PSI FUNCTIONING WITHIN A SIMPLE COMMUNICATION MODEL

ROBERT L. MORRIS

Defining What We Mean by Psi

Recently several writers have questioned the relevance of communication models for the understanding of psi (e.g., Braude, 1979; Edge, 1978; Stanford, 1978). Part of the problem appears to involve the variety of definitions that can be given the concept of communication. In an earlier paper (Morris, 1975), I defined psi in the following way. "Psi involves one of two things: (a) an organism appears to receive information from—to be influenced by—some aspect of its environment through means not presently understood; or (b) an organism appears to impart information—to influence—some aspect of its environment through means not presently understood. If we combine the two, psi becomes apparent communication between organism and environment across barriers to known means of information exchange. The term "apparent" is used because the communication is implied, by an observed correspondence between organism and environment, but cannot be proved because no means can be specified. We cannot designate a specific path of information flow, and we therefore cannot conclusively label the true origin or true recipient of the apparent communication." (p. 117)

If one examines the various anecdotes and experimental studies labeled as psychic by the parapsychological community, we find that in each case there is an observed organismic event or set of events that appears to resemble an environmental event or set of events in some meaningful way, meaningfulness determined by observer criteria.

Observed organismic events can range through several categories: (1) the complex contents of intense personal experiences and their associated verbal reports; (2) simple impressions and their behavioral representations; (3) choice from known alternatives, plus a behavioral manifestation; (4) generalized somatic impressions and their

behavioral and/or psychophysiological manifestations; (5) a variety of behavioral and physiological manifestations not readily identified with any specific experiential event; (6) an intentional experience such as simple wishing for a simple goal, or complex wishing for a complex goal, often accomplished by specific imagery of the goal and/or processes leading to the goal, plus a behavioral representation of that experience; (7) an externally assigned intention and the objective record of that assignment; and (8) an inferred need and/or wish, which inference is based upon a set of assumptions about the needs of an organism with respect to a set of conditions in its environment. In each case, there is an aspect of the organismic event which renders it observable to a third party. For events in the last two categories, the organismic event is inferred on the laws of input observables; for events in the first six categories, the organismic event is inferred on the basis of output variables.

What is the range of environmental events? Basically the limits are determined only by our own capacities to attribute meaning. The environmental event in a psi experience can be literally any event that we can relate in a meaningful way to one or more of the organismic events listed above.

Let us examine this conceptual linkage in action by considering an anecdotal and an experimental example for each of our eight categories of observables.

(1) Verbal reports of complex mental events. Anecdote: A sensitive enters an old house and describes in detail having experienced a child whose "spirit" remains behind in the house; a check later reveals a child similar in many respects to the sensitive's description had died in the house two years earlier. Experiment: a subject attempts to generate imagery about a concealed target picture and then draws a picture of an axe and labels it; later a blind judge rates the subject's imagery as much more like the target picture, a peace pipe, than the other control pictures, because the shapes were almost exactly the same and both represented an implement frequently associated with American Indians.

(2) Behavioral concomitants of simple impressions. Anecdote: A woman starts to worry about her baby and wheels the baby's carriage inside; moments later slate falls off the roof onto the pavement where the carriage had been. Experiment: a subject is asked to press a button every time he feels he is being stimulated by a remote event; later analysis shows that he tended to press the button more often when the experimenter was being actively stimulated than during control periods.

(3) Behavioral manifestations of choice from among known alternatives. Anecdote: An individual consistently makes money off the bookies by selecting winners at the horse races. Experiment: A subject consistently guesses correctly the identities of individual cards in a deck of ESP cards a day in advance of the actual composing of the deck from a table of random numbers.

(4) Behavioral or physiological manifestations of somatic impressions. Anecdote: A young woman feels sick to her stomach and has to be excused from school; when she gets home she learns that her father was seriously injured at work and is in the hospital. Experiment: A subject is asked to be responsive whenever a sender is stimulated; the subject shows stronger physiological arousal each time the sender receives an electrical shock than during a control period.

(5) Behavioral and physiological manifestations not readily identified with any conscious experience. Anecdote: A man in an elevator "absent-mindedly" gets off a floor earlier than he intended and runs into the very person he was intending to meet. Experiment: A subject, unaware he is in an ESP study, is given a simple pursuit-rotor task to perform; he makes significantly more errors during periods in which a close relative is being shown slides of congenital deformities than during control periods.

(6) Behavioral representation of wishing for a specific goal. Anecdote: One person shouts a curse at another, stating that a close relative will shortly die; two weeks later the cursed one's mother breaks a leg and recovers very slowly. Experiment: A gambler, using a throwing cup and banking board, announces in advance that he will throw sevens; he proceeds to throw significantly more sevens than would be expected by chance.

(7) An objective record of an externally assigned intention. Anecdote: One individual tells another she will make the second person's horse win a race; the second person shows the first which horse he had bet on, and that horse then wins. Experiment: A subject is given a written set of target instructions about how to bias the behavior of a colored light display governed by a random event generator; the behavior of the lights then conforms to the subject's assignment to a statistically significant extent.

(8) Environmental circumstance designed to induce a need and provide an opportunity for it to be fulfilled. Anecdote: Several people are on board a lifeboat which careens out of control towards shore; in a seemingly "miraculous" fashion, the boat barely slides through the only gap present in the treacherous reefs. Experiment: Chicken eggs about to hatch and in need of heat are placed under a heat lamp which is either

activated or kept off each minute by a governing random event generator; the light stays on significantly more times when the chicken eggs are present than during control periods.

