward the subject for success we can build up a successful reinforcement schedule. We shall, however, be rewarding for the observed high scores ninety-nine per cent of whose variance is irrelevant to the issue, so that we are mostly reinforcing the wrong things. I don't say the problem is insoluble; I do say that we have not honestly and consistently faced it.

So I turn now to a fourth favorable factor which we think we have detected in the research literature; namely, the factor of a certain splitting of the mind, a dissociation or abstraction, or indeed we might at times say a concentration by which part of the mind or part of the functioning system is directly preoccupied with the task and the target, while the rest of the total living system is split off, minding its own business, having nothing to do with the target. In this way the vast array of distracting factors can be picked up, so to speak, and neutralized by those major portions of the organism which are not playing the game at all. This is, of course, a negative way of looking at a phenomenon. It may mean that most of

the time we are too preoccupied with other activities to have any energies for the ESP target at all. From this point of view disinhibition or the inhibition of an inhibition, as in the Pavlov system, would be the shortest road to success. In the early days of S.P.R. experimentation in London and Cambridge, hypnosis and semi-sleeping conditions were frequently used for this purpose and of course the recurrence of the concern with altered states of consciousness may give us a battery of devices for lulling to sleep all the scattered stray energies of the hapless individual who would like to hit a distant target but is actually busy in his own intrapsychic conflicts and preoccupations.

It cannot be claimed, however, that anybody today knows how to dissociate or split a mind so as to get a consistently positive result. It must, of course, be noted that the high concentration used by Rhine and his associates (16) in the early Duke days may well have succeeded because of the morale of the laboratory group and the very considerable fear of failure. In other words, the dissociation factor was not working by itself but contaminated by the motivation factor. It is quite possible that the same high motivation without any expressed dissociation could produce the same result. Some of you will remember the name of Margaret Pegram Reeves, one of the pioneers in the Duke group. Noting her brilliant application of Lewinian "field theory" to paranormal phenomena (15), Laura Dale and I tried to find out whether the decline curves that were being observed everywhere could be due to satiation, boredom, or something of the sort—that is, lack of motivation—or to confusion and inhibition from working with the same targets over and over again. We therefore made up an inordinate number of batches of targets so that fresh target material could be used for every call, thus eliminating the confusion factor and supposedly helping the motivation factor to work without contamination (5). Actually we got no significant results at all. This was one of the many things which led me in those early years of work at the A.S.P.R. to doubt whether enough is known about favorable conditions to set up any replication likely to be successful while using ordinary subjects. And this pointed to the need for finding big-time subjects who could rattle off 8's, 9's, and 10's per deck with some regularity. Actually, I have never seen anything like this. I did, however, see the extraordinary case of Lillian Levine of which I will remind you here (22). Using a two-way signaling device, she pushed a telegraph key to indicate the target in a study in which Laura Dale was acting as agent, with Ernest Tayes observing. Miss Levine went on a binge and made fifteen consecutive hits which, if you do a little arithmetic, you will find is very much too high a score to expect even if every man, woman, and child in the world had a series of,

tries at this task. Then, as suddenly as ESP walked in, it walked out. Of course Miss Levine was asked to come back and tell her life story, take various psychological tests, etc. What we did learn from this was that during the moments of success she had seen the coils of a steam radiator take on the form of wavy lines, the corner of the table seemed to indicate a star, a design in the rug turned into the shape of a cross, etc. It was one of those cases where the external form was not too firm to allow the subjective factor to play a large part in structuring it.

However, the point which I must sadly stress in the present connection is that this not only did not repeat; it didn't even last for one working session. It is nowhere near as good as the Brugmans performance (3) which, as you will remember, lasted a number of weeks, nor as good as the Moss performance now in its third replication, nor the Schmeidler performance now in its twenty-seventh year. But it was a big-time phenomenon for all that.

Where do we stand, though, regarding those psychological laws we thought we had to start with, the laws of motivation, of intensity, and of dissociation? I think they remain benign hunches from about the time of the French and British workers of the 1880's; I do not think that we have taken them seriously and have not dignified them to a point where we can even state them in firm, clear, quantitative form, ready to be given a scientific test.

