

COMMENTS

PSI METHODS REEXAMINED

BY J. B. RHINE

Today, more than ever, the question is being asked: Where does parapsychology stand now? Professional appraisers—sociologists and historians of science, psychologists, philosophers, science writers, and physicists—are studying this field more objectively than at any time in the past. This year (1974) will have seen various institutional advances: Dr. Martin Johnson became Professor of Parapsychology at Utrecht; Dr. Robert Morris was appointed to teach parapsychology at the University of California at Santa Barbara; and in Britain the distinguished periodical *Nature* (Oct. 18) published a report of ESP test carried out at Stanford Research Institute. Even more important is the persistent interest young scientists are showing in psi research as a career field.

Naturally I wish I could go on now to announce the founding of the full-fledged university graduate school for parapsychology so urgently needed in the U.S.A. (beyond the important steps in that direction already taken abroad at Utrecht, Andhra, Freiburg, and Edinburgh). It would be gratifying also to say that the sound economic backing this field now deserves is more noticeably becoming a reality. But these further advances, although invisible now, must certainly be on the way, for slowly but surely the signs become more favorable. A few more job openings are becoming available, more scientific literature is being published in more countries and languages, and quite a few more linkages with the academic and technological establishments have already been forged.

What, then, could it be that still retards the respect and understanding parapsychology should be expected to receive? Let us give ourselves and our research field the candid self-examination needed to put it in a more properly revealing light. It is especially timely to do this right now while one of our main issues (experimenter deception) is being held up so glaringly before the world. Let us ask ourselves frankly how sound the psi research field really is in all its primary requirements for acceptability, whatever they are; one weak link in the chain of valuation can be as limiting as another.

With this in mind I have undertaken a review of what seem to me

the three principal current threats to the total security of psi research. First in mind, of course, is the recently inflamed honesty issue. Next is the more quietly pervasive question of whether the really scientific questions are being asked. Both of these have already been discussed at some length but not enough. The third, however, is a new sort of disturbance: the question whether psi investigation may not be beyond adequate experimental control, since the ability is now admittedly not physically controllable.

The main reason for the choice of these three issues is their importance in a current appraisal of parapsychology; they are timely questions this field has to deal with. Another reason, however, has led to this grouping of the three problems in this one review. As it happens, all three have a rather critical need of better experimental method; and surprising as it may be, the solution to one of these issues has almost equal importance for another, and even promises to provide (with some development) a new type of solution to the third. This common thread of a need for improved methodology runs through the sections of this paper that treat the specific types of problems, but it does not stop with these three areas. Parapsychology has naturally had to be a heavy borrower of means and aids to research in the course of its long, slow advance in the technology of inquiry. It now looks, however, as if its own more distinctively unique technology of experimentation will be more emphasized in the future.

Section I

THE ISSUE OF EXPERIMENTER HONESTY

"Psi watchers" will probably agree that 1974 looks like the "year of experimenter deception"—that is, the year of *talk* about it—in parapsychology. Ironically, I cannot recall ever having experienced so active an exchange on the subject of parapsychology, especially with the popular news media, as I have had since the news of the Levy exposé came to the attention of the press. Not even the most exciting controlled laboratory experiments have ever stirred such concerted press interest.

I had hoped that making the essential facts available to those professionals who needed to know them would be enough in the way of publicizing this unfortunate event. But as it turned out, the tragic personal angle was seized upon by the press—as if that professionally less important aspect were the main point—and Levy, the Institute, and the field as a whole achieved instant notoriety on an international scale that, it would seem, far outweighs for the time the total positive

contributions of the entire research. It would appear that scandal is a better seller in the news media than any of the discoveries so far made in the field of parapsychology.

But while the restless waves of publicity keep on rolling, serious efforts to recover from the professional blow must continue. Clarification is still needed in many quarters because of the scientific interest that had been shown in Levy's work, all of which now of course must be considered suspect. As stated in my June article in this section (Rhine, 1974a), judgment must be suspended about all research reported under Levy's name (or otherwise) in which he participated.

The emphasis now must be on the adoption of further preventive measures designed to make dishonesty in research impossible. One can even see some benefit from the Levy affair in the stimulus it gives everyone to cooperate now more fully in making parapsychology a model field with respect to reliability in research. Such resolution, however, as readers of this section should be aware, is by no means a sudden change of attitude. Nor will it constitute any crushing weight to be thrust suddenly on the conscience of the beginning researcher. Naturally, it will take time, training, and some careful trying-out to implement the improved safeguarding that now must be considered on a wider front than ever before. On that account I am especially hoping for cooperation in unifying respect for experimenter security and all aspects of reliability in the field. I think it is likely that we can do now what I could only hope might one day be accomplished when I wrote about experimenter deception (Rhine, 1974b) just a few months before the Levy case broke.