There may well be other categories; the above are sufficient to illustrate the range of coincidences capable of being labeled as psychic. (It is understood that the above examples are brief and in any given case may well be amenable to explanation in quite ordinary terms.) As stated earlier, the meaningfulness of the resemblance between the organismic and environmental events is defined by observer criteria. It can range from a simple duplication of pattern in the psychophysiology of two organisms, with no apparent conceptual translation required, to two events whose relationship must rely on considerable conceptual translation from one to the other for meaning to be detected. The events need not be contemporaneous; they need not resemble each other perceptually; they need not resemble each other thematically. All that is necessary is that they resemble each other in some way that matches an observer's criterion for meaningfulness. If the observer has a good imagination, then a great range of anecdotal material may be labeled as evidence for psi. For controlled research, the researcher presumably has specified in advance the criteria in operation during any specific research project; otherwise evaluation of the role of chance would be impossible.

If we are going to define psi in terms that will allow the application of communication models, we must close off the apparent open ends to this conceptual system. The most effective way to do this, a way strongly tied in with the method of analysis of psi studies, would be to rule out of court all aspects of meaning not specified in advance in accord with criteria linked to the intended mathematical evaluation procedure.

Our final definition of psi, then, becomes: any resemblance between the objective record of an inferred organismic event and the objective record of an environmental event that has been linked conceptually in advance to the organismic event by an external observer, such that (a) the resemblance cannot be accounted for in terms of presently identifiable channels of information flow, and (b) the resemblance is strong enough that chance can be reasonably ruled out through the application of accepted mathematical procedures.

Selecting a Useful Definition of Communication

Communication is a term derived from our ordinary language system and as such it has assumed a variety of meanings. MacKay (1972) has described five uses of the concept of "communication"

between A and B: (a) a correlation between events at A and B; (b) a causal interaction between A and B; (c) transmission of information between A and B regardless of the presence of a sender or recipient; (d) an action by organism A upon organism B; and (e) a transaction between organisms A and B. Each constitutes a subset of the one preceding it. All five have frequently been used, and although MacKay prefers to restrict the concept of communication to uses (d) and (e), it would appear that the usage truest to our definition of psi would be (a). All of the others imply that one of the events causes (or at least contributes to) the other. Such causation is not compelled by our definition of psi; recent surveys of the major attempts to theorize about psi (e.g., Chari, 1977; Rao, 1978) provide several examples of attempts to construct acausal theories of the operation of psi. Perhaps the best known of the latter is the notion of synchronicity, an "acausal connecting principle," developed most extensively by Jung and Pauli (1955) to explain anomalous coincidences. Our broadest, most flexible definition of communication, then, would be any meaningful correlation between two sets of events, with meaning to be defined in accord with criteria developed by the observer/reporter and whomever else wishes to state that communication has taken place.

Researchers who prefer to construe psi as involving at least contributory causality, may prefer a definition of communication at levels (b) or (c). Some researchers, such as Stanford (e.g., 1978), posit a causal connection between A and B, but not true transmission of information between A and B. Stanford construes psi as occurring when a disposed system (generally an organism with needs) is confronted with a random event generator (REG) of some sort which is capable of generating outcomes that will be favorable to the disposed system. Stanford explains: "The selection of the favorable REG outcome occurs precisely because if that REG outcome occurs it will eventuate in a favorable circumstance. This is proposed to occur because of how the world is built, not because of any kind of internal guidance by the organism. The model does not propose how this occurs, but only that it does occur under specified circumstances. Which output of the REG is selected is a function of which outcome would eventuate in the favorable circumstance, not of guidance mediated through the mind or central nervous system of the organism. It is as though nature anticipates the consequences of various outcomes and weights the probabilities accordingly." (p. 208)

Stanford's model thus is descriptive, and, although it makes predictions which are falsifiable, it does not offer a true explanation of the cause-effect relationships it posits. As Standford explains elsewhere in the article, he does not feel that information transmission

is part of that cause-effect interaction since there are many findings in parapsychology (which we will consider below) that appear incompatible with information transmission models.

Stanford does not regard his model as a model involving communication, preferring to define communication at MacKay's level (c), in which information transmission takes place. Most communication theorists prefer level (c) also, as it allows liberal usage of the concepts that have evolved from Shannon's (1948) classical model of a communication system: an information source (not necessarily organic) places a message of some sort in a transmitter; the transmitter sends an encoded signal through a channel to a receiver; the receiver decodes the signal, thus allowing the message to reach its destination (also not necessarily organic). The message is capable of being distorted to varying degrees by noise in the channel. A crucial concept in information theory is the notion that the clarity with which the message is transmitted is dependent upon the signal to noise ratio—the greater the ratio, the clearer the message, all other factors being constant. Given the apparent overall weak strength and inconsistency in psi, there is the often declared hope that information theory could produce some very useful ideas in this area.

Can parapsychology make use of the sophisticated advances in information theory? To do so, it must define its communication at least at MacKay's level (b) and ideally at level (c), assuming for the latter that psi involves genuine information transfer, complete with source, receiver, encoding and decoding, channel, and noise. There are problems with making these assumptions, given the present data base in parapsychology, problems which will be discussed shortly.

As for defining communications at levels (d) and (e), the levels preferred by MacKay, these levels require the involvement of organisms at both the sending and receiving ends and thus would have insufficient generality.

In the next two sections, I will consider the methods and findings of parapsychology in more detail, to assess further the appropriateness of defining psi communication at levels (a), (b) and (c).

The Methods of Parapsychology and their Implications for Causality

As we research the question of psi, we appear to do business using procedures which themselves imply cause-effect relationships. Science itself is an active process; researchers do not stand by as simple observers of an unfolding acausal drama. Rather, they formulate hypotheses, design studies to implement them, conduct the studies and assess and interpret data. All of these acts represent attempts to cause

events to happen, to set things in motion, vary conditions and observe the effects.

In almost all parapsychology studies conducted to date, the researcher conceptualizes, at least in crude, general form, a message to be sent, selects a source for the message and a receiver for the message, and erects barriers to all known means of information transmission (channels) that might link the source and receiver. Following this the potential communication system is activated; the receiver is monitored and later compared with the source. Thus the research community tries to approximate level (c) in all respects except for the existence of the channel.

