THE JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH

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CONTENTS

THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH, Inc.
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PURPOSE AND SCOPE OF THE SOCIETY

1. The investigation of claims of telepathy, clairvoyance, veridical hallucinations and dreams, psychometry, precognition, dowsing, and other forms of supernormal cognition; of claims of supernormal physical phenomena, such as raps, telekinesis, materialization, levitation, fire-immunity, poltergeists; the study of automatic writing, trance speech, hypnotism, alterations of personality, and other subconscious processes: in short, all types of the phenomena called psychic, mediumistic, supernormal, parapsychological and metapsychic.

2. The collection, classification, study and publication of reports dealing with the above phenomena. Readers are asked to report incidents and cases. Names must be given, but on request will be treated as confidential.

3. The maintenance of a library on psychical research and related subjects. Contributions of books and periodical files will be welcomed.

4. Cooperating in the above tasks with qualified individuals and groups who will report their work to the Society.

MEMBERSHIP IN THE SOCIETY

Members, who receive the Proceedings and the Journal, pay an annual fee of $10. (One may become a Life Member or endow a Memorial Membership on payment of $200.) *Fellows,* who receive all publications of the Society and are afforded special library privileges pay an annual fee of $25. (Life Fellowship, $500.) *Patrons and Founders*: For those who wish to make a still larger contribution to the Society's work, these classes are open at $1000 and $5000, respectively.

It is to be remembered that membership in a scientific society means more than merely a subscription to its publications. The work must be carried on largely through the income from membership fees. Therefore members, old and new, are urged to make their membership class as high as they feel they can. *If a comparatively small proportion of the present members went one class higher, the money available for research would be more than doubled.*

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Fiftieth Anniversary Celebration

The American Society for Psychical Research will celebrate its Fiftieth Anniversary on Friday Evening, March 2nd, at the Women's National Republican Club, 3 West 51st Street. Members who wish to dine at the Club before the meeting will kindly make their reservations with the secretary at 880 Fifth Avenue. The program of addresses by leading psychical researchers will begin at 8:15 P.M.

The meeting will commemorate the founding of the Society by Professor James Hervey Hyslop (1854-1920), following the death in 1905 of Dr. Richard Hodgson who had served as secretary of the American Branch of the English Society which was dissolved shortly thereafter.

Professor of Logic and Ethics at Columbia University, Professor Hyslop resigned his post in 1902. For over a decade he had recognized psychical research as a subject of great and growing importance. As director of the newly-founded American Society he brought the technical methods of the scientist to the investigation of psychic phenomena. He personally selected and edited all the material that appeared in the vast volumes of *Proceedings* and JOURNAL and published many books and articles in magazines for the ordinary reader. He delivered lectures on psychical research throughout the country, attended to all financial matters, secured about $185,000 of endowment as a nucleus for a permanent Foundation—all this wholly without compensation. He provided for a competent successor to

carry on his work by engaging Dr. Walter Franklin Prince as his assistant in 1917.

It was after prolonged and arduous experiment that Professor Hyslop discarded his erstwhile advocacy of telepathy as an explanation of the results he obtained, and he became convinced that survival of personality after death was the more reasonable interpretation. After he reached this tentative conclusion, he adopted it as a working hypothesis.

Apart from his abilites as an investigator Professor Hyslop had a genius for organization. When the first JOURNAL of the Society appeared in January, 1907, his list of members comprised 300 names from 32 states in this country. This was a single-handed achievement. Before his death the list was quadrupled.

Now that the Society has reached the half-century mark it is well to pause and pay tribute to its Founder. The organization he built so wisely has withstood the vicissitudes of time and cicumstances and assured the continuity of the Society to which he devoted the best years of his life.

Notice of Annual Meeting of the Voting Members of the American Society for Psychical Research, Inc.

The Annual Meeting of the Voting Members of the American Society for Psychical Research, Inc., will be held at the office of the Society, 880 Fifth Avenue, Suite 1A, Borough of Manhattan, City of New York, on Tuesday, January 31, 1956, at 4:00 o'clock in the afternoon, for the election of five Trustees and for the transaction of such other business as may properly come before the meeting.

Lydia W. Allison, *Secretary*

Psi and the Problem of the Disconnections in Science [1]

JULE EISENBUD, M.D.

I

Parapsychology deals with correspondences between events. These correspondences are occult in the sense that whatever is responsible for them is, in line with a standard dictionary definition of the term occult, "not immediately known; perceivable only by investigation."

Up to recently it was customary to designate such correspondences as telepathic, clairvoyant, precognitive, psychokinetic, or by roughly equivalent terms. In the past few years, however, the term "psi" has come into increasingly widespread use in relation to the class of occult correspondences formerly encompassed by such terms as those given above. Most investigators have come to feel that there are no compelling grounds for making any assumption other than that behind the various phenomenological forms and manifestations in the realm of occult correspondences there is one underlying process. This fundamental what-is-it, which they presume to be the common noumenon behind the phenomena of telepathy, clairvoyance, psychokinesis, etc., they agree to call psi.

It is felt that the term psi succeeds in eliminating some of the possibly misleading implications with which the older terms were burdened. In designations like "a psi event," "a psi relationship," "psi phenomena," it is supposed to be understood that psi refers to nothing more than the still unknown means or process by which a given event, relationship, or phenomenon is brought about. It is often more or less automatically assumed, however, that this means or process is an actual something-or-other which in principle can be positively identified (that is, not solely by exclusion) and differentiated from a set of non-psi processes. The term psi, in other words, is not limited to the designation merely of a *class* of events which lends itself to differentiation solely on certain phenomenological grounds from other classes of events; it is frequently used to refer to a quite special and even, as far as all the natural forces and processes now known to science go, uniquely operating principle which is postulated to be the particular thing-in-itself that, working from below, as it were, is inherently responsible for the fact that this class of events can be more or less sharply differentiated from other classes of events.

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[1] A condensation and abridgement of a paper presented at the First International Conference of Parapsychological Studies, University of Utrecht, Holland, July 30 — August 5, 1953.

Despite the lack of clarity surrounding the concept of psi and the looseness with which the term is often used, in the minds of some parapsychologists it has come to be looked upon as a force of some kind — a "spiritual force," a "psychic force," a force vaguely associated with thought — at all events, a "non-material force" insofar as it is generally held to be "independent of time and space." Somehow, the notion of a *psi force* has taken deep root, and it is this "force" that, in one form or another, is held to provide the causal connection between the events that make up those correspondences that would otherwise be completely inexplicable. (One need not cite the parapsychological literature at length to show that this is a widespread notion about "psi," whether or not explicitly stated as such.)

It would be highly desirable to be able to arrive at a more precise conception of what we mean by "psi." Although we do not know what inertia is, or momentum, or electronic charge, we nevertheless have a high degree of clarity about what we mean to signify when we use these terms. The meaning of terms like these, and of many others of similar stock, is derived directly or indirectly from a group of congruent pointer readings which vary consistently in relation to each other under circumstances which we are able to describe with great exactness. From this we are able to infer the highly consistent — in fact predictable — quantitative variation of *that which we agree to call inertia* (or momentum or charge) under varying conditions that we are able to define and specify in terms of other pointer readings with a high degree of precision. The underlying principle, the "thing-in-itself" behind the dynamical concept, we have no way of getting at; but so long as we have a consistent means of mathematically relating events in a given domain, we are content to speak of a "force." The notion of force has no other meaning. It is a metaphysical skin drawn over a skeletonized set of mathematically stateable relationships.

It is the absence of relationships of this kind, however, that characterizes the correspondences dealt with by parapsychology. It is this lack — and mostly this lack, moreover — which seems to set these correspondences apart from most other correspondences known to science. If not for this lack, perhaps, there would be nothing that would be considered occult, nothing "paranormal" about any of the phenomena we now classify under psi. What measurements we do make have to do with chance (telling us merely that we have correspondences on our hands, nothing about what is responsible for them) and latterly with personality characteristics. But neither of these classes of measurement is of the type of those tightly interlocking pointer readings which provide us with the most familiar basis on which we conventionally define in a positive way a so-called causal

connection between events.[2] As a matter of fact, all we have according to this way of structuring events are what might be termed causal *dis*connections. These disconnections in our process map of science — our simple inability to account for any of our observed correspondences by means of "force" assumptions that are in any way explanatorily continuous with those used to link other correspondences known to science — are all that the term "psi" can legitimately be construed to refer to.

Perhaps the gap that confronts us represents nothing more than some inadequacy in the grammar of the language with which we describe nature, a developmental anomaly resulting in a baffling lack of congruence between our descriptions of various aspects of the whole, or at least what we have always been tempted to assume to be, despite our clear difficulties in picturing it as such, a well hung-together and smoothly running whole. But however this be, we cannot make a disconnection into that type of connection we call a "force" by simply giving it a name and imputing to it a negative qualification — something that tells us how "it" does ***not*** act — like "psychic," "spiritual," or "non-material." Such terms still fail to state anything more than that we are confronted by a contentless hole (descriptionwise) in our process map of nature, an abrupt and disconcerting chasm separating our techniques of describing certain correspondences from our techniques of describing other correspondences.

Have we then nothing more in "psi" than a new name for something essentially indistinguishable from the demons of old, which too were "force" assumptions invented to fill gaps in the presumed causal fabric of things but which lacked the positive identifying credentials which alone are capable of being harnessed for the work of scientific inquiry? Is "psi" nothing more than a modern-dress latecomer to the demonological hierarchy?

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[2] The closest we have come in appearance to such measurements are those reported by Crawford. (*Experiments in Psychical Science*, New York, Dutton, 1919; *The Psychic Structures at the Goligher Circle*, Dutton, 1921). A fundamental methodological difficulty in this work, however, is that the measurements reported by Crawford were not confirmed by a sufficient number of independent investigators to minimize, if not to obviate, the possibility that the medium was simply producing data to order. Crawford was an engineer who thought entirely in terms of mechanical "forces," beams, cantilevers, and the like. It is entirely conceivable that the medium, who after all was demonstrably capable of a wide range of physical manifestations, was unconsciously serving as an accomplice "before the fact" by acquiescently dishing up data in a form that Crawford, an engineer, would find most comprehensible and satisfying.

This methodological difficulty is inherent in all our experimental investigations of psi. Once events of the psi class are assumed, moreover, it becomes necessary to redefine the basic concept of measurement as it applies in all science.

II

Before attempting to deal with this question, let us first look at types of disconnections met elsewhere in our process map of nature. As it happens, such breaks in our map are not rare, and we do not have to look far to find other areas of experience where we have never really succeeded in getting behind a description of events in terms of correspondences that remain — for all our efforts to account for them coming into existence *as* correspondences — occult. These correspondences do often enough group themselves into certain regularities and can to this extent be described with more or less satisfying consistency; but they nonetheless remain correspondences of unknown origin, correspondences with, so to speak, no visible or, so far as our conventional means of structuring experience go, invisible means of support.

The most familiar example, and the one ostensibly in closest connection with our present problem, is the broad group of psychophysical correspondences that has always been a central preoccupation of philosophy under the heading of "the relation of mind to matter." The disconnection here has been dealt with mainly in two ways: One, the postulation of a connecting "force" principle which has been given various names (the soul, for example) and which is essentially demonological; two, the attempt to restate the problem in such a way that what appears to be a duality can be reduced to unity. The latter method of attempting a resolution of the issue might be quite satisfactory if the "body-mind" problem existed alone, isolated from the other data and problems of science. But it does not; in fact the more data we gather from other fields, the more this problem, in one form or another, intrudes into everything. Of this, more later.

Another area (or possibly the same projected onto different coordinates) in which we are confronted by a major theoretical disconnection is that of the relation to each other and to the whole of all the events in nature to which the theory of probability is applied. There are currently several differing approaches to probability theory, but the objective of each is to arrive at statements which describe the ways in which events which are axiomatically held to be genetically independent fall out in relation to each other. Herein lies the paradox. One starts with events that are assumed to occur completely independently of each other and ends up with events that, insofar as they fall out in the aggregate in over-all conformity to definite patterns of expectation, seem to show a mutual dependency of some sort that has developed en route, a lawfulness that has crept into the situation somewhere between the initial state-

ment of the rules of the game and the final scoring of results. At any rate, while it may be possible to *describe* with the consistency demanded by logic what takes place when a hundred or a thousand events, each independent of every other at the start, somehow whip themselves into a regimented outcome, it is not possible to say how this comes about. For all our devices of logic, there remains a descriptionless gap between one toss of a coin and any other (to take the most familiar case).

No less enigmatic than the theory of probability upon which it rests are those aspects of quantum theory which provide descriptions of events on the microphysical plane. Exactly the same difficulty is encountered here since our only method of describing what occurs in this realm is to utilize the language of the mathematics of probability. Just how it happens that ruggedly individualistic waves or particles, each conceived of as a law unto itself, somewhere along the line decide to waive their potentially anarchical individual rights and prerogatives and swing good-naturedly and with remarkable *esprit de corps* "into accordance with" the laws of probability, thus transforming themselves into highly respectable and well-ordered macrophysical events — this we cannot account for. We are left again with nothing more than simple, unexplainable correspondences between events that cannot be related except in terms of probabilistic correspondences which themselves have in some inexplicable fashion "jes growed," like Topsy. We have the spectacle of one disreputable character vouching for another.

The one other major segment of nature where we have had to put up with a kind of disconnection which has never been successfully resolved is that in which the law of gravitation is held to apply. Here we have a set of correspondences capable of mathematical definition but with no assumptions as to the causal or dynamical connections involved. "Force" turns out to be an unnecessary concept in the geometry of mass and field, just as it is in the theory of probability, and the technique of description used outflanks the "action-at-a-distance" difficulty by ignoring it.

III

We see thus that, as far as our means of describing what goes on in "causal" terms go, the gaps between our correspondences in the thought-action sphere, between any two events in a probability series, between two definable states of an electron, and between any two bodies in a gravitational system, are just as wide, just as empty of the "stuff of causality" and, in this sense, just as occult, as any two events that go to make up a correspondence of the psi class.

The question now arises: Do we have any particular justification for treating events of the psi class in isolation from the events which make up the occult correspondences in other spheres, and in postulating a special principle applicable primarily to these "psi" correspondences and without reference to the disconnections encountered in our descriptions of other segments of nature?

This question can of course also be posed the other way around; but let us simply recognize the fact that nobody but parasychologists would ask it this way, since the data of parapsychology have no official standing in science. Before going further, however, let us take a preliminary look around to see what other people are doing about *their* disconnections.

The first thing we notice is that in regard to what assumptions it might be convenient to make about this or that disconnection, no uniform convention has been agreed upon by scientists of diverse fields or even by scientists working in the same field. The positions adopted in regard to the treatment of disconnections seem to be a matter of individual preference, and the three main categories into which these positions can be divided appear to be correlated along vocational lines in as little degree as political attitudes.

One convention which has been in high favor among quantum physicists, relativity physicists, and biologists is to treat the problem as one without immediate relevance to the tasks in hand. For working purposes, anything on the order of a "causal" bridge over the disconnections met with is considered to be an unnecessary assumption. In each area the technique of description is held to be adequate to the events dealt with to the extent of providing the necessary framework for the continuance of scientific operations. So long as the structural formulae for relating observed events to each other and for the prediction of as yet unobserved events seem to work, nothing further is demanded. The question of whether or not to impute a causal character to whatever reality may exist beyond the observable data themselves is held to be not a scientific question but a metaphysical one; and the fact that scientists nevertheless spend their time endlessly debating this question is held to be not germane to what they are supposed to be doing *as* scientists.

A somewhat different position, which spread rapidly from the quantum physicists to scientists in all areas after Heisenberg enunciated in 1927 his famous indeterminacy principle, is to deny not only the operational necessity of the causality principle but the very existence of any such principle in nature itself. To the indeterminists, what has looked like causality is at best an illusion, a statistical artefact. There is no nature, no reality, say they, apart

from what we observe; and since we can observe only by means of metrical devices and techniques of description which embody fundamental discontinuities in the mathematical sense and disconnections in the dynamical sense, the grounds for belief in a causal "beyond" in which any such nonsense as "ultimate" process in nature may be imagined to take place simply disappear. The only way we can define nature is in *a*causal terms; and that, they state with perhaps the maximum degree of certainty they can muster, is that!

An inability to accept either of these first two positions has led a growing number of scientists to a third view which, as it turns out, is simply a revival, with very few modifications, of one of the most ancient and widespread attitudes toward the problem of causality in the history of mankind. But first as to the grounds on which the first two positions are sometimes found wanting.

That there can be a sharp distinction between science and metaphysics is disputed. Science, it is claimed, cannot even begin to formulate physical theories — or theories dealing with any type of event, for that matter — without making the most sweeping metaphysical assumptions and without taking into account unobservables and unmeasurables of all kinds. What is more, the distinction between what is observable and what must be assumed is becoming so increasingly blurred that to imagine that we are in a position clearly to say what are "necessary" working assumptions and what are not is a hazardous presumption.

There are more fundamental grounds, however, on which the utter abandonment of something like causality, whether as a working principle or as an ontological one, is unacceptable to many. Such a theoretical position, it may be pointed out, involves a peculiar contradiction in the entire structure of science and scientific method. These have but one consistently tenable objective, an objective to which all part-operations are necessarily subordinate: The construction of hypotheses which replace chance with definable nonchance factors in the description of the universe. The establishment and the carrying out of this program on a level of conscious awareness is *the* feature which distinguishes scientific activity from all other activities of man. To have all our operations, however, culminate in an hypothesis which accords chance a fundamental and sovereign role in all events is just as absurd (for science) as to set up a government at whose apex sits a supreme court which decides every issue in jurisprudence on the assumption that all government is false and by right does not exist. It may be claimed, moreover, that what the acausalists and indeterminists *actually* do is accept government *de facto* while denying it *de jure*, which puts them in a

position hardly less anomalous than that of the woman who, when asked why she didn't finally marry the man who was the father of her ten growing children, replied, "He just doesn't appeal to me."

Now when men of science begin looking for a causal principle they can't quite put their fingers on, which they can't define in terms of an unambiguous set of symbolic relationships, they not infrequently end up with the idea of God, or at least some transcendental principle which amounts to the same thing. This is, of course, exactly what every major metaphysician from Aristotle through Whitehead has held to be the inevitable logical consequence of the global approach in which things and events are considered all together. While today a high degree of sophistication seems to be required before this idea takes hold, this was not always the case. The men of science of Newton's era took it for granted that nature had a Supreme Author, and that their efforts were of course merely attempts to piece together His Work. The mystery and awe they felt were not the blank confusion of today, since there was no mystery in principle: No disconnections existed in the Mind of God.

At any rate, this is a third way of attempting to deal with the problem, an increasingly popular way among men in the top ranks of science and mathematics who are becoming less and less diffident in expressing their conclusions. The disconnections are recognized, held to be a legitimate concern, and dealt with by imputing all occult causal bridges — both in their logical and dynamical aspects — to a Divine Principle or the Mind of God (in the metaphysical sense). These people may agree that the God concept may be as unnecessary for the adequate performance of the daily nine-to-five tasks of the scientific investigator as is the concept of the workings of a huge corporate structure to the petty bookkeeper in one of the branch offices at the periphery of the system. On the other hand they find it difficult to account for the peripheral transactions except in terms of the existence of the parent corporation.

IV

There are then these three major positions on the problem of disconnections taken by scientists today. And since there is no uniformity in the treatment of this problem, it should, needless to say, be permissible in all fairness for parapsychologists to adopt any one of the three positions, which we might characterize as the "No need to worry about it," the "There *is* nothing to worry about," and the "Let George do it" positions.

The matter could conceivably rest this way indefinitely were it not for the fact that even the small measure of tolerance shown among

scientists by the adherents of one doctrine toward the heresies of the other groups does not extend toward the parapsychologists when the latter attempt to adopt one of these positions in relation to their own problem.

It is just as feasible, for instance, to adopt the first position — that for working purposes one can do without metaphysical luxuries — in the case of correspondences of the psi class as it is in the case of the occult correspondences in the other areas cited. Up to this point, in fact, this has been effectually the state of affairs. It has been possible (1) to classify data, (2) to devise experiments, and (3) to make a promising beginning toward the integration of psi data with the major hypotheses of currently acceptable psychological theory — all solely in terms of correspondences of unknown origin established according to the conventional rules for the making of correspondences. The hypothesis of a psi "force," as the form in which the assumption of causality is put, is virtually impertinent to the actual tasks of investigators in these areas; and the question of a causal bridge over the disconnection has had, whether or not acknowledged as such, the essential status of a metaphysical issue.

But the license to ignore what they consider to be metaphysical questions which many statisticians, physicists, and biologists allow themselves they do not grant to the parapsychologists. Physicists may state: "Our only way out seems to be to take for granted the fact that space has the physical property of transmitting electromagnetic waves, and not to bother too much about the meaning of this statement"(3); but they are not at all willing to say to the parapsychologists, "You just go ahead and make what order you can of the events you feel justified in arranging as correspondences, and don't worry too much about how it is possible for these correspondences to occur at all." Scientists in divers fields can and do shrug off *their* unexplainables in this way, but the no whit more "miraculous" data of the parapsychologists are considered by them to be outside the province of science precisely because they are unexplainable in terms of conventional causal assumptions. It would appear that among scientists the absence of sauce for the goose is one thing while the absence of sauce for the gander is quite another.

It is also possible — or at least permissible on comparative grounds — to espouse a more thoroughgoing doctrine of acausality in relation to events of the psi class. In this case too, the adoption of such a credo, or rather *non-credo*, would make little difference as far as the present program of classification and research in parapsychology goes. It is plain that if we are so disposed as to be able to conceive of what is termed the fundamentally statistical

character of events, then psi data, like physical data, need nothing further to justify them than the probability determinations they now have and in terms of which they are in fact construed as significant data.

Here again, however, it has become plain that the ranks of scientists close into a solid wall of opposition to any such point of view. An attempt on the part of parapsychologists to rest squarely and exclusively on their statistical determinations (it makes no difference in such a case whether an outright doctrine of acausality is explicitly asserted or not) seems to call forth a protest of impossibility which stems plainly from the implicit attitude that statistics are not enough in the absence of a justifying causal concept. This, as is well known, has led to a type of attack on the experimental and statistical procedures employed which has not been similarly the case in an effort to batter down the causeless and in this sense equally "impossible" data of the physicists. Even the most determined anti-determinists seem to resist the logical extension of their doctrine (or dogma) to the data of psi, which is as absurd as one atheist claiming that *his* atheism is a higher atheism, one more worthy of faith and belief, than the insubstantial and positively ungrounded atheism of his neighbor.

Historically the most popular tradition in parapsychology, from the days when it was all psychical research, is that which corresponds to the third, or "let George do it" tradition. George has been variously conceived of and has had different *noms d'action* under one or another spiritistic hypothesis, has even achieved the status of Deity, and has now become quasi-mechanized under the alias of "psi"; but George is still the same old George for all that — a creation, a moving principle, a "force" with plenty of *raison d'être* and yet with no more positively identifiable existence than the illustrious Putois of Anatole France's allegorical extravaganza of that name. Putois, whom nobody had ever seen, became endowed with a reality in the minds of the simple villagers who created him as indubitable as that of their own selves, simply because he came to be the perfect and natural explanation of a whole series of otherwise inexplicable events, such as the pregnancy of one of the town's most unassailable virgins. The fact that there was not a shred of first-rate evidence that any one had ever seen Putois, talked to him, heard him, felt him, smelled him, or received a bona fide kick in the seat by him, did not render him any the less credible as the actual agency of a great many otherwise disconnected happenings.

It is questionable whether there is much of a difference between psi, conceived of not solely as a category of phenomenological classi-

fication, but as some sort of dynamical "force," and the transcendent principle which metaphysicians and latter-day scientists identify generically as God. Perhaps the main point of distinction is the former's provincial character, like one of the old tribal gods who later ceded to Jahveh. Psi does things only for the parapsychologist, while the God of the metaphysician has a blanket commitment to do things for all logicians and natural scientists, to fill up all blanks, gaps, holes, discontinuities, and disconnections wherever they may be. In any case, it is precisely the unlimited flexibility of such a concept that has bothered the practical scientist who likes to be weighing and measuring and who prefers a specifiable datum in hand (even if the datum be his own hand) to a deity in the bush (even a burning bush). And it is highly doubtful, furthermore, whether the very scientist who today falls back upon the God concept when he comes to the end of his technical and theoretical tether would grant the same sanctuary to the equally desperate parasychologist.

At all events, it appears that all of science is in the same boat as far as its fundamental assumptions about the interconnections between events go, with parapsychology neither more nor less adrift than other disciplines. Most of these deal with correspondences between events which cannot be accounted for except in statistical terms; but the theory of probability behind this system of accounting turns out to be based upon a set of correspondences no less obscure than those they are used to "explain." It is like finding that all the alleged gold in Fort Knox is nothing but paper money after all, and that all our transactions have been carried out without real collateral.

V

Several scientific authors have recently suggested compromise solutions to the problem of the disconnections within their special fields of investigation but have failed to recognize that the problem they are dealing with cannot be given a narrowly limited treatment. Schroedinger and Lillie, speaking for biophysics and biology respectively (14, 10), have tended to make an implicit distinction between living and non-living systems, and have given the impression that the central problem, as they see it, is simply that of accounting for the directive factor which opposes and alters, as it were, the general laws of probability in the special instance of life. That the problem, however, is exactly the same in non-living systems, where even what we call randomicity stands in need of some sort of explanation, has been brought out by Margenau in his inquiry into what makes quantum-mechanical systems work (12).

The one who most clearly saw that it made no difference whether we were dealing with living or non-living systems, with anti-chance or chance, and who tried to formulate the problem squarely in terms of probability theory itself, was the philosopher-mathematician Karl Marbe (11). This author saw in the paradox of the fact that probability had laws at all and that it worked, what many metaphysicians had already grasped in terms of ontological and cosmological necessity — that nothing in the universe would work, neither atoms nor animals nor astral systems, in the absence of what amounts to a communicating dependency of each event upon every other event and upon the whole, a state of affairs that more or less implies (if we are to avoid unconditional surrender to the "Mind of God" hypothesis) cognitive, reactive and "volitional" aspects of every element in nature, whether as single atoms or as complex polyatomic systems, living or non-living. In his exhaustive study of "what makes probability run," Marbe was led to deny the statistical independence of successive trials in a coin-toss series and came to the same conclusion in regard to every other type of statistical series: i.e., that in a sense nature *does* have a memory (at least he suspected that the answer to the riddle lay somewhere in the psychological sphere, just where he could not finally say). Had Marbe given some thought to the implications of the psychical research of his day, he might have come somewhat closer to unravelling the puzzle which he saw so clearly.