That earlier discussion made no headlines in the media and, in fact, might have stirred little concern even among present-day parapsychologists because it referred to work carried out thirty or more years ago, with methods and other conditions that have long since been considerably outdated. In this regard, some aspects of the Levy case are more telling. The deception not only has the freshness of recency, but it also was caught "red-handed" and was admitted; and it involved test methods closer to the best conditions in use in research today. The harpoon really struck home, as perhaps never before in our field; and this should be one of the salutary aspects of the situation. It allows immediate and forthright emphasis on the changes needed and, as I have said, encourages coordinated effort in achieving them. The problem itself concerns us all.

But there is another positive note to add: research is becoming easier rather than more difficult with all the more recently developed advances in safeguarding. With improved electronic equipment and

automatic recording, it is not only easier to make psi tests; it is also more convenient for experimenters to team up on activities that take time and special skills which can be handled better when they are shared. Now, too, it is increasingly likely that more researchers will be willing and interested in trying to repeat the work of their own and of other laboratories.

The Way of Maximal Security

The principal consequence emerging from the recent agonizing reflections over research and security, however, is one that I made in that same article on experimenter honesty in the March issue of 1974, in a section entitled "Incidental Evidence: The 'Signs of Psi'." There I referred to evidence that had unexpectedly appeared in certain psi experiments, unknown both to the experimenters and subjects until discovered later, usually by other researchers. Thereafter, with the objective records, often in duplicate, it was possible to make independent analyses as many times as desired. The cases I used to illustrate these peculiar "signs of psi" were the declines in scoring rate in the test run, or other position effects such as U-curves of hits in the run. I might have cited many other examples of such effects that were not a part of the experimental design, but which have been found reflected in numbers of other test series that used similar test conditions. Often they were not found until after the experiments had been completed and reanalysis of the results by other experimenters had been made in search of such evidence. In these instances the ordinary questions about honesty do not properly arise; if there is doubt about the analyses, they can always be repeated *ad lib*.

The point I made in the earlier article was that such findings as these were extremely good evidence against the common weaknesses that might be encountered in psi-testing but that they were, as stated, largely incidental, that is, unplanned. We were looking for an original test design that ruled out experimental error, deliberate or unintentional.

I recognize that while I had based my own judgment of the psi research findings most of all on these incidental blocks of data (like those showing psi-missing, diagonal declines, and other position effects), I also wanted a more easily adaptable and currently manageable technique for the elimination of the fraudulent type of insecurity. I thought (and still think) that this is an attainable objective if we want to work and wait for it.

But as I have studied the "tracer" type of evidence (signs of psi) further in recent months (with other aims in mind, as will be seen), it is

becoming clearer that these incidental patterns of hidden evidence are more varied and prevalent than I had supposed. More than that, we are less dependent on the fact that such evidence is incidental (unexpected by the experimenter) than I had at first been inclined to think. It does now look as though this kind of "earmark" proof of psi can probably be developed so as to take care of the psi security problem very well, aided by independent recording, and double-blind safeguarding. Also, studies will now be made to see how adaptable the design can be when new tracer methods are combined with these other security devices.

First of all, however, a thorough reappraisal and a broad discussion are needed, along with a roundup of the whole range of psi peculiarities, old as well as newer ones. I think first of all of the many types of position effects, of all the known peculiarities of psi-missing, the varieties of displacement, the psi-patterning effects evident in consistent-missing, the psi-differential effect, and a range of other target-response associations. It is quite stimulating to think of all the psi quirks and "finger-print peculiarities" that have already emerged even before anyone began to look for them.

Fortunately, a few workers in other laboratories are already involved in these incidental and secondary psi manifestations for other reasons and their results are among the more challenging features of the field. The historically most extensive block of these studies, was one that was made on the large collection of PK research records at the Duke Laboratory in the early 1940's and reported by Dr. Betty M. Humphrey and myself (Rhine & Humphrey, 1944a, 1944b), with reanalysis by Dr. J. G. Pratt (Rhine, Humphrey, & Pratt, 1945). So strongly conclusive is this type of evidence that a cautious student today wishing to test his own judgment as to the occurrence of psi could hardly do better than to start by first thoroughly rounding up this type of "incidental evidence." Such a roundup will be supplemented by the present reexamination of the other uses to be made of these signs of psi, as will be discussed in Section III. But the evaluation of psi research methods of dealing with specific problems will continue on through Section II.

Section II

ARE WE ASKING THE RIGHT QUESTIONS?

At this point it may appear to some readers that now, while the patient is sick in bed, I am taking advantage of him to try to save his soul. But no, I reply, that does not happen to be true. Rather, para-

psychology has two serious illnesses, and either one could be fatal. It is strategic, it seems to me, to treat them both without delay. Furthermore, it will be shown in a later section that the prescription for one could be applicable, at least in part, to the other. The present section, however, is concerned wholly with illness No. 2.

In the preceding issue of the *Journal of Parapsychology* there was an article (Rhine, 1974c) on the telepathy problem in which I briefly reviewed again the difficulty of designing a telepathy test by means of which a reliable conclusion could conceivably be reached. Then I went on to list a number of other research issues concurrently being pursued that similarly cannot logically be brought to solution by current methods—e.g., post-mortem survival (PMS), astral projection, and retroactive psi. The difficulty in each case was briefly indicated. I characterized the many studies of these problems over the past hundred years as largely resulting in a great amount of lost time. Not one of them has proved its intended case, so far as I know, even to parapsychologists. As I have repeatedly stated it, these researches should therefore no longer be continued as before, since logically no known test design can yield acceptably conclusive evidence. On the other hand, they ought not to be dismissed either; rather, they should be indefinitely shelved until, if possible, an effective approach can be devised.