Since the researcher must rule out all known means of information transfer that might link source and receiver, this means that information transmission between source and receiver must be blocked, *and* that source and receiver must both be informationally isolated from any third set of events that might impart information to the other two, thus synchronizing source and receiver such as to simulate communication between the two. One cannot, for instance, allow the agent to select what to send, even from a restricted target pool. In free response studies, for instance, an agent allowed to select which of four target pictures to send might select the one most relevant to the news of the day (e.g., a fireplace, if there had been forest fires in the news); the receiver might be likely to select the fireplace for the same reason the sender did; both would have been influenced by an external third event. This is an old problem, of course, and to eliminate it today's researchers know they must randomize the material that is to serve as the message.

The experimenter must set in motion a set of events that will "cause" the message selection to be adequately random, in hopes therefore of "causing" the receiver to behave as though it had received the messages as selected. Any well-done study must have such an experimenter involvement, and its logic thus assumes that manipulations of the message at the source served as a contributary cause of the behavior of the receiver.

This is not to assert that psi involves no acausal relationships; I'm simply asserting that our methodology compels us to collect data that reflect those elements of psi that can reach expression in functional, causal interactions. And to the extent that we seek to understand and control the phenomena we observe, we are constrained to the study of cause-effect relationships.

The same is not necessarily true for studies that rely exclusively upon natural observation. So long as we remain passive observers, simply recording coincidences and describing the relevant events fully, then

we are not necessarily participating in a study of causal relationships. This raises an interesting point. Conceivably, psi may involve both acausal and causal relationships. If so, then perhaps we may learn of both by doing both naturalistic and controlled studies. If we spot an apparent relationship in nature that we cannot obtain under controlled circumstance, that might provide some evidence for acausality in the observed relationship. A major problem with this, of course, is that purely observational studies by their very nature leave open the possibility of presently understood factors accounting for the correlation observed. This has consistently put limits on our ability to conclude much on the basis of naturalistic observation alone (see Morris, in press, for a further elaboration of the problems involved in drawing conclusions from naturalistic observations).

A final possibility should be mentioned briefly—it is conceivable that even the experimenter is involved in some greater, acausal coordination scheme, such that his/her intentions do not truly represent the deliberate manipulation they may seem to; that the details of target selection were themselves somehow present within the context of some sort of acausal system. Such an assertion is at present impossible to falsify or verify.

The Data Base of Parapsychology and Its Implication for Information Theory Modeling

As mentioned above, the procedures used in parapsychology not only imply the study of causal relationships; they also stay very close to the core concepts in a standard Shannon-style communication system, save for the channel, which is procedurally eliminated, but, for many researchers, still conceptually implied. As also mentioned above, however, many of the findings of parapsychology on the surface appear to be incompatible with the notion that psi represents information transmission from source to receiver. Let us consider these purportedly incompatible findings in turn.

(1) Psi appears independent of space. Standard information transmission models involve information flow along a channel having spatial characteristics, and distance traveled is a variable. Yet it is often claimed that psi is independent of distance between sender and receiver. There are two sets of points here. First of all, depending primarily on the properties of the channel, distance may have little if any effect upon the clarity of the message, and the notorious inverse square law pertains only in certain circumstances. Second, the relevant data base in parapsychology is not at all clear. No single study has ever compared psi success when target distance was systematically varied and all other factors were held constant. Given that distance was varied,

many would argue that subjects could become aware (perhaps even psychically) of distance as a variable, such that the subject's psychological characteristics, such as confidence, feeling of challenge and so on, would covary and render interpretation of results unclear. The only extensive survey of studies comparing two or more distances was Osis' (1965) survey. Although he found a systematic decline in percentage of hits as distance increased, he neglected to note that the study involving the furthest distances (Osis and Pienaar, 1956) indeed found considerable evidence of psi in the opinion of the authors, in that two separate running conditions produced psi hitting on one condition and an equal amount of psi missing on the other. In my opinion, the distance data overall do not at present seriously threaten the applicability of information-theory models (nor do they overtly support it).

(2) Psi appears independent of time. For standard information theory models, order is important and time is clearly a relevant variable. Yet the data from precognition studies suggest that information can flow backwards in time, that messages can be received before they originate and so on. Once again, in my opinion, the data base bearing on the concepts of precognition and retrocognition is unclear. Specifically, it seems to be that all procedures by which one might hope to infer information flow forwards or backwards in time are amenable to interpretations involving alternate paths of information flow within the present. The alternatives are easy to see for retrocognition. If a subject is asked to respond to information supposedly hidden in the past, any response the subject makes can only be assessed for accuracy by recourse to some record of the past event that continues to exist in the present. Given that such a record exists, it could just as easily have served itself as the actual source of information.

With regard to precognition, the alternatives are more complex. In a precognition procedure, a subject makes a response to an event (target) that will be determined at some later time. Unfortunately, all ways of determining that later event are potentially amenable to psychic influence themselves. Suppose someone makes a decision of some sort to determine the target; that decision might reflect ESP on the part of the decider, such as to construct a match between guess and target. If a random physical event is used to determine the target, that event may be influenced by psychokinesis, such as to produce a match. If a complex set of random events already in existence at the time of the subject's guess is reprocessed through a fixed set of rules, such that no new information is generated after the subject's guess, then the subject may simply have used ESP of those present events. Certain procedures for selecting targets stretch such alternative explanations rather far.

For instance, one could have the targets generated by digits drawn from the published records of stock market transactions, such that the human decisions that finally determine the target are all drawn from humans who not only do not know of the study, but are highly motivated to be serving their own interest in other directions. I have not been able to obtain clear-cut evidence of precognition under such circumstances, and the precognition literature is not strong when such procedures have been used.

For present purposes, the important point is that precognition may not be a phenomenon that must be accounted for by any information-transmission model.