VI

As far as I know, the first major attempt to view the data of psi in connection with the data of both physics and psychology was that of the physicist Jordan (8). Drawing a parallel between the data of these disciplines on an epistemological basis, Jordan attempts to resolve all contradictions between the three sets of data in terms of Bohr's principle of complementarity and the concept of the unconscious, as described by Freud in connection with the phenomenon of repression and as extended by Jung into the idea of the collective unconscious. Jordan sees no causal connection between these sets of data but holds one to be complementary to another in the sense that what is actually done by the observer in the act of measurement determines and at the same time limits the type of datum that results. He extends the psychological concept of repression to the notion that one set of data "represses" another. In terms of the logical relations involved, he feels, this can be asserted just as validly in respect to the position and velocity of the electron as it can in relation to the two phases of a dual personality, only one of which is objectifiably in evidence at a given time. He feels that in some way "the Uncon-

scious" is the missing link. At any rate, paranormal phenomena he holds to occur in the "collective unconscious," which stands in complementary relation to the "collective conscious" space in which physical phenomena (e.g., those linked by radiations of one sort or another) take place. Although what happens in these two "spaces" may be said to be in complementary relationship, there is no causal relationship between the two. This, states Jordan, follows from the fact that physics can assert with complete certainty that there are no undiscovered radiations which can now or ever be brought forth to explain what happens in the parapsychical realm.

A theory somewhat similar to that of Jordan has recently been brought out by Jung (9). This author too points out that the conventional notion of causality is inapplicable to psi phenomena insofar as these are independent of space, time, and energy transfer. He proposes as a hypothetical "explanation factor" in the case of psi phenomena a kind of relationship which he terms *synchronicity*. Synchronicity may be said to obtain where events are linked together in a meaningful but in nowise causal manner, the meaningfulness of a relationship being defined in terms of archetypal ideas existing in the collective unconscious. In other words, archetypal ideas, springing from the collective unconscious, manifest themselves as psi occurrences. Jung too falls back on what amounts to a principle of complementarity in holding the two types of order in the universe — the familiar causal order and the order of events linked by synchronicity — to be mutually irreducible.

Both Jordan and Jung do indeed succeed in showing that the disconnections in data of the psi class are in principle no less embarrassing in the total picture of science than the disconnections in any other class of data; but at the end they leave us with a jigsaw puzzle which somehow fails to fit together. Jordan stresses the epistemological limitations which make it impossible to assert that parapsychical phenomena *cannot* take place, and Jung merely postulates a new principle, a sort of paracausality, by way of justifying the existence of these phenomena. But at this point both leave the world in pieces, divided into the things that are Caesar's and the things that are God's, and with little concern over the fact that a big problem remains so long as Caesar and God are left still not communicating with each other but either "repressing" one another or working side by side, amicably enough and on the whole not getting in each other's way, but in the last analysis quite independently.

The problem, however, is not simply that of staking out the relative domains of differing types of correspondences in a universe big enough and variegated enough to include them all, but of accounting

for the fact that there exist correspondences to be accounted for in the first place. This now brings us back to the question posed earlier: Are we justified in treating all the occult correspondences with which we have to deal, including those we designate as of the psi class, in isolation from one another?

VII

It appears that this question can be argued from many points of view. In many quarters, moreover, it is conceded to be unlikely that a "solution" of the problem of the whole will eventuate solely from the methods of science. Many feel that there will always be something left over, something which cannot be assimilated or even accounted for by any system, however self-consistent in itself, which is forced to abstract for the sake of logical comprehension that which is, if it is anything at all, ineffable, unnameable, and perhaps even unthinkable in the ordinary sense. This is the difficult-to-express insight which the great mystic philosophies have all tried to convey.

What we might strive to achieve, however, is at least a degree of consistency in our ordering of experience that does more than simply admit correspondences of the psi class into a crazy quilt structure on the assumption that one more unassimilable patch isn't going to make much difference. It may be possible to show that it *does* make a difference, and that the difference is in the direction of an increased ability to describe and order experience as at least a fictional continuum. To my mind a proposition which has been asserted so often that it has almost become the watchword of parapsychology — that "paranormal phenomena will not fit into a niche in the existing scheme of science" (Tyrrell) — does not possess the validity that appears to many so self-evident. It can be argued, on the contrary, that paranormal phenomena *do* fit into such a niche, that the niche in fact calls for something like paranormal phenomena to fill it.

For the following argument, a few ground rules must be laid down and clarified for at least minimal working purposes. Actually these are all highly controversial and admit of almost endless debate, but for our purposes let us try provisionally to accept them as axiomatic.

All we ever do in science is arrange and order sets of correspondences in ways that result in the maximum of meaning to us. *We* establish the rules for the making of correspondences, and *we* are the ultimate arbiters of meaning. We have worked out certain techniques (for both these aspects of our behavior) which have at least the capacity for facilitating maximum agreement *among ourselves* on the question of what the rules are to be and what we mean

by "meaning." In this wise we have arrived at working fictions which we term lawfulness, chance, causality, force, and other abstractions.

When we make (or, as we customarily term it, "observe") a correspondence, we are utterly without means for ordering it in relation to other correspondences we have made except in terms of still other correspondences we have made. This system of kiting correspondences is all we have, and is as close as we ever get to the bottom of notions like "force" and "causality," and to explaining what takes place in a solar system, a mechanical system, a kidney, or a group of people acting in a specifiable way. In none of these areas can we do better than "explain" (i.e., order into what *we* consider to be a meaningful arrangement) one set of correspondences in terms of another. One set constitutes, so to speak, the warp of our fabric, another the woof. The warp and the woof hold each other in place, order each other, as it were, but do not constitute, except in the sense of the patterned arrangement that results, a continuum. In logic we can make any number of ordered arrangements which satisfy in this way our criteria of meaning. In pragmatic science we are to a great extent limited by a set of firmly established correspondence-making habits which are altered only slowly in the development of new habits.

Now events which we have come to designate as of the psi class constitute a set of correspondences which we have so far had little success in meaningfully ordering in terms of those correspondences which we abstract into the familiar notions of space and time. As a matter of fact we have much greater success in establishing correspondences of the psi class if we abandon completely any attempt to relate events according to time and space measurements. We can make an analogy here to the superfluity of spatial measurements in many other correspondence systems — for instance that of probability itself.

When we adhere to our program of abandoning completely any reference to spatial and temporal coordinates (i.e., correspondences) and view certain events against a backdrop of a system of correspondences developing from the attempt to order the data of human behavior according to the correspondences that result when we relate the self-observed behavior of the individual to his non-self-observed behavior (i.e., according to the so-called psychodynamic principles based on the concept of "unconscious mental functioning") we find a very rich field for the making and ordering of correspondences which, according to our rules, we may reasonably presume and

designate to be of the psi class.[3] This system of making correspondences appears provisionally to justify the following inferences, according to the published work of several psychoanalytic investigators (1, 2, 4, 6, 7, 13, 15) and the still largely unpublished studies of my own: (1) a large and variegated (i.e., in respect to phenomenology and conditions of appearance) segment of events can now be brought into the class of psi correspondences which otherwise would pass unremarked and unconnected; (2) a much larger than heretofore believed probable segment of the general population (in fact, by not unreasonable presumption, the entire population) lends itself to a certain ordering of behavior in terms of psi correspondences; (3) a reasonable model for the interaction of the members of psychologically defined groups can be constructed along the lines of psi correspondences to supplement but also to integrate descriptively with models of so-called "interpersonal relationships" developed along the lines of other systems of correspondences.

VIII

What can be construed from the study of psi correspondences made from the psychoanalytic observation of small groups of individuals is that people appear to be in effective, albeit "unconscious," communication through other than so-called normal means and in ways in which they mutually influence each other's behavior (thoughts, feelings, actions). This is not continuously observable in the case of any individual or group, and thus we cannot strictly determine from our data whether this "psi-conditioning" of behavior (for lack of a better term) *occurs* only under certain conditions or can be *observed* only under still but poorly defined conditions in the observer as well as the observed. Suffice it to say, however, that many of our data show this type of psi-conditioning of behavior under conditions so similar to what appears to obtain in the most prosaic and humdrum circumstances of life — so far from what are usually considered catastrophic or stressful circumstances — as to invite more than the suspicion that this sort of subliminal determination of behavior may indeed be a general feature of all interpersonal relationships. For reasons which are gradually becoming clear, this presumed state of affairs has to be denied by

[3] Here, however, is precisely where some parapsychologists seem to balk. Despite a good deal of abstract talk about the need to apply the psi hypothesis along broad lines, the moment this is attempted in this or that concrete instance in an effort to achieve a meaningful ordering of otherwise incomprehensible human behavior, an outraged cry arises as if the psi hypothesis were being rudely wrenched from its secure moorings in a glass case and being made off with. People who insist on their money earning interest should not complain that they cannot also have it as gold in their stockings.

what we conventionally regard as the conscious part of the self. This part of us has to adhere to the fiction that we are perforce constrained through natural limitations to live for the most part cut off from, and unreactive to, what goes on about us in a chance-ridden universe, and that the avenues of determination for our behavior at any given moment constitute at best a comparatively thin network in the vast oceans of freedom around us. This view of things is given great support by our seemingly fresh and "unpremeditated" reactions to events which break upon our awareness in the so-called normal manner, and our seeming inability to avert "unforeseen" dangers or to achieve professed goals successfully and without misadventure. But psychoanalytic studies of psi-conditioned behavior tend to show that these are often "cover reactions" at least partly designed to support the fiction adopted by the conscious part of the self. This fiction, at any rate, is easily enough rationalized, as only rarely do psi correspondences which require no particular psychoanalytic decoding come spontaneously to light, and when these do, as every parapsychologist knows, everything in the conscious ego (so to speak) of civilized society seems to conspire to discredit their authenticity, their significance and their power seriously to affect the prevailing belief in the essential separateness of individuals. Some light on the origin and nature of this resistance can be shed by psychoanalytic and anthropological studies on an aspect of psi that appears to be decidedly unwelcome even to the parapsychologist — namely, its active and destructive potential.

One must remind oneself of course that such a schema as has been suggested here of the kind of patternization that can be achieved in interpersonal relationships by means of a psi-correspondence assumption is ideal — ideal in the sense that one cannot actually pick out at random a segment of historical reality and plot it out completely, and with no lacunae left to account for, in these terms. But this is true in one degree or another of any assumption we might entertain and apply.

What is perhaps of more relevance at the moment is this: If we do make the generalized assumption that the human macrocosm can be descriptively ordered along these lines, and if we stop worrying for the moment about what the psi correspondence "really" is or represents, is this in any way helpful to us in ordering other segments of experience? True we have, one way considered, a basic disconnection on our hands. Can we, however, manage to make do with something on the order of this one basic disconnection in other areas of our scientific picture map, or need we continue to have many disconnections, with disconnections between these disconnections? Can we in some way turn several into one?

IX

These questions can at present be only tentatively explored, and any suggestions that can be made are perhaps far yet from the stage where they can be rigorously formulated for decisive testing. But since we are at the moment primarily interested in a general descriptive framework in terms of which we might be able to order the data of several areas of scientific correspondence-making with greater consistency than obtains at present, we might do worse than attempt to apply toward this end a provisional model developed from the study of psi-conditioned human interrelationships. For this, several provisional assumptions would seem to be required, of which the two major ones appear to be as follows: (1) It is possible to order any given set of meaningful correspondences in terms which satisfy certain criteria for meaning in the ordering of any subset, and vice versa. Here we shall have to pass over the difficulties involved in the definition of sets, subsets, criteria for meaning, etc. We shall simply take it that all our present scientific data can be ordered as a set of correspondences, and that the data we divide into physical, biological, parapsychological, etc., can be ordered as homologous subsets. (2) The correspondence we now designate as that of the psi class can be taken as the prototype of every correspondence we are able to and in fact do make in science.

If now we take as a model of all event systems (i.e., all correspondence systems) that segment of the macrocosm which includes the observer and correspondence-maker himself, we are able to discern some method in the madness of the elements of any set. What we are attempting by taking the model of psi-conditioned human interrelationships as a model for all relationships which can be statistically defined is to introduce the notion of a dynamical system geared not to the categorically indeterminate vagaries but to the intercommunication-based responses and "strivings" of the individual element in such relationships. We can, in short, arrive at a means of describing the ways in which the laws of probability are carried out.

This is a crucial advance. We are perhaps as far from understanding the ultimate necessity of these laws as we are from comprehending the "whys" behind seemingly universal constants like e, i and pi, but we at least glimpse a mechanism whereby these laws, which we must accept simply as givens, are maintained. With the assumption of psi-intercommunication between individuals, this is in principle no more incomprehensible than is any directed activity in a freely intracommunicating group. The essential trick behind the "miracle" of probability has been exposed. In embodying the behavior of the

individual as well as of the group in one system of description, furthermore, we are able to bridge the gap between statistical regularity and individual variation.

There is another aspect to this about which we might say a word. When we follow out the implications of the model of how events come to happen that we have derived from our psychological study of psi-correspondences in humans, we are more or less forced, I believe, to a picture of things essentially similar to that which has been poetically expressed in the great mystic philosophies, those of Lao-Tse, Meister Eckhart, and others. In these systems the growth of a seed, the mind of man and the light from our most distant star are, if I might so put it, reciprocally influential, mutually determinative. This way of expressing things suffers, of course, from our almost ineradicable proneness to view things from the outside, which Lao-Tse and others caution us against when they tell us we must strive to nullify the gap between the "I" and the "not I" in any attempt to "know" what is what. When, however, we stick to the method of description in which we use the correspondence of the psi class in the manner suggested, we can break down to a great extent the distinction between the "I" and the "not I" at the same time as we are forced step by step to connect in one system the doings of the electron, the strivings of man, and the motions of the stars. An anthropocentric bias in our conception of the psi correspondence sooner or later becomes untenable except in terms of ourselves as the map makers.

X

We have tried to minimize the number of separately conceived disconnections with which science has to deal. We as parapsychologists term the disconnection we deal with the correspondence of the psi class. Physicists and biologists no doubt would prefer to call it something else. But whatever it is called, it performs the same service descriptively in linking events which can be ordered meaningfully but not in terms of those congruent pointer readings to which we give the shorthand designation of "force." We have tried to outline a system whereby it might be possible to take care of several unknowns with one basic postulate, much as the Federal Reserve Banking system has been able enormously to expand the monetary credit structure with a single mobile cash reserve. If there be some who prefer to believe that the kiting of correspondences that we do in these maneuvers corresponds to something "inside" or "outside" that may be called causality, I can not personally foresee in this any undesirable consequences. At least such a belief might constitute a "cause," so to speak, serving to unite various factions in an other-

wise loosely knit household, like a decrepit old family servant who no longer gets the stairs swept but whose welfare nevertheless constitutes the one effectual point of liaison between the otherwise mutually disinterested members of the younger generation. One way or the other, however, it no longer appears strictly necessary to divide the universe into two mutually irreducible principles, causality and chance, or causality and synchronicity.[4] These apparent opposites can be satisfactorily brought together and reduced into mutually meaningful terms by the consistent use of our correspondence-making system.

In regard to the question of, "Is there or is there not a power behind the throne," it may indeed be that with so much concrete "causality" *in absentia,* we are led inevitably to a metaphysical God concept. But such a concept need not be a static one, however much of a hard, unfilterable residue may be yet left over after all its transformations. The fact is that the metaphysical God of Whitehead is not quite the same as that of Aristotle: One might say that he has shrunk somewhat (although very little, to tell the truth) in mystery as he has gained in cleverness, much as a magician who has bit by bit revealed his secret methods of legerdemain. In a sense he remains the master magician whether he leads you to believe in the immanent entelechy of his strange automatons or tells you finally that he does it all by cybernetics. But even if one felt one had to rest upon the principle of an ultimate Godhead, it should be possible to do more than proclaim its utter unfathomability and unknowableness. Perhaps the Godhead can be seen and known by means of certain tricks and devices, technical and operational mirrors, if you will, in many modalities now still unperceived or uncomprehended. By progressive devacuumization and definition, by substitution of sharp lineament and positive specification for hazy aura, we can perhaps trade ultimate for penultimate mystery, approach more closely to The Asymptotic Essence. In such an approach, which we might make somewhat in the spirit of Pascal, the psi correspondence would appear at the moment to give us our maximum leverage.

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[4] It seems to me that in any case Jung's distinction between causality and synchronicity is not very helpful. What is generally referred to as causality has fundamentally all the earmarks of what Jung terms synchronicity in that it is found to "consist of," so to speak, nothing but a certain type of meaningfulness; and what Jung terms "synchronistic" occurrences have as much right to be considered "causally" connected, insofar as we can succeed in ordering them at all, as any other events. Whether or not they have a connection "in themselves" may be beyond our power to ascertain; but so long as *we* are able to connect them in ways which we regard as lawful, we have a right to view them, if we so please, as "causally determined." We can do no more in the case of the heavenly bodies.

XI

Before concluding, I would like to say something about those psi correspondences to which the term "precognition" has generally been applied. Most parapsychologists have tended to regard the data of these correspondences as requiring radical alterations in our constructs of time and causality. It is almost axiomatically assumed that on the face of things a complete revolution in our ways of ordering events in relation to these parameters will be required before a satisfactory framework for "precognitive" phenomena will ever be found.

However, parapsychologists have failed fully to explore the logical implications of all the psi data they have, and have given short shrift to hypotheses advanced by investigators who have tried to do so. The most consistent hypothesis of this sort was put forth by Tanagras (16, 17), who attempted to account for all chronologically extraordinary psi correspondences (a term which I prefer to "precognitive phenomena") on the basis of the more readily accepted psi "processes" such as telepathy, clairvoyance and psychokinesis in conjunction with hypotheses involving telepathic influence and control. Malevolent wishes on an unconscious level, corresponding to hexing and the "evil eye" on a conscious one, he believed to play a hidden but none the less greatly determinative role in the affairs of mankind. Tanagras' point in essence was that in the case of all psi correspondences which appear to be "precognitive," the future is not foreseen but created, brought about by means of the agencies he described.

I do not believe that Tanagras' theory, or any other theory that rests exclusively on such simple and ready-to-hand notions as this author invokes, can account successfully for all types of psi correspondences which appear to be chronologically extraordinary, particularly long term correspondences such as those which some of Nostradamus' predictions might conceivably be classified as.[5] However, I do believe that Tanagras' basic hypotheses are in the right direction, and that those of the parapsychologists who feel obliged to postulate novel, hidden "dimensions" (or what not) in regard to time are completely indefensible from every point of view.[6] The

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[5] There is enough ground in the studies and commentaries on Nostradamus to date to warrant a much more searching historical and linguistic investigation of his quatrains than has been done. To write these off as unsuitable for further research is an unjustifiable presumption.

[6] Some of these postulates have been presented in terms which look mathematical, but C. T. K. Chari has shown in a recent study (Time as Minkowski's Fourth Dimension: *Indian Astrological Magazine*, Annual Number, 1952) that there is not a shred of evidence anywhere in mathematics to support the notion that any sense they seem to make in this form is anything but illusory.

necessary assumptions (assumptions about "mind," if we may so put it, rather than about "time") that would be required to correct Tanagras' theory and to bring it into concordance with our data are not only logically conceivable but even positively justifiable in terms of science's present stockpile of correspondences, including those of the psi class.[7] On the other hand, the radical assumptions about time which have been suggested to account for "precognitive" phenomena are irreconcilable on all fronts with all other correspondences known to science. On analysis these assumptions turn out to be magic wands which the looseness of our language permits us to wave about in all seriousness and solemnity.

One of the more significant consequences of bringing the psychoanalytic set of correspondences to bear upon the problem of chronologically extraordinary psi correspondences ("precognition") has been that a number of pieces of a complex puzzle have one by one begun to fall into place, revealing something of the depth and the interrelationships of the all-but-unconquerable resistances to psi which tend to narrow and distort the perspectives not only of science in general but also of the parapsychologists themselves. The working out of this puzzle has in many respects been like the unfolding of a mystery novel, with the resolution of one apparent paradox after another once the long hidden clue has been brought to light (5). The clue in this case seems to be related to one of the most important consequences of the origin somewhere in the dim past of the primordial family institution, with its much prolonged period of maternal dependency, namely, the development on man's part of more and more ineradicable guilt over his destructive tendencies and his consequent need to project further and further from himself both the responsibility for his deeply repressed aggressions and the awareness of his active role in the events that he comes to conceive of as occurring "outside" of and apart from himself. This can be related on the one hand to the peculiarly obscure "now you see it, now you don't" phenomenology of psi and, on the other, to man's ostensible need to project the entire notion of causality step by step from within outward, beginning with the primitive conception of *mana,* progressing through magic and witchcraft to parallel developments

[7] It will be necessary, however, for many parapsychologists first to untether themselves from the completely unwarranted assumption, implicit in much of the current literature, that our best source of information upon which to base inferences regarding the possible range and scope of psi is the laboratory. As a case in point we might cite the scandalously rigid attitudes toward PK which seem to prevail. One might gather that the reporting of observations on the behavior of dice constituted science, while the reporting of observations on the behavior of people — by such disingenuous dabblers as Crookes, Lodge, Barrett and others — constitute merely unreliable contributions to folklore.

in science and religion, and culminating in our day in an epistemological device for getting rid of the basic concept altogether.[8]

I believe that the time may soon be ripe for a counter-revolution in which both the data of psi correspondences and the data of psychoanalysis, particularly those relating to man's hidden aggressions, his defenses against them and the techniques with which he attempts to minimize and deny them, will be brought into a framework for the ordering of experience much more unified and much more self-consistent than now obtains. The problem of the disconnections in science will not then disappear, but it will have a decidedly new look about it.

[8] See in this connection, Kelsen, Hans: *Society and Nature*, University of Chicago Press, 1943. This author has been able to arrive through anthropological and historical research at a thesis which checks precisely with the inferences to be drawn from a psychoanalytic approach to the problem of the disconnections in science (which can be said to include the problem of chronologically extraordinary psi correspondences). This thesis is that the conventional concept of causality had its origin in primitive fears of retribution for wrong done. Kelsen is able to show, among other things, that the original words for guilt and cause were identical in both the early Indian languages and in Greek, whence they were taken over into juristic forms and later into the writings of the early Greek scientist philosophers.

901 Sherman Street
Denver 3
Colorado

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Eusapia Palladino in Retrospect

J. FRASER NICOL

Of the many mediums who have professed to demonstrate the physical phenomena of psychical research, the claims of Eusapia Palladino are in some respects the most impressive. Over a period of twenty years she was investigated by a greater number of scientific committees than any other medium in history.

One of the last chapters in her long career has only now been revealed in full by the publication of the transcripts of her American sittings of 1909-1910.[1] As this new account is likely to be the last substantial addition to the enormous literature on the mediumship that we are ever likely to see, it may be worth while to glance back at some of the highlights of this woman's surprising history.

Born of peasant stock in Southern Italy on January 21, 1854, Eusapia was orphaned at an early age and eventually went to make her home in the 1860's with a family who had evinced a lively interest in the new craze for table turning. Eusapia was invited to join their circle, and after ten uneventful minutes, the table took to the air. In the course of further séances it became clear that the source of the manifestations was Eusapia Palladino.

Not until many years had passed, however, was Eusapia subjected to scientific investigation. As a result of an earlier study by Professor Chiaia of Naples, Professor Cesare Lombroso with a group of scientists and scholars held two sittings with Eusapia in 1891 and reported favorably to the medium's claims. Other groups observed the phenomena at Milan and Naples, and Eusapia was soon famous throughout Europe. She travelled widely—St. Petersburg, Rome, Warsaw, Paris—and gave sittings for various international groups. Three highlights of her career best illustrate the enigma of Eusapia's mediumship: her sittings for Charles Richet, the S.P.R. study at Cambridge, and the famous Naples investigation of 1908.

Eusapia's skill in cheating had been detected and studied in the early eighteen-nineties by Torelli Viollier, Richet, and Ochorowicz. Her methods were well known and steps had been taken (e.g., by Richet) to prevent them. It was against this background knowledge that in 1894 Richet, Oliver Lodge, F. W. H. Myers and J. Ochoro-

[1] Hereward Carrington, *The American Séances with Eusapia Palladino.* Pp. viii + 273. Garrett Publications, New York, 1954. $3.75. (Note the spelling of Palladino. In the early literature the name is spelled "Padalino" and later "Paladino." Eusapia spelled her name with two "ll's," as can be seen from the facsimile signature in H. Carrington, *Eusapia Palladino and Her Phenomena,* New York, 1909.)

wicz sat with Eusapia at Richet's home on the Ile Roubaud in the Mediterranean. So impressive were the phenomena observed that Richet later held additional séances near Toulon at which Professor and Mrs. Henry Sidgwick and Baron von Schrenck-Notzing were also present. As a result of these two groups of sittings, the investigators—notably Lodge and, a trifle more cautiously, Mrs. Sidgwick—presented accounts highly favorable to the paranormal hypothesis. Surveying these reports[2] now, however, it does seem clear that while indeed some of the telekinetic effects may have been, and probably were, genuine, there was a proportion that could be very plausibly laid to the credit of the medium's adroitness in hoodwinking her sitters. Throughout her long career, Eusapia's most useful piece of legerdemain consisted in approximating her two (controlled) hands until, with a deft movement, she caused the controllers to share the control of *one* hand while the other slipped free. Her extreme restlessness at the séance table made this trick easy to perform—at any rate with inexperienced sitters.

So far as I am aware, however, Eusapia was never found guilty of fraud prepared in advance of a sitting. On various occasions in her later history she was thoroughly searched and nothing suspicious was found.

The S.P.R. group at Richet's sittings appeared to have been satisfied that, even after making full allowance for Eusapia's tricks, her phenomena warranted further study. Accordingly they invited her to Cambridge, England, the following year, 1895. In the meantime, however, Dr. Richard Hodgson expressed dissatisfaction with the report of the French sittings on the ground that the control of the medium at crucial moments was insufficiently described. Hodgson, then Secretary for the S.P.R. in America, was invited to the Cambridge inquiry.

The Cambridge sittings form one of the most perplexing chapters in the history of mediumship. Eusapia was repeatedly detected in fraud, principally by her hand-substitution trick. Sidgwick wrote that "the investigators unanimously arrived . . . [at the conclusion] that systematic fraud had been used from first to last, and that there was no adequate reason to suppose any supernormal agency whatever."

The report of the Cambridge investigation[3] is most disappointing, for, whereas Hodgson had sharply criticized the French report on the point of inadequate reporting of controls, the Cambridge

[2] *Journal* S.P.R., Vol. 6, 1894, pp. 306-360. For the ensuing controversy between Hodgson, Lodge, Myers, Richet, and Ochorowicz, see *Journal* S.P.R., Vol. 7, 1895, pp. 36-79.

[3] *Journal* S.P.R., Vol. 7, 1895, pp. 131-135 and 148-159.

report is even less informative. Only one of the sittings was published in detail and more than two-thirds of it consists of notes added by some of the sitters at *later* dates. One of the investigators, Oliver Lodge, in a letter subsequently published, made it clear that he did not wish to have his name associated with the "Cambridge investigators." Hodgson and his colleagues had discovered (as Richet pointed out) only what was already well known from the work of previous investigators.

While Sidgwick spoke of Eusapia's "mischievous trade," the Continental researchers were less inclined to moralize. They employed—or professed to employ—methods of control whereby fraud would be excluded. Reports continued favorable (though, to later readers, not always so satisfying as appeared at the time).