This is not, of course, a moral issue; but it is quite a serious one for parapsychology. As I look at the research field and see all of its limitations in men, money, space, institutional status, and scientific respectability, I would estimate that preoccupation with these inconclusive problems has handicapped parapsychology more than all the known falsification of data by experimenters throughout its history. Accordingly, one of the biggest advances this field could make would be to improve the selection of the research problems undertaken in the name of parapsychology and to choose for the present stage only the more viable ones. No other step would seem so crucial to the sound objectivity essential to the experimental type of study.

And yet, before I go further let me balance the issue fairly. I have no hostility toward these unsolvable problems. To most of them I have given some attention at one stage or another of my own research career. I realize too that many devoted parapsychologists have been working on these questions, and they all have personal reasons acceptable to them for going ahead, even without any clearly foreseeable way of answering the questions.

There is, I realize, a kind of irresistible impulse that seems to these

able, experienced, and comparatively well-supported investigators to justify them in following deliberately, year after year, a path which they must see, at least dimly, leads to no possible logical assurance of a definite solution. And yet, this unreasonable hope is an abandonment of the objective thinking that is essential to science. Therefore I urge such researchers again to investigate the types of problems that have long been yielding definite conclusions. If they will help to find out more of what we need to know about the basic psi capacities, it should be possible in time to take up effectively some of these currently unanswerable questions.

An Alternative Approach to Post-Mortem Survival (PMS)

After I had first written this section and was at work on the one that follows, it dawned on me that the new light on psi methods appearing there (in Section III) seemed relevant to some of these questions of Section II that I have long been saying are currently unanswerable. This was to me surprising indeed; for although I had anticipated in a general way that when we knew enough about psi we well might be able to design a better approach to the PMS question (and perhaps to some of the others that similarly lack good methods at the present stage), I had thought of no such approach. Now it began to look as though this stage was coming into view, at least in terms of logical feasibility, which was the very quality missing in the older methods.

Let me recall that the main difficulty in regard to all of the PMS research had been the lack of methods of identifying the medium's source of "supernormal" (ESP) information. There was always the alternative hypothesis, of course, that she got it from "terrestrial" sources by basically the same ESP ability as that assumed by the idea of mediumship. But after finishing Section III of this paper, I realized that this PMS difficulty was in principle close to what Section III itself was mainly about—namely, more efficient methods of psi discrimination. There was enough similarity between these problems to start a new line of thought about the PMS problem.

Here I will leave the PMS question for the present while turning to the rather formidable-looking difficulty psi researchers have been forced to consider in recent years in the interpretation of test results.

Section III

THE PROBLEM OF PSI INDETERMINACY

My annual report for 1973 in the *Comments* section of the *Journal* (Rhine, 1973) ended with what was headlined as "The Problem of the Year." This was the question of whether or not we were approaching

an experimental dead end in parapsychology. The fairly typical instance was cited of an experimenter engaged in the testing of animal subjects for psi ability. The problem was that in the end the experimenter could not reliably tell whether he or the animal was responsible for any significant results produced, since either one or both might possess psi ability. This type of question concerning the indeterminacy of psi had also come up with human subjects in the course of many years of investigation; but in the recent work with animal subjects it was fast becoming almost a regularly recurrent issue in the interpretation of the results.

Certain steps were taken, of course, to meet the difficulty. The experimenter would usually introduce certain physical conditions aimed at discriminating between himself and the animal as the possible contributor of the psi results; however, each new step that was made to try to bar the experimenter's influence was met with the fact that it was well known that psi could not thus be physically excluded. If, let us say, the experimenter went to another room in order to isolate himself, it had to be conceded that the intervening wall and distance could not safely be assumed to block the psi process. Or if he shifted the timing of his presence in the laboratory, leaving the experiment to someone else who did not know the experimental problem and design, this also had to be considered inadequate because (since there is strong evidence of precognition) a time gap, like the one of distance, did not necessarily prevent psi exchange. So the baffling problem remained—the problem of finding a method of discriminating conclusively between the psi ability of the man and that of the animal, or more generally, between the experimenter and the subject (human or animal).

It was logical, of course, to advance as far as possible toward the complete automation of the test (including computer control of the various stages of the subject's participation or lack of it) with the aim of searching for indications that the psi results obtained were (or were not) produced by the animal. But even this left at best a somewhat blurred distinction regarding the source of the psi in the case of such experiments as one reported by Dr. Schmidt (1974) in which the aim was to test whether or not psi could penetrate exceedingly complex combinations of equipment and method. As had often been suggested earlier, it was found that complication *per se* did not seem to be a barrier. In fact, when results like that and also all the considerations of precognition tests are taken into account, tests in which the selection of the future target order is to be made on the basis of intricate randomization processes and apparatus, including quite a variety of models of

random number generators, the conclusion has to be that the psi process is not to be regarded as containable even by complex physical barriers.