(3) Cognitive complexity of target material does not seem to matter. Probably the most difficult findings for present-day information theory to model are those that deal with changes in the cognitive complexity of target material. One of the earliest examples of the problem was provided by Foster (1940), who compared open matching and blind matching procedures. In open matching, the subject is presented with a deck of individually concealed target cards, and a set of face-up "key cards," one for each kind of symbol. The task is to place each target card in front of the key card corresponding to its identity, such that to be successful one need only know the identity of the target card. Blind matching is more complex, in that the key cards are concealed so that their identity is not known; to be successful, the subject must now use information from both key card and target card. Foster noted that subjects tended to do much better on blind matching than would be expected, given their rate of scoring on a comparable open matching task.

Some of the blind matching research (e.g., Rao, 1964) asked subjects to match target cards in one language with key cards bearing the same words in an unknown language using an unknown alphabet. It was almost as though one were saying to a subject, "Your goal is to place each card in the correct pile, as we define 'correct,' and that's all you need to know." Subjects showed evidence of being able to match words within a language, even in the unknown language, but not necessarily across languages.

This procedure intrigued me a great deal, as I was one of Rao's subjects who did well matching across languages. I thus decided to push the point even farther, choosing as my psi task a procedure used by many researchers to rule out the presence of psi in the selection of precognition targets. The procedure, described more fully in Rhine and Pratt (1957), involved a way to generate a six-digit entry point into the Rand Corporation's million digit random number table (Rand

Corporation, 1955). The procedure was as follows: take a two-sided die; throw it twelve times, thus producing four three-digit numbers; multiply these four sets of numbers together; multiply that product by itself backwards; take the result and extract the square root to six digits of the last ten numbers on the right; convert the first digit to 1 or 0 and the fifth to 0 or 5, on an odd-even basis; use the resultant six digit number to enter the random number table; use the numbers following the entry point to determine the precognitive target order. The idea was that the experimenter could not use PK to make the dice produce a favorable entry point because there would be no way to know what numbers to make the dice come up, assuming that even with ESP operating the experimenter would still have to work back through the numerical manipulations. However, if we hypothesize that ESP operates in some sort of all-or-nothing or goal-oriented way (diametrically, to use Foster's term), then such a task becomes quite possible.

I thus set up a study with myself as subject and a coworker, Cynthia Weaver, as experimenter. My job was to generate twelve digits which, when processed mathematically in the way described above, would produce an entry point which would in turn produce a predominance of odd or even numbers, in accordance with a preset target sheet. Half of the time I threw a die twelve times. Half of the time, in a counterbalanced way, I called digits off the top of my head. I had no idea how many numbers in the table I might be responding to, so for the pilot study I arbitrarily decided in advance to score the results of the first five numbers after the entry point, the first ten, 25, 50, 100, 200, 500 and 1,000. This would allow further assessment of how complex a task could be done, e.g., if it only worked for the first five numbers, that would be informationally far simpler than if it worked throughout the first thousand. My results for the pilot study were encouraging for the verbal calling method only; the die throwing results were at chance. A positive result was obtained at every tallying point overall, to a statistically significant extent for the first 50 and first 100 numbers following the entry point.

Having obtained a positive result, I then repeated the procedure, this time stating in advance that my criterion for declaring the significance of the second study would be based on an analysis of the results for the first 100 digits only, since that cutoff point had produced the strongest results initially. The results for the second study confirmed the first—the results at the 100-digit stopping point were quite significant (p .01, two-tailed) for the verbal calling procedure. The dice results once again were quite close to chance (Morris, 1968).

This study has not been systematically pursued, primarily because I wish to use someone other than myself as subject, so I can vary conditions and test some hypotheses, and I have had a hard time finding others who are interested and motivated by the procedure. I have described the study in some detail because I feel that it and studies like it represent a real challenge to our attempts to provide models for psi based on information theory.

There are other areas of research in which the link between source and receiver have been conceptually complex. Several studies (surveyed by Palmer, 1978, pp. 188–193) have involved covert ESP tasks—aspects related to the target of which the subject was unaware, as well as studies in which the subjects were not aware that they were participating in an ESP test at all. Something of meaning to the subject was somehow embedded in the general experimental context, and evidence found that the subject nevertheless responded to it. Such a finding is similar to the implied communication in many spontaneous case reports, in which individuals suddenly respond in some way to a remote tragedy to a loved one whom they had no ordinary reason to suspect was in trouble. In these cases, it is as though the subject were somehow scanning the environment, processing and filtering out what would seem to be incredible amounts of information, responding in strength only to those signals that take on strong meaning for them. This happens all the time in ordinary perception, of course, but on a much smaller scale in terms of the potentially available information to be screened out.

Within the area of psychokinesis several analogous challenges exist. An issue raised by Rhine (e.g., Rhine and Pratt, 1957) concerns the fact that subjects appear not to need to have much information about their targets to exert an influence upon them. This is especially true of recent work in which the targets are minuscule, short-duration events in the circuitry of random event generators. Subjects can even succeed when the REGs are generating information at extremely fast rates (e.g., Schmidt, 1976, whose REG generated 300 trials per minute), so long as the information is displayed to the subject fairly slowly. It should be noted that, although many REG's select and display only one or two trials per second, their generation elements can be producing potential decisions available for selection at rates in excess of several hundred times a minute.

Of additional interest is the fact that several PK studies (surveyed by Stanford, 1977) have involved concealed target instructions, such that subjects would presumably have to use ESP to know how to influence the target. Despite the increased complexity of the task, subjects

performed at least as well in these studies as in studies in which subjects had more information available about the target.

A final area noted by Stanford in his survey is the presence in several successful PK studies of targets whose very existence was not known to the subjects—subjects were frequently not at all aware that they were in a PK study.

The overall impression from these studies is that increasing the informational complexity of the psi task does not necessarily appear to reduce performance, save for some mild evidence that high-speed PK may not work so well and that cross-language matching does not appear to work as well as within-language matching. Additionally, psi testing procedures appear to work even when the information processing necessary to make a correct response would seem considerably beyond the organism's capacities.