For a dozen years Eusapia continued to be the wonder of Europe and the flow of reports endorsing her phenomena encouraged a new and rather younger S.P.R. group to new efforts at Naples in 1908. The investigators were the Hon. Everard Feilding, W. W. Baggally, and (from the American S.P.R.) Hereward Carrington. They were a remarkable group: two of them, Baggally and Carrington, were skilled amateur conjurors; two, Carrington and Feilding, had been investigating alleged physical phenomena for ten years—Baggally for thirty-five years—and they had never experienced a genuine physical phenomenon. Carrington was the author of what is still the standard work on séance-room fraud.[4]

The Cambridge and Naples sittings differ in two important respects. The former took place in almost complete darkness and the medium was given opportunities to cheat. At Naples "many of the phenomena took place in light when her hands and her whole body were in full view."[5] Also the intention of the investigators was to exert a control so rigid that cheating would be impossible. In this last respect Feilding and his colleagues met with much success if one is to judge from the evidence of the shorthand transcript of the sittings.[6]

In these circumstances the table repeatedly rose a foot or more in the air when apparently no person was touching it. In the curtained cabinet behind Eusapia a bell rang, a guitar was strummed, and a table carrying various objects was moved about. The famous

[4] *The Physical Phenomena of Spiritualism.* First Edition, 1907; 3rd Edition, 1920. In the first edition Carrington adopted an "extremely skeptical" view of Eusapia, an opinion which he retracted after his Naples experience in the following year.

[5] W. W. Baggally, *Journal* S.P.R., Vol. 14, 1910, p. 209.

[6] E. Feilding, W. W. Baggally, & H. Carrington, "Report on a Series of Sittings with Eusapia Palladino," *Proc.* S.P.R., Vol. 23, 1909, pp. 309-569.

breeze from a scar on Eusapia's forehead was detected and a hand was repeatedly seen carrying objects out of the cabinet while her normal hands were held by the investigators.

One of the most interesting things about this report is the persistent skepticism of all three researchers. On the morning following a sitting, they would re-read their notes and again lapse into doubt on the ground that since these events were in conflict with the known laws of nature they could not have happened. Nevertheless, having excluded all explanations known to them (including collective hallucination), they delivered a unanimous report affirming the paranormal origin of the phenomena.

In those Naples séances Eusapia reached the zenith of her career. After that came the descent, the first signs of which were in the American sittings now reported.

She arrived in New York on November 10, 1909, and sensational accounts appeared in the newspapers next morning of demonstrations of her powers given by Eusapia on shipboard. The first séance in the United States was given for the benefit of newspaper reporters, Dr. Carrington believing that this favor would keep the press quiet for the ensuing months of Eusapia's visit. This hope was falsified by events. And the story of the American venture is mixed up with the mishaps that so often attend newspaper publicity in psychical research. When journalists were not at hand, some sitters could be depended upon to describe their supposed experiences to the papers and magazines. Professor Hugo Münsterberg wrote a sensational article for *Collier's*. Professor Joseph Jastrow attended one séance and wrote two articles about it. A professor of philosophy retailed his findings in the *New York Times*, and so on.

Such was one burden. Another and perhaps more important one lay in the inexperience of so many of the sitters. So far as one can see, almost the only investigator competent to deal with the problems at issue and to forestall the risks of fraud was Dr. Carrington himself. But, through the seven months of the sittings, though Carrington was usually present in the séance room at Lincoln Square Arcade, New York, he left the control of the medium largely in the hands of inexperienced visitors. No doubt his course may have seemed desirable at the time, since he mentions that his relations with the sitters were "to some extent financial as well as scientific" (p. 98), and elsewhere he is described as the medium's "manager" (p. 172). Whatever the reasons, the absence of skilled controllers impairs the value of the report. Carrington did, however, send a wise letter to all prospective sitters describing "Eusapia's chief method of trickery" and advising as to the best methods of preventing it.

In spite of the clear instructions, one has frequent doubts as to the adequacy of the control. On the rare occasions on which Carrington himself came forward to take a direct hand in the control, the thoroughness of the detail in the report increases remarkably. For example, on one occasion (January 8, 1910) there is a succession of supposedly striking phenomena occurring over a period of ten minutes, but not a word from the controllers, Professor Busch and Professor D. S. Miller, until it is all over, when they claim that "the control has been continuous." By contrast with these blanket assurances, there is the sitting of November 14, 1909, in which Carrington plainly kept a firm grip on the scientific requirements. He gives an almost minute-by-minute account of the medium's whereabouts and actions, and admonishes other sitters to beware of tricks. At one point the left-hand curtain of the cabinet behind the medium is blown out over the table, and Carrington says, "That is the sort of thing which happens when she has released one hand." We have Carrington giving minutiae of his control, whereas with regard to other controllers, we have to be satisfied too often with vague and belated assurances.

Still, taken all in all, and keeping in mind the authenticity of the accounts presented by the S.P.R. group at Naples, and by the Psychological Institute in Paris, it is readily apparent that many of the phenomena asserted to have occurred in America were of the same form. They may be summarized thus:

1. Telekinesis. The séance table (weight 12 lbs.), was observed in fairly good red light to rise as much as two feet in the air on a number of occasions without visible means of support. Eusapia always sat outside the curtained cabinet; yet within it, a small table and musical instruments were knocked about and sometimes projected into the room.

As a subdivision of the telekinetic phenomena, we may note the production of sounds from the musical instruments. Thus, while Eusapia would flick her fingers back and forth in the air, a mandolin in the cabinet behind her would sound in unison with her movements. This had apparently been shown at earlier times not to be due to any thread or other object connecting the medium with the instrument. If true, it suggests that Eusapia could exercise *conscious* control over the queer events occurring in her presence. Indeed when she felt that some phenomenon was about to appear, she would often cry out for closer control.

2. The appearance and disappearance of objects of unknown origin. These were of irregular structure, sometimes hand-like in shape, and they usually emerged from between the curtains. These

are the most inexplicable, and indeed the most suspect manifestations, and yet when the medium was searched, nothing was ever found to account for them.

3. The "breeze" from a scar on the right side of Eusapia's forehead. This had been noticed at séances and in clear light at other times by many investigators. It could not apparently be explained by any novel use of the medium's breathing apparatus, and its source remains to this day one of the quainter mysteries of Palladino.

In January, 1910, the medium's services were transferred to a group of scientists and philosophers at Columbia University. Four sittings were held in eight days. Twelve persons were present at these séances. Carrington was excluded, and the status of the eminent men who took part can be described without offense as that of earnest apprentices. There must be few séance records so disappointing as those of the four Columbia sittings. Time and again one is led to suppose that the control was "perfect" (a favorite Columbianism) and yet when we want to know the whereabouts of Eusapia's hands and feet and of the scientists' own hands at the exact moments of the phenomena, the sitters forget to tell us.

The truth of the matter seems to be that they were inadequate to their task, and that in the first three sittings they were undergoing a hasty education in the complexities of the séance room. At the fourth and final sitting they determined to do better, and imposed what they apparently believed were methods that would make "trickery absolutely impossible." In a statement published in *Science*, May, 1910, they declared that under those conditions "none of the so-called evidential phenomena took place." So far as I am aware, no full account of their sittings was ever published by the Columbia group, but the stenographic report is now before us, and here their contemporary remarks to the stenographer are plainly at variance with their comprehensive assertion in *Science*.

With regard to the controls, there are several examples of sitters disputing each other's assurances. With regard to the phenomena, no doubt the medium was being put on her mettle, and the demonstrations were certainly of a reduced variety. Nevertheless there were innumerable curtain movements, some table tilting, "touches" were felt by sitters—all under supposedly good conditions of control.

It is perhaps not surprising that the work of the Columbia committee was so unsatisfactory, for Carrington tells us (p. 232) that "not a single member of this committee had taken the trouble to inform himself on the history of the case," nor had they read any of the reports written by scientists more distinguished in psychical research than themselves.

Accounts of thirty-one séances in all are given in the book, including the shorthand transcripts of twenty-eight of them. What conclusions can be drawn? Because of the inexperience of the sitters, the records are extensively imperfect, the controls are often inadequate, and there seems no reason to doubt that fraud was frequently perpetrated. Examples, however, can be found—the first and tenth séances are instances—where some of the manifestations cannot easily be fitted to a "normal" hypothesis. If they stood alone, the American séances could satisfy no critical-minded reader, but taken in conjunction with earlier and better investigations, it may be supposed that some at least of the positive but restrained views expressed by Dr. Carrington can be supported by fairly good evidence presented in the book.

Eusapia spent seven months in the United States, and when she sailed for Naples on June 18, 1910, her career was virtually at an end. She gave a few sittings for an S.P.R. group in November-December of the same year, but the conditions were lax, and the results unsatisfactory.

Over a period of years her health seems to have undergone a slow decline, and she died on May 16, 1918.

As to the *nature* of the telekinetic force which she ostensibly demonstrated, her contemporaries were hardly any wiser at the end of her career than at its beginning. Instrumental and psychophysiological methods, occasionally employed, yielded inconclusive results for the most part. The value of Eusapia Palladino to psychical research lay elsewhere. Her readiness to be investigated by competent observers permitted the development of rigorous methods of control and of exact recording of facts at the moment of their occurrence to a degree wholly unknown before her time. Experimenters of the next generation profited from the impressive standards created by Feilding, Baggally, Carrington, and others. Let us hope that their example will not be lost sight of in the future.

An Examination of the Humphrey-Nicol Experiments on the Feeling of Success in ESP

ROBERT H. THOULESS

Dr. Humphrey and Mr. Nicol have made a praiseworthy attack on a problem which has been incidentally considered by a number of other parapsychologists, as to whether subjects who are doing ESP experiments can to any extent know whether they are guessing correctly or not.[1] The general opinion has been that even very good ESP subjects are unable to make correct reports as to which of their guesses are correct in the experimental situation although spontaneous cases of ESP suggest that those experiencing them often have also a conviction that these do represent reality. It is obvious that failure in the past to demonstrate any knowing by the subject of when he is guessing correctly is insufficient ground for conviction that nothing of the sort will be discovered in the future and the question remains open as a research problem.

The authors of this paper present evidence which they consider shows that their subjects were able to some extent to make this discrimination between right and wrong guesses. If this result is confirmed, it will be an important finding. Unfortunately most of the evidence presented cannot be regarded as proof of this hypothesis since the results obtained could be explained in another way. Some of the statistical estimation also appears to be faulty. There remains at least one strong indication (in Table 12) that the subjects were succeeding to a small extent in the task of discriminating between right and wrong answers.

The subjects were asked to call "check" when they felt sure that the guess they had made was correct, and the question the authors asked was whether the percentage of checked responses was greater amongst the hits than amongst the misses.

The main part of the evidence is presented in Table 6 in which the totals of checked and unchecked hits and misses are shown for the series of experiments with 34 subjects under "unknown" conditions, i.e., when the subject was not told of his success or failure till after the completion of the run. The results are as follows:

[1] Betty M. Humphrey and J. Fraser Nicol, "The Feeling of Success in ESP," JOURNAL A.S.P.R., Vol. XLIX, 1955, pp. 3-37.

	Hits	Misses	Total
Checked	407	1431	1838
Not Checked	905	4057	4962
Total	1312	5488	6800
Per cent Checked:	31.0%	26.1%	

$\chi^2 = 13.13$
$P = .0003$

This is only part of the story since there was also a series of experiments under "known" conditions, in which the subject was informed of success or failure immediately after guessing, in which there was no significant tendency to check a higher proportion of hits than misses. Even considered, however, as the best of two series of experiments, the "unknown" results are impressively significant and, at first sight, appear to support the authors' conclusion that their subjects were able to distinguish to some extent between success and failure.

This conclusion is, however, fallacious since the results might be due to a very different cause. If those subjects who showed high scoring also happened to call "check" more often, the above result might follow even though any one subject were no more likely to call "check" for his hits than for his misses. It is obvious that such a tendency for higher scoring subjects to call "check" more frequently both for hits and for misses would not imply any paranormal capacity to know that they were scoring highly; it could be determined by their previous experience of having high scores. In fact an individual who habitually found that he had high scores might very well develop a higher degree of confidence which led him to say "check" more often.

This possibility may be illustrated by a fictitious numerical example. Let us suppose that two subjects A and B have each made 100 guesses with the following results:

A.	Hits	Misses	Total
Checked	4	16	20
Not Checked:	16	64	80
Total	20	80	100
Per cent Checked:	20%	20%	

B.	Hits	Misses	Total
Checked	16	24	40
Not Checked	24	36	60
Total	40	60	100
Per cent Checked:	40%	40%	

It is obvious that in neither case is there any evidence of the subject knowing when he is right. Both subjects are as likely to check misses as hits.

But when these two sets are added together, the following result is obtained:

A + B	Hits	Misses	Total
Checked	20	40	60
Not Checked	40	100	140
Total	60	140	200
Per cent Checked:	33.3%	28.6%	

The mixed scores look like the results of the Humphrey-Nicol experiments shown in the earlier table, and would seem to suggest that subjects knew to some extent when they were guessing right. This appearance is, however, misleading since neither A nor B showed any signs of knowing when they guessed right. It is due

merely to the fact that B both guessed right more often than A and also checked more often than did A.

All the evidence of this kind presented by the authors must therefore be dismissed as fallacious. More cogent evidence could be obtained either by treating the results of each subject as a unit and seeing how many subjects showed a higher proportion of checks on hits than on misses or by treating each run as a unit and performing a parallel calculation. The authors have presented evidence of the first kind. It is, however, not statistically significant.

The evidence is given at the bottom of page 23. In the "unknown" experiments, 23 subjects gave a greater number of checks to hits than to misses while 11 gave a greater number of checks to misses than to hits. The chance expectation is, of course, 17 in each group. For this discrepancy from expectation chi^2 (with Yates's correction for discontinuity) is 3.56. This gives $P = .06$ which would be only marginally significant even if this were the only set of experiments to be considered. But there was also the "known" set of experiments which gave no indication of the effect. If we consider that the present experiment is the more striking result of two sets of experiments, we must ask what is the probability that at least one of two sets of experiments will show a deviation from expectation equal to or greater than this. The required probability is .12 which is clearly beyond the margin for significance.

The authors give what appears to be a considerable over-estimate of significance when they say (p. 23) "the probability here is .012." It is to be supposed that the quantity referred to is P/2, the probability of a deviation of at least this size in the observed direction occurring in a chance series whereas the chi^2 method gives the probability of a deviation in either direction, but even for P/2 this estimate does not seem to be correct. It is true that the chi^2 method gives only a close approximation to P, and the exact criterion in this case is, as the authors point out, obtained by summation of the binomial expansion of $(\frac{1}{2} + \frac{1}{2})^{34}$. They say, however, in a footnote that summation of the binomial is laborious, so they used the approximation given by Pearson's incomplete beta distribution. They perhaps exaggerate the labor of summing the binomial series. I have done so and find that P/2 is .03, which gives the same value of P as that given by the chi^2 method. The choice of the incomplete beta distribution was perhaps unfortunate since the effect of using a continuous function for a discontinuous variate is to overestimate the significance, although I should not have expected the overestimation to be as great as that represented by the difference between .03 and .012.

In view of the fact that, when correctly estimated, the value of P for the results as treated on p. 23 does not reach significance, this part of the work also cannot be regarded as evidence for the reality of any capacity for discriminating between right and wrong responses although a significant result here would have been valid evidence. There is, however, better evidence in favor of the authors' hypothesis in an interesting table on p. 24 which shows the number of checks given to right and wrong answers in runs with different scores.

The first point of interest in this table is that one may hope to obtain from it some indication as to whether the main results could have been due to high scoring subjects giving the greatest number of checks both to hits and misses. The indications are not altogether clear but there is some suggestion that high scoring runs contain a large number of checks. In the runs with a score of 9, for example, there are 9.33 checks per run which is the largest total number, the next highest being 7.62. In these runs too there is the largest proportion of checked misses, 35.4%. At the other extreme, the runs with a score of one show the lowest number of checks per run: 6.36. There is not, however, any continuous gradation between these extremes; the runs with a score of 2 give the second largest average number of checks per run. The general appearance of the table is not inconsistent with the hypothesis that the main results could have been due to high scoring subjects giving the most checks.

On the other hand this table yields one piece of evidence which cannot be explained by this hypothesis. As the authors point out, all collections of runs with the same score show a higher proportion of checks on hits than on misses. This result is significant ($P = .004$) and even when we take into account that no such effect is reported for the other "unknown" series, the result remains comfortably significant (P less than .01).

While this may be regarded as an encouraging indication that there may be some power of discrimination between right and wrong answers in this experimental series, it must, of course, be treated with some reserve as evidence. It was not the way in which it was intended that the experimental results should be tested and the appearance of significance may be illusory since if a sufficient number of unexpected oddities of the data are examined some will be found to have low values of P even if there is no real cause producing a departure from randomness. It cannot be regarded as proof but as a pointer for further research. No claim for any value as proof of this peculiarity of the figures is, of course, made by the authors who are careful not to base conclusions on unanticipated characteristics of their data.

SUMMARY.

The main argument on which the authors base their conclusions is fallacious since their results could be explained by the possibility that high scoring subjects made the most checks without any assumption that subjects could discriminate between right and wrong calls.

The argument based on the number of subjects calling "check" more often for right answers than wrong would be valid if the observed deviation from expectation were significant. It is, however, wrongly claimed by the authors to be significant.

The observation that all run lengths show a higher proportion of checks for right answers than for wrong ones is significant but its value as evidence is limited by the fact that this was not the way in which it was intended that the data should be examined.

2, Leys Rd.,
Cambridge,
England.

An Answer to the "Examination"

BETTY HUMPHREY NICOL AND J. FRASER NICOL

If we understand it correctly, the argument in the above "Examination" runs as follows:

1. The statistics may involve a fallacy.
2. Therefore they do involve a fallacy.

Concerning 1) we are given the imaginary and extreme examples A, B, and their Total. Concerning 2) we have the statement: "All the evidence of this kind presented by the authors must therefore be dismissed as fallacious."

It will be shown that what is weak in logic is empirically baseless.

Before ever the "checking" experiments began, close study was given to the statistical problems of estimation. At the conclusion of the experiments further work was done, both theoretical and practical. We were fully aware of the controversies that have enlivened the history of contingency table methods in the last half century, and we are not so unacquainted with the terrain as to slip into a pitfall that was exposed by Udny Yule more than forty years ago.

Our answers to the above criticisms will be taken up under three headings:

1) Relation of checking *frequency* to run scores.
2) Relation of checking *frequency* to subjects' total scores.
3) Relation of checking *success* to run score levels.

Relation of Checking Frequency to Run Scores

The statistical "fallacy" is conjectured to have arisen by high PNC (paranormal cognition) scores receiving *many* checks while low scores received *few*. Combine these two categories and the checking "fallacy" appears.

Let us then compare the *number* of checks given on high run scores with those given on low run scores. The "Examination" presents extreme examples. In our Unknown data (the section in which the checking effect appeared), the most extreme cases we can find are one run with 11 hits and one run with zero hits. On the former there were no checks; on the latter, six. Comparing these, we have:

	PNC SCORE		
	0	11	Total
Checked Trials	6	0	6
Unchecked Trials	19	25	44
Total	25	25	50

By direct computation the probability associated with this table is .022. This is significant, but the distribution of the checks is directly *opposite* to that predicted by the "fallacy" theory.

The number of trials and checks in the above case is small; and it is incumbent upon us to examine other extreme comparisons. Thus we may compare other pairs of extreme scores. The next step then is to sum the number of checks on the above score of 11 with the checks made on run scores of 9 (there were no run scores of 10 in our data). Those are to be compared with the number of checks given on runs with scores of 0 and 1. This test gives the result:

$$\chi^2 = 2.45 \qquad P = .12$$

This also does not support the "theory." There is no significant difference between the number of checks given on these high and low scoring runs.

Let us go further by comparing the number of checks on run scores of 11, 9, and 8 with those on run scores of 0, 1, and 2. Result:

$$\chi^2 = 0.386 \qquad P = .53$$

This also is a random effect. Comparing now the number of checks given on run scores of 11, 9, 8, and 7 with those of 0, 1, 2, and 3 gives

$$\chi^2 = 0.588 \qquad P = .44$$

The difference is again very close to chance expectation.

To complete the examination, we compare the number of checks on *all* run scores above chance ("high") with the number of checks on *all* the run scores below chance ("low"): that is, checks on scores of 11, 9, 8, 7, 6 compared with those on run scores of 0, 1, 2, 3, 4, Result:

$$\chi^2 = 0.0276 \qquad P = .87$$

This chance result is conclusive.

In brief, the difference between the number of checks given on low scoring runs and the number given on high scoring runs is a triviality. (Per run: High, 6.81; Low, 6.86.) Thus so far as *runs* are concerned there is nothing in the theory at all.

In our papers we have presented a larger amount of numerical data than was strictly necessary. The justification was that if doubts arose in the thoughts of inquirers, they could compute other statistical results from the printed data. In the paper under discussion all the data necessary for the above calculations were quickly available from Table 12 and the facing page.

Relation of Checking Frequency to Subjects' Scores

The issue relating to the number of checks on high and low scoring *runs* having been disposed of, we now turn to the classification of high and low scoring *subjects*. Each person in the experiment did eight runs (Unknown). The question raised is: Were those who, in their eight runs, scored high more prodigal with their checks than those who scored low? According to the "fallacy" surmise, the answer is Yes. The facts are as follows:

In the context of the 34 subjects none of the PNC scores was particularly high or low; around an expected mean of 40, they varied from 31 to 52. Ten subjects scored above the mean, twenty below the mean, and four scored at the chance level. They were irregularly dispersed and do not lend themselves to the tidy "converging" comparisons which we have applied to the runs. However, for what it may be worth, we may compare the quantity of checks given by those whose PNC score was six or more points above chance with the number of checks given by subjects whose PNC scores were six or more points below chance. The result is:

$$\chi^2 = 0.0091 \qquad P = .93$$

This disagrees with the "theory." There is relatively no difference in the number of checks given by the highest and lowest scoring subjects. A more extensive test is to compare the checks of *all* sub-

jects whose PNC score was above the expected mean with the checks given by *all* subjects who scored below the mean. This gives:

$$\chi^2 = 2.06 \qquad P = .15$$

Thus there is no overloading of checks by high scorers or parsimony of checks on the part of low scorers.

Finally to complete this section, we considered the sixteen PNC score categories observed in our data, these ranging as stated from 31 hits to 52 hits (there were several blanks), and determined whether the number of checks varied between these classes. Analysis of variance provided:

Source of Variance	Degrees of Freedom	Sum of Squares	Mean Square	F
Between PNC Scores	15	2295.463	153.031	1.60
Within PNC Scores	18	1724.420	95.801	
		P = .17		

The probability of the computed value of F is .17, which being insignificant, leads to the conclusion that within the range of random variation, the checks are impartially dispersed through the subjects' PNC scores 31 to 52. The notion that high scoring subjects gave a disproportionately large number of checks and the low scorers disproportionately few is empty. Had there been a positive correlation between number of checks and size of PNC score, a different type of statistics would have been called for.

Relation of Checking Success to Run Scores

Now, having disposed of the problem with respect to the simple *amount* of checking on run scores and on subjects' total scores, we turn to a more interesting aspect of the whole matter. This will serve a double purpose by 1) giving further evidence in refutation of the "fallacy" speculation, and 2) giving evidence of the extensive nature of the *checking effect* in action.

We consider the actual success or failure of checks at all run score levels from 1 to 9. This is done by the matching method (see footnote 2, page 7 of the original report) by which it is easy to derive mean chance expectation, deviations from this, and the variance for each score level. The following table gives the necessary information:

Run Score	1	2	3	4	5	6	7	8	9	Total
No. Runs	11	21	38	49	53	45	27	20	6	270
Deviation from Expectation	+4.40	+6.40	+3.68	+11.92	+11.60	+37.36	+22.32	+1.92	+3.68	+103.28

It will be noted that the checking effect is positive at every scoring level. The probability that nine results should all be in the same direction is .0039. Chi square derived from summing deviations and variances is 13.34 ($P = .00026$) which is in close agreement with that found from the 2 x 2 Table 6 in the report ($\chi^2 = 13.13$).[1]

Since it was previously found that the *number of checks* approximates to equality for the nine scoring levels, it is appropriate to inquire whether the "strength" of the *checking effect* is greater at some scoring levels than at others. The answer to this question is obtained by first of all 1) computing χ^2 for each of the nine score levels and summing them, and then 2) computing χ^2 for the total. For the former, $\chi^2 = 19.71$ with 9 degrees of freedom. For the latter, $\chi^2 = 13.34$, as previously found. Subtracting the latter from the former, we get $\chi^2 = 6.37$. There are 8 degrees of freedom and $P = .61$ showing that, so far as these data go, there is no reason to suppose that checking is more effective at one scoring level than at another; and that the claim in the "Examination" that "All the evidence of this kind presented by the authors must therefore be dismissed as fallacious" is baseless.

A curious contrast to the above results is obtained when we apply the same methods to Examples A and B of the "Examination." The results are:

Example	Run Score Average	Checks: Deviation from Expectation	χ^2	Degrees of Freedom
A	5	0	0	1
B	10	0	0	1
Sum		0	0	2

The χ^2 for the two tables combined in one (A+B, not given in the above table) is small (0.45), but it precludes taking the analysis any further, since subtraction of this χ^2 from the χ^2 of zero (at foot of the table) would give a *negative* χ^2, which is absurd since χ^2 is essentially positive. As the table stands, the results are shown to be consistent only in their nullity, and being frankly acknowledged "fictitious" seem to have no relevance to the experimental realities.

The chief criticism being disposed of, the others being subsidiary to the main statistical result are now of less significance. With regard

[1] Table 6 in the report contains two more runs than the above analysis because in the latter the one run with zero PNC score and the one run with zero checks had to be omitted. Such data are not susceptible to individual computation since they involve multiplication by zero.

to the Incomplete Beta-Function, there is an error in our paper and a misconception in the "Examination." A word or two about the I.B.F. table (for short). This is no ordinary table, for it runs to 430 pages and some one hundred thousand entries arrayed in about two thousand five hundred columns. By malign (but fortunately not fatal) influence, our collective finger appears to have waywardly side-slipped among the mass of figures and provided P = .012, whereas the proper entry gives P = .0288. We are glad to make this correction and express our thanks.

When, however, the "Examination" notes as "unfortunate" the choice of the I.B.F. as a source of binomial probabilities, it is sufficient to mention that that view is in sharp conflict with the writings of Professors Karl Pearson, E. S. Pearson, M. G. Kendall, and Sir Ronald Fisher.

The I.B.F. gives probabilities to seven decimals. We have summed the binomial distribution and here give the two results for comparison:

	Probability
Binomial Distribution	.0288063
Incomplete B-Distribution	.0288063

The probabilities are identical and do not support the opinion that "the effect" of using the I.B.F. "is to overestimate the significance." We believe this fact ought to be stressed for it would be regrettable if new and inexperienced computers coming into psychical research were to be unnecessarily discouraged from using a most valuable and time-saving tool.