Since it appeared therefore that psi is to such an extent unrestricted by physical conditions that no test design based on them can adequately isolate one subject from another (or, for that matter, one test object from another), this lack of physical containment appeared to present a generally baffling problem for experimental parapsychology. It meant that physical controls were in principle bound to be ineffectual in most, if not perhaps in all, psi experiments. The situation thus looked like one of general psi indeterminacy, a status that, as we have already seen in Section II, logically leads to the need to shelve the problem concerned, no matter how important it may be.

The recognition of this question of psi indeterminacy is only relatively recent. It arises out of new interest in the psi process beyond the mere establishment of its occurrence and the other more elementary findings about its nature. With human subjects the question has arisen most frequently in the course of PK experiments in which other persons besides the subject were present (experimenters and assistants) whose PK ability could just as well be supposed to contribute to the test results as that of the subject himself. In most experiments it did not matter at that stage whether the experimenter or the subject was the actual contributor; the result would still be PK. But it actually *can be questioned whether any PK experiment has ever been reported in which, if an experimenter or other person was involved, his influence on the results was adequately ruled out by the test design.* And even if the experimenter was physically separated from the target objects (e.g., dice) he could have been aware of them by ESP, and his own PK ability could still have influenced the results.

On the ESP side of the research there seems at first to be somewhat less of this difficulty since there is at least less likelihood that the experimenter enters into the subject's guessing response. But the Kreitlers' (1972, 1973) recent work, as well as that of Stanford (1973) reminds us that a subject in the ESP test situation who is having difficulty identifying marginally perceptible targets can draw information by ESP, even unconsciously, from another person nearby (even in another room) who has the needed information. It appears, therefore, that a wide reach of possible psi influence has to be recognized. None of the known physical barriers can be considered effective against psi interaction. This, then, raises the question *whether reliance on physical barriers has any place at all in the researches of parapsychology; or if it does, just where is the line to be drawn?*

How Extensive is the Problem?

Does psi research need physical controls? The answer is yes. The very proof of the occurrence of psi depends for the present, at least, on physical controls. Such controls are required in every psi test in order to exclude sensorimotor activity, which by nature is physically based. In fact, the occurrence of psi could not in any known way have been proved conclusively without the necessary physical conditions to exclude sensorimotor activity as a counterexplanation of the test results. Moreover, it is equally evident that the need for strict physical test conditions will (according to present knowledge) continue in every psi test ever to be conducted. This, at least, is the way the matter stands until further knowledge of psi provides, if it ever does, ways of recognizing that ability by its own uniquely characteristic features or signs.

But is this single universal need for physical controls in psi research the only one that exists? In other words, is this the only place for physics in psi methodology? Logically it should be so. Since psi itself is non-physical it should need physical barriers only to keep sensorimotor abilities from intruding into and confusing the psi test situation. The severest test perhaps is the question whether proof of the distinction of the types of psi may not also be dependent on physical methods. Does it not (for example) take different physical conditions to distinguish evidence of precognition from that of PK? The answer is no, that the very same physical conditions that would be used for such separation are those that have repeatedly been experimentally shown not to block psi activity. In fact, that is exactly how we came to know that psi is not physical. Rather, what it is that makes the test for the different subtypes of psi effectively discriminate between them is quite another kind of method; call it for the present psychological—one that can be recognized here as at least distinct from physical. It will suffice now to say that this other (i.e., nonphysical) methodology appears to be logically the very kind needed to solve this problem of psi indeterminacy. This I will now undertake to describe.

Psychological vs. Physical Methods

As I have just now stated, the testing of psi communication between a person and his environment needs physical controls only to exclude sensorimotor effects; all such tests that reach beyond that requirement must depend on psychological conditions of control. Such a distinction of methodology would now appear rather essential to parapsychology and therefore needs to be carefully considered even while we proceed to develop it, implement it, and, in a provisional way, to test it.

This differentiation of psi test methods into physical and

psychological types has not hitherto been generally emphasized; as a matter of fact, only within recent years has it been found necessary to recognize it explicitly. As stated at the beginning of this section, the issue emerged as an immediately urgent one in 1973 and was associated first with the difficulty encountered in animal researches in parapsychology in excluding possible psi influences by the experimenter. But for that matter, general psychology itself, with its limiting preoccupation with sensorimotor behavior, has largely passed over this distinction between physical and psychological methods of test controls. Psychology, however, has had no objective methods of finding where physical methods ended and something distinctively mental (i.e., nonphysical) began—that is, not until psi testing began to be taken seriously.