But I said that there was another approach to the problem of replication, namely, one which is conceived in individual terms. That is to say, a question of the relative standing of a person in relation to others working at a task. This is the kind of thing that would appear if we asked whether individuals maintain the same rank or even the same scoring level from one performance to another. I think it is shocking that there is almost no work of this sort to report. I can think of a few tiny little fragments, but not enough even to establish whether there is any real retest reliability at all. I do not, for example, even know whether Schmeidler's sheep contained any individuals of relatively stable superiority on retest. But considering what I said about the fact that ESP is only contributing one per cent or less to the original high scores, it is scarcely probable that the long-time work of looking for such retest reliability would pay off. It would, of course, be possible to show continuity in mediumistic performance during the lifetime of Mrs. L. E. Piper, or the big-time ESP performances of Gloria Stewart or Pavel Stepanek. This, however, is a type of reliability rather different from studying a person's relative position within a group, thus permitting personality dynamics, life history and other factors to be consistently related to outcomes. I do not disparage the importance of individual long-range studies. I say, however, that

they belong to the very important sphere of biography, and that in order to give us scientific data on the conditions under which phenomena appear we need to combine them with studies in which groups of individuals and their relative performances can be studied. There is still one other type of quantitative approach to reliability possible here, namely, the one used by Riess (17) in the case of his high-scoring subject in which internal consistency in the performances, run by run, was reported; that is, a tendency for odd-numbered components and even-numbered components of the same task to vary concomitantly is further evidence that we are not dealing with sheer flukes or so-called runs of luck.

But perhaps I have said enough to suggest to you why I think our case for replicability is today quite weak despite the brilliant thrusts into the dark that have here and there been carried out. I am going to turn now to a more positive approach in what I believe to be a feasible program for the development of true and extensive replicability of ESP research material. In a way it will be long and hard, but none of us in this field has asked for anything easy. I am going to begin by a reference to a series of studies in the general field of altered states of consciousness, particularly in reference to the observed subjective or personal world of the participating subject. Years ago this would have been called introspective data, or Titchener, fifty years ago, would call it existential. The later and more modern uses of the term existential and the term phenomenological could also be brought in if you like. Perhaps the word experiential is the best of all. For I am referring now simply to getting all the possible views of the person's own experiences, both as naively given and as structured, altered, and even distorted by the attempt to describe it. I am speaking of the world of inner knowledge which has been so greatly enhanced by modern studies of yoga and Zen and most of all by those feedback techniques of Kamiya (9), Brown (2), Green (8), and others in which one can directly see on a panel the course of inner activity—proprioceptive, enteroreceptive, or EEG—which reflects the inner psychophysiological world. One thus learns, by anchoring the experiences on particular physiological indicators, to get more and more voluntary control and more and more systematic describability. We have, of course, the great tradition of German and American introspective psychology; the distinctions between sensations, images, and feelings, and the more subtle "determining tendencies" and the "thought elements" of the Würzburg School; the Galton studies of individuality in imagery; and then we have the area of feelings described by existentialists and phenomenologists. We encounter the problem of a mode of observation and a language appropriate to the fleeting and complex character of this inner world. We have

learned much from the psychoanalysts, and other clinically-oriented students, of the world between the conscious and the unconscious, whether we call it preconscious or whatever our predilections may be. Kamiya and Brown have explicitly sought to extend and enrich and pin down so far as possible this inner world as it relates to a world of EEG and the world of the processes by which these inner phenomena play their part in the triggering of the EEG phenomenon. Exciting possibilities have been suggested, as for example the possibility that in certain parts of the EEG spectrum there may be regions specially favorable for the reception of paranormal messages. I do not know whether we are ready to leap ahead that fast, but we are certainly ready to dig in and make systematic and disciplined studies of this vastly important inner world, side by side with the psychophysiological world, which students of relaxation, students of hypnosis, students of drugs, students of peak experiences have taught us to regard with the most poetic excitement on the one hand and the most desperate seriousness on the other. Brown and Kamiya have hoped that we could vastly increase our knowledge and our objectivity in the reporting of these subjective states by pinning them to physiological changes; and we may, through a good description, proper grouping, proper developmental analysis, proper factor analysis perhaps, disentangle, regroup, reinterpret, and freshly test the series of psychophysiological states which may be relevant to the subtle paranormal processes which we need to understand. There needs to be a taxonomy of these psychological states, using natural groupings and true phenomenological categories of experience as well as true physiological units or factors. Such factors must, I suppose, ultimately agree at some level with the factors found at the psychological level. At least this much isomorphism of the maddeningly complex physiological with the maddeningly complex psychological seems to be a premise with which we must indulge ourselves for the present.