HISTORY OF THE SOCIETY

The First American Society for Psychical Research was formed in 1885, in consequence of a visit by Sir W. F. Barrett to this country, and Prof. Simon Newcomb became its President. In 1887 the Society invited a man of signal ability, Richard Hodgson, A.M., LL.D., sometime Lecturer in the University of Cambridge, to become its Executive Secretary, and he accepted.

This organization later became a branch of the English Society under the very able guidance of Dr. Hodgson until his death in 1905. The American Society for Psychical Research was then re-established with James H. Hyslop, Ph.D., formerly Professor of Logic and Ethics in Columbia University, as its Secretary and Director.

THE ENDOWMENT

The American Society for Psychical Research, Inc., was originally incorporated under the Laws of New York in 1904 under the name of American Institute for Scientific Research, for the purpose of carrying on and endowing investigation in the fields of psychical research and psychotherapeutics. It is supported by contributions from its members and a small endowment fund. The income of the Society pays only for the publications and office expenses, but does not enable the Society to carry on its scientific investigations. A much greater fund is required before this work can be carried forward with the initiative and energy which its importance deserves.

The endowment funds are dedicated strictly to the uses set forth in the deed of gift and are under control of the Board of Trustees, the character and qualifications of whom are safeguarded, as with other scientific institutions.

Moneys and property dedicated by will or gift to the purposes of the American Society for Psychical Research, Inc., whether to the uses of psychical research or psychotherapeutics, are earnestly solicited. The form which such dedication should take when made by will is indicated in the following:

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THE JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH

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THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH, Inc.
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PURPOSE AND SCOPE OF THE SOCIETY

1. The investigation of claims of telepathy, clairvoyance, veridical hallucinations and dreams, psychometry, precognition, dowsing, and other forms of supernormal cognition; of claims of supernormal physical phenomena, such as raps, telekinesis, materialization, levitation, fire-immunity, poltergeists; the study of automatic writing, trance speech, hypnotism, alterations of personality, and other subconscious processes; in short, all types of the phenomena called psychic, mediumistic, supernormal, parapsychological and metapsychic.

2. The collection, classification, study and publication of reports dealing with the above phenomena. Readers are asked to report incidents and cases. Names must be given, but on request will be treated as confidential.

3. The maintenance of a library on psychical research and related subjects. Contributions of books and periodical files will be welcomed.

4. Cooperating in the above tasks with qualified individuals and groups who will report their work to the Society.

MEMBERSHIP IN THE SOCIETY

Members, who receive the Proceedings and the Journal, pay an annual fee of $10. (One may become a Life Member or endow a Memorial Membership on payment of $200.) *Fellows,* who receive all publications of the Society and are afforded special library privileges pay an annual fee of $25. (Life Fellowship, $500.) *Patrons and Founders:* For those who wish to make a still larger contribution to the Society's work, these classes are open at $1000 and $5000, respectively.

It is to be remembered that membership in a scientific society means more than merely a subscription to its publications. The work must be carried on largely through the income from membership fees. Therefore members, old and new, are urged to make their membership class as high as they feel they can. *If a comparatively small proportion of the present members went one class higher, the money available for research would be more than doubled.*

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Fiftieth Anniversary Celebration

The Fiftieth Anniversary Celebration of the Society, as announced in the January JOURNAL, was held on the evening of March 2nd in the George Washington Hall of the National Republican Club for Women. An overflow attendance of members and guests participated in the occasion. A good representation of prominent psychical researchers came to New York from distant cities to join in the festivities.

Dr. George H. Hyslop, President of the Society, presided at the meeting. Addresses were made by Professor C. J. Ducasse of the Philosophy Department, Brown University, Dr. Waldemar Kaempffert, Science Editor of *The New York Times*, Dr. Gardner Murphy, Chairman of the Society's Research Committee, and Miss Gertrude Ogden Tubby, Professor Hyslop's Secretary. All the speakers paid fitting tribute to the Founder of the Society, James Hervey Hyslop.

Messages of congratulation and good wishes for the future were received and read by Dr. Hyslop from the Council of the Society for Psychical Research and Mr. and Mrs. W. H. Salter, London; Mr. G. Zorab, in behalf of the Council of the Dutch Society for Psychical Research, The Hague, Holland; Mr. H. Addington Bruce, an old

friend of Professor Hyslop's and a former Trustee of the Society; Dr. J. B. Rhine, Parapsychology Laboratory, Duke University; and Miss Beatrice Hyslop, daughter of the Founder, now in Paris.

The meeting was preceded by a broadcast in the afternoon on which Mrs. Alma Dettinger interviewed Professor Ducasse and Dr. Murphy on the meaning and importance of psychical research.

Annual Meeting of Voting Members

The Annual Meeting of the Voting Members of the American Society for Psychical Research, Inc., was held on January 31. 1956, at the Rooms of the Society. The President, Dr. George H. Hyslop, presided at the meeting. Voting Members also present were: Mrs. E. W. Allison, Mr. L. C. Andrews, Mr. Edward N. Ganser, Dr. William A. Gardner, Mrs. Lea Hudson, Mrs. Lawrence Jacob, Dr. S David Kahn, Mr. Gerald L. Kaufman, Mr. Alan F. MacRobert, Dr. Russell G. MacRobert, Miss Margaret Naumburg, and Mrs. Henry W. Warner.

The following Trustees of the Society whose terms of office had expired were re-elected for another term of three years: Dr. George H. Hyslop, Dr. Gardner Murphy, and Dr. Montague Ullman. Mr. Richard DuBois and Dr. Robert W. Laidlaw were elected Trustees.

The Annual Meeting was followed by a meeting of the Board of Trustees for the election of officers for the year 1956. The following officers of the Society were re-elected: President, Dr. George H. Hyslop; First Vice-President, Dr. Gardner Murphy; Treasurer, Mr. Gerald L. Kaufman; Secretary and Assistant Treasurer, Mrs. E. W. Allison. Professor C. J. Ducasse was elected Second Vice-President.

Committees for 1956

The President has appointed the Chairmen of Standing Committees to serve for the year 1956 with power to select the other members of their respective committees.

Research Committee: Dr. Gardner Murphy, Chairman; Mrs. E. W. Allison, Mrs. L. A. Dale, Dr. Jan Ehrenwald, Dr. Jule Eisenbud, Dr. S David Kahn, Dr. E. J. Kempf, Dr. R. A. McConnell, Dr. Montague Ullman, and Dr. J. L. Woodruff.

Finance Committee: Mr. Gerald L. Kaufman, Chairman; Mr. Benson B. Sloan, and Mr. Harold W. Updike.

Publications Committee: Mrs. E. W. Allison, Chairman; Mrs. L. A. Dale, Professor C. J. Ducasse, Dr. R. A. McConnell, Mrs. E. de P. Matthews, Dr. Gardner Murphy, Dr. J. B. Rhine, and Dr. Gertrude R. Schmeidler.

Precognition: An Analysis[1]

W. E. COX

Introduction

In the realm of psi phenomena, precognition has been one of the most difficult to understand and to integrate with other capacities. The search for a more incisive approach toward knowledge of its fundamental principle could well include at this time a fresh examination of the evidence of several varieties of spontaneous precognition.

Past efforts have dealt mostly with (1) the collecting of reports of premonitions according to the *manners* in which they were perceived (p. 57, nn. 23, 24), and (2) the laboratory variety of controlled, statistical treatment. In the present survey the objective is to point out new avenues of approach toward a better understanding of this enigmatical class of psi function and of the part it may be playing in our everyday lives.

Examination of the basic phenomenon will be considered under the following subheads: I., General Precognition: A., Trivial, B., Beneficial, C., Detrimental; and II., Subliminal Precognition.

Section 1. General Precognition

(*Section II, "Subliminal Precognition," will appear in a later number of the* JOURNAL.)

This consideration of precognition might be prefaced with the question of just how valuable are precognitive experiences. The scope of this writer's answer to such question, and (in Section II) a statistical search for fresh evidence of precognition, account for the arbitrary subdivisions of this paper. A new approach to theoretical considerations is also presented. The definition of precognition will be restricted to include only those experiences which are clearly unlikely to be due to clairvoyance (or telepathy).

A. Trivial Precognition

The trivial variety of precognition, wherein some puerile event comprises the whole of it, is often consciously experienced at just the moment of its fulfillment. The greater portion of each in this popular class is likely to unfold only a few seconds before its confirmation in fact, but quite often sufficiently prior thereto to enable the percipient to note—as the fulfillment transpires—that it is indeed

[1] Appreciation is expressed for constructive suggestions on this paper by Drs. Hornell Hart, Gardner Murphy, R. H. Thouless, C. J. Ducasse, and J. B. Rhine. Responsibility for the opinions expressed, however, rests with the author.

precognition.[2] From this form of the phenomenon there can be posited a subconscious occurrence of the precognition at some earlier hour or day, perhaps in the fleeting dream-state. Just as many other dreams are forgotten when we awaken, so could precognitive dreams be lost in the subliminal until the real events begin to yield a conscious conviction of the precognition.

There has, as yet, been offered no suitable way of measuring such varieties of precognition or clairvoyance as do *not* cross over the threshold of consciousness, other than by means of conventional ESP tests and the like.[3] Nevertheless, the nature of precognition provides a gateway, subsequently to be discussed, through which may be admitted the fact that psi may occur much more often than we realize.

The frequency with which precognition occurs may exceed considerably the portion that is consciously realized from (1) the moment of incipience, or from (2) some subsequent moment when fulfillment unfolds. There is no theoretical reason why a precognition should not occur without any conscious realization at all as such, directly or indirectly. As an example, A. could dream that X would happen to B., and the precognition could be fulfilled without A.'s ever knowing of it. A.'s subsequent direct knowledge of the event is no requirement; and it would make little difference whether or not A. consciously can recall the details of the dream next morning or at any future time. Such a case could be one wherein the percipient dreams of the name of a winner in a certain horse-race, and, perhaps remembering the dream but being disinterested and not of a betting temperament, never learns of the accurate fulfillment of his unrecognizedly-precognitive dream when this horse subsequently runs and wins. One of the cases reported by Myers is of this sort, involving a soldier's precognitive dream of a Civil War sequence which was accurately fulfilled only after his own death (itself included in the prophecy).[4]

This example illustrates the dreamer's ability precognitively to perceive (or, more precisely, to prehend a present image of) future events themselves rather than to perceive *quoad hoc* his ultimate sensory knowledge of the same. That it was correctly precognized is a fact which does not stand or fall upon the percipient's own

[2] An alternate hypothesis, of course, is that the subject might perceive telepathically, from the mental intentions of the "agent," knowledge of the latter's immediately-subsequent actions, and at once misconstrue it as precognition; but this would not hold true in most cases involving only inorganic or otherwise unpremeditated actions.

[3] And also in the manner to be introduced under "Subliminal Precognition."

[4] *Proc.* S.P.R., Vol. XI, 1895, pp. 582-585.

ultimate knowledge of its accuracy—or any other person's.[5] Similar examples may be found in certain prophecies, such as the long-term ones of Nostradamus, etc.[6]

In this paper, the term "realized" precognitions should be taken to mean those of which the percipients personally observed fulfillment, whether consciously remembering them from incipience or failing to recall them until the beginning of their fulfillment. "Realized" is accordingly synonymous with "subsequently realized." The term "unrealized precognition" may be given a nominal place in this paper, and would simply connote such precognitions as occur (either consciously, but without subsequent recognition, or subconsciously), but which are never confirmed to have been such. The theoretical horse-race example would come under such a classification.

B. Beneficial Precognition

Precognitive experiences which are of simple and conscious benefit to the percipient need not be given particular attention. This discussion will be confined, instead, to more complex varieties—not to becloud our meager understanding of precognition's operation, but rather to seek out ways of doing the opposite.

In some precognitions which may be placed in the "beneficial" category, not only has the "time element" occasionally been misinterpreted (*e.g.*, in selected "warning" cases where resultant anxiety is so immediate as to provoke premature precautions), but there have been precognitions of an unpleasant experience which, although perceived a good number of times in advance of it, at the last fail to "come to the rescue." Hyslop reports one such example[7] (here abbreviated) :—

Mrs. D. experienced a vague but powerful impression that an unusual "burden" would befall her family. The premonition frequently was repeated, becoming even more intense and tending to indicate involvement of her small child. At odd times when planning something for the child's future, she heard an internal voice say, "She'll never need it," and the like. About a week before the

[5] Compare laboratory tests for precognition, wherein subjects often are never informed of the significant quantities of their hits which a correlation of the specially-shuffled ESP decks later may reveal. And if, let us suppose, all the shuffled decks were somehow to disappear before their correlation, this would not affect the matter of their having contained perhaps a highly significant total of correctly foreseen card orders. It is axiomatic that vanished or disarranged cards (and, to be sure, the soldier's *not* having informed any of his associates—*if* that had been in fact the case) prevent empirical confirmation of such precognitions.

[6] H. J. Forman, *The Story of Prophecy*, Tudor Publishing Co., New York, 1940.

[7] *Proc.* S.P.R., Vol. XIV, 1898-1899, pp. 266-270.

catastrophe the mother thought she smelt fire at night so she began to be very cautious about matches within reach. Then one day as she took the child to its crib for a nap, the voice warned "Turn the mattress." She, in her haste, did not heed this brief warning, and a little while later flames enveloped the crib and child.

The enigma in this case is why the voice did not urge her more strongly to take action at the last such as by adding "now!" or some other explanation—and without ever having maintained that its worrisome prevision would be consummated.

In another category we of course find those occasional premonitions of disasters that are "nipped in the bud," prior to their would-be horrible terminations, as a direct result of precognition.[8] In the cases of Mrs. D. and Lady Z. we have a relative paradox within precognition itself: in the former, a futile urge to take action about a fixed and "inevitable" future; and in the latter a "conditional future," one not inevitably fixed but rather precognized in "whole" and thwarted in part.

Two other short considerations may be presented concerning the "indirectness" of the operational procedures of precognitive phenomena. The first is illustrated in the following case reported by Dr. L. E. Rhine.[9]

> I dreamed I was down town on Main Street and stopped to look in the window of a dress shop. Just inside the shop an old school chum of mine came out dressed in a black coat. I said, "I am sorry to hear about your mother's death, Helen." A few weeks later Helen's mother did die, and one day as I was down town in the same spot of my dream the whole thing happened exactly as dreamed.

Here the dream only casually alluded to an important occurrence, which, indeed, might well have been remembered and related to her chum.

Secondly, "indirectness" in precognition is illustrated in quite another—indeed a more popular divergence—by the occasional experience of precedented symbolic dreams, wherein, *e.g.*, one "usually dreams of" teeth, of bathing, or a Celt of "death lights," etc., shortly before the death of a relative or friend. There is, too, the occasional disparity between the location or the appurtenances in a precognitive dream and these details in its fulfillment; but this need not par-

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[8] E.g., "Lady Z. and the Coachman," *Proc.* S.P.R., Vol. XI, 1895, p. 497; see also cases in Vol. VIII, pp. 391 and 401-402.

[9] L. E. Rhine, "Frequency of Types of Experience in Spontaneous Precognition," *Journal of Parapsychology*, Vol. 18, 1954, p. 102.

ticularly concern us, for reasons expressed in what Tyrrell aptly has termed "artist's license."[10]

There are other precognitions of specific benefit, which either may be subliminal (or otherwise unrecallable) or, if consciously recalled as a former dream or waking impression, may never be realized actually to have been precognitive. If I were to dream of my car in an accident, and as a result of dwelling on it delay an impending journey for even a moment, or if purely intuitively I should on occasion alter my speed, this quite conceivably might avert what would have become the identical accident "so precognized." There is fairly strong "parallel" evidence for this contention and likewise against it; and, although the recognizable evidence is mainly in its favor, we must acknowledge the possibility of both (see Section II and Detrimental Precognition).

The task of interrelating the divergencies of such precognitive experiences as are herein considered is a difficult one, especially when we include the "detrimental" variety illustrated below. It would seem, then, that we must find out as much as we can about the full ***nature*** of foreknowledge before trying in renewed earnest to discover its principle.

There is another aspect of precognition which may be given brief mention: the problem whether this faculty is not also the root-cause of those rare capacities of mathematical prodigies. Can Myers' "manifestation of the indwelling computative power"[11] (which prevails in the very young), however literate or illiterate the prodigy, be reduced to something *other* than precognition (or clairvoyance)? (*If* it could, would we not then have a new and unparsimonious postulate for what is accepted to be psi?) Precognition *per se* may not be the actual form of psi involved: we cannot safely rule out clairvoyance, for in a pure mathematical problem the answer, though not yet routinely determined, is of course inherent in the problem. Conceivably, at least, the psi function here posited by which the correct answer itself might be ascertained in advance of any computation, is no more *certainly* that of precognition than is, say, the capacity to guess correctly the number of beans in a jar before they are counted. The "limitations" of conventional psi capacities can

[10] G.N.M. Tyrrell, *The Personality of Man*, Penguin Books, Ltd., Middlesex, 1947, pp. 87-89.

[11] F. W. H. Myers, *Human Personality and Its Survival of Bodily Death*, Longmans, Green, New York (last edition, 1954) Vol. 1. pp. 79-85. Myers does not try to attribute such prodigious calculations to any form of psi, but rather to an "accidental adaptation" (p. 118), and he terms it as "merely a subliminal calculation . . . expressing itself supraliminally" (p. 87). One well may wonder what would result if a problem in calculus—or one in the binary or even a "heptary" numerical system, instead of only the denary—were presented (and, in addition, a test for precognition itself).

hardly be held to comprise adequate indication that neither precognition nor clairvoyance and the capacities of *prodigia mathematica*, however continuous the latter, are wholly unrelated.

The fundamental questions which man's diverse precognitive experiences pose—questions of whether precognition results from a "timeless subliminal consciousness," the "specious present," extra dimensions, or none of them—remain quite debatable, though these have been given considerable attention in the literature. But what the most adequate postulate ultimately might turn out to be is not the purpose of this analysis to consider.[12]

C. Detrimental Precognition

Among the enigmas of psychical research there is another variety of precognition, of much less than average familiarity; one which is an anomaly in itself. In it a consciously realized precognition of some undesirable eventuality is a cause of its own fulfillment. I do not refer to psychological "cases" which can be resolved to the exclusion of psi, but to cases where there were resultant efforts made to thwart the fulfillment, efforts which were not only of no avail but were *per se* a major contribution to the cause of the misfortune precognized.

There are a number of such cases, to which might be ascribed the title of "detrimental precognition." Because of their comparative scarcity, four (of nine known to this writer) are here related. The eventuality in three of these was death, and in one, trivia.

The first is taken from a biography of Robert Morris, noted American financier during the Revolutionary War and a framer of the Constitution.[13] His father, Robert, Sr., was an agent for a Liverpool shipping firm, and it was to him that the premonition occurred.

> It was customary, after the arrival of a ship from a foreign port, for the captain to . . . entertain the consignee and his friends, and as a compliment, to fire a salute at their departure from the vessel . . . The night before the gala event (aboard the *Liverpool*, at Oxford, Md.), however, Morris dreamt that, after a pleasant day on shipboard, he had received a fatal wound from the salute fired in his honor. Try as he would, he could not shake off the memory of this dream . . . All morning he brooded . . . and before noon had decided not to join the party. Calling Captain Mathews into his office, he very apologetically related his dream . . . and offered this as an excuse

[12] See Theoretical Discussion, pp. 56 ff., however.

[13] Eleanor Young, *Forgotten Patriot, Robert Morris*, Macmillan, New York, 1950, pp. 5-7. Quoted by permission. Original records of this experience are in a "Narrative of (Capt.) Jeremiah Banning," and a letter by Morris, Jr.'s daughter, in *Boogher's Repository*.

> for not attending the celebration that afternoon. The captain, indignant, rebuked Morris, [who] replied . . . "Call it superstition if you like, but our family is reputed to have the gift, or curse, of receiving premonitions of impending disaster."
>
> Mathews at last promised that no salute would be fired. Thereupon Morris reluctantly consented to attend the celebration . . . After the festivities he called Morris aside. "The crew don't take kindly to the arrangements, sir. They insist on firing the usual salute. You know a glass of grog is served every sailor immediately afterward."
>
> ". . . Very well," Robert Morris agreed, "but do not fire the salute until I or someone else gives the signal."
>
> "I will accompany you to shore, sir, and give the signal myself when we are far enough away." Captain Mathews thereupon instructed the gunner not to fire until he raised his hand . . .
>
> . . . A fly lodged on the captain's nose before the boat had rowed clear of the gun range. Thoughtlessly Mathews, forgetting for the moment his responsibility, raised his hand to brush the fly away. Seeing the motion and considering this the signal, the gunner fired the salute. The next moment Morris felt a sharp pain in his upper arm . . . The wadding from one of the guns . . . [had struck] his arm above the elbow, breaking the bone, and lodging in the flesh . . . No physician was available . . . [and] a few days afterward mortification set in.

Morris died July 12, 1750, and is buried at White Marsh churchyard, Oxford.[14] In this case, no alternative to precognition seems to exist.

The case to follow involves a comparatively trivial "detriment." It was first reported by Myers[15] in the words of the percipient, Mrs. C.

> I have an intense horror of monkeys—I seldom look at one if I can help it . . . I dreamed that I was persistently followed by one such as I had never seen before, but which terrified me extremely, and from which I could not escape. . . . I mentioned it to my family, and my husband recommended a short walk. In consequence, and *quite contrary to my custom,* I arranged to take my children for a short walk, without their nurse accompanying me, and as their favorite walk was up Nightingale-lane . . . past . . . the high walls of Argyll Lodge, the residence of the Duke of Argyll, I agreed to take them there, and when we arrived . . . what was my horror to see on the roof of the coach-house the very monkey of my dreams! In my surprise and terror, I clasped my hands and exclaimed, much to the

[14] Morris' epitaph, which still stands (restored in 1898), is aptly worded *in re* this incident.

[15] *Proc.* S.P.R., Vol. XI, 1895, pp. 488-489.

amazement of a coachman waiting outside, "My dream! My dream!"

This I suppose attracted the attention of the monkey and he began to come after us, he on the top of the wall, we beneath, every minute I expecting he would jump upon me, and having precisely the same terror I experienced in my dream. . . . We could not go fast, which added to my distress, but we succeeded in escaping it, and on my return home I sent a servant to enquire if a monkey had been seen there, for my state of nervousness was extreme. She was informed that that morning a rare and very valuable monkey belonging to the Duchess had got loose, and so the incident was explained. But my dreaming of it previously remains unexplained.

An explanation of this case in terms other than the precognitive would require a complicated set of less defensible hypotheses. Mrs. C. could well have perceived clairvoyantly, *e.g.*, that at that moment a monkey was at large in the community; and, being horrified, awakened and so decided herself to exercise the children. But clairvoyance surely played no part in their directing and her timing the sojurn by the Duke's high walls during the monkey's stance upon a perch visible beyond. On the contrary, such a variety of clairvoyance should have caused her to avoid those walls, and this psi experience then would never have reached total fulfillment, or—more particularly—the annals of psychical research.

The third case, entitled "The Locksmith's Apprentice," is reported by Owen,[16] who states that it occurred (*circa* 1859) "in Hamburg [and] was given at the time in the newspapers of the day."

The apprentice of a . . . locksmith . . . Claude Soller, one day informed his master that the night before he had dreamed that he had been murdered on the road between Hamburg and Bergedorf. His master laughingly told him that he had just then 140 Rix-dollars to send to his brother-in-law in Bergedorf; and, to prove to him how ridiculous it was to believe in such omens, he . . . should be the bearer of it. The young man, after vainly remonstrating, was compelled to set out . . . Arrived half-way, at the village of Billwaerder . . . he called upon the baillie . . . related . . . his dream . . . and begged that some one might be allowed to accompany him through a small wood that lay in his way. The baillie, smiling at his fears, bade one of the workmen go with him as he desired. The next day the body of the apprentice was found, his throat cut . . . The man who accompanied him . . . was apprehended, confessed his crime, and declared that it was the recital of the dream which had prompted him to its commission.

[16] Robert Dale Owen, *Footfalls on the Boundary of Another World*, Lippincott, Philadelphia, 1875 (also 1859), pp. 145-146.

In commenting on this class, Owen states that of these (two cases are given) "we may dispose, preliminarily, of one class as evidently susceptible of simple and natural explanation." He is referring to their bringing about their own fulfillment.[17] But strict examination does not reveal such explanation to be adequate; for even if the probability of the workman's being thievish is presumed (learning as he did of the money), it will be recalled that the original dream was that of murder between Hamburg and Bergedorf, and not of money or his master.

Another case of this type is reported (without corroborating evidence) by Crowe.[18]

> A Scotsman dreamt of people removing a body from a lake, and that he upon approaching perceived that it was himself. Alarmed, he resolved never to venture on that lake again. Later it became indispensable that he do so; and as the day was calm he yielded, on condition that he be put ashore at once on the opposite side whilst the rest of his party proceeded to their destination where he would meet them. This done, amidst their derision, he was standing on the bank [at destination] as they neared. But the little promontory supporting him had been undermined by the water; it gave way beneath, and life was extinct before he could be rescued . . .

The possibility of accounting for this case through coincidence is weakened by the dream's including a group of people specifically retrieving the body, with which the fulfillment was in accordance.

The comparative rarity of the "detrimental"—and the "non-beneficial"—types of precognitive experiences makes all the more necessary their full conformity to the given definition of the category in which each is here placed. Strict search accordingly was made against their justifiably being other than "irreducibly prepresentative" precognitions. To attempt further to reduce this detrimental type to the same category or "order" of precognition as the more popular varieties would be to predicate the same upon the hypothesis that each precognition, and a fulfillment of it, would have occurred anyway—even if after the precognition no express effort had been made to thwart fulfillment. To make this interpretation, however, is to beg the entire question of precognition. But is it correct *ad hoc* to accept precognition as being basically ***responsible*** for the very misfortune which, in these cases, it was "designed" to prevent? In this there

[17] *Cf.* H. F. Saltmarsh, *Foreknowledge*, G. Bell & Sons, London, 1938, p. 36n. He mentions several wherein the precognition "helped bring about" that which was anticipated, and ascribes them to autosuggestion. They involve states of health, however, and accordingly might well be purely psychosomatic.