In parapsychology one can now see better how, in earlier testing for subtypes of psi, certain purely psychological elements of design could enter into a test; for example, in a test of precognition the subject could try to guess the card order in the deck at the time or as it would be at a set future date! Or he could try to register either ESP or PK on the Schmidt push-button machine, the essential difference being a subjective one, with the physical apparatus essentially similar in both cases. Likewise, the role of subjective attitudes in ESP tests gives another example of mental factors operating differentially under the same physical set-up. Of this type, Dr. Schmeidler's comparison of "sheep" and "goats" (subjects with pro and con attitudes, respectively, toward ESP) in group clairvoyance tests was one of the first. Only the sensory screening of the cards was "physically" controlled; the comparison of attitudes was subjective, and this alone affected the results. So it was a nonphysical, psychological test method. Now, of course, this distinction is easily seen, but no one was looking for it then.

As it had been, and still is for the most part, the whole cultural drift of all the sciences had favored the ignoring of such possible differences of methods as this one. After all, no way had been found of sharply proving experimentally whether or not there was a verifiable limit to physical methods in psychology—none, again, until parapsychology came onto the scene. It is not surprising, therefore, that psi research went on as long as it did, not expressly raising the issue over this distinction of methods between physical and psychological controls. As already stated, it was a *practical* empirical problem in the animal work that raised the question.

Psi-Missing

My own first experience with qualifying psychological factors in psi

testing came to mind while testing A. J. L. for clairvoyance one day in 1932. In this investigation a sharp drop in scoring rate occurred, ranging from a level well above to a level below theoretical chance expectation. I did not then know I was keeping the subject overtime, but he knew and was anxious. As a result, his normal psi effect was reversed and, by *missing* the targets, he showed a rate of deviation below "chance" (psi-missing).

Later analyses of test data showed further that under some conditions some subjects even made systematic errors in this target avoidance; e.g., stars were by some ESP subjects confused with crosses. Thus there sometimes appeared individual patterns of consistent missing on certain targets (Timm, 1969). Hence, in a variety of tests it was indicated that distinctly *psychological* factors determined, not only the level of scoring rate but also, and almost equally important, whether it was above or below mean chance expectation. It became increasingly obvious that it was not any sort of physical determinacy that governed psi; rather, throughout its varying manifestations, the ability responded to psychological determinants such as the individual mental states, attitudes, and traits of the subjects. These lawful mental effects were in fact *characteristic signs* of the psi test results given under certain conditions, and in themselves were especially fraud-proof evidence of the presence of ESP (or PK).

Position Effects

Still another peculiar sign of psi was the subject's reaction to the structure of the test, especially to the record sheet he used. The position effects that resulted were especially marked if the test runs were long and the testing became repetitious. The rate of success tended to decline, especially if the subject recorded his own calls or responses. Usually the first segment (e.g., of five trials) of the run would show more hits than later segments; the first run on the page would be likely to show the highest score. Such effects of the position of the trial depended of course on the subject's psychological reaction to the framework of the test.

These findings on position effects and psi-missing, like all those showing the signs of psi under discussion here, came to light first in ESP experiments rather than PK tests because of the historical priority of ESP research; but later on, when the accumulated records of the PK tests with dice were also examined for patterns of hitting, the results of these analyses showed similar effects of both types, psi-missing and position effects. If anything, the latter were even more regularly recurrent in PK than in the ESP experiments. It was not, however, until

many years of PK testing had taken place that the examination of these curves of hit distributions was made on the PK records. Meanwhile, the prior attention given to the position effects in the ESP work had thrown some light on the conditions favoring them. It had been found, for example, that it was the *subject's awareness of, and response to, the structure of the record sheets that caused position effects and determined the patterning*. This explanation had come out most plainly in the comparisons beginning in 1941 showing that if the subjects in an ESP test did not see the record sheets (and thus were unable to keep track of the position of the trials) the curves did not appear (Rhine, 1941). The curves, therefore, seem to have been produced psychologically by the subjects, and especially when the subject was the only one concentrating attention on the sheet.

Position effects not only became a fairly obvious earmark or "sign of psi," but (as can now be appreciated) they could also be considered a useful clue as to who had contributed the evidence of psi which was shown by the test results. The subject's differential psi response was plainly a reaction to *psychological* conditions. It was the subject's lawful mental reaction to the pattern of the record page. Now, however, we can see that incidentally, in giving a special indicative sign in the form of position effects, the psi function itself provides an experimentally manipulable method for use in tracing the operation of psi. As has been mentioned, the position effects had already been giving evidence of psi under conditions that were proof against dishonesty on the part of either subject or experimenter. Now they may be seen to serve the further purpose of being a *useful identifying sign of psi* in the exploratory technology of parapsychology.

By 1943 the patterns of hits in the large collection of accumulated PK test data (as mentioned at the end of Section I) had been discovered quite independently of any knowledge of them on the part of the original experimenters, who had collected the test results with other aims in mind. These extensive analyses confirmed the position effect evidence of the ESP work and when completed, confirmed, rechecked, and published they furnished one of the most reliable and extensive bases of support for the occurrence of psi on record. The point I am making now about these position effects and the various other characteristics of psi test data, however, is that they are criteria peculiar to psi that may be used in the experimental discriminations the research most acutely needs to mark the field as a science. Here is a type of method on which to build future psi test designs that do not depend on the indeterminate criteria of physical controls. *The significance of the method rests on the psychological factors that produced the position effects*, in this case

mainly diagonal declines on the record sheet. These effects, as stated, provide the strongest type of proof of the occurrence of psi; but more to the point of this section, the quarter distributions (QD's) of scoring on the page and the subunits of the page shown in these PK data constitute the best evidence of a distinctly psychological method, one on which psi research is now dependent and which it has itself developed; hence it is at least primarily a parapsychological method.