Although the data base in this respect is still somewhat sparse, the evidence is strong enough that consideration of such studies and their implication must be borne in mind by anyone hoping to generate models for psi based on the assumption that information transmission is involved.

Modeling Information Flow Pathways

Assuming that the considerations raised above have not deterred us from construing psi in information theory terms, we must now confront a very important implication of the apparently loose constraints placed upon psi functioning by such factors as space, time, and informational complexity—namely, that in any given study we may design and conduct, there are many potential pathways for information to flow. As we set up our studies, we designate a source, message and receiver, put information into the source and monitor its potential arrival at the receiver. Yet we may have several different potential sources and receivers, in addition to the ones the researcher has chosen, which may be responsible for whatever evidence of communication we observe.

Consider the organismic events and environmental events we discussed earlier. For present purposes, these will represent the sets of events whose measurements will eventually be compared by the researcher to test for the presence of psi communication. Each set of events has a set of factors which contributes to its final characteristics, its contributory causes. In the usual study, the experimenter controls the contributory causes for the events to serve as designated sources, e.g., the selection of a target pool and random selection of a specific

target for each trial. The receiver event has a set of contributory causes also, some due to constraints placed by the researcher, some due to a host of antecedent variables affecting the receiver, some of indeterminate origin and some, ideally, due to psychic information transmission. Good experimental design means ruling out all presently understood means by which the contributory causes of the designated source event could transmit information to the contributory causes of the receiver event, and vice versa. If the contributory causes to source and receiver events share information, that shared information will increase the likelihood that source and receiver events will be meaningfully related. (Consider our earlier example in which a forest fire in the news constituted shared information for contributory causes of both source and receiver events.)

Now comes the problem: even though we now have what appear to be quite adequate procedures for preventing such information sharing by presently understood means, we are totally unable to prevent potential psychically mediated information sharing. As we examine each possible contributory cause, we must consider that each may conceivably be a psi receiver, influenced by some external event (a psi source) not necessarily under the control of the researcher. If a contributory cause related to the designated source receives information psychically from a contributory cause related to the designated receiver, or vice versa, then there is psychically mediated information sharing which will make designated source and designated receiver likely to resemble each other, such that we will mistakenly decide that information flowed directly from our designated source to our designated receiver.

Any attempt to model information flow in any given study must take into account the multiplicity of information flow pathways that may conceivably join designated source and designated receiver. The success of any such attempt will be greatly dependent upon our ability to understand the full range of contributory causes involved in any specific study. (Analogous reasoning can be applied to our attempts to describe any spontaneous report or event observed in the field, the main difference being that in the field we are likely to be less able to assess the contributory causes involved.) Additionally, source events may in due course generate further events, representations of the source event in experimental records and so on; any of these further events may, themselves, serve as the actual source.

Let us explore these notions by considering some experimental procedures presently in use, which represent extremes with regard to the number of known contributory causes at work. A favorite

procedure for illustrating these problems (see also Morris, 1975) is the chair test (e.g., Eisenbud, 1973), occasionally used as a precognition test for selected sensitives. Someone designated by the researcher as a psi receiver is given the identifying number of a chair in a lecture hall and asked to describe the person who will sit in that chair at a later lecture. The chair number is sealed inside an envelope; the transcript of the receiver's statements is sealed in a second envelope; and both are placed together in a third envelope. Later, notices about the lecture are distributed, people decide to attend, they arrive at the door and are assigned chair numbers, they take their seats, and at some time during the lecture they hear the receiver's transcript read aloud. The members of the audience are asked to assess how much the transcript resembles themselves; they are asked to stand up if they consider the transcript very accurate. If the person sitting in the correct chair stands up, the study is considered successful. One can also statistically compare the correspondence rating of the correct person versus those sitting in the other chairs.

When such studies have been done, they have been regarded as precognition studies, with the source being the person in the correct chair and the receiver the person asked to generate impressions about the source. However, there are alternatives. Once the receiver has given a set of responses, these responses become represented in different ways. Perhaps they were tape recorded or written by hand; this original record would then be typed up, perhaps edited; by the time it gets sealed in the envelope and closed off informationally, its contents exist in the memories of at least two people and perhaps more.

All of this information may serve as source material for those involved in the contributory causing of who finally sits in the target chair. Once the lecture notices are sent out, a variety of potential lecture attenders start making contributory decisions; should I attend? when should I depart? when should I arrive? and so on. At some similar time, people conducting the lecture may be making contributory decisions about how to assign seat numbers to the incoming members of the audience. Of those potentially attending the lecture, of course, some may resemble the transcript description more than others. Thus, one set of alternative information flow pathways would be those extending from the original impressions to those who decide who will sit in the target seat, such that someone who resembled the impressions would arrive in time to get assigned the target seat.

Perhaps the above does not come to pass, and whoever sits in the correct chair has no special resemblance to the transcript. However, once the transcript is read, whoever sits in the chair may psychically

receive the information that he/she is in the correct chair and will thus be inclined to detect more resemblance than those in other chairs. Other procedural decision makers may also have had opportunity to contribute to the outcome. If we accept the possibility of precognition, that opens even more possibilities.

The chair procedure involves many people and many complex events spread over a considerable time period. Now let us consider another procedure with these dimensions considerably compressed. The subject is placed in front of a random event generator attached to four display lights. The REG is fed by a noise diode and displays a new decision once a second. Which light is selected for display on each trial is recorded by computer and the data are tabulated automatically. The subject's task is to increase the number of doublets (consecutive identical decisions) during the experimental period, which always lasts from fifteen minutes after the hour to thirty minutes after the hour. The REG's performance during this 15 minute period is always compared with one of the other 15 minute periods in the hour, on a rotating basis in accord with a sequence set up at the start of the study. Human contributory decisions are kept to a minimum. A set of rules for target selection was chosen at the start of the study, and all the data collection, storage and analysis has been handled by computer from then on. The main problem with this study, as with all PK studies, is that the PK source could easily be anyone involved at all with the study at any conceptual level. Schmidt (1975) has argued that anyone who observes the outcome of a PK trial may contribute to its success, even though that observation may take place considerably after the REG events that nominally determined the display. By this line of reasoning, anyone who later reads a writeup of a study may conceivably be an observer-influencer.