[18] Catherine Crowe, *The Night Side of Nature*, G. Routledge & Co., London, 1854, p. 41 (3rd edition).

can be no certainty, as we cannot safely take for granted the apparent condition that these "precognitive episodes were deliberately designed for specific purposes, any more than any other natural event should be so interpreted"; therefore the detrimental type of precognition "deserves much further exploration, for the whole problem of causation as related to precognition is a fundamentally important one if we are going to clear up the philosophical aspects of psi."[19]

An appreciable number of European and Asiatic folk tales are of similar motif, in which someone's death is prophesied in detail and preceded by unsuccessful effort to prevent it; and it is a feature of certain classical tragedies, such as the Oedipus cycle. The preventive acts (whether or not as "prophesied") seem generally to have been such as did not in themselves contribute to the cause of the deaths; but they may have been evoked by actual cases fairly analogous to the examples under discussion. Herodotus also relates an appropriate example, one involving King Croesus and the death of his son.[20]

The detrimental form can be but little more incongruous than is precognition alone. Though in the latter, knowledge of the effect appears to precede its cause, it is worthy of note that in the detrimental variety the cause seems partially to precede the effect. In the case of Mrs. C., had she not distinctly exclaimed, "My dream! My dream!" the monkey might well not have been led to follow her. Thus it becomes an interesting conjecture as to whether her exclamation was likewise "foreordained" (it did not comprise part of the dream, nor could it conceivably), or whether the monkey would have spied and pursued her anyway.

Theoretical Discussion

We turn now to a short consideration of the specific value which the foregoing classifications of precognitive experiences collectively may have. The opinion of this writer is that they can be of relative value to the research, and for at least one basic reason: *viz.*, a need for a fresh classification of precognitive cases, one which is based on their inherent natures instead of upon only the comparatively superficial effects which comprise their rise to conscious registration.

The painstaking efforts required in order to progress much further toward the discovery of the *modus operandi* of precognition might stand to benefit from such treatment. The speculative theories which past efforts have educed, as mentioned by name only at the end of the preceding section of this paper, appear each to have reached an

[19] Quotations are from a letter from Hornell Hart to the author.
[20] Herod., Bk. 1, c. 34-45.

"impasse," if I may presume to use that term. There will be lent to the effort, then, through the classification here offered, a platform which may render appreciable aid in an ultimate extrapolation of a unified theory for precognition. Space does not permit of a specific attempt at a theoretical development of these observations, but only in drawing attention to their potential *a posteriori* value to the precognition problem.

The appropriate categories, or types, would be the following:

1.	Beneficial[21]	
	a. Directly	(cases of simple, conscious benefit; no example given.)
	b. Indirectly	(*e.g.*, dream of informing chum *re* regretting a death, p. 50.)
2.	Non-beneficial[22]	(*e.g.*, "She'll never need it" case, pp. 49-50.)
3.	Trivial	(no example needed.)
4.	Detrimental	(*e.g.*, Robert Morris case, *et al.*, pp. 52-53.)

Most of the classifications which already have been made illustrate primarily the *paths* through which precognition reaches the level of consciousness, and the psychological conditions, etc., which are most conducive to its occurrence; but they leave uncharted the *inherent* differences which are pointed out in this discussion. The former have been assimilated fairly adequately by L. E. Rhine,[23] Saltmarsh[24] and others, and are of course of great value as far as they go. But the factors they stress are the devious *modes* in which the contents are *presented* through psi to the conscious. The factors which are stressed in the foregoing categorization, however, are wholly of "purpose-and-result" transitional orders. There may be other categories of similar value; for example the "directly beneficial" (1,a) might be divided into "positive" and "negative," so to speak, of which the latter would refer to all cases that are either wholly aversive or of the "nipped in the bud" variety (see p. 50).

The issue of "time" may itself be of small consequence: since an irreducible precognition is distinctly on the other side of the "time

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[21] This would include both the "General" and "Subliminal" (see Section II) varieties. An "Indirect" example of the latter, at the conscious level, is the Stevick case.

[22] Such "non-beneficial" cases may be few enough to justify doubt as to whether they represent a genuine type.

[23] *Op. cit.*, pp. 93-123. Her precognitive classifications are the following: Intuitive, Hallucinatory, Unrealistic- and Realistic-dreaming; also "Conviction" and "Non-conviction."

[24] *Op. cit.*, p. 15. His classifications are as follows: Dreams, Borderland, Impression, Hallucination, Mediumistic, and Crystal Vision.

line," its distance beyond it is secondary. There is, in fact, little evidence that the comparative rarity of known cases involving from one to many weeks' (and years') foreknowledge is due to anything other than (1) our natural tendency to forget their content much more frequently than that of "shorter ones," and (2) a conceivably longer time during which complete "psi-aversion" might come to pass.[25]

The "limiting aspects" illustrated by two of the above categories, the Non-beneficial and Indirectly beneficial precognitions, appear not to have been accorded as much importance as they *may* turn out to deserve in the study of precognitive dynamics. Even our understanding of telepathy and clairvoyance might likewise benefit from a similar categorization (*viz.*, Trivial *vs.* Beneficial). Through the widened scope of the "comparative anatomy" which a categorization of the different types would seem to afford, further progress in the valued efforts to attain the underlying objective of this quest might the sooner be achieved.

[25] Some theoretical implications of this category are summarized in the concluding paragraphs of a paper entitled "The PK Mechanism," by C. B. Nash, *Journal* S.P.R., Vol. 38, 1955, pp. 8-11.

The Significance of Gestalt Psychology for the Problem of Immortality[1]

JAMES C. CRUMBAUGH

It is usually assumed that the concept of survival requires a belief in dualism of mind and body. The present paper describes a point of view, drawn largely from the implications of *Gestalt* psychology, wherein the concept of both immortality and physical monism could be accepted. So far only Wheeler[2] and Murphy[3] appear to have seen these relationships, and neither has developed them from the present point of view.

It is not difficult for the popular mind to assume the truth of the concept of immortality, for most people unconsciously adopt the belief in the dualism of mind and body which has typified the everyday thinking of almost all of the world's culture patterns from earliest times to the present. We find this dualism clearly defined in Plato, adopted by the Apostle Paul, and integrated into Christian theology, and later restated in the modern period of philosophical thought in Cartesian philosophy. But modern psychology, growing for the most part out of the British Empiricist philosophies of Locke, Hartley, Hume, Berkeley, and Bain, and becoming a separate discipline under the leadership of the great nineteenth-century German physiologist, Wilhelm Wundt, became increasingly skeptical of this dualism. As some wag has put it, "Descartes separated mind and body, and psychologists have been trying ever since to put the two back together again."

The usual position on the mind-body problem among the early psychologists—Wundt, his pupil Titchener and their successors—became that of psychophysical parallelism. This view, said to have been originated by Bain but clearly apparent in the work of Leibnitz, holds that mind and body run parallel, being separate but correlated in activity. The interpretation made by psychologists, however, became something like Spinoza's "double-aspect" theory: mind and body were two aspects of the same thing, rather than two separate things, just as a lens may appear concave from one side and convex

[1] I am deeply indebted to Professor C. J. Ducasse of Brown University for a critical reading of this paper and for very cogent suggestions concerning treatment of some of the major points herein.

[2] Raymond Wheeler, "Organismic versus Mechanistic Logic," *Psychological Review*, Vol. 42, 1935, pp. 335-352.

[3] Gardner Murphy, "Field Theory and Survival," JOURNAL A.S.P.R., Vol. XXXIX, 1945, pp. 181-209.

from the other, still being the same lens. With the advent of Watsonian behaviorism in psychology, one of these aspects disappeared: "Mind" became simply an abstraction for the functioning of the physical organism as it adjusts to its environment—in short, mind was synonymous with behavior, and conscious experience no longer existed.

The Watsonian era has now passed, but it left deep marks upon American psychology—deeper, in fact, than most present-day psychologists seem to realize. While conscious experience has crept back into psychological thought, mind is still a functional abstraction and does not exist apart from the physical organism. The "psychophysical axiom," one of the fundamental assumptions of modern psychology, states that all psychological processes have a physical counterpart in the biological nature of the organism. For example, when you think, some neurophysiological process is taking place in the brain. This view is consistent with both psychophysical parallelism and extreme behaviorism, although genuine parallelism would hold that the neurological process is not the same as—but merely correlates with—the mental process, whereas behaviorism would identify the two. The latter view has become the most widely accepted concept among American psychologists. As a result they have seldom subscribed to the possibility of survival, for they have seen no way in which mental processes could exist apart from the physical organism.

One system of psychology, however, offers concepts which make possible the acceptance of both survival and physical monism. This is *Gestalt* psychology, originating in Germany around 1912 with the work of Wertheimer, Koffka, and Köhler, and spreading thereafter to this country. While this system has nothing officially to say of survival concepts, one of its exponents, Wheeler,[4] has seen that its data can be interpreted in terms which harmonize with them. The leading proponents of *Gestalt* psychology have bent over backwards to avoid any metaphysical coloring of their system because in its early years it was accused by the behaviorists of being based upon speculative and somewhat subjective concepts that were difficult of experimental verification. But while the *Gestaltists* evaded the metaphysical implications of their concepts, we may make use of these in our search for a common ground between psychology and metaphysics.

Let us briefly examine the principal tenets of the *Gestalt* system. First and foremost is the general approach to the study of mental phenomena that is implied by the name *Gestalt*, which in German

[4] Wheeler, *op. cit.*

means "whole," "pattern," "form," "configuration," or "total arrangement." Mental processes are to be studied as a unit or totality, not analyzed or dissected into their elements or component parts. Analysis may be of value at times, but it is only by consideration of the whole that we understand the meaning of the mental process. The four sides of a square have no meaning of squareness when dissected and observed separately; they constitute a square only by being brought together in the relationships necessary to yield a unitary whole which is perceived as a square. Thus the true meaning is observed only in the relationships, not in the discrete elements which form these relationships. And so it is with all aspects of life—moral, spiritual, aesthetic, intellectual, physical; the meaning is to be found only in the totality of relationships within each of these areas.

The *Gestalt* psychologists believe fundamentally in the interpretation of psychological events from the standpoint of the total pattern of relationships which constitute these events. They believe we perceive and learn by this principle, which they have called the law of *Prägnanz* or *filledness*. They speak of a perceptual or learning "field," which implies that mental activities are not made up of a series of discrete acts but rather of a total unitary complex or field of mental relationships. The key to understanding this idea lies in the term "organization." A psychological field is organized; all elements of the field are related in such a way as to form a total pattern.

This principle has a number of corollaries which we shall not deal with here, but there is one derived concept which is of fundamental importance for our purposes. This is the concept of "transposition," which holds that the total complex of relationships in a given field can be reproduced in a different medium composed of entirely different elements, and that the psychological *Gestalt* or meaning will remain the same. For example, we may play "Auld Lang Syne" on the piano in the key of C in the octave below middle C. Then we may play the same tune in the key of F, in the octave above middle C. The second rendition will be immediately recognized by anyone who is familiar with the first, yet the actual elements (the tones) that composed the first have been entirely changed in the second. What have the two in common? It is obviously the total pattern of relationships, the total field. If we are asked to name the tune in each case, we promptly respond that both are "Auld Lang Syne." We detect a difference in the elements, but the tune remains the same. The elements constitute the medium of expression, but the psychological experience of "Auld Lang Syne" is in the relationships between the elements, not in the elements themselves. This experience can be reproduced in many different media. We can record the tune on a phonograph record or on motion picture film

or on magnetic tape. The recording media may be so different as to make it hard to see how each could express the same musical relationships, but these different media can reproduce the same tune so accurately that it would be impossible to tell by listening to it whether it was recorded on tape or in wax. We may destroy a particular medium, for example the wax record upon which the tune is impressed; but this does not destroy the tune. It can be reproduced in another medium by the influence of some second medium in which it has been recorded at the same time. If, for instance, both a tape and a wax recording were originally made of the tune, upon destruction of the wax record the tape could be used to reproduce the tune in more wax.

Now let us draw an analogy between this tune and human personality. The conscious awareness and past experience of a given individual may be compared to the melody on a record; the physical organism is analogous to the wax record. If the physical organism should be destroyed, the organization of the conscious awareness and past experience which constitute the personality—if this organization should exist in another medium isomorphic to the first—could still continue to function. This second physical medium could be either the same type as, or of a different type than, the original. In other words, the isomorphic personality might constitute another biological organism such as we now know, or some entirely different type of organism of which we have not yet conceived. All that is required is that the organic medium be capable of retaining and expressing the same relationships which exist in the personality of a given individual. The idea here is very similar to that of the so-called "etheric double" which has sometimes been proposed.

In the metamorphosis of insects we see a change from one physical form to an entirely different physical form, although the individual presumably retains a continuity of psychic experience, albeit on a very primitive level. The experience changes in the adult stage, to be sure, correlating with the changes in the physical organism—*id est*, crawling experiences are replaced by flying experiences, and so forth. We do not know whether memory of the previous stage is retained in the later stages, but we can conceive of dynamically changing organisms in which this is so.

Even in an identical medium the dynamic changes may be as great. Long ago Ernst Mach[5] pointed out that human personalities change so much from one period in life to another as to be unrecognizably different in these different periods. A person may, in fact, be more

[5] Ernst Mach, *The Analysis of Sensations* (translated by C. M. Williams), The Open Court Publishing Co., Chicago, 1914.

different from his own personality in childhood than from another individual's personality in the adult stage. The only common thread between successive stages of an individual is to be found in his memory for a previous stage.

The first law of the universe is change. Heraclitus saw this: **τὰ πάντα παρέρχονται** . . . "All things pass away." The Romans expressed it: *Tempus, edax rerum* . . . "Time, the devourer of things." The *Gestalt* principle of change, development, dynamic unfolding is a fundamental law of nature; the laws of celestial motion, the laws of thermodynamics, and the evidences of an expanding universe attest this.

According to this concept "soul" ceases to be thought of as either *a particular group of molecules of matter* or a *particular* "non-material" substance arranged into a pattern of relationships that constitute an entity, and becomes instead simply another name for the pattern of relationships which constitute personality. An entity, as Professor C. J. Ducasse has pointed out (in personal correspondence), is "a *Gestalt* of capacities, dispositions, etc., each of these being a tendency to behave in some manner B upon some change of kind A in a context of kind C." The human body is such a *Gestalt*. But while the relationships which constitute this *Gestalt* can be destroyed in a given medium, the isomorphic pattern for them exists in the same way that the formula for a chess gambit exists independently of the destruction of the board upon which it is played. In fact, all of the as yet unsung songs, as yet unplayed chess games, as yet unlived personalities exist potentially in nature regardless of whether they are at a given moment expressed in any medium. The number of possible combinations of relationships is, of course, too staggering to comprehend, but probably finite if the universe itself is finite, which seems to be a growing view in physics.

In this connection we may recall the Scripture, John 1:1: "In the beginning was the Word, and the Word was with God, and the Word was God." If we translate "Word" as "law," the passage fits perfectly the concept at hand. The original Greek employs *logos* (**λόγος**), which may be rendered as "reason" or "intelligence," or perhaps more loosely as "law." In other words, in the beginning was the law which made possible all patterns or arrangements. Even though a personality may act under free will, the law determines what it will want to do.

This view of "soul" makes impotent the frequent scientific objection that every fiber of the human body has been carefully dissected and no soul has been found. Just as the relationships which constitute love or hate are not found by observation of the discrete elements which have entered into these relationships, so the total complex of

relationships which constitute "soul" are not found in these same discrete elements. But this does not preclude the reproduction of these relationships in another medium, according to the *Gestalt* concept of transposition. Among the *Gestalt* psychologists, only Raymond Wheeler[6] seems to have seen the implications of transposition for the doctrine of immortality; but this thinking is implicit in the *Gestalt* concepts.

We may now raise the question of the nature of the medium in which reproduction of the bodily *Gestalt* would take place. A number of hypotheses are possible here, but to the writer the most tenable appears to be the concept of an "etheric double" which coexists with the body. This is not, of course, a new idea. This etheric double may be conceived in either of two ways: (1) as a material medium in the sense that it obeys the known laws of matter (which view is consistent with physical monism), or, (2) as a non-physical medium in the sense employed by J. B. Rhine in relation to the ESP hypothesis, to which the known laws of time, space, mass, and so forth would not apply. Since the first view represents a lesser departure from orthodox science and, if tenable, is a more parsimonious one, it is probably wise to exhaust the possibilities here before yielding to the second view.

We may thus assume tentatively that each change in the bodily *Gestalt* registers simultaneous isomorphic changes in an etheric double which, although as yet undiscovered, is susceptible of discovery and reconciliation with known physical laws. This is like a master tape recorder which makes a record of what is played in various media (such as radio, phonograph, film sound track, and so forth) and which can reproduce any of these *Gestalten* in another medium at a later time. This concept may be susceptible of experimental verification, regardless of whether the medium be physical or non-physical. If such a master medium exists, it may be that it can be discovered and changes in it can be found which coincide with changes in the bodily medium. Or it may be that the laws that govern *psi* capacities—which laws give some evidence of being independent of the laws that govern the medium of the body—may also govern this master medium. If so, when more is known about the medium in which ESP and related capacities are expressed, it may be possible to find evidence that changes in the bodily medium produce isomorphic changes in this master medium, and to study the nature of these changes with a view to determining whether they result in a reproduction of the bodily *Gestalt*.

This last named line of thought would, of course, represent a

[6] Wheeler, *op. cit.*

deviation from physical monism; the latter could be maintained only by assuming that the isomorphic medium is a part of presently known media, or at least of media whose laws are the same as those of the known physical universe.

Of course many tough-minded scientists will be unable to make any of these assumptions in the absence of experimental evidence for them, but at least the assumptions do not conflict with any of the established facts, as scientists have generally considered to be the case with the usual concept of soul in which a non-material entity is assumed to exist in the form of a *particular* group of "psychic elements" which might be compared to the particular group of molecules which make up the body. And these assumptions are sufficiently open to such possible experimental demonstrations as to permit all who have not eliminated faith and imagination from their intellectual make-up to integrate the ideas into their thinking without loss of scientific respectability.

Department of Philosophy and Psychology
Memphis State College

An Appraisal by Mrs. W. H. Salter of "The Haunting of Borley Rectory: A Critical Survey of the Evidence"

By ERIC J. DINGWALL, KATHLEEN M. GOLDNEY, and TREVOR H. HALL[1]

It has always been one of the functions of psychical research to strip away from the basis of genuine paranormal phenomena the strange accretions imposed on it by credulity and fraud. To this necessary, though sometimes uncongenial, task the authors of "The Haunting of Borley Rectory" have made a notable contribution. "The tale of the Borley haunting," they tell us, "developed into a really good ghost story because the legendary skeleton became clothed with a body of material which passed for reality and anything that weakened the flimsy structure was glossed over or treated as of no importance." The clothing of the skeleton owes much to the late Harry Price. "As a scientist," he wrote, "I can guarantee you a ghost."

The story the authors have to tell is long and involved (it covers a period from 1863 to 1953), and they have felt it necessary to tell the story in great detail. Consequently the reader may find it difficult sometimes to see the wood for the trees, and a reviewer may provide a useful guide by setting out the salient features of the story and the main conclusions to which the authors have come.

The original foundation for the haunting was an old local legend that Borley Rectory, in Essex, was built on the site of a thirteenth century monastery, that a monk from Borley eloped with a novice from a nearby nunnery at Bures, that they were caught and the novice bricked up in her own nunnery. It is revealing to find that as late as 1936 Price enthusiastically accepted the theory that Borley Rectory was built on a monastery site, but dropped it like a hot brick when in 1938 after careful enquiry the theory was finally disproved. The apocryphal novice from Bures probably provided a *point de repère* for an equally apocryphal character, Marie Lairre, a French Roman Catholic nun from Le Havre. Neither for Marie Lairre's existence, nor for the paranormal character of the phenomena she was alleged to produce does there appear to be a shred of valid evidence, but an elaborate story was built up about her by Dr. Phythian Adams, Canon of Carlisle, and accepted by Price. It seems

[1] *Proceedings* of the Society for Psychical Research (London), Vol. 51, 1956, pp. XIV+181. Simultaneous publication of trade edition, Gerald Duckworth & Co. Ltd., London. 16*s*.

a pity that educated men of good standing should be ready to lend their support to such taradiddles.

To return to the chronological building up of the Borley haunt—the next contribution was made by the "Bull incumbencies." The Bulls, father and son, held the living for 64 years, 1863 to 1927, and according to Price one of the most convincing features of the haunt was the long period covered by the manifestations. As the Bull incumbencies occupy the greater part of this period and the "story of the rectory apparitions was firmly established in Borley by 1928, when the living passed to a stranger, the Reverend G. Eric Smith," it is obviously important to consider whether there is any good evidence for paranormal occurrences during the Bull period. The authors discuss this question in detail and the following points may be noted: (1) Concerning apparitions alleged to have been seen by Henry Bull (father) and Harry (son) there appears to be no first-hand testimony. (2) The atmosphere of the rectory in Harry Bull's time was spiritualistic and we are told at second hand that he believed himself to have seen apparitions. He is however an unsatisfactory witness (see p. 21 of the Report). (3) Henry Bull's daughter, Miss Ethel Bull, had only one or two vague experiences which she recorded many years after the event. The alleged physical phenomena of this period are negligible, "stones falling about," "boots found on the top of a wardrobe," a French dictionary thrown onto a bedroom floor. (4) In view of the fact that Henry Bull had fourteen children and that other young people were frequent visitors at the house, which was already reputed to be haunted, some allowance must be made for the force of suggestion and for practical jokes. Attention may be called at this point to the geography of the rectory, admirably adapted as the scene of a haunt, a huge rambling old building of some twenty-three rooms, in very bad repair, infested according to several witnesses by rats and mice, badly lighted, and hung all about with exposed bell-wires. The authors conclude that up to the death of Harry Bull in 1927 the alleged haunt amounted to no more than a well-established local tradition with no real substance behind it, but "the stage was set for what was to follow."

With the incumbency of the Reverend G. Eric Smith in October, 1928, the haunting of Borley took on a new character owing to the well-intentioned but unfortunate action of Mr. and Mrs. Smith. Troubled by the local reputation of the rectory as a haunted house, they sought advice from the *Daily Mirror* with a view to "laying the ghosts." So far from laying them the *Daily Mirror* brought upon the scene a spate of phenomena, a horde of sight-seers, and—Harry Price. Concerning the part played by Price more will be said hereafter, but it may be noted here that with his advent the physical

phenomena associated with the rectory took on a quite different character from anything observed before, and were undoubtedly objective. The conclusion reached by the authors of the Report is that when the evidence for these alleged wonders is critically examined, there is nothing that could not be explained by normal agencies, probably eked out on one or two occasions by deliberate fraud. The situation is complicated by the fact that Mr. and Mrs. Smith were not consistent in the opinions they expressed at various times as to the nature of the phenomena. The authors have felt it necessary to discuss these inconsistencies in detail, and perhaps they were right. But it might have been helpful to the patient reader if some part of this discussion could have been relegated to an Appendix.

We come now to the Foyster incumbency (1930-1935). The Reverend L. A. Foyster at the time he became rector was a middle-aged man in poor health with a young and physically attractive wife to whom Borley Rectory is not likely to have been a congenial home. According to Price's published opinion in *The End of Borley Rectory* "it can be said without fear of contradiction that the Foyster occupation coincided with the most violent and most dangerous period in the whole recorded history of the Borley manifestations." He wrote of the "variety and violence of the phenomena," and of the meticulous record made by Mr. Foyster of "every paranormal incident which came under his notice." It may come as a surprise to the reader to learn that at the time when these incidents were occurring and for some years afterwards Price attached little, if any, importance to these phenomena as evidence of the paranormal and was of opinion (privately expressed) that "Mrs. Foyster was responsible for the trouble, though it is possible that her actions are the result of hysteria." It is apparent from the very careful investigation of the facts carried out by the authors that Price's private opinion (which he himself suppressed) came pretty near the truth. For detailed evidence of this the reader must study the Report itself. Points that may be noted here are: (1) Nearly the whole of Mr. Foyster's "meticulous record" was made months and even years after the alleged events and examples can be found of the fallibility of his memory and a tendency, as time went on, to enhance the paranormal character of the incidents. He records only one incident (of no evidential value) as occurring in the absence of Mrs. Foyster. (2) The testimony of other witnesses, e.g., Edwin Whitehouse and Mr. d'Arles, is for various reasons unconvincing and suspect. (3) For many of the incidents Mrs. Foyster is the sole witness; unless we are prepared to accept her testimony without question, there is no reason to suppose that these incidents were paranormal. (4) With

regard to the mysterious wall-writings, all of which seem to have appeared during the Foyster period in the summer of 1931, in view of the absence of any careful enquiry *at the time* all that can be said is that there is no good evidence for supposing them to have been of paranormal origin. They could have been produced normally and the burden of proof is on those who suggest that they were *not* so produced. There is evidence of a similarity between some of the writings and Mrs. Foyster's normal script. Attention may again be called in this connection to "Price's repeatedly expressed private opinion that Mrs. Foyster was the conscious source of all the phenomena during her period at Borley." It may also be noted that during Mrs. Foyster's absence from the rectory for a considerable period *no* phenomena were observed with the exception of one trivial incident, which was almost certainly not paranormal.

After the departure of the Foysters the next phase in the history of Borley was Price's tenancy, May 1937 - May 1938. The reader may be wearying by this time of the reiterated statement that Borley *still* provided no evidence of scientific value for the paranormal, but that after all happens to be the truth, a result which might perhaps have been anticipated from the fact that most of the observers during this period were chosen by Price on the curious theory that the less they knew about psychical research the more valuable their observations were likely to be! I am reminded of a saying of Alice Johnson's, than whom no one was better qualified to express such an opinion, that the art of psychical research is best learnt by practising it. Forty-eight "official observers" took part in this enquiry and of these only two produced statements which the authors thought worthy of careful consideration.

In February 1939 the rectory was destroyed by fire and in 1944 the ruins were finally demolished. This last period is described as "a sort of Borley 'silly season' of extravagant theory . . . and extreme credulity." No further comment seems called for. The only attempt at serious investigation during this time was made by Dr. A. J. B. Robertson and a group of Cambridge undergraduates.[2] The phenomena observed were mainly auditory and inconclusive, and Dr. Robertson himself called attention to the possible effect of suggestion when conducting "an enquiry into a supposedly haunted ruin." The part likely to have been played by suggestion in all phases of the Borley haunt should be borne in mind.

So far then as concerns evidence for the paranormal the Borley report is negative. It is however of great positive interest on several

[2] "Some Recent Investigations into the Borley Rectory Case," *Journal* S.P.R., Vol. XXXIII, 1945, pp. 107-110.

grounds. It affords a remarkable example of the growth of a legend. We can see this phantastic snowball gradually piling up and wonder at the enthusiastic will-to-believe with which it was accepted. The revelation of the part played by Price is also of great importance. For many years Price was widely (though not universally) accepted as a leading authority on psychical research, but where does he stand now? The authors have very properly expressed regret that they have found it necessary to bring accusations of *mala fides* against a man who is dead, but "the duty of establishing the true facts in this difficult region far outweighs personal considerations," and much of the evidence on which they rely was not available in Price's lifetime. Of the part he played in building up an imposing façade of falsehood, by gross inaccuracy and exaggeration, on the one hand, and suppression of what he himself apparently believed to be the truth, on the other, there can be no question. That accusation has been proved to the hilt and Price's reputation as a responsible enquirer is thereby destroyed. The evidence for his deliberate faking of phenomena is not quite so conclusive, but it is, to say the least of it, damaging and a good deal better than any evidence adduced for the haunting of Borley. One example is given from Price's first appearance on the Borley scene during the Smith incumbency (see p. 46) when in his presence a glass of water was turned to ink, a well-known conjuring trick, and Price was a skilled conjurer. Other examples are the evidence of Mr. C. Sutton (p. 31) concerning the throwing of a large pebble, a statement by Lord Charles Hope, an experienced investigator, that he left Borley "with the definite suspicion that Mr. Price might be responsible for some at least of the phenomena which had occurred while I was present" (p. 33), and the incident of the brick, adduced by Price in *The End of Borley Rectory* (p. 284) as a paranormal occurrence, but described by a witness as "barefaced hocus-pocus on the part of the late Harry Price."