But (as an afterthought inserted since this section was first written) I wish to add that these findings of the QD's just mentioned do not even depend on the physical conditions of the tests for the ruling-out of sensorimotor counterhypotheses, as I had earlier thought (and said in tentative terms above). It was possible neither for the subjects to have produced the results by cheating, nor for the experimenter to have done so by falsifying the data. It could only have been PK that gave the "signs," those indicative and oft-confirmed diagonal declines, first on the page and then independently confirmed in the sets and half-sets. It now seems to me that for the identification of psi this opens up the future prospect of reliance upon parapsychological test methods pure and simple. If so, it is a forward step, or at least a fortuitous stumble which, even a few pages back, I had not anticipated. But while warning is in order against hasty acceptance, it spurs one to think hopefully about further advances in methods. We may yet have in psi testing a rather more unique methodology than has been anticipated.

How Well the Psychological Methods Apply

If now we turn to see how workable it would be to utilize such psi-identifying devices as position effects in exerting experimental control over the psi process, a number of possibilities present themselves already, even at this early glance. For one type of example, the experimenter who wants to shield his subject's test record from his own (or other) psi intrusions could do so by adapting the test to conditions that would be expected to induce a certain type of position effect in the subject's results. No one else would be subjected to that special set of conditions, and consequently there should be no confusion over the source of the resulting psi influences. (The subject could even be given a personal choice of certain designated "sign" techniques to add further to the isolation of his performance from experimenter influence.)

For the use of psi-discriminating devices in animal tests I will make only elementary suggestions now, since we know as yet too little about the oddities of animal psi. Some of Dr. W. J. Levy's reported findings will do to illustrate, even though they are still awaiting replication. He

reported a deterrent effect on psi success when on the preceding trial the animal was shocked for missing; also, the animal was said to have missed more often when it jumped back and forth frequently; and again, the more it stuck to fixed habits, the less psi ability it showed. Now the more these special psi effects become known, the more readily identifiable the animal's own psi performance should become. It will be best, however, to attempt to acquire more special animal psi characteristics than are known as yet before trying to design tests with a wholly conclusive separation of experimenter and subject in the animal work.

It will not be entirely easy in general to develop the test procedures needed to discriminate clearly between different participants in the test situation until we learn more about the full range of psi indicators and earmarks. But the variety of "signs" is already considerable, in addition to the many types of position effects such as I have mentioned. For example, there are the varieties of psi-missing effects including consistent-missing and its related phenomena. Displacement, focusing effects, and response bias belong in this category of special psi response modes. There may, of course, be numerous other odd features yet to be discovered and put to use; it is probable that several more have been found and forgotten, the present interest in them not having been awakened at the time.

However, it is sometimes necessary to distinguish more than experimenters and subjects in psi testing; special methods are needed even to separate the results obtained with two or more target objects or other elements in the tests. For instance, in an experiment reported by W. E. Cox (1971) the senior experimenter wanted to compare the effect of PK on two weights of dice (lead and celluloid). To minimize conscious bias, he painted both types the same color and, by using a mechanical throwing device, arranged to conceal the other signs of physical differences from the subjects and from the recording assistant. The subjects nevertheless unconsciously differentiated the two weights as shown by the results, the heavy ones giving negative deviations and the lighter ones, positive deviations, with both deviations about equally large and with a significant difference between them.

Thus it seemed safe to say that the PK effects on the two weights of dice were nearly equal in magnitude but that the subjects reacted differently to them subjectively by producing deviations on them in opposite directions. The subjects thus unconsciously showed a psychological distinction (through the sign of deviation) in their PK effect on the two different weights, even while showing an equal amount of PK in both. The mass thus did not affect the extent of the PK influence but it did have a psychological effect. So this (first-of-its-type)

experiment offers still another way of surmounting the barrier of psi indeterminacy by using a peculiarly psychological test design to obtain unconscious differential psi response to two types of targets when compared experimentally. At least a beginning has been made that might be useful in the comparison of still other physical conditions besides that of the mass of the targets in PK tests.

Developing Psi Methodology

The basic idea, then, in dealing with the physical indeterminacy of psi is to design into each experiment the psychological controls that are known to differentiate the results desired. For this, as is now evident, it is necessary to go beyond mere physical control of targets, personnel, or other conditions. Since the principle being tested is nonphysical, it can be experimentally controlled only through psychological factors and conditions. The success of such control is best evidenced by the identifying signs of psi that have been discovered incidentally over the years of experimental testing and analysis.

It is true, we do not yet know much about the signs of psi—their number, range of variation, and the like—important though they are as indicators of that ability. As must be evident, we are only now beginning to recognize the peculiar psychological oddities of the psi function. But the more we learn about them in the future, the easier it should be to design definitive test procedures. As it is now, no discernible limits to the range of subjective methods of exploring psi have been found.