Thus, even apparently simple, straightforward experimental designs are amenable to alternative information flow pathways. To the extent that we wish any precision in our modeling, therefore, we may be compelled to develop and refine procedures that involve as few opportunities for contributory causation as we can.

Summary

Given the basic nature of parapsychological concepts, methods and findings, it would appear that the most productive uses of communication concepts lie at MacKay's levels (b) and (c). Level (a), in which acausal connections are considered, is very difficult to conceptualize and construct testable hypotheses on. The very process

of hypothesis testing relies upon acceptance of at least contributory causality, insofar as it relies upon deliberate manipulation of experimental variables to observe their outcome. This problem is well illustrated by the Everett-Wheeler-Graham "Many Worlds" view of reality (e.g., DeWitt and Graham, 1973), in which all quantum uncertainty is removed by positing that all possibilities in fact happen, each in its own universe, such that the number of universes expands at an infinitely large rate. Such a view is concomitant with the apparent goal-oriented nature of psi—a psychic event happens when an observer tracks a universe in which the goal was accomplished, regardless of "how" it got accomplished. The problem with the EWG view is that it cannot be tested (e.g., there's no way to falsify it) and it does not discuss the nature of the observer adequately. As for the Jung-Pauli "acausal connecting principle," they do not at any time demonstrate the acausal nature of their phenomena, and in truth it would be impossible to say anything more of two coincident events than that one has not at the present time located any point of information sharing among elements in their sets of potential contributory causes. One way to show this problem is to consider that each coincidence put forth as evidence for synchronicity can easily have more than one path of psi-mediated information flow constructed for it, as we did above for the chair test. In short, all phenomena describable in terms of level (a) are potentially modelable at levels (b) and (c).

We can now consider some major conceptual systems that would appear to occur at level (b), causal interactions, such as Stanford's "conformance behavior"; various notions of direct mental or spiritual action, parallel to the laws of physics, but not encompassed by them; and the various notions of what I have called earlier (Morris, 1975) "message ubiquity," which state that there exists some sort of universal repository of all information and that psi occurs simply when the designated receiver accesses the repository.

Such idea systems have relatively little difficulty in accounting for the capricious and complex nature of psi. However, even they are at present difficult if not impossible to test, largely due to problems in determining which informational pathways may be operating. They also do not preclude the transfer of information in the sense of level (c), transmission of information; the strongest arguments against information transmission are those put forth earlier, and they are merely problematic, not conclusive at this time.

In many respects the stongest evidence for psi functioning at level (b) would be the consistent failure of intelligent modeling at level (c). Intelligent modeling at level (c) must bear in mind the full range of

phenomena presently encompassed within parapsychology's data base. It should take into account the likelihood of alternate information flow pathways, and it should emphasize, for the present, research into these procedures in parapsychology which represent the hardest for modeling in information theory terms: the apparent sparsity of space/time and informational complexity constraints. If we hope to find a channel, we must articulate constraints. The longer we go without constraints, the more our modeling and theorizing will founder on vague and untestable concepts; the more potential constraints we eliminate, the more evidence we provide for the absence of a true channel and the need for modeling at level (b). And that task I leave to those far wiser than I.

BIBLIOGRAPHY

Braude, S., "Objections to an information-theoretic approach to synchronicity," *J. Am. Soc. Psych. Res.*, 1979, *73*, 179–194.

Chari, C. T. K., "Some generalized theories and models of psi: A critical evaluation," In *Handbook of Parapsychology* (Wolman, B., ed.), New York: Van Nostrand Reinhold, 1977.

De Witt, B., and Graham, N. (Eds.), *The Many-Worlds Interpretation of Quantum Mechanics*, Princeton, N.J.: Princeton University, 1973.

Edge, H., "A philosophical justification for the conformance behavior model," *J. Am. Soc. Psych. Res.*, 1978, *72*, 215–232.

Eisenbud, J., "A transatlantic experiment in precognition with Gerard Croiset" *J. Am. Soc. Psych. Res.*, 1973, *67*, 1–25.

Foster, A., "Is ESP diametric?" *J. Parapsychology*, 1940, *4*, 325–328.

Jung, C. G., and Pauli, W., "*The Interpretation of Nature and the Psyche: Synchronicity and the Influence of Archetypal Ideas on the Scientific Theories of Kepler*, New York: Pantheon, 1955.

MacKay, D., "Formal analysis of communicative processes," in *Non-Verbal Communication* (Hinde, R. A., ed.), Cambridge: Cambridge University Press, 1972.

Morris, R. L., "Obtaining non-random entry points; A complex psi task," in *Parapsychology Today* (Rhine, J. B. and Brier, R., eds.), New York: Citadel Press, 1968.

Morris, R. L., "Tacit communication and experimental theology," in *Research in Parapsychology 1974* (Morris, J. D., Roll, W. G., and Morris, R. L., eds.), Metuchen, N.J.: Scarecrow, 1975.

Morris, R. L., "Building experimental models," *Journal of Communication*, 1975, *25*, 117–125.

Morris, R. L., "An updated survey of methods and issues in ESP research," in *Advances in Parapsychological Research, Vol. 3* (Krippner, S., ed.), New York: Plenum, in press.

Osis, K., "ESP over distance: A survey of experiments published in English," *J. Amer. Soc. Psych. Res.*, 1965, *59*, 22–42.