The legend of Borley Rectory is now firmly established in the public mind (not without some assistance from the B.B.C.) and it has had a long time to take root, a fact for which delay in publishing the Report must take some responsibility. It is hardly to be hoped that a single shot, however well directed, will suffice to kill the legend. But the future student, who wants to learn the facts of the case, so far as it was possible to ascertain them, will know where to look.

Comments by Dr. Jan Ehrenwald on Professor F. C. Dommeyer's Report "Some Ostensibly Precognitive Dreams"[1]

I am glad that the Editor of this JOURNAL has offered me the opportunity to comment on this interesting paper. It is a report of apparently precognitive—or telepathic—incidents observed under unusually favorable circumstances. It is based on occurrences of a spontaneous nature and is supplemented by attempts at their experimental verification. What makes it particularly interesting to me is the fact that the argument in favor of a "paranormal" interpretation of the evidence hinges on the apparent correspondences between a certain recurrent element featured in a series of dreams and a series of well-authenticated actual events in the dreamer's life. In the Dommeyer series the "element" in question is the perception in dreams of excrement pertaining to an unidentified source or a baby in diapers. Alternatively, it was represented by a cat's regurgitated gastric content. The ostensibly corresponding event was the receipt by the dreamer (Mrs. Dommeyer) of money which, as Professor Dommeyer rightly points out, psychoanalysts usually equate with excrement, that is, with the first "gift" that the baby, in the course of his toilet training, is supposed to surrender to his mother or nurse.

The claimed correspondence—precognitive, telepathic, or otherwise—raises a number of questions. First, is the correspondence regular and persistent enough to warrant a parapsychological interpretation? Secondly, if so, what are the criteria on which the claim of the assumed correspondence between dream and waking event can be based? Thirdly, if such a correspondence is taken for granted, does it call for a precognitive interpretation or can it conceivably be accounted for by reference to some other hypothesis?

Professor Dommeyer's article emphasizes the close connection between the "predictive symbol" and the subsequent event. The five instances described are certainly impressive enough. But there are three possible objections which can be raised to their paranormal interpretation: (1) The "predictive" dream element is of such frequent occurrence in the subject's dream life that its coincidence with a certain type of actual event might be due to chance. (2) Her good fortune in being the apparently habitual recipient of smaller or larger monetary gifts might be considered a factor which would further

[1] JOURNAL A.S.P.R., Vol. XLIX, 1955, pp. 109-117.

weaken the suggested psi-explanation. (3) One might even go so far as to wonder whether or not there is a deeper psychodynamic, characterologically determined connection between the recurrence of a certain element in the subject's dream life and her apparent good fortune which makes so many dollar bills flutter—or at least so many nickels and pennies roll—her way.

As for the claimed telepathic (or precognitive) nature of a certain dream element, I have pointed out elsewhere[2] that this can be based on a number of well-defined criteria. First, there is the criterion of uniqueness (e.g., names, dates, numerals, etc.) contained in both dream and reality. Needless to say, the predictive symbol discussed here lacks this criterion. A second criterion is the presence of a combination of specific distinctive features in both dream and reality. It is readily understood that the material reviewed here falls short of this criterion. Nevertheless, the recurrence of Mrs. Dommeyer's "predictive feature," i.e., the excrement, in the manifest content of a series of dreams, in conjunction with a similarly patterned series of real events, is strongly suggestive of a psi factor being involved in the occurrences. The excrement contained in the dream can be compared to a tracer element causing a Geiger counter to tick revealing the presence of radioactivity. This is what I have described as the telepathic "tracer effect."

But even if we are satisfied with the *prima-facie* evidence of psi suggested by our criteria, how can the telepathic (or precognitive) nature of such a series be verified without resort to the statistical method? It is at this point that what I have described as the criterion of *psychological significance* may come to our aid. I have pointed out that this criterion can be established by psychoanalytic inquiry into the deeper meaning of the particular dream element (1) in relation to the dreamer, i.e., the percipient, and (2) in relation to the agent involved in the incident. If such an inquiry reveals the presence of a dynamically meaningful relationship between, on the one hand, the "tracer" element contained in the dream and, on the other, the agent's personality and emotionally colored preoccupations, then the suggested paranormal interpretation may indeed be regarded as psychologically significant. It may then be used as an added criterion to justify the telepathy hypothesis.

In psychoanalytic practice, for example, we may find that by granting the telepathic interpretation of a given dream we may arrive at a better psychological understanding of it, whereas failing to do so, the dream would remain utterly unintelligible. This means that

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[2] *New Dimensions of Deep Analysis*: A Study of Telepathy in Interpersonal Relationships, Grune and Stratton, New York, 1955.

introducing the telepathy hypothesis fills a gap in our approach and thus in itself justifies resorting to the telepathy hypothesis.

Professor Dommeyer's report indeed leaves us with the impression that detailed psychoanalytic exploration of Mrs. Dommeyer's dreams might well give added weight to their telepathic or precognitive interpretation. Whether or not the demonstration of its psychological significance would then satisfy the skeptic is, of course, another question. In any case, it might satisfy the psychiatrist anxious to join hands with observers concerned with the same problem in neighboring fields of research.

One more remark regarding the telepathic versus the precognitive interpretation of the incidents may be in order. Clearly, the first four instances can readily be explained in terms of telepathy to Mrs. Dommeyer from Professor Coyer, Professor Ducasse, or the dreamer's mother. They "thought" of sending a money gift to Mrs. Dommeyer, whereupon she produced the "predictive symbol" in her dream. The difficulties are greater in Instance 5. Yet whenever we are prepared to give serious consideration to the hypothesis that telepathy has occurred between Mrs. Dommeyer and her various agents, we might do well to extend the same courtesy to the hypothesis that Mrs. Dommeyer might have the capacity to affect her friends and relations in a similar way, i.e., telepathically. If so, one might assume that Mrs. S., forcing her nickel on Mrs. Dommeyer despite her protests, may in fact have acted as a percipient and carried out what amounted to a telepathic command given her by Mrs. Dommeyer as an agent. In this case, her dream would not be precognitive; it would in effect have brought about its own fulfillment. In so doing, it might even have fulfilled a wish—conscious or unconscious—present in the minds of many students of psychical research: the wish to obviate the need for accepting evidence in favor of true precognition with all its disturbing philosophical implications.

Analysis and Interpretation of a Pair of Presumably Telepathic Dreams

GERTRUDE R. SCHMEIDLER AND ESTHER FROMMER

In 1916, on a sunny day in an open trench, Hirsch Brand fell asleep very thirsty, dreamed of drinking beer, and woke refreshed. The dream was so vivid that he described it to his family, and has remembered it to the present. At about the same time (but not at exactly the same time) his seven-year-old daughter Jetta had a dream closely resembling her father's, which was so vivid that she described it to her mother on waking and, also, remembered it to the present. The similarity of the dreams is striking enough to make them appear telepathic. Discrepancies between them seem to be psychologically meaningful, and the pattern of similarities and discrepancies carries implications for the dynamics of the telepathic process.

Statement of the Dreams

In the spring of 1955, one of us (EF, Jetta's daughter) was a student at City College. In a discussion of psychic phenomena with GRS, she described the pair of dreams summarized above. At GRS's suggestion, EF interviewed separately her mother and grandfather. She recorded in shorthand their accounts, and typed them precisely as they were stated. Mr. Brand then read and signed his account, and Mrs. Frommer read and signed hers. Supplementary information was also recorded by EF, a short time later, in separate interviews with the two participants. The verbatim reports follow.

> This happened in Strassbourg in 1916. I was seven years, the oldest of four children. My father was in the war. I was very much attached to my parents.
>
> I dreamed one night that my father came home from the war. He said that he was very, very thirsty. He asked me to go down across the street to the grocery and get him a bottle of beer. I went down to get it. After drinking, his thirst was quenched and he was very happy and relaxed.
>
> Two to two and a half weeks later we got a letter in which he told us that he was lying in a trench and that he was almost dying for thirst. And still he could fall asleep. And he dreamed about his terrible thirst, coming home, and asking me to go down, across the street to the grocery and get him a bottle of beer. He drank it out and his thirst was relieved. After he woke up he wasn't thirsty any more.
>
> YETTI FROMMER
> (Mrs. Jetta Frommer)

It was during the first World War, it was a hot day. It was in May, 1916, and we were in Lucke, Poland. It was midday, the sun was burning, and I was very thirsty. I tried to fall asleep and forget my thirst. I got a dream that I went home to Germany, to Strassbourg, to my wife, and I told her, "I have such a thirst." My wife sent Jetta down to the store to get a bottle of beer. She went and bought a bottle. My wife opened it up and poured me a glass of beer. I drank one glass, then another, then woke up. I wasn't thirsty any more.

This same dream my daughter told my wife. I found this out when I got home from the war. We didn't remember the exact day, but we did remember the month and the year. I do not remember writing any letter.

HIRSCH BRAND

Supplementary questions fell into three categories, designed (1) to find whether the dreams were unusual, for these two people; (2) to check on the points where the two accounts differed; and (3) to explore the family relationships of the Brands. The answers are summarized below.

1. Both Mrs. Frommer and Mr. Brand state that the dreams were unique in their experience. Neither remembers a similar experience.

2. (a) Mr. Brand dates his dream in May, 1916, at midday. Mrs. Frommer remembers that her dream occurred in 1916, at night, but does not recall the month.

(b) In her original statement, Mrs. Frommer said, "I dreamed . . . that my father *came* home from the war." On questioning, however, she says that the homecoming was not a part of her dream, but that she remembers her father was in uniform. This is similar to Mr. Brand's report; he also has no recollection of actually coming home, in his dream. Both, then, begin with the father at home, thirsty.

(c) In Mr. Brand's dream, the whole family was present. In the daughter's dream, no one figured except her father and herself.

(d) Mr. Brand's dream detailed his wife's pouring him one glass of beer and then a second. The daughter has no recollection of this process; she does not even remember whether the beer was poured into a glass or drunk directly from the bottle. She has, however, a vivid memory of her father's happiness at quenching his thirst, and even of the phrases with which he expressed satisfaction.

3. Both Mr. Brand and Mrs. Frommer report that the father and daughter loved each other dearly. Mr. Brand says he "was always proud to walk with Jetta, she was so beautiful." Mrs. Frommer says she loved both her parents equally — and continues

by pointing out, critically, certain faults of her mother, then referring more vaguely to, and condoning, the corresponding faults of her father.

Evaluation of Evidentiality

Supporting the Telepathic Interpretation

Both participants recollect their astonishment at the similarity of the two dreams. In addition Mrs. Brand, to whom little Jetta told her dream on waking, fully confirms that the statements of the two dreams were made independently.

The dreams had many common elements. In both, the father was home; he said he was thirsty; he asked for beer to be bought; his daughter went to buy the beer and returned with it; he drank the beer and his thirst was quenched.

If (as stated by Mrs. Frommer) the father's letter was received two or three weeks after her dream, it seems likely that the time gap between the two dreams was small.

Both Mr. Brand and Mrs. Frommer describe their dreams as unique. They were not, then, a part of a long series of reported dreams, where we might expect eventually to find a chance relationship which, taken out of context, would seem evidential.

The fact that the dreams do not agree in all details, and that Mr. Brand does not recall writing the letter which his daughter still states that she recalls, implies an obstinate honesty on the part of both participants. It gives the ring of truth to the points on which there is agreement.

Opposing the Telepathic Interpretation

There is no documentary evidence, other than the accounts dictated almost forty years after the experience.

The two dreams occurred at different times. One was at night; one was at midday. There is no way to determine whether the time difference was a few hours or many days.

There are several discrepancies between the two accounts. Mrs. Frommer reports that a letter came from her father describing his dream, while Mr. Brand does not remember writing the letter. In Mr. Brand's dream, his wife and all the rest of the family were present, and his wife figured actively; in Jetta's dream only she and her father were present. Mrs. Frommer inaccurately excludes mention of her mother in recounting her father's description of his own dream.

Evaluation

Let us begin with a rough four-point scale for evaluating spontaneous cases, with the points designated as very good, good, inconclusive, and suspect. The absence of written or fully attested statements of the two dreams, each made before there was knowledge of the contents of the other statement, immediately prevents this case from being in the "very good" category.

Using previously recorded cases as a standard of comparison, we can dismiss as unimportant the lack of simultaneity in time. Many very good cases show a time lag or a precognitive effect.

By the same criterion, the absence of her mother and the rest of her family in little Jetta's dream need not be considered important. Myers reports a similar discrepancy, in a case which he seems to consider very good.[1]

The difference between what Mr. Brand recalls, and what Mrs. Frommer thinks that Mr. Brand recalls, indicates that the two did not compare notes, and jointly build up a better story than the original one. The contradictions on minor points thus seem to strengthen the agreements on the major ones.

The unusual nature of Jetta's dream, to which her mother testifies, the uniqueness of Mr. Brand's dream, and the fact that the two dreams agree on the essentials of the story they tell, seem to place the case as a "good" one.

Interpretation

When a very thirsty man falls asleep in the sun, we should expect him to wake even thirstier than before. But when instead, after dreaming of drinking, he wakes with his thirst quenched, we may speculate that either his body acted out the dream and, through autonomic activity, released body fluids in sufficient quantity to relieve the thirst sensation, or else that autosuggestion as powerful as a deep hypnotic trance changed his conscious experience. Neither explanation, of course, precludes the other; and either suggests that an important emotional meaning was carried by the dream. Turning to its manifest content, we find that it describes a scene with his intimate family, in which his wife, helped by his daughter, ministers to his needs. The dream, then, tells of his being cared for by those he loves best.

Put in these terms, the fact that only herself and her father are in Jetta's dream becomes a meaningful misrepresentation of the father's dream. We believe it natural that a little girl should learn to be

[1] F. W. H. Myers, *Human Personality and Its Survival of Bodily Death*, Longmans, Green and Co., New York, Vol. I, Case 425 B. (last edition 1954).

feminine by identifying with her mother — and even have come to believe it natural that, much as she loves her mother, she should at times be jealous of the special intimacy that her father feels with his wife, and should sometimes imagine herself as being in her mother's place. Thus Mr. Brand's dream told a story that he presumably wished were true: instead of being at war, he was at home, cared for by those he loved best; and Jetta's dream told a story that she (according to widely accepted current theories) like other little girls presumably wished were true: that she was the one her father loved best, and that she was caring for him in her mother's place.

The provocative question in all spontaneous cases is why, when two people are capable of telepathic experience, a particular experience should be the one that "comes through." The most reasonable explanation, in this case, is that the experience was initiated by the integration of Mr. Brand's strong body needs for coolness and something to drink, with his strong emotional need to be cared for by the family he loved. That this integration, expressed in his dream of being supplied with beer, was an unusually powerful experience, is attested by the extraordinary body change that followed it. According to this interpretation, his ability as a telepathic agent would not ordinarily be strong; but the combination of intense physiological and emotional need was sufficient to make it strong at this one time.

Turning, then, to little Jetta, our interpretation must be that she was an effective percipient in this case but not in others, because her strongest unsatisfied emotional need, while her father was away, involved being in the closest possible relation to him: caring for him and being rewarded by his satisfied approval. Fitting herself into the place of both wife and daughter, in the dream story that he told, satisfied this need. The dream story that he was sending, therefore, was (with the one change that she made in it) the story that she was most ready to receive.

City College of New York

"Mystical" States as a Subject for Psychical Research

MRS. MARION WILKERSON

I wonder whether a substantial effort has ever been made in psychical research to study "mystical" states of consciousness or those perceptions which claim intuitive knowledge of the universe as a whole.

On the surface, at least, the mystical state would seem to derive from a full or complete extrasensory power of which simple telepathy, clairvoyance or precognition are lesser representatives and, if this is true, a study of the mystical states would seem to constitute the most direct approach to the study of ESP.

A number of facts suggest that the traditional mystic experience may not be merely illusory — that it may be at least generally objective in reference and therefore suitable as a subject of parapsychological research.

The mystical claim of direct knowledge of a non-physical reality seems to be fairly well supported, for example, by modern science. The intuitive view of the unreality of time and space as viewed by common sense seems to be reflected by the scientific concepts of relativity, space-time, and the fourth dimension and, similarly, the intuition of wholeness and of the non-mechanical nature of various forces and influences would seem to echo the physicist's concept of field. The concepts of teleology, wholeness, form and field in biology and of gestaltism in psychology combine with the findings of physics and especially with those of parapsychology to paint a picture of reality not essentially different from that held by the mystic. Too, many of the individuals who have made claims of mystical knowledge of reality have possessed demonstrable and often striking telepathic and clairvoyant abilities and occasionally, it seems, powers of levitation (or PK powers) as well. While the demonstration of such powers does not establish the validity of the undemonstrated abilities, it at least suggests that these may also actually exist.

One wonders whether the mystical perceptions might not be the source of the more specific intuitive abilities so often found in the mystic and, further, whether it might not be possible to strengthen ESP through stimulating the more general intuitive states. It seems possible that research along these lines could further the ends of parapsychology in several important ways:

1. Help to strengthen ESP so that it could be more easily observed and experimented with.
2. Help to strengthen and control ESP with a view to its possible practical applications in everyday life.
3. Support belief in the spiritual reality affirmed by religion and, by increasing knowledge of how to stimulate mystical states, encourage more widespread participation in that unitive type of consciousness which constitutes the living core of all religion.

   Dr. Rhine has emphasized the importance to morality and world peace of the intellectual acceptance of a spiritual reality, but I wonder whether the actual mystical experience with its feeling content of love and "at-one-ness" might not foster social harmony a good deal more surely and to a much greater extent than intellectual belief.
4. Possibly, use the mystical faculty as a window through which to view directly the landscape of non-physical existence — thus aiding the study of non-physical reality and its relationship to physical existence.

   If (a) the researcher could really learn to produce these states or could find subjects among mystics

   (b) he could devise means of accurately recording their content and of translating it into intellectual terms (perhaps the most baffling and crucial problem in such a project)

   (c) he could work out means of checking the content with traditional scientific procedures

   then an introspective study of mystical states might prove to be of great value in the effort to grasp intellectually the nature of "a universe which combines non-physical operations and the physical world."

   In any such undertaking the dangers of illusion, wishful thinking, and general subjectivity would, of course, be very great. On the other hand, the intuitive faculty which helps to demonstrate the existence of a non-physical reality appears also to be the natural faculty for contacting that reality. While conventional scientific methods are indirect, dependent upon inference, and more or less fumbling as to direction, the intuitive method, if it could be properly developed and critically used, should be direct, to the point, and perhaps capable of pointing the way to essential facts and the relationships between them.

Might it not be the special task of an expanded and matured psychical research to re-discover the "lost half" of human nature, that inner or intuitive part which apparently relates Man directly to the universe as a whole, and its further task to reconcile the intellectual and intuitive elements in human experience, both the faculties themselves and the views of reality which derive from them?

In undertaking any such research the great initial problem would be, of course, that of finding subjects with mystical mentality. If these were not available there would have to be found some method of producing the desired states in more ordinary subjects. Such a method might be called for in the long run anyway if the findings should indicate, as it seems that they might, that social harmony could be greatly increased through stimulation of mystical experience in large numbers of people.

A few signposts already exist, of course, which might be useful to a researcher in his effort to produce the necessary states. Parapsychology, first of all, indicates that the intuitive faculties are unconscious. The spiritual disciplines of both East and West, too, show that these faculties do not reside in the intellectual part of the psyche but that they appear only when the intellect — together with the affective elements which arise from it — are in abeyance or, in physiological terms, that they do not arise in the forebrain.

Assuming traditional psychological methods of spiritual development such as the Yoga to be too slow, difficult, and uncertain in effectiveness for use in laboratory research (or for widespread social application), one would begin to look for physiological rather than psychological methods of producing the desired states.

The psychiatrist Dr. Trigant Burrow,[1] in seeking a means of breaking up the conflict-producing human tendency to depend too exclusively on the fore or "part" brain (the intellectual part of the psyche) claims to have devised an automatic method of stopping the affective images from the intellect and starting a different type of imagery — constructive, inclusive, and impersonal — which derives from the harmonious working of the "whole" brain. He claims to have done this by a simple kinesthetic device — the balancing of the ocular muscles to keep the closed eyes in a state of equilibrium.

Dr. Burrow's aim — to develop the art of living wholly and in unison — to restore man's primary pattern of motivation — is decidedly non-mystical in intent but it does suggest that physiological devices may be used to produce far-reaching psychological results. I am not even sure that Dr. Burrow's device, which apparently fosters the rounded and harmonious life of the *natural* man, might not also, through its tempering of the intellectual part of the psyche, tend to open the door to mystic states as well.

The science of electroencephalography suggests another possible non-psychological approach to the problem of stimulating mystical

[1] Trigant Burrow, *Science and Man's Behavior*, Philosophical Library, Inc., New York, 1953, p. 372.

states. By allowing a flickering light of carefully adjusted frequencies to strike the eyes of his subjects, the English physiologist Dr. Grey Walter[2] was able to break down the barriers between different parts of the brain and alter the brain wave rhythms. Subjects "saw" moving patterns, felt whirling sensations, or had sudden vivid recollections of past experiences.

If such physical means can produce psychological results, one wonders whether we might learn in time to produce almost any desired mental state through altering (by electrical impulses?) the brain wave patterns and rhythms. Dr. Walter does, in fact, recognize the possibility of physiological training of brains.

Clues as to the exact nature of the brain wave modifications required to produce a mystical state might be found first in the knowledge of the inhibiting effect of the forebrain on such states and next, perhaps, in electroencephalographic studies of the brain-wave patterns of mystics (if such can be found), of lobectomy patients, of subjects who have been given drugs reported to stimulate such states or of subjects during the well-known "anesthetic revelation."

Whatever direction the research might take, it does seem that an attempt by psychical researchers to explore the mystic condition — its stimulation and content — might prove to be of great value in furthering the ends of parapsychology — by strengthening ESP for research purposes; by strengthening and controlling ESP for possible development of practical applications; perhaps, by fostering greater social harmony through making possible the widespread stimulation of the more unitive states of mind; and by supplementing traditional methods of discovery with intuitive ones in the effort to understand intellectually the nature of a universe that combines non-physical operations with the physical world.

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[2] W. Grey Walter, *The Living Brain*, W. W. Norton & Co., New York, 1953. Chap. 4, p. 269 and last chap.

Reviews

MAN'S PLACE IN A SUPERPHYSICAL WORLD. By W. W. Coblentz, Pp. X+233. Sabian Publishing Society, New York, 1954. $4.00.

Dr. Coblentz is a physicist, recipient of many scientific honors, who for more than three decades carried on research at the National Bureau of Standards, where he specialized in selective radiometers for the interception and measurement of thermal radiant energy.

Early in life, he became interested from a scientific point of view in the phenomena of mediumship and in other paranormal occurrences; and for some sixty years he has availed himself of such opportunities as he could get to observe and study them. Attempts which he made to develop extrasensory perception in himself met with some degree of success along the lines of automatic writing, telepathy, clairvoyance, and significant dreams, and had at least the value of giving him some personal insight into the nature of the psychological states of mediums and sensitives.

Being wide awake to the fact that many of the persons who purport to be mediums are frauds, Dr. Coblentz made it a point to inform himself as thoroughly as he could regarding the methods, the apparatus, and the psychology of conjuring. However, in describing his contacts with a psychic seamstress, who gave out such clairvoyant impressions as came to her while sewing for his family, Dr. Coblentz makes a remark which, on the one hand, mediums, and, on the other, scientists, could well ponder: "She found that all scientists were not seeking newspaper publicity by exposing the shortcomings of mediums, and one particular scientist [to wit, himself] was learning that not all psychics are fakers" (p. 38). The sensible attitude which he accordingly learned to adopt is put by him in the following words: "Certainly a predetermined judgment would be of little help to an investigator. It would destroy his chance of real participation in the séance, on the one hand, and would make it impossible for him to learn anything of consequence, on the other. Hence, from the beginning I tried to play a fair game, even if I was imposed upon — as no doubt I was, quite frequently, both known and unknown to me . . ." (pp. 45-6).

In commenting on the mediums and other persons he talked with in Spiritualist groups at various times, Dr. Coblentz states that he found — as others, including this reviewer, also have — "a general grooving in conventional methods which I was unable to divert."

On one occasion, at Lily Dale, "I had brought a set of magnets with me, for . . . tests on the aura . . . but it was impossible to get anyone interested in my project. It was not a question of fear, or anticipation of injury, but sheer disinterest in anything different. . . . each one . . . wanted to give me a mediumistic reading in which I was not interested, and had a mind quite closed to any other suggestion" (p. 33). This disinterest in questions of the kinds a scientist would like answered is of course not peculiar to Spiritualists, but is common among persons whose concern with these and related matters is, like that of most Spiritualists, essentially religious: the majority of devout Christians would be similarly disinterested in a project to make a chemical analysis of a consecrated host.[1]

The book's third chapter describes some experiments, employing magnets and various metals, which Dr. Coblentz conducted with the collaboration of a medium who claimed the capacity to perceive the aura. Also, the attempt to record on a gramophone the voice of a communicator, purporting to be William James, who had died four months before. Whenever the gramophone was on, however, a communicator purporting to be R. W. Emerson — who had died thirty years earlier — came through instead of James. So the intended test of identity — by playing the recorded voice to persons familiar with that of James — could not be made.

In Ch. 4 Dr. Coblentz gives an account of the numerous purported materializations he witnessed, chiefly at "dark" séances with the medium Pierre Keeler, to whom he had been introduced, not as a scientist, but "as a government employee," of whom there were several others at the séances. Thirteen of the séances were in 1911-12 and two in 1915, at which time Keeler broke off relations in consequence of Dr. Coblentz' expressed desire to obtain a sample of "ectoplasm," such as — while Coblentz was operating the music box at the right of the curtain — had appeared under the box and had then "suddenly, with a hissing noise, and high speed . . . started up the right-hand side of the cabinet, or edge of the curtain and then, still hissing, passed along the top of the curtains, and down the left-hand side, where it disappeared" (p. 86).