Thus far we have been thinking mainly of how to trace the personal source of psi test effects obtained in an experiment—whether, for example, the experimenter or the subject, or both can be proved to have been responsible. However, in Cox's case (mentioned above) the psi distinction between two types of dice was at stake; in others, a comparison of various other physical properties has been attempted. As was mentioned above, too, one of the inviting possibilities that may lie ahead would be to find out how to trace the source of a psi message reliably, that is, to identify (in the laboratory test itself) the origin of a given bit of psi information, just as can be done to some extent in more developed types of communication. Only small beginnings have thus far been made with ESP. Finally I will add to this the need to develop a technique of confidence rating for psi test results. (This would consist of a sampling device for estimating in advance what the success of the final results would be.)

Section IV

A NEW METHOD FOR THE POST-MORTEM SURVIVAL PROBLEM

So far, I have indicated that a type of psi evidence already more than thirty years old, can now be recognized as not only highly dependable with regard to the long-standing risk of experimenter fraud, but also can counter the threat of psi indeterminacy as well. I go on now to add that these two major consequences of this type of psi methodology are not the whole story of its advantages. As another of its virtues, it should be an added spur to the search for other identifying marks and properties of the psi function in general.

Still another turn to be expected is that of the successful testing of old problems that have been shelved (or stalled) for lack of a method with which to attack them, as in the case of the question of post-mortem survival (PMS). It is at least logical to think this stage may be approaching now. Not only do we have more knowledge of psi ability than ever before—and this ability may, of course, be assumed to be the essential principle of communication underlying the PMS hypothesis—but also we have a better setting for the science of parapsychology in which to present the problem of PMS. It can now be made a rather logical question for biology, just as psi ability has logically come to be regarded as a part of an extended psychology.

Moreover, the PMS question can now be more neutrally stated: Is there any recordable sign of personal continuity beyond death or does every trace of mind, including psi, disappear? This question allows a fairly good possibility of an answer, yes or no. Up until now there has been only the search for a possible affirmative answer, sufficient for first attempts at exploration. A negative answer could not have been expected. Now it becomes a more basic question as we inquire what the total role of the psi system is in the organism, how it functions on the various levels and stages of life and embryonic development. Does it have any sort of bodily localization, or any necessary physiological accompaniment? Does it show any degree of somatic interdependence in any way? Or any verifiable independence whatsoever? More specifically, what is the relation of psi to sleep (natural or induced)? Does this mental function ebb and flow with fluctuations of vigor, health, and illnesses of various types in man and the many animal species that can now be counted on for a broad basis of evidence? With all the advances in equipment and methods now available we can well expect to find out what the entire psi side of life is like, how closely identifiable and how far separable, if at all, from the rest it may be.

The PMS question may be made easier today by asking as a first step

if *any* living creature really has *anything* deathless in its make-up—anything registrable. If some function, once recognizably vital and personal does survive, it would most likely, almost necessarily, be psi-like, or even more likely some kind of psi-system. Psi is nonphysical; it is not known to be localized in the body. Thus psi at least would probably be a part of any immortal element, if only a communicating link or function. Then since a number of animal species have shown psi ability (at least in some members), we could with many advantages indeed start with the “guinea pig” approach. It would of course be just one of the ways of proceeding.

The key difficulty with the PMS question in the past has been that of adequately identifying the source of messages suspected of having had a post-mortem agency. The methodology about which I am now hopeful encourages the researcher to move ahead on acquiring distinctive signs of psi for tracer application to just this type of situation, quite as much as any. Whereas in earlier PMS research we sought methods of identifying memories and other personal characteristics to verify the implied source of the medium’s messages, the new approach would fix *first-step* attention on the hypothetical communicator’s most identifying signs of psi. In the beginning the aim would be to work with animal subjects to develop a design that would allow the study of long-range, gradually lowered states of consciousness and eventually of the terminal stages of life. The various curves of life processes may be expected, on a non-PMS hypothesis, to be closely paralleled by the curve of psi activity. On the other hand, the best possible techniques of psi communication will objectively trace and graph any indications of independent continuity of peculiar personal signs that persist on into and beyond the final stages of declining vitality—if any of them do. We may well hope to have all related sciences working together on the problem—much as has developed in the counterpart study of the *origin* of life.

If, in one animal (and species) after another, nothing of the distinctively indicative psi communication exhibited in the state of highest vitality keeps on manifesting its identifying messages after lower and lower levels of vital activity are reached, the evidence would lead in time to a negative conclusion. On the other hand anything showing continuity beyond life’s end would challenge the method to its utter extremities—that is, to show whether other living persons other than the subject himself could be contributing the evidence in question—that is, back again where we began in Section III. It is that dilemma which I think can now be successfully resolved.