Kamiya hopes that a greatly enriched new psychology of the subjective world might result in this way. Perhaps some of our old hunches having to do with motivation, emotional intensity, dissociation, and stable individual differences may be supported by these new studies looming on the horizon. But perhaps light will be thrown more directly upon a series of phenomena which we have not yet learned even to notice. It may well be that the more important characters in the play are still in the wings and that the whole action will change when they come upon the stage. It may be, for instance, that there are devices which we all use to keep these characters off the stage, so that not even a whisper of the reechoing footsteps far back in the wings can be heard. It may be that we shall

learn by these new feedback skills where we have been driving them away; we may learn the nature of the defensive operations that have been tightening up our muscles and flattening our affects—the cut-off affects which we have handled sharply as by a cookie cutter to keep us from understanding them in their whole context. They will all begin to force their way upon the stage as we learn how to disarm our own defenses, learn how to minimize the massive forces in our society and in our own individual character which make us unwilling to confront the paranormal, even unwilling to confront the very signals by which the presence of the paranormal might be hinted. The existentialist, the psychoanalyst, the physiologist will join hands with new workers from unguessed future psychological schools to enrich our understanding of that vast system of the here-and-now which has been so unwilling to come to terms with the there, the far-away, the displaced in time which is a special preoccupation of parapsychologists. I would especially emphasize the problem of pattern or system, the problem of interrelated aspects of our life and of the deep reciprocities between our inner life and the social world of physical and cultural ecology beyond which lies the world of energies not yet guessed, the world of cosmic relationships which unconsciously pushes up like a suboceanic volcano to break into the world of experience, whether of cosmic consciousness or of telepathy, clairvoyance and precognition, or of other functions for which we have no names. I would specially emphasize also the conception of interaction in R. A. Fisher's sense (7), the probability that no single variable will turn out to have coercive control of the main-springs from which the paranormal makes its entrance, but rather the more delicate problem of interactions in which major cues, as yet unguessed, may still cooperate with essential minor cues of many sorts, of which we have preliminary and fragmentary knowledge.

What a lot of work you think I am asking. Yet, look back to the physics of 1600 in which Gilbert did his experiments on magnetism, and Francis Bacon wrote about inductive methods, a world even before Harvey, and long before Newton. That world of 1600 is about where we stand with regard to any real systematic understanding of the paranormal, and the fortresses will not fall easily to the blunted shafts of today's scattered arrows, but only to the battle-axes of a very much more formidable and determined scientific effort. Even when the laws begin to line up in an orderly way, they will still, like all things biological and psychological, display staggering and bewildering individual differences which lie partly in the depths of molecular biology and genetics, and partly in unique ecological relationships between persons and certain fields as yet unguessed.

Repeatability, then, depends upon the identifiability of components and of the requisite interactions considered systematically. Very broad generalizations—for example, about relaxation and motivation—are probably just barely worthy of being mentioned again, but most of the needed work will probably be of a more specific and refined type. It is certainly clear today that the gross ESP phenomenon does not ordinarily appear just by providing large quantities of the obvious variables—motivation, emotion, relaxation, dissociation, and the rest. Challenge me if you like. Set up an experiment tomorrow using these things and get a clear-cut positive result! You know that it will not be that easy, and you know that vast numbers of unsuccessful experiments lie cluttering the desks and files of many parapsychologists. Repeating the experiments of A, B, C in the hope of getting the results M, N, O reminds me of Crumbaugh's heroic attempts (6), with the help of two of his graduate students, to replicate the beautiful series of studies of ESP in the classroom carried out by Anderson and White (1). Using a very adequate method and with very adequate groups, a clear confirmation was obtained in one of the Crumbaugh replications, and no results at all in the other. If this is true of our best studies, what shall we say of our ordinary studies? We will say that the data have not been analyzed in anything like sufficient detail to permit real replication. It is that for which I am pleading.

For good performances just won't replicate without the series of steps, the analytical work for which I have pleaded, both at the phenomenological and physiological level, and at the mathematical level of a careful study of factors and particularly of interactions. One experimenter with both genius and patience may replicate his or her own work, but it is very much more than this that we must demand. Outstanding today, in addition to the work which I have already mentioned, are the efforts of Ullman and Krippner (23). But these experiments, based upon the largest personnel and upon the most extensive pilot investigations, are still very far indeed from permitting prediction as to results of the next replication study which will be carried out. The Ullman-Krippner data especially emphasize the individual factor, the replication results with particularly gifted subjects which is, of course, a strength of method in dealing with this kind of phenomena, but which will make replication in the usual routine sense doubly difficult. It will take a long time to find out whether the results are traceable to a series of working conditions or only to those conditions when interacting with particular experimenters and particular subjects whose work is being replicated.