The author kept voluminous notes of the séances, and, because the question whether confederates could be responsible for what he observed was constantly in his mind, he paid particular attention to such features as the "unmistakable demonstration of the building up and disintegration of the form outside the cabinet, close by me . . ." (pp. 81, 110, 113). He does not commit himself to the genuineness

[1] Cf. Prof. J. H. Hyslop's penetrating chapter on "Spiritualism, Religion and Science" in his book *Contact with the Other World*, Century Co., N. Y., 1919.

of any one of the purported materializations he observed, but is apparently convinced that Keeler had authentic mediumistic powers even if some of his phenomena were fraudulent.

In the next chapter, he describes in detail some of the occurrences at two of Keeler's "light" séances, the first at a private house under conditions precluding the possibility of confederates. The occurrences are similar to those which Hereward Carrington observed some years earlier at a séance with Keeler and pronounced fraudulent; but, in the light of some of the details Coblentz gives concerning his own séance with Keeler, the explanation Carrington offers (A.S.P.R. *Proceedings*, Vol. II or what *he* saw at his séance does not seem applicable to what Coblentz observed.

The last part of this interesting book is given to the author's account of his attempt to develop psychic faculties in himself, and of the modest degree of success this met with.

C. J. Ducasse

Brown University

PROCEEDINGS OF THE FIRST INTERNATIONAL CONFERENCE OF PARAPSYCHOLOGICAL STUDIES. Pp. XX+136. Parapsychology Foundation, Inc., New York, 1955. $3.00.

These *Proceedings* are a valuable compendium for the reader who wishes to be au courant with the international situation in psychical research. They present summaries of about fifty papers delivered at the First International Conference of Parapsychological Studies held at Utrecht, The Netherlands, in the summer of 1953. The authors of the papers are drawn from fourteen nationalities and include psychologists, philosophers, sociologists, physicians, physicists, chemists, biologists, engineers, and mathematicians.

Readers of this Journal will recall Dr. Gardner Murphy's "Summary Report" of the Utrecht Conference, in the October 1953 number, in which he described the four working groups that were established there: "the first dealing with quantitative experimental studies of the various classes of paranormal activity, including studies of the physical and physiological aspects of paranormal phenomena; the second with interpretation of material gathered in the practice of psychiatry; the third with qualitative and spontaneous (not experimentally controlled) phenomena; and the fourth with the psychological study of those persons who appear to display a relatively large amount of paranormal sensitivity (special sensitives)."

In addition to the summary of papers which occupy the major part of the volume Dr. Murphy, who served as Chairman of the Conference, contributes an Introduction in which he writes: "From the perspective of 1955, it appears that this Conference achieved its major purpose in moving toward a high level of seriousness in parapsychological science, enabling scholars from different lands to meet with one another, and orienting the public to the fact that a great university may today be proud to give its utmost hospitality to these studies so rich in challenge, so far-reaching in ultimate implications." Dr. Murphy's opening address to the Conference on "International Parapsychology" precedes the summaries of the papers.

The last pages of the volume are devoted to the "Resolutions" adopted by the Conference; a "Statement Regarding the International Congresses held Between the Two World Wars"; and "Follow-Up" Conferences in France and England in accordance with the Resolutions of the Utrecht Conference. These Follow-Up Conferences included one on "Philosophy and Parapsychology," the other a "Study Group on Unorthodox Healings," both held at Saint Paul de Vence, Alpes Maritimes, France, in the spring of 1954; and a "Conference on Spontaneous Phenomena" organized by the Society of Psychical Research, London, at Newnham College, Cambridge, England, in July, 1955. The *Proceedings* of these conferences will be published by the Parapsychology Foundation at a later date.

All four Conferences on Parapsychological Studies were made possible through the generous support of the Parapsychology Foundation in New York, of which Mrs. Eileen J. Garrett is President.

L.W.A.

The "Science" Magazine Controversy

In an article entitled "Science and the Supernatural," which appeared in the August 26, 1955 number of *Science*, Dr. George R. Price, research associate in the Department of Medicine of the University of Minnesota, challenged the validity of the findings of parapsychologists, with special emphasis on the work of Dr. J. B. Rhine of Duke University and Dr. S. G. Soal of the University of London.

Dr. Price attempted to show that fraud was logically conceivable under the conditions of the experiments, and he quoted Tom Paine ". . . is it more probable that nature should go out of her course or that a man should tell a lie?"

In his article Dr. Price wrote, "My opinion concerning the findings of the parapsychologists is that many of them are dependent on clerical and statistical errors and unintentional use of sensory clues, and that

all extrachance results not so explicable are dependent on deliberate fraud or mildly abnormal mental conditions. . . . Since I cannot prove, all I can do is try to convince by showing that ESP is incompatible with current scientific theory."

The January 6, 1956 number of *Science* contained comprehensive replies to Dr. Price's arguments by Dr. Rhine, Dr. Soal, and other scientists. According to Dr. Soal, ". . . Price makes these suggestions of fraudulent collusion without being able to produce the least fragment of factual evidence that any such fraudulent malpractice ever took place. It is, I think, safe to say that no English scientific journal would have published such a diatribe of unsupported conjecture. *Nature,* the leading English scientific weekly, has nothing but praise for our work in a recent book review.

"Price begins by saying that 'In his early work as a psychic investigator, Soal published excellent papers reporting negative findings and showed himself to be a meticulous and ingenious experimenter, expert at uncovering trickery. . . . Apparently Price considers the early experiments to be 'excellent' merely because they produced only negative findings."

Dr. Rhine commented: "Strange though it may seem, the publication of the George Price paper . . . is, on the whole, a good event for parapsychology. It is not merely that it is better to be attacked than it is to be ignored. According to the ways of American science, a revolutionary finding has to be cuffed and kicked through the entrance to gain admittance. When unorthodox issues are concerned, only critical articles, and the rougher the better, are likely to be accepted by the scientific periodicals. In fact, one can easily fancy (as some readers have) that Price deliberately undertook to sell parapsychology to American science by disguising a really informative article as a slanderous critique, with charges so utterly exaggerated that they would not be believed even by skeptics of ESP. At any rate, as a way to get a lot of instruction on parapsychology into *Science,* it worked as well as if it had been planted."

Dr. Paul E. Meehl, Chairman of the Department of Psychology at the University of Minnesota, and Mr. Michael Scriven, research fellow in the Minnesota Center for the Philosophy of Science, contribute a joint paper to the controversy entitled "Compatibility of Science and ESP," and Professor P. W. Bridgman, emeritus professor of physics at Harvard University, discusses "Probability, Logic, and ESP." The same issue also contains a rejoinder by Dr. Price and a final comment by Dr. Rhine.

The Editors of the *Journal of Parapsychology* have received a number of contributions to the controversy. These, together with abstracts and rejoinders appearing in *Science* are published in the current (March) issue of the *Journal.*

L.W.A.

Notice to Members

THREE PAPERS ON THE SURVIVAL PROBLEM. Pamphlet Containing Three Articles by Gardner Murphy. Reprinted from the January, April, and October 1945 numbers of this JOURNAL. Edition of 300 copies. Pp. 90. $2.50.

Members who are interested in the question of the survival of human personality after death are reminded that Dr. Murphy's three papers on the subject, which originally appeared in the JOURNAL in 1945, are again available.

The first article, "An Outline of Survival Evidence," presents the various classes of evidence in organized form.

The second article, "Difficulties Confronting the Survival Hypothesis," is concerned with the problem of finding evidence for survival which cannot be explained by some other hypothesis.

The third article, "Field Theory and Survival," discusses the implications of field theory (complex organized wholes cannot be fully understood in terms of ingredient parts) for psychical research. The interpersonal nature of telepathic and clairvoyant processes is considered and the hypothesis is extended to relate to the future and the past. The most cogent types of survival evidence are indicated.

Throughout the three articles important cases serve as illustrations for the subject matter under consideration.

HISTORY OF THE SOCIETY

The First American Society for Psychical Research was formed in 1885, in consequence of a visit by Sir W. F. Barrett to this country, and Prof. Simon Newcomb became its President. In 1887 the Society invited a man of signal ability, Richard Hodgson, A.M., LL.D., sometime Lecturer in the University of Cambridge, to become its Executive Secretary, and he accepted.

This organization later became a branch of the English Society under the very able guidance of Dr. Hodgson until his death in 1905. The American Society for Psychical Research was then re-established with James H. Hyslop, Ph.D., formerly Professor of Logic and Ethics in Columbia University, as its Secretary and Director.

THE ENDOWMENT

The American Society for Psychical Research, Inc., was originally incorporated under the Laws of New York in 1904 under the name of American Institute for Scientific Research, for the purpose of carrying on and endowing investigation in the fields of psychical research and psychotherapeutics. It is supported by contributions from its members and a small endowment fund. The income of the Society pays only for the publications and office expenses, but does not enable the Society to carry on its scientific investigations. A much greater fund is required before this work can be carried forward with the initiative and energy which its importance deserves.

The endowment funds are dedicated strictly to the uses set forth in the deed of gift and are under control of the Board of Trustees, the character and qualification of whom are safeguarded, as with other scientific institutions.

Moneys and property dedicated by will or gift to the purposes of the American Society for Psychical Research, Inc., whether to the uses of psychical research or psychotherapeutics, are earnestly solicited. The form which such dedication should take when made by will is indicated in the following:

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THE JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH

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THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH, Inc.
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PURPOSE AND SCOPE OF THE SOCIETY

1. The investigation of claims of telepathy, clairvoyance, veridical hallucinations and dreams, psychometry, precognition, dowsing, and other forms of supernormal cognition; of claims of supernormal physical phenomena, such as raps, telekinesis, materialization, levitation, fire-immunity, poltergeists; the study of automatic writing, trance speech, hypnotism, alterations of personality, and other subconscious processes; in short, all types of the phenomena called psychic, mediumistic, supernormal, parapsychological and metapsychic.

2. The collection, classification, study and publication of reports dealing with the above phenomena. Readers are asked to report incidents and cases. Names must be given, but on request will be treated as confidential.

3. The maintenance of a library on psychical research and related subjects. Contributions of books and periodical files will be welcomed.

4. Cooperating in the above tasks with qualified individuals and groups who will report their work to the Society.

MEMBERSHIP IN THE SOCIETY

Members, who receive the Proceedings and the Journal, pay an annual fee of $10. (One may become a Life Member or endow a Memorial Membership on payment of $200.) *Fellows,* who receive all publications of the Society and are afforded special library privileges pay an annual fee of $25. (Life Fellowship, $500.) *Patrons and Founders*: For those who wish to make a still larger contribution to the Society's work, these classes are open at $1000 and $5000.

It is to be remembered that membership in a scientific society means more than merely a subscription to its publications. The work must be carried on largely through the income from membership fees. Therefore members, old and new, are urged to make their membership class as high as they feel they can. *If a comparatively small proportion of the present members went one class higher, the money available for research would be more than doubled.*

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THE JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH

Responsibility for the contents of any article appearing in the JOURNAL rests entirely with the contributor and not with the A.S.P.R.

VOLUME L JULY 1956 Number 3

Important Notice to Members: The Rooms of the Society will be closed during August.

How Can Psychiatry Help in the Study of Spontaneous Phenomena?[1]

JAN EHRENWALD, M.D.

In February, 1955, Professor Gardner Murphy gave us an illuminating lecture in which he mapped out plans for organized research in the field of spontaneous psi occurrences.[2] At the same time he described a few illustrative cases seen by himself or collected by Mrs. Laura Dale.

I propose to discuss with you tonight the contributions the psychiatrist, or to be more specific, the psychotherapist, can make to this problem. In so doing, I wish to confine our discussion to spontaneous incidents of a telepathic nature. To begin with, let me try to give you a composite picture of a typical spontaneous incident of this kind as it was described by the early workers in psychical research: Still dreaming—or just awakening from sleep—Mr. X

[1] This paper was read by Dr. Ehrenwald at a meeting of the Society on March 31, 1955.

[2] Gardner Murphy, "Plans for Research on Spontaneous Cases," JOURNAL A.S.P.R., Vol. XLIX, 1955, pp. 85-98.

hears a voice crying out for help. The voice may be calling his name or merely sighing in a barely audible manner. The dreamer is immediately aware that an absent friend or relation is in a state of crisis, in mortal danger, or has met with an accident. In some cases the percipient interprets his experience as an apparition. He may then describe the scene in great detail and include information which he could not have obtained through "ordinary" sensory channels. In other cases it is merely a spasm of sudden baseless anxiety apparently connected with an indefinite danger threatening the absent friend, and although detailed information may be lacking, subsequent inquiry may show that the apprehension brought about by the "premonition" was justified.

Needless to say, observations of this kind have to meet the rigid criteria of proper authentication and corroboration by independent contemporary witnesses, written records or diaries, etc., before they can be accepted as valid scientific evidence.

The fact is that a good number of the observations reported by the early workers does measure up to these standards. I need only to refer to some of the incidents recorded in *Phantasms of the Living* by Gurney, Myers, and Podmore,[3] in *Human Personality* by Myers,[4] or in the "Census of Hallucinations."[5]

There is one more characteristic these incidents have in common: they seem to follow a consistent pattern—they all conform to a certain style of their own. With a few exceptions to which I will revert later on, they can be described as incidents of the *grand* or *classical* type, carrying a deep emotional impact upon all those involved. Now you may rightly ask at this point: What can the psychiatrist, shut in as he is between the four walls of his clinic or consulting room, contribute to this far-flung field of research? What can he add to the vast mass of spontaneous evidence psychical researchers have accumulated over the past seventy-five years?

Well, it all depends on what you mean by spontaneous evidence. If the term is to include all psi occurrences which come our way without our having set a trap for them, as Professor Murphy put it, then, I submit, the psychiatrist or psychotherapist is indeed in a position to make a contribution to the problem. Telepathic incidents in the psychoanalytic situation are of comparatively frequent occurrence and they are not, as a rule, planned for. In this respect they are in no way different from occurrences which happen outside the therapist's office. But they have the definite advantage that the therapist may be able to catch them red-handed, as it were, under

[3] Trübner and Co., London, 1886.
[4] Longmans, Green and Company, New York, 1903. (Reprinted 1954.)
[5] *Proc.* S.P.R., Vol. X, 1894.

favorable conditions of observation. More than that: when they occur, they do so in a setting containing all the prerequisites for their methodical psychoanalytic investigation, that is, for the inquiry into both the patient's and the therapist's—the percipient's and the agent's—states of mind at the time of their occurrence.

But if you contrast the incidents reported in the psychoanalytic literature with what I called spontaneous phenomena of the grand or classical type, you may conceivably wonder whether they are really the same thing—and even if they are, whether they are altogether worth your interest. Like the big-game hunter who scorns the petty pursuits of the entomologist trying to catch a butterfly or a grasshopper in his net, you may feel inclined to belittle the findings of the psychotherapist. I must admit that on the face of it these findings seem to be much less intriguing and of an entirely different order than those reported by the early workers in psychical research. More often than not they are of a trivial, pedestrian, uninspiring nature, containing fragments of apparently indifferent, inconsequential information. As a general rule their telepathic nature passes unnoticed by the patient and more often than not it is only the therapist's special interest in the phenomena which makes him aware of the fact that he himself happened to play the part of the agent in relation to his patient. This, in effect, is one of the reasons why telepathy in the psychoanalytic situation is still regarded as a comparatively rare event, and, if it occurs at all, as an event deserving at best the smallest print in the psychiatrist's textbook—not the chapter headings and italics reserved for observations of major importance.

Let me give you an example of a telepathic incident of this kind.[6] Mrs. M., a lady of fifty-two, was suffering from a depressive state. This had developed when she felt she had reason to suspect her husband, two years her junior, of marital infidelity. Mr. M., a writer, is of quiet and unassuming manner. The couple lived in moderate circumstances. Psychotherapy was at first of no avail and the patient received a series of electric shock treatments. Shortly after her release from the hospital and following resumption of psychotherapy, she had the following series of dreams which occurred during the same night:

1. We were living in a penthouse. Caspar and Louise came to visit us. We were serving tea.

2. I am in my cousin Charlotta's house in D. I look at a brass tray. It is used to hold calling cards. I see a card with something written on it in Spanish.

[6] This incident is abridged from Jan Ehrenwald's *New Dimensions of Deep Analysis*: A Study of Telepathy in Interpersonal Relationships, Grune and Stratton, New York, 1955.

3. My husband and I drove to a place. It is like a hotel or a big apartment house; I showed my husband the way to drive to the front door. But he drove to a kind of parking place at the back of the house. He smiled and said: "This is the way I always drive here."

The dreams introduce a number of characters who require elucidation. Caspar and Louise are a married couple, friends of the dreamer and also of the therapist. Incidentally, it was Caspar and Louise who referred Mrs. M., the patient, to the present writer for treatment. Caspar's wife, Louise, is, like the patient, older than her husband. The marriage is successful, however, and Caspar has never been involved with other women. They are also much better off than the dreamer and her husband. They, Caspar and Louise, live in a beautiful penthouse apartment in which rather expensive parties have been the order of the day. The patient also mentioned her cousin Charlotta in her dream. Charlotta too has done much better than Mrs. M. and lives with her husband in an expensive apartment house in D. with all the comfort and luxury the patient could wish for herself. You may note that all this accounts for the dynamics of a substantial part of the dream material. The first two dreams express the dreamer's wish to change places with Louise on the one hand and with Charlotta on the other. Both have been more successful in their married lives than Mrs. M.

You will note, however, that so far I have not tried to fill the gaps left in our dynamic understanding of certain elements in the second and third dream fragments. The fact is that no such information was supplied by the patient's associations. Yet when we turn our attention to the situation on my side of the picture, we can see a striking correspondence between these so far unaccounted-for dream elements and certain events occurring in my own private life on the night of the dream.

As it happened, Louise and Caspar had been my guests on that particular Sunday night. It was the first and only occasion they had been in my house. Mrs. M., though she knew of our acquaintance, was unaware of Louise and Caspar's visit to my home. She had never been in my private residence and knew nothing of my personal life. Nor had she been in touch with Louise and Caspar since her discharge from the hospital. Tea and cold cuts were served during the evening and the conversation revolved around two principal subjects: (*a*) Mrs. M. and her unhappy married life and (*b*) telepathy in psychoanalysis and in everyday life. Those present at the party included Mrs. Laura Dale of the A.S.P.R., and all of us contributed some personal experience in parapsychology. There was general agreement as to the reality of psi phenomena. Every-

body was sorry for Mrs. M., who had made such a "mess" of her life and was on the point of ruining her husband's career.

There is one more detail which should be mentioned. To their annoyance, Caspar and Louise arrived half an hour late at the party. They came by taxicab and tried to explain to the driver how to get to their destination, but he refused to listen. He said he knew the place well and needed no directions. As a result, they had first landed at the wrong (the back) side of the large suburban apartment house on whose second floor I lived at that time. The back was occupied by garages and had a driveway in front. On Sunday evenings it was usually blocked by parked cars, giving the impression of a parking lot.

Comparing the parts of the dreams reviewed so far with the presumably corresponding real events, there are indeed several distinctive features common to both dream and reality on my side of the picture. In short, we have a situation suggestive of telepathy. But if this is so, you may well ask why any dreamer, whether emotionally well-balanced or disturbed, should take the trouble to reach out telepathically for such trivial and inconsequential information as was seen to be contained in Mrs. M.'s dreams.

It is at this point that the psychoanalytic approach to our data comes to our aid. Seen from the psychoanalytic point of view, it is immediately apparent that the patient, by picking up the information contained in her dream, joined as it were in the social activities of Caspar and Louise on the one hand, and of myself and my family on the other. To be more specific, the patient projects herself into a situation which amounts to a composite picture of my own family situation, of Caspar and Louise's married life, and of her cousin Charlotta's home. The three dream fragments represent a condensation (1) of Charlotta's house, (2) of Caspar and Louise's penthouse apartment, and (3) of my own house. Yet at the same time it is the patient who runs the show in the dreams. She is in possession of the penthouse apartment; she entertains the guests; she serves the tea. She also seems to know better how to get to my home than the obstinate cab driver, who was in turn fused into one person with her fumbling and ineffectual husband.

There is only one trivial element left in one of the dreams which has remained unexplained so far: the motif of the Spanish calling card lying on a brass tray in her cousin Charlotta's house. The patient had no associations to offer in connection with either the tray, the calling card, or with the Spanish words printed on it. Neither did there seem to be anything on my side of the picture which could possibly be linked up with these items. This gap in the understanding of the manifest dream content was so puzzling

to me that I felt I had to shelve the whole incident and its suspected telepathic implications together with many other inconclusive observations of the same kind.

Three weeks later my daughter's eyes were caught by a quaint little match box she found lying in a large copper tray on an end table in our apartment. At first I was not quite sure where it had come from. Then I remembered that Caspar had used what I thought were miniature candle-shaped matches during his visit to our house. "They are called *fiamiferi Italienissimi,*" I explained to my daughter in a somewhat nostalgic reminiscence of pre-war travels in Italy. On taking another look at the box, however, I was surprised to find that far from being of Italian make, it hailed from Mexico. It had a Spanish inscription and showed, among other things, the manufacturer's name in bold letters: Mendizabel Y Cia Sucs. Cia Cerillera Mexicana S. A. 4a del Pino No. 169, Mexico, D.F. The face of the match box carried a picture of the Venus of Milo and of the Parthenon in the background. Its back was adorned with a color reproduction of Goya's *La Maja Desnuda.*

Assuming that the dream is of a telepathic nature, the belated discovery of the match box left behind by Caspar, the "caller," would well account for the motif of a calling card as seen by the patient in the dream. It is true that we have to substitute copper for brass, and a rectangular match box carrying the manufacturer's business address for a calling card. But this is a well-known characteristic of what I have described as the telepathic scatter effect. Notwithstanding this telepathic scatter, it is the combination of a number of undistorted, distinctive features common to both dream and reality which, in conjunction with the meaningful nature of my suggested telepathic interpretation, justifies and even demands such a reading.

Let us pause here for a moment to re-evaluate our case. The dreams described indeed contain a number of apparently trivial elements whose telepathic nature could easily have passed unnoticed by both patient and therapist. But once we are prepared seriously to consider their relevance to the dynamics of the case they suddenly appear in a new light. They express a deep-seated emotional need of the dreamer: her desire to become involved in her therapist's private life, to be admitted to the friendly group gathered in his home, to be there as an unseen and uninvited guest, but present just the same in the "spirit" if not in the "flesh." Psychologically speaking, her attitude is familiar. It is a feature of the patient's positive transference to her therapist. At the same time her telepathic dreams fulfill another function: they picture her as competing successfully with her cousin Charlotta, the rival of her childhood years. In a

similar vein she can see herself on equal footing with Louise, the happily married, wealthy socialite, as well as with the therapist's wife, at whose party she, the patient, is serving the tea.

You may see from this glimpse into the dynamics underlying Mrs. M.'s dreams that the telepathic elements contained in their manifest content are not so inconsequential after all. In the light of psychoanalytic inquiry, the dreams can be seen as serving a specific emotional need of the patient. They go far beyond the task of merely transmitting pieces of irrelevant information from an involuntary agent to an unrelated percipient who just happens to be around.

Now, the telepathic incident just described is typical of a great number of observations reported in recent years by I. Hollós, H. Deutsch, E. Servadio, N. Fodor, J. Eisenbud, G. Pederson-Krag, M. Ullman, G. Booth, J. Meerloo, myself, and, last but not least, C. G. Jung.

If I may outline to you a composite picture of this type of telepathic incident in contrast to telepathic occurrences of the classical, heroic type, I should describe it as follows: At a given stage of psychotherapy the patient produces a dream whose manifest content contains a combination of distinctive features which show a striking parallel with a simultaneous set of mental events or other occurrences in the therapist's life situation. In many cases the correspondence between dream and reality is so far-reaching that it cannot reasonably be attributed to chance alone. Moreover, the analyst's evaluation of the dream indicates that the occurrence of the telepathic element in the dream meets a deep-seated emotional need of both the dreamer and the therapist. Unfortunately, the dreams discussed here do not lend themselves readily to illustrate the part played by my own emotional involvement in the incident. But such an involvement is always there. It is part of what psychoanalysis describes as the analyst's counter-transference. Another characteristic of this type of incident is that, as a general rule, the percipient is unaware of the telepathic nature of his own production. To make matters worse, blind spots in the mental make-up of the therapist may make him unaware that he has played the part of the agent in relation to his patient. Needless to say, in such a case there is no one left to tell the tale of an otherwise highly suggestive telepathic incident. This, in effect, is the reason why I believe that only a small fraction of the telepathic incidents occurring in the psychoanalytic situation or, for that matter, in everyday life, comes to our notice. The rest remains "unwept, unnoticed and forever dead"—and certainly unrecorded by the student of spontaneous phenomena, psychoanalytic or otherwise.

What, then, is the psychiatrist's—and more specifically the psychoanalyst's—contribution to our issue? As we have seen, he is in a position to study a special class of spontaneous incidents which occur under his very eyes in a more or less controlled setting, that is, under circumstances ideally suited for their closer investigation and verification. I also stated that this class of phenomena seems to conform to a structural pattern of its own and can be contrasted with telepathic incidents of the grand or heroic type described by the early workers in psychical research.

But let me emphasize once more that the underlying dynamics of the apparently trivial incidents studied by the psychoanalyst spring from essentially the same deep-seated emotional needs of the individual as the phenomena of the heroic, classical type. In fact, their study may help toward a better understanding of a number of the less dramatic spontaneous incidents contained in the older reports of Myers, Gurney, Podmore, and others. The student of spontaneous phenomena occurring in ordinary life situations comes across a great many incidents of an unassuming, trivial type. They may appear puzzling to him just because of their trivial nature. But there is little doubt that here, too, their methodical psychoanalytic investigation will reveal the same underlying dynamic factors as in the cases seen in the psychotherapist's office.

The psychiatrist's most readily available contribution to our problem lies, then, in the methodical psychoanalytic investigation of both the agent's and the percipient's personalities and mental states at the time the incident occurred. This, at least, should be the ideal objective of a planned cooperation between the psychiatrist and the worker in psychical research. I realize that it might not always be easy to obtain the consent to such a course of those involved in a given spontaneous incident. But let me remind you that the psychiatrists working in this field have already set the pattern for such a procedure. The study of telepathy in the psychoanalytic situation is based on the constant analytic soul-searching and self-scrutiny by the therapist himself. Then and only then is he able to remove his own blind spots and arrive at the proper dynamic understanding both of his own and his patient's role in an individual observation.

There is one more aspect of any given spontaneous incident where, I believe, the psychiatrist can be of help. Let us return once more to Mrs. M.'s telepathic dreams. Even though sketchy, I hope that my description of the incident has made it sufficiently clear that it was by no means confined to a two-way communication between the patient and myself. It was not just a strictly circumscribed private affair, channeled into the narrow straits of the transference-countertransference relationship. You will recall that those present on the

critical evening showed a positive attitude toward the possibility of psi phenomena and this included not only myself and the members of my family but also all the guests. In effect, it may well be that in relation to the "calling card" with the Spanish inscription which appeared in Mrs. M.'s dream it was not I, but my wife or daughter who played the part of the agent. There is a number of observations in my files which indicates that individual telepathic incidents may have been helped along by similar highly complex psychological situations. They were facilitated by the presence of a generally sympathetic atmosphere in the social setting in which they occurred.