Osis, K., and Pienaar, D. C., "ESP over a distance of seventy-five hundred miles," *J. Parapsychology*, 1956, *20*, 229–233.

Palmer, J., "Extrasensory perception: Research findings," in *Advances in Parapsychological Research Vol. 2: Extrasensory Perception* (Krippner, S., ed.), New York: Plenum, 1978.

Rand Corporation, *A Million Random Digits*, Glencoe, Ill.: Free Press, 1955.

Rao, K. R., "Studies in the preferential effect. IV. The role of key cards in preferential response situations." *J. Parapsychology.*, 1964, *28*, 28–41.

Rao, K. R., "Theories of psi," in *Advances in Parapsychological Research, Vol. 2, Extrasensory Perception* (Krippner, S., ed.), New York: Plenum, 1978.
Rhine, J. B., and Pratt, J. G., *Parapsychology: Frontier Science of the Mind*, Springfield, Ill.: Thomas, 1957.
Schmidt, H., "Toward a mathematical theory of psi," *J. Am. Soc. Psych. Res.*, 1975, *69*, 301–319.
Schmidt, H., "PK experiment with repeated, time displaced feedback," in *Research in Parapsychology 1975* (Morris, J. D., Roll, W. G., and Morris, R. L., eds.), Metuchen, N.J.: Scarecrow, 1976.
Shannon, C. E., "Mathematical theory of communication," *Bell System Technical Journal*, 1948, *30*, 50–64.
Stanford, R. G., "Conceptual frameworks of contemporary psi research," in *Handbook of Parapsychology* (Wolman, B., ed.), New York: Van Nostrand Reinhold, 1977.
Stanford, R., "Toward reinterpreting psi events," *J. Am. Soc. Psych. Res.*, 1978, *72*, 197–214.

DISCUSSION

STORM: What is the difference operationally or specifically between causal interaction and transmission of information?

MORRIS: That is one of the problems we have to tease out. I'm not sure how to construe a cause/effect relationship that does not involve information transmission. I have never been able to do that. I think mainly that it could be possible to demonstrate cause/effect type relationships without being able successfully to define a particular form of information transmission. So we would have a failure at that point, which wouldn't mean there is no such information transfer, but basically would say, "We haven't found one yet." I think we can demonstrate cause/effect relationships and it seems to me that by doing so, we probably also demonstrate information transfer relationships, but I don't think we can assume we do.

STORM: Would you say that the concept "cause-effect" is an interpretation placed on the events by us?

MORRIS: The notion of a classical cause/effect relationship, I think, has been shown to be conceptually pretty weak. What I have in mind here is an idea of a contributory cause or factor, such that we may not, for any given effect, be able to specify the complete set of contributory causes. Just because we found a contributory cause wouldn't necessarily prove, in and of itself, that there was no additional acausal component in the system.

STORM: My personal life is lived in a world of which space and time are very definite aspects. I can think of very little in my life that is not

essentially dependent on the space/time coordinate system. How do I understand what it means to say that psi phenomena or space/time are independent, when all my rational experience is with space/time?

MORRIS: You're best off addressing that to someone other than myself, because it seems to me that all of the studies that have been conducted are done by people dealing in space/time. What they do is find coincidences; they find anomalies which look as though they defy our present understanding of space/time. And I think it can be demonstrated that any procedure for demonstrating precognition is amenable to alternative information flow pathway modeling, which is not independent of space and time. I think this is an assertion that was made all too quickly by key people in the history of parapsychology. It may be true. I'm not denying that it's true. I just simply say it's not been demonstrated yet. It has not been demonstrated that parapsychology and its findings are independent of space and time, as many of us would love to label them.

RUDOLPH: I'd like to address the point about the relationship between information theory and information transmission models. One of the things I'm trying to work out for myself is whether Shannon's Information Theory itself requires an information transmission model. My current thinking is that it does not. Shannon probably had information transmission in mind when he developed his theory, and most people think in terms of information transmission, but I don't think the mathematics requires it. Certainly, the concept of a channel as simply two variables which are correlated would come under your category (a) of communication. When you add a source and a destination, and an encoder and a decoder, then it seems as though you would have to drop down to level (c). But I don't think so. I just concocted a little communication system which has a channel with two outputs which we normally think of as input and output; it has an encoder and a decoder and the source and the destination both receive information. I think the mathematics apply to that. So I would just like to propose that perhaps Shannon's Information Theory really can be used to model processes at communication level (a).

MORRIS: Right. I won't comment on that until after your paper. I think you have a very interesting point.

NASH: Communication and information theory is an attempt to provide terminology and principles of communication in order to make it understandable, manageable and predictable. Evidence is convincing that information transfer is sometimes mediated by psi and,

in the absence of an adequate explanation of psi, it is desirable that communication theory be examined as a possible explanation of paranormal intercourse. I see no evidence, however, that communication and information theory provides an understanding of what basically takes place when one person's thoughts become those of another, even when the event is brought about by normal means, much less when it occurs through psi.

MORRIS: Yes—I think it's a tool. It's not an attempt to understand. It's a procedure by which understanding presumably can be enhanced, and that's the function it served in terms of increasing our understanding of other communication systems.

RUDERFER: You mentioned Dr. Osis' survey of 1965 which, as I recall, was a statistical survey in which he found that, from the data he analyzed, ESP fell off as a two-fifth power of the distance. Dr. Osis did some experiments after that with a number of receivers throughout the world in which he found that there was essentially no change with distance, and that's more relevant than his survey because it's an actual experiment. He did find in one of his correlations that there may have been a slight drop-off with distance, but it was very small. I pointed out to him that his experiments were sufficient to eliminate the possibility that ESP falls off as the square of the distance or as a two-fifth power of the distance, and that they indicated that ESP is space independent.