Now of course I will agree with any critic who would interject at

this point the fact that a computer of average transience (CAT) could probably comb out some slight generic factor that is usually present in successful ESP experiments. That is, one might find a way to short-cut with modern techniques the enormous mountain range of complexities which I have suggested. Let us, however, think twice about this. If it is really true that some slight effect can be teased out by a CAT, we will still have the problem of finding out what it is in the organism or in the organism-environment relations that made that particular effect appear. If we knew what particular parameter in our experimental procedure is causing this positive result, we would be more than half way to our goal, but that is exactly what we do not know. We shall have to do the hard psychophysiological work to find out and give a name to a particular aspect of our procedure, expression of which will then appear through the CAT. We might as well, indeed, be looking for such major components right now as we use sensitive equipment. In fact, the CAT will be a wonderful cane to assist us in walking, but a very poor crutch if we expect to swing along with its unaided support. It is only because elaborate procedures like those of Ullman and Krippner are available that there is any serious chance that some delicate positive factor will emerge in sufficient strength to be noted and measured. The computer will help us if we are already marching along.

But what can we do to prepare ourselves for what is needed at this juncture? What are our practical strategic possibilities? I think they are two:

The first strategy might be as follows: Just try patiently and energetically to replicate exactly whatever experiments have been most successful. Go back and do all over again the Brugmans experiment, the Estabrooks experiment, the Tyrrell and Soal experiments, the Pearce-Pratt, Carington, Schmeidler, Osis, Moss, and Ullman-Krippner experiments. When you get the thing running smoothly, then drag in reluctant scientific collaborators if you can find them, saying, "If you do exactly the following in the necessary order you will get confirmatory results." Then you could go after funding on the assumption that a hard-bitten scientific collaborator who has gotten results somewhat like your own will be an active, or at least a passive, support to you with NIMH, FFRP, NSF, or any other alphabetic support to which hard-nosed investigators can appeal. This, I think, is mostly what we have been doing and the results have not been exactly spectacularly rewarding. In fact, it has been the maverick individual, not the foundations and not the federal agencies, that has had the sensitivity of ear, the far-flung imagination, or the tender heart, or some combination of all these, to support us in the pinch. Maybe

it is only the maverick, spaced far out in time, that can ever be expected to support our efforts.

But there is another strategy involving three major steps: (a) Disentangle the fact, the ESP, the psi phenomenon, by all the phenomenological and other methods described above, getting the picture reasonably full and clear as to what the components in the successful method actually are; then replicate, bit by bit, and all the bits together, with a full, complex multivariate design to see the component and interactive aspects of the replicated successful outcome. (b) Then, as you replicate over and over again, disentangle some factors for specially close study, distinguishing between generic results which call for human subjects in general and on the other hand individual results which relate to the biology and ecology of a single individual who is nevertheless capable of replicated positive work month after month. (c) Then systematically separate out and also combine two or three or more factors which appear to be responsible for your success, until you get something that is intelligible as well as repeatable; that is, something which is rational in the sense that it can be conceptualized both for itself and in its relation to other scientific conceptualizable principles, something which hangs together and can be described so as to keep your colleagues informed and motivated to attempt replications from one research center to another, using not only grossly similar methods but a comparable design. What is replicable, then, in the narrow sense becomes replicable in the broader sense, not limited to particular times, places, and persons. Go after your funding for your broad support by serious scientific bodies, insofar as something has actually met the replicability test. Then, with a very, very deep breath, ask for the kinds of support that will be offered to anyone who, after this running of the marathon, this running of the gauntlet, accepts this ordeal by staff and study sections, interdisciplinary committees, editors of *ANAS* and *APA* journals, official and unofficial spokesmen of the whole array of pundits, who will determine what science can and cannot allow to exist, and will be ready to acknowledge that you have reached the goal. By that time you will have gone through all the steps requisite to build a model, i.e., a general theory of parapsychology. You will have done what Conant and Kuhn asked you to do: you will have marshaled not only a brilliant little jewel of a fact which you want everybody to acknowledge, but you will have done the hard work in putting together a new system of observations and concepts in which your jewel, like any self-respecting jewel, is properly set and can shine to advantage. Replication will not come your way as a prize for ingeniously copying some new experimental technique. It won't copy, won't replicate, it won't

even know itself when next you try to show it to your skeptical colleagues. Replication will come when the context of the event is well understood; and when it comes, it will be a fulfillment of a long series of immature fragmentary incomplete replications, none of which was ready to become the authentic fulfillment of our hope, until the phenomenon was so well understood that it created for itself a context in which it could live.

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