Whatever the results might be with animals, the method could be applied to man, at least with terminal patients. But it can well be done with anesthetized persons, either under medical treatment or even better, with normal healthy volunteer subjects. With advanced psi test methods for tracing results to a given source, this ought to have a fair prospect of realization; at least nothing about it is beyond the rational expectation that might follow from the record of psi research, so far as I know it, today. If we are now going to be able to determine whether it is the subject or the experimenter who is producing the results in the psi laboratories, it would seem safe from now on to expect that we could with increasing expertise trace the connection of psi correlates with states of somatic deterioration, temporary or irrecoverable. In any case, it is not necessary now to be highly confident; it is enough to see a clearly logical design possible, one that can be followed up and one that should justify a trial. But I need hardly add that such a research can be well justified on grounds other than interest in PMS alone.

It would be a proper first step into this PMS program to explore and develop the best possible psi indicators, especially for animal test procedures. But without delay, the roundup of the whole wide range of parapsychic signs should be undertaken. Such an inclusive catalogue of psi quirks and earmarks is needed, not just for the new biological attack on PMS, but for almost everything important still to be discovered in this field.

While the field stands to gain much from such a long-view project, there is little yet to indicate the probable outcome. Let us recall, however, that a whole century of effort has thus far led at most only to a possibly improved method of exploring the question. Still, we need on balance to remember that a much longer period than that, with hundreds of times the exploratory personnel and resources of scientific inquiry, have hardly scratched the surface of man's most nearly comparable problem, that of the natural *origin* of mind on the planet. Had that more massively concentrated effort to discover the introduction of life and mind been a highly successful one, psi explorers today could do much better with this question about the other end of the story of life than it is possible now to do.

What we can expect, I think, at the very least is a definitive answer one way or the other in the course of time. One little reliable sign of PMS as showing the extrabiological nature of psi, either in animal or human subjects, that would stand out as clearly as its extraphysical nature has done over the years, would electrify the whole field of parapsychology as nothing ever has. However, there is now no firm

basis for a prediction either way. If, on the other hand, the discovery should eventually be made that there is absolutely no discernible sign of a surviving element of personality to be found when the entire problem area is fully explored and charted, it would have to be accepted—and even regarded in time as fully as much of a discovery for human knowledge as if the conclusion had gone the other way. It now seems clear on the basis of present knowledge that it could go either way, according to “the way it is in nature.” What mankind really most needs to know is just what his actual nature and destiny are in all their fullness and factuality, whatever that may be. Illusion, however pleasant, could have but low survival value even for the lay world today.

Yet we are still so vastly ignorant about life and mind and their origin and functioning that I doubt that anyone has a reasonably close guess (or rational inference) as to the great ultimate universal truth about them. It is likely to be beyond present power to comprehend when and if eventually it is revealed to the sciences. What matters most today, in any case, is that we faithfully preserve this indescribably wonderful privilege of exploring as best we can, intelligently and responsibly, on ahead into the great unknowns of human nature and destiny with all the endless reach of curiosity, method, and design the expanding sciences can command.

REFERENCES

- COX, W. E. A comparison of different densities of dice in a PK task. *Journal of Parapsychology*, 1971, **35**, 108-119.
- KREITLER, HANS, & KREITLER, SHULAMITH. Does extrasensory perception affect psychological experiments? *Journal of Parapsychology*, 1972, **36**, 1-45.
- KREITLER, HANS, & KREITLER, SHULAMITH. Subliminal perception and extrasensory perception. *Journal of Parapsychology*, 1973, **37**, 163-188.
- RHINE, J. B. Terminal salience in ESP performance. *Journal of Parapsychology*, 1941, **5**, 183-244.
- RHINE, J. B. Annual report on parapsychology. *Journal of Parapsychology*, 1973, **37**, 228-240.
- RHINE, J. B. A new case of experimenter unreliability. *Journal of Parapsychology*, 1974, **38**, 215-225. (a)
- RHINE, J. B. Security versus deception in parapsychology. *Journal of Parapsychology*, 1974, **38**, 99-121. (b)
- RHINE, J. B. Telepathy and other untestable hypotheses. *Journal of Parapsychology*, 1974, **38**, 137-153. (c)
- RHINE, J. B., & HUMPHREY, BETTY M. The PK effect: special evidence from hit patterns. I. Quarter distributions of the page. *Journal of Parapsychology*, 1944, **8**, 18-60. (a)

- RHINE, J. B., & HUMPHREY, BETTY M. The PK effect: special evidence from hit patterns. II. Quarter distributions of the set. *Journal of Parapsychology*, 1944, **8**, 254-271 (b)
- RHINE, J. B., HUMPHREY, BETTY M., & PRATT, J. G. The PK effect: special evidence from hit patterns. III. Quarter distributions of the half-set. *Journal of Parapsychology*, 1945, **9**, 150-168.
- SCHMIDT, HELMUT. Comparison of PK action on two different random number generators. *Journal of Parapsychology*, 1974, **38**, 47-55.
- STANFORD, REX. Extrasensory effects upon associative processes in a directed free-response task. *Journal of the American Society for Psychical Research*, 1973, **67**, 147-190.
- TIMM, U. Mixing-up of symbols in ESP card experiments as a possible cause for psi-missing. *Journal of Parapsychology*, 1969, **33**, 109-124.

Institute for Parapsychology
College Station
Durham, N.C. 27708