MORRIS: I think the main problem would be whether or not he had the strong possibility of a confounding variable with differential motivation with different distances, even though the people involved in his studies were not aware of what distances they were operating on at the time. I think that, also, those studies and others like them show that information appears to occur over rather extensive distances. I don't think they would indicate, in and of themselves, that we could then conclude that we now know that it's independent. I think that goes beyond the data.

RUDERFER: I thought you were talking about a survey only, not his experimental results after 1965.

MORRIS: No. I described the survey in my remarks. I was taking into account also his experimental data.

IRWIN: Bob, I have a certain amount of sympathy with your idea of distinguishing precognition and retrocognition from other forms of ESP, but how would you account within your present system for precognition of natural disasters in which there is no apparent human factor?

MORRIS: Let us consider something like a landslide. There it would be a matter of whether or not the person was in some sense making an integration of presently available information. Landslides don't occur in certain kinds of rock formations: they occur in others, and there is a fair amount known about the antecedent conditions for them. And one of the alternative kinds of hypotheses to precognition—actually, the one that I didn't mention, is the possibility of an integration of presently existing information in such a way as to offer a "best guess" estimate of future events. This sounds, on the surface of it, somewhat absurd. It's made slightly less absurd because of the problems with regard to the apparent psi success at informationally complex tasks already, like some of the PK data. We're not in a position to rule out of court right now that people may be able to process very complex information in that way. As a matter of fact, the study that I did on myself addresses itself to that issue. I was using a procedure frequently used to determine precognition targets, and I think that some of the anecdotes from some of the psychics may be of some use here. I recall Douglas Johnson describing to us that he had two totally separate levels of anticipations of the future—one of which he regarded as the one he had most frequently, and that was very probabilistic in nature. He got the feeling that if he told a person, "Look, as of right now, you're about to die," that that information could interact with the person in such a way as to lead him to do things to reduce the probability of that death's occurrence. However, he said upon rare occasions he felt he got a truly precognitive experience, which was of something that was unalterably going to happen, which couldn't be interfered with.

If you anticipate a plane crash, perhaps what you're anticipating is your anticipation, or your experience may occur at that moment at which somebody involved in fueling the plane is getting into a violent fight with his wife, such that he's going to come to work madder than hell and screw up the way that the fuel is put in. Those are the kinds of alternative possibilities one has to look at, and I think they are testable. I think the timing of a precognitive experience is important. Why do you have it now? When did the information arrive? Has it been with you since your embryonic days and only came forth into experience at a certain time? Or is it possible that one has a precognitive experience at some sort of final moment, at which time factors determining that future event have now raised its likelihood of occurrence close to certainty? You could do that kind of study with random event generators. We've done something a little bit like it that I don't really want to get into right now, but I think that possible explanation at least could be falsified experimentally.

RUDERFER: I want to come back to the subject of distance for a minute. Distance is a physical variable and if you can establish some variation with ESP and distance, this is very important, as was pointed out. In looking through your paper, I don't see any reference to Dr. Osis' work after 1965, and I just wanted to clear that up. Were you discussing his 1965 survey and his paper with Turner in 1956 only?

MORRIS: In the paper, I was citing that as the overall survey of a set of studies in which he had thought he had found a decline. In my remarks later on in the paper and in my summary statement in the distance section, I was taking into account all of the distance research that I know of, although I did not list them in the bibliography.

RUDERFER: So you're saying that there is possibly a defect in his actual tests which he reported in 1972?

MORRIS: Yes, I'm saying they could be confounded by motivational variables.

RUDERFER: And that includes his test that he reported in the *Proceedings* of the ASPR in 1972, I think?

MORRIS: Yes. Unfortunately, when you vary distance, it is very difficult to say for sure you have not also varied the motivation of the people. I've discussed this with Dr. Osis, and he was trying to get at that question in a way that I think is very useful. In one of the studies he did, he administered to people the Rosenzweig Picture Frustration Test. His hypothesis was that someone who would be more frustrated, and would show up as officially more frustrated, would be the kind of person who would be more frustrated at trying to work over a long distance, and you would expect his results to be different from the others. And I think this issue could be addressed by further work along that line, because if you say that increasing distance produces a motivational confounding, you should be able to get at that. In some informal research that I was doing with myself as subject, I was led to the possibility of pre-testing people on a task having different levels of ostensible complexity, and then selecting people who showed a clearcut decline when they were faced with a more complex task, and a set of subjects who showed a clearcut incline, as though they rose to the challenge. Then take those two sets of subjects and try the distance work with them. If they both leveled out to flat even, then I would say that was better indication that distance didn't matter. But what I'm saying is that distance might not have mattered because of some sort of canceling of two different kinds of motivational systems which could

have been activated. I think the issue is resolvable through further research.

RUDERFER: Then what you're hypothesizing is that a subject's success is dependent on his emotional way of considering distance—he might improve with distance or he might decline with distance, but you have no experimental data.

MORRIS: That kind of experimental data has not been provided yet. I think that a person's motivation, his attitudes towards distance, etc., should contribute to the intensity of his score.

RUDERFER: The last point I want to make is that Dr. Osis used about a hundred people. Now, only if there was a great difference between these two emotional aspects would it show up in an experiment, otherwise, with a hundred people, statistically it would pretty well average out.

MORRIS: That's right—that's the problem.

RUDERFER: So from that point of view, the evidence does favor that psi is fairly independent of the distances we have on earth.

MORRIS: But if you used a large number of people, then these two alternative factors, as you say, would be more likely to cancel out, and that would produce results of the sort that he found.

RUDERFER: All I'm saying is that the objection that you have would be canceled out with a large number of people, unless it was very one-sided.

MORRIS: Possibly so.

RUDERFER: You would be left only with the variation of distance independent of the emotional factor.

MORRIS: In order to know that, you'd have to really test further with the people involved. I'm not unsympathetic to the notion psi is independent of distance. I just don't think that the data at present have really demonstrated it. It's been demonstrated, I think, that psi can occur over considerable distances, and I don't see its independence yet.