Mrs. M.'s case illustrates one more important characteristic of psi occurrences of this type. Several members of the Medical Section of the A.S.P.R. have found that psi incidents are apt to occur in their daily practice when their interest in the subject matter of telepathy was sparked by some special occasion: by a meeting or seminar devoted to the problem, by a lecture or major controversy in which they had to defend their position or otherwise had an axe to grind. Dr. Eisenbud and I have both published several observations of this kind. One similar observation is contained in Jung's book on Synchronicity. Under such circumstances psi incidents are likely to occur in little droves and clusters over a period of days or weeks. They often show a peculiar interlacing pattern of telepathic dreams or other psi incidents which sweep across the boundaries of the doctor-patient relationship and involve several patients or other persons outside the therapeutic situation, tied together and brought into focus by the therapist's personality.

What, then, is the relevance of this type of observation? It shows the importance of the broader, interpersonal—indeed all-encompassing—aspects of telepathy observed both in the psychotherapeutic situation and in ordinary life. It indicates that the occurrence of spontaneous psi incidents depends on a multiplicity of predisposing factors; the particular dynamic relationship between agent and percipient is only one of these factors. Another factor is the presence of a special atmosphere of approbation in a given social context or "subculture" comparable to that described by Dr. Schmeidler in her "sheep and goat" experiments.

But let me hasten to add at this point that focussing attention on the dynamic flux of the relationship between agent and percipient and the ever-changing situational factors in the existing social context shows only one side of the picture. The other side is made up of what might be described as the more or less enduring structural aspects of the personalities involved. It is needless to say that the study of these personality traits as they can be ascertained by clinical observation and through the use of various projective tests

is an equally challenging task for the psychiatrist interested in parapsychology. As you may remember, an important part of Dr. Murphy's lecture was devoted to this aspect of the problem.

Summing up, let me add my voice to those who hold that psychical research must be based on the broad study of three major aspects of both spontaneous and experimental phenomena: (1) the dynamic, (2) the interpersonal, and (3) the structural aspects.

This, I believe, defines in at least a tentative way both the scope and the limitations of modern dynamic psychiatry in the study of spontaneous phenomena. But after all this has been said, I feel it is high time for the psychiatrist to stop weighing and measuring, analyzing and dissecting, his own observations. If he does not, I am afraid he will soon run out of the very phenomena he is anxious to analyze. And those left will in the meantime have certainly lost their spontaneity.

Precognition: An Analysis, II

W. E. COX

Subliminal Precognition

The kinds of precognition we have thus far considered are, for the most part, those which have entered the consciousness "by gleams and flashes," leaving some degree of evidence of their presence. ESP-card and other laboratory experiments, however, have shown that with humans it can occur and be measured without their "hits" (or "misses") ordinarily making any conscious impression whatever. One of the questions which the valuable success of statistical psi research seems to engender is whether spontaneous precognition measurably can be perceived *and acted upon* in certain situations without consciously having been observed to enter the picture at all, just as can precognition in the laboratory.

For its study, to which we now turn our attention, it is here identified as "subliminal precognition." Concisely, the working-hypothesis may be stated as follows:

In advance of a sudden misfortune, a wholly subliminal precognizance can prevent the percipient's involvement.

In the large majority of one's ordinary experiences or avoidances of misfortune, there is no appropriate way in which a statistical yardstick can be applied to test whether or not a given precognition never arising as such to conscious level *did* prevent a certain misfortune from involving that person; or, if he did "accidentally" escape the same, whether this was due to chance or to a subliminal form of precognition. There does, however, appear to be at least one category of unfortunate events which, through involvement of many people in each single case of its kind, can provide the raw data that are required. It is that of railway passenger trains involved in accidents. The procedure, as here presented, is to obtain the particular information called for below, centering around each "major" accident:

(1) The total number of passengers on the involved train, as ascertained in the last routine count made prior to the accident.

(2) The total number aboard the train on that same "run" during each of the preceding seven days, and on the 14th, 21st, and 28th day preceding, as determined by the identical routine count as in (1).

Parallel trains too far in advance of the date of the accident would allow increasingly numerous normal and ulterior events to affect the

statistical value of the quantities of their travelers. The 8-day and the 5-week records here described are arbitrary limits. All necessary data are gathered and kept on file for several years by a good number of railroad companies.

In a study of this sort, sources such as ship sinkings are too infrequent, and the demand for ship passage too often exceeds capacity. Nor are bus and subway accidents, etc., appropriate, as accurate population data are not customarily recorded. (*In re* plane accidents, however, see p. 107.) Only the railroads can make any variation in the capacity of a "single carrier" by increasing or decreasing the available space (cars) according to demand.

In an exploratory application of this procedure, reported on the following pages, the stipulated count in each run included all passengers present at the time of the counts, separately in Pullman and in Coach, regardless of whether all were aboard at the place of the accident (in case the latest official count had preceded it by an appreciable distance), and whether injured or not. The definition of "passengers" (*in re* the inclusion of "pass-" and "dead-head-" or just "paying-passengers") was of course constant for the daily run of any train, as was the place where all its relevant counts were made. In this research an arbitrary minimum of 10 injuries per accident was set in advance.

The source of information which was used is the United States Interstate Commerce Commission's annual lists of all railway accidents on "Class I" Roads, and of the numbers of killed and injured passengers (Statement No. M-400), for the years 1950-1954. During 1953-55 there ensued correspondence between this writer and a Vice-President or Traffic Manager of each Road contacted.[1] Some Roads were unable to furnish the desired data; either because of their not maintaining them (assimilably or at all), or for reasons of policy.

The entire research was conducted for Coaches and Pullmans (or "sleepers") separately, for two reasons: (1) Most Pullman figures are not maintained by the individual Roads, but are recorded by The Pullman Company of Chicago, to which I am indebted for berth-passenger populations at the station nearest above the scene of most accidents involving Pullman cars; and (2) Pullman passengers customarily book passage in advance, while Coach passengers do not, and hence the former are (*a*) less likely to cancel them for superficial reasons (e.g., those instigated by subliminal precognition), and (b) more likely to order the original booking several days

[1] Appreciation has been expressed to the Railroad Companies listed in Tables 1 and 2, *et al.*, and to the Association of American Railroads and The Pullman Company, for their cooperation.

earlier when psi theoretically might have a weaker effect upon one's travel plans.

All of the Coach passenger counts represent those at each Road's nearest "check-point" some distance above the accident. The distances varied widely; but since all figures for the daily runs of a given train were obtained from the same point, its distance from the accident is relatively immaterial. The theoretical (and actual) error in the counts, resulting from this interposition of distance, obviously would tend to be "on the safe side" as far as the purpose of this investigation is concerned. There are also, of course, chances of other inaccuracies on the part of the railroads, from whose voluminous records the population data were extracted; but such can hardly be presumed particularly to have "favored" the working-hypothesis.

Results

Of the 131 principal ("Class I") railroads in the United States and Canada, 35 were approached; viz., those having one or more accidents since the beginning of 1950. Twenty-eight sets of 11-day data were received, 11 Coach and 17 Pullman. (There was also one "combined" and one "unfinished" set which were thus unusable. Train #2960 had only partial operation.) Pullman and Coach data were analyzed separately, even when both comprised one "train," for reasons already given.

Table 1-*B* shows all Coach-passenger totals on the appropriate dates for each of the 11 accidents, and Table 2-*B* all Pullman berth-passengers in 17. In both tables Column "D" represents the number aboard on the day of each accident, as recorded at the customary check-point. "D-1" signifies the identical run the day before, "D-7" that of one week before, etc. Trains with least passengers aboard on D-day are starred, and in this study constitute a full "hit"; i.e., a case wherein (for the 8 consecutive days) no other run of that train carried as few or fewer passengers. These 8 runs of one train thus comprise one "trial," if all-Pullman or all-Coach, and two "trials" (one listed in each table) if it was a combined train. So do the 5 week-apart runs (on a weekly basis, referred to below), which are listed in Table 1-*A* and 2-*A*.

The tables are inserted to enable the reader to have an over-all picture of the findings, even though the total quantity of data obtainable is regrettably small. From both Tables 1 and 2, however, interesting results are apparent, and are in accordance with the basic hypothesis. Chance-expectation (c.e.) is 11/8 for the 11 Coach "trials" in Table 1-*B*, but the hits as defined are practically three times this amount. For "Pullman trials," c.e. is 17/8, and the hits

TABLE 1

COACH-PASSENGER POPULATIONS ON STATED RUNS OF 11 TRAINS INVOLVED IN ACCIDENTS

Line	Train	Date of Accident	*A* (by Weeks) Populations					*B* (by Days) Populations						
			D-28	D-21	D-14	D-7	D	D-7	D-6	D-5	D-4	D-3	D-2	D-1	D
Boston & Maine	#60	5/7/53	32	33	32	36	28†	36	83	53	44‡	34	34	32	28*
"	#1115	12/18/51	166	179	182	182	203	182	163	173	166	173‡	173‡	151	203
Canadian Pacific	#2960	2/21/53	—	—	105	123	126	123	85	62	54	46	46	80	126
Central Vermont	#332	7/15/51	25	40	187	173	147	173	131	94	75	124	140	187	147
Chicago & East. Ill.	"Georgian"	6/15/52	54	53	55	35	9†	35	70	62	48	53	60	68	9*
Chi., Mil., St. P. & Pac.	#15	12/15/52	66	91	69	150	55†	150	136	100	120	87	118	86	55*
Louisville & Nashville	#99	2/2/52	128	107	63	77	85	77	110	33	48	51	52	133	85
New York Central	#5	3/27/53	61	119	97	109	55†	109	84	161	49	84	86	81	55
"	#12	3/27/53	45	94	98	86	109	86	84	185	66	65	60	82	109
"	#27	10/4/50	150	94	86	57	53†	57	93	87	79	90	62	59	53*
Western Maryland	#2	2/14/51	3	5	5	6	9	6	7	2	10	7‡	7	6	9

* Accident ("D") day population was the lowest of the 8 consecutive days recorded.

† Accident-day population was lower on that week-day than on the same day of any of the 4 preceding weeks.

‡ No Saturday and/or no Sunday schedule. Each number so indicated is thus artificial, and equals the cross-sectional average.

TABLE 2

PULLMAN BERTH-PASSENGER POPULATIONS ON STATED RUNS OF 17 TRAINS INVOLVED IN ACCIDENTS

Line	Train	Date of Accident	*A* (by Weeks) Populations					*B* (by Days) Populations						
			D-28	D-21	D-14	D-7	D	D-7	D-6	D-5	D-4	D-3	D-2	D-1	D
Atlantic Coast Line	#2	4/20/53	49	57	58	53	52	53	50	55	52	54	41	45	52
Atchison, Topeka & Santa Fe	#2	9/22/54	79	86	71	73	48†	73	78	50	71	77	75	66	48*
"	#4	9/6/54	14	11	14	11	3†	11	11	14	6	12	6	8	3*
"	#15	11/13/53	50	54	42	55	63	55	61	50	38	49	70	49	63
"	#19	8/22/54	36	54	45	43	35†	43	55	44	51	58	42	52	35*
"	#20	11/24/54	54	47	54	32	40	32	41	82	46	36	60	37	40
Chicago & East. Ill.	"Georgian"	6/15/52	128	49	73	106	159	106	56	49	45	55	83	89	159
Chi., Mil., St. Paul & Pacific	#15	12/15/52	34	45	35	69	46	69	41	35	53	63	50	29	46
	#16	5/31/53	20	20	15	18	19	18	25	15	13	16	24	13	19
	#16	9/5/54	36	45	30	28	42	28	36	53	55	32	62	56	42
Kansas City Southern	#1	8/10/51	9	13	16	30	12	30	30	13	15	12	20	16	12(*)‡
Louisville & Nashville	#99	2/2/52	33	22	47	66	25	66	37	36	33	49	62	56	25*
New York Central	#5	3/27/53	45	33	54	48	31†	48	23	43	35	26	31	44	31
	#12	3/27/53	65	73	88	103	96	103	49	129	100	54	69	73	96
"	#27	10/4/50	140	144	111	129	96†	129	159	143	135	109	95	77	96
Pennsylvania	#17	9/6/53	8	13	15	16	11	16	9	16	13	12	20	15	11
"	#173	1/15/53	169	68	57	182	161	182	132	114	56	196	176	180	161

* Accident ("D") day population was the lowest of the 8 consecutive days recorded.
† Accident-day population was lower on that week-day than on the same day of any of the 4 preceding weeks.
‡ Train #1 is a "half-hit." See Table 3.

are double this quantity. If these few trials be any criterion, and if we may for convenience inspect them just as one would ESP-card test results, it easily may be surmised what degree of signifiance might be attained were much more similarly suggestive data available.

The inclusion of Columns D-7 through D-28 enable comparison of the D-day populations with those of the *same* week-days for a month preceding. There were 10 such hits, as indicated in Tables 1-*A* and 2-*A* (about twice the pooled expectation).

The statistical method used in analyzing the railroad data is that of the binomial. In view of the impossibility of knowing specifically what nonrandom factors other than psi affected the number of passengers aboard a train during any run, it seems reasonable to assume that each one of the dates considered is equally likely to be that having the smallest number of passengers for a given train. This assumption immediately leads to the binomial distribution.[2]

The critical ratios derived from Tables 1 and 2 are assembled in Table 3, together with other data. Either the daily or weekly Coach analysis (from Table 1) shows a marginal significance of 2.4 CR. The Pullman results are less significant, however, as had been hypothesized. ("Correction" of P by doubling its value is necessitated by the originality of the research effort.) Also in accordance with anticipation are the CR's of the pooled findings, which show the "by days" (from Tables 1-*B* and 2-*B*) to exceed the "by weeks" (from Tables 1-*A* and 2-*A*).

TABLE 3

SUMMARY OF RESULTS, FROM TABLES 1 AND 2*

Train Category	C. E. ($= \frac{\text{trains}}{\text{days}}$)	Total "Hits"	Dev.	CR	Corrected P-values
Coach, by Days	11/8	4	+2⅝	2.4	.016
Coach, by Weeks	10/5	5	+3	2.4	.016
Pullman, by Days	17/8	4½†	+2⅜	1.7	.088
Pullman, by Weeks	17/5	5	+1⅗	1.0	.317
Coach & Pullman pooled, by Days	28/8	8½	+5	2.8	.004
Coach & Pullman pooled, by Weeks	27/5	10	+4⅗	2.2	.028

* A single "trial" consists of the whole order of runs of a single train. A single "hit" consists of each train in whose order of runs the D-day population was the least.

† Where a D-day population was exactly the same as a prior day's, this is classed as a "half hit" for reasons of convenience.

[2] Appreciation has been expressed to Dr. T. N. E. Greville, a Statistical Editor of the *Journal of Parapsychology*, who was consulted *in re* this treatment of the data.

In this interpretation, the entire data are tantamount to less than *two* "ESP runs." Computation of CR's through tabulation in terms of actual populations instead of relatively by trains per se is not appropriate, due to distortion that may be caused by the relatively great variability of any individual total (as might occur when, say, members of a Sunday School outing augment a normal population).

There well may have been one other of the 8 consecutive days when a given train's population was by chance less than D day's, even when psi did reduce the latter. Hence it is not illogical *ad hoc* to observe that "by days" the D-day figure is *either* the least or next to the least for 5 Coach and 6 Pullman (pooled c.e. is 7); and "by weeks" it is least or next to least for 5 Coach and 10 Pullman (pooled c.e. is 10.8).

Discussion

Though their small quantity restricts the significance of the present data, discussion of these results is given in order to enable a fuller consideration of the working-hypothesis, in this effort to illustrate what appropriately may be termed "applied psi." A number of tentative conclusions may be drawn from them, presuming that corroboration of the exploratory findings ultimately ensues (within a country whose trains are adequate enough to avoid habitually being filled to capacity).

Hypothetically, there is a much larger quantity of cases of accident-avoidance, *et al.*, that stem from subliminal precognition than of those which result from conscious ones. Psychological studies specifically have shown that not only do we have an "unconscious mind," but that we also possess various levels of unconscious mental activity which influence our behavior and which are only subsequently in untraceable manners revealed to the conscious. Perhaps the seat of these unconscious cerebrations has closely associated with it a sort of "subliminal pan-awareness" which can possess information of relatively imminent "dangers" without any actual picture depicting a consciously comprehensible vision of the specific misfortune that may be (or, shall we say, "otherwise may have been") involving us. Nor can the precognized imminence of misfortune be presumed to occur only to, or predominantly with, that portion of the would-be travelers whose fate would have been personal injury or death: others who could have experienced the subliminal precognition are those whose journey the annulment of the ill-fated train would have undesirably disrupted.

Perhaps it should be stated that consciously realized precognitions, of which many dramatic experiences never reach the annals of psychical research, indeed may have accounted for *some* train can-

cellations; but there is no reason why we should presume precognitive psi thus consciously to have been perceived by any large percentage of the so-called "would-have-been" travelers. A word might also be added concerning "chronic masochists" who actually may have *boarded* the doomed train as the result of subliminal precognition of its misfortune. Those would seem to be quite scarce, in comparison, if the trend of these findings is any indication.

"If only the engineer had been able so to perceive and prevent the entire accident," one might ask. The answer to this, no doubt a redundant one, is that such accident may well have been "the one time" that subliminal psi failed him—with his prior "subliminally-precognitive avoidances" of train misfortunes having come to pass unnoticed.[3]

There are in the literature many illustrations of subliminal precognition wherein the percipient's psi-reactions were *only subsequently* recognizable as such. Reference is given to five which happen to concern railroads.[4]

A different case, but one which likewise illustrates a welling-up of subliminal precognition into the conscious level, was experienced by a businessman, Wade Stevick, of Southern Pines, N. C., with whom I am personally acquainted, and was related to me at first hand.

> There were a number of circumstances just prior to my mother's sudden death which seem to me providential. The previous day I had made several collections, but for some reason I had kept in my safe that day much more money than *ever* before. (I usually paid all bills by check.) I had finished to a quite unusual degree all carpenter and repair work, procured ample necessities, and, to make a summation, there was not one

[3] This could be true in normal courses of events as well. In such cases, "subliminal psi" would have its effect upon what we take to be purely conative actions, e.g., automobile driving, business administration, rearing of children, and multitudes of our "human needs," wherein minute variations of a "would-be" course of direction or action continually can be resulting in very great reductions of the probability of our encountering such misfortunes as otherwise might have been our fate. It is of no small consequence to note the general degree of attribution of the same to divine guidance (cf. Ps. 91: *passim*) and to faith (see Heb. 11:1, 8-11, 32-34). In fact, do not some people seem conspicuously to have gone through life experiencing a string of "lucky breaks" which repeatedly pulled them through physical dangers? Historians, both ancient and modern, have given passing reference to such characters (and likewise to their antipodes).

[4] *The Annals of Psychical Science*, Vol. 8, 1909, pp. 148-151 and pp. 311-313; and *Proc.*, S.P.R., Vol. XI, 1895, pp. 416-417, pp. 419-420, and pp. 559-560. The two in the *Annals*, reported by Cervesato and G. Elliott, concern accidents which ensued; and the last three, reported by Myers, concern accidents that were prevented. But Myers' first two are among cases he hypothesizes as possibly due to sensory hyperacuity.

detail left undone which would have caused someone else to do anything unusual in connection with my business while I was away.

About 7:30 P.M., Saturday (Dec. 30, 1944), a telegram revealed that my mother, at her home in Ohio, had dropped unconscious (never revived) from a cerebral hemorrhage. This caused me to be away from my business until January 10.

In this instance (which was not the only one he experienced), Stevick's failure to precognize the actual fatality as such, which itself appears never to have risen above a subliminal level, differs from the railroad in only one basic respect: his case is, in a word, an illustration of "psi-causation," while the railroads were cases of "psi-aversion." In the latter, the abortive measures by would-be travelers may have been prefaced either by basically similar causative thought and action or by less definitive varieties. Such, as it were, would be comparable to what earlier has been described as "nipped in the bud" conscious precognition, since the involvement was "nipped" but not the wreck itself (see Section I, pp. 50 and 57).

Although railroad accidents are incorporated in this initial study, that particular source of raw data may not comprise the most appropriate channel of inquiry. Other specific fields might more adequately support the hypothesis through more easily accessible data. Among those which perhaps are suitable for studies of this kind, but which have not been investigated owing to procedural difficulties and personal limitations, is, e.g., that of the air travel and/or of airline passenger cancellations. It may be that the number who actually are aboard a scheduled plane generally is too nearly commensurate with its limited seating capacity, naturally enough; but by observing personal *cancellations* initiated by would-be passengers (and disregarding any seats that are hastily "resold") for planes involved in accidents, etc., the objective might be attained. The relative amounts of travel insurance purchased just prior to flights of ill-fated planes may be especially worthy of consideration. Other possible sources might be those of hotel fires (when great enough to cause evacuation), and of hail damage to tobacco farms. In the latter, subliminal precognition could result in the purchase of hail insurance by a larger percentage of farmers who subsequently suffered hail damage than by those who did not. One difficulty in this approach, in addition to the factor of precedent or habit, is the widespread absence (in the United States) of accurately assimilable records of the number of non-insured farms whose tobacco was not damaged; but it is listed here in order to familiarize the reader with potential sources of data, and with hindrances that might not be avoidable. Perhaps, nevertheless, in other countries quite different situations

may prevail, making more feasible a procedure involving (and/or not involving) Acts of God. If, however, a carefully selected field is found wherein advance scientific predictions *cannot* be made (perhaps the hail, e.g.), the findings of a correlative study of total annual insurance *vs.* total subsequent loss per insured acre to policy holders, for given years, may yield some evidence of this sort.

There also is a novel and relevant experiment I wish to put forward. It is that of assigning insects to sudden death if, in their movements, they should violate a certain place-time edict. Specifically, one quarter or some other fraction of an enclosed surface upon which a certain number of ants, e.g., freely roam, could be designed to electrocute such as are not traversing elsewhere at that instant. The quadrant and instant would have to be randomly selected, of course, except when the machine is used for such other *non*-precognitive tests that (presuming psi in insects) readily may suggest themselves.

The extent of the basic existence of apparent precognition suggested by the railroad research may not itself be held to be very limited just because otherwise mortal chaos conceivably would result. The delimiting factor may be more likely the *rise* of this subliminal precognition into the supraliminal, or unconsciously into conation—suppressed perhaps by a Freudian "censor," or as implied in Bergson's "inattention to life" concept. It is not my purpose to discuss the logic or causation of precognition, but rather to rationalize the possible effect of subliminal precognitive knowledge upon ensuing conscious actions; and, where possible, to point out supporting evidence of this.

Evidence and theory of the acquisition of precognitive knowledge, which admittedly is one of the most difficult problems facing present researchers, already have been given considerable space in psi literature. It is my hope that the difficulties attendant upon such efforts, if only to a modest degree, may be made less insurmountable through consideration of the observations herein set forth. Through field investigations of subliminal precognition we can be hopeful of attaining a deeper understanding of its general dynamics, and of the integral part which it long may have been playing in, literally, the very ordering of our daily lives. As psi is more deeply rooted in our mental organization than can readily be indicated by the "non-subliminal" surface-phenomena we recognize today, such investigation as the above, to which I think Rhine's generic term of "psi-control" is applicable, may be one of the "broad lines of method [which] would lead to a full study of the natural manifestations of

psi (and anything resembling it) and thus to learning everything possible about the conditions that hinder and facilitate its operation."[5]

The railway-accident approach should point the way to a number of methods of *public verifiability* of what I have presumed to term "applied psi." Those which have been mentioned may comprise collectively just one category of several involving subliminal precognition; for it is just as logical for such a faculty to expedite our general advancement, e.g., instead of only to minimize our physical dangers. Such of these particular psi capacities as can be subjected to verification would be included, of course, in the classification in Section I of this paper (p. 57) under "Type I—Beneficial," as its "subliminal" adjunct.

Such orders of the effects of psi upon any and all creatures are fairly readily conceivable. The possibilities of our recognizing and measuring one or more of these subliminal orders, however, is what this part of the present paper is intended chiefly to ascertain.

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[5] J. B. Rhine, "Editorial: Some Considerations of Methods in Parapsychology," *Journal of Parapsychology*, Vol. 18, 1954, p. 80.

Box 936
Southern Pines, N. C.

Psychic Healing in Organic Disease

FREDERICK W. KNOWLES

The main experiences to be presented here date from times before I had the opportunity to study medicine, and in fact before I had attained as much as a school certificate, as my schooling was irregular owing to my parents' travels.

In India twenty years ago, I became acquainted with an old Parsee gentleman, Mr. A. F. Khabardar, best known as a poet. In his younger years he had achieved a considerable reputation in North India as a healer, and of his successes I had had eye-witness accounts.

In his youth, the poet had lived for a time with a cave-dwelling yogi, who imparted to him certain secrets of a little-known aspect of yoga: treatment of disease. The yogi himself had been initiated into this with the promise never to impart the secrets except to one chosen disciple. That was the poet A. F. Khabardar. He in turn after some deliberation decided to let me be the one to carry on his secrets with the same promise.

I lived in a hut near the poet's home for some months during which he taught me, and I spent most of the day carrying out his prescribed meditations and exercises, visiting him regularly for instruction. This he gave freely without any charge or compensation, as is the rule with these yogis.

Briefly, he described a vital force, called in the Sanskrit language "prana," permeating living things, its source being the sun. The practices he taught me were intended to make me able to obtain an excess of this prana from the sun, in order to be able to release it upon patients, who would benefit by it. To do this, I was to make passes over the patient with my hands whilst willing that this prana should exude from my fingers to permeate the patient and restore him. There is nothing particularly secret about this, except that he gave me a special method by which this prana was to be controlled more effectively. I was aware of the resemblance of this with mesmerism, of which I had read a little. I was also aware that mesmerism had been discredited by medical men and replaced by hypnotism. It appeared from my reading however, that modern hypnotists were not getting effects as useful as their mesmeric predecessors. This suggested that something might be gained by restudying mesmerism.

Returning to England, I obtained access to the British Museum Library and ploughed through the old literature on mesmerism, which

can still be found there. At the same time, I worked in various minor capacities in factories and found my patients mainly among my fellow workers and their relatives and friends. At first, I followed exactly the system I had learned in India, and treated any kind of disease I could get the opportunity to treat. Soon it became apparent that the most striking immediate effect was pain relief. There were a variety of other apparent benefits, but they seemed more difficult to assess and therefore I decided to confine myself mainly to pain relief. A. F. Khabardar, my teacher, had told me that this treatment always relieved pain in his experience, but I regret to say I had a few failures. Fortunately they were in the minority, otherwise the discouragement they always gave me would have brought my efforts to an end.

Among my earliest patients I remember particularly a lady who complained of severe pain in one shoulder and arm since several weeks. It was a steady ache, aggravated by movement. Her arm was in a sling. The doctor told her she had neuritis, and she claimed that she was never free from pain, that certain tablets gave only partial relief and that various hospital out-patient treatments did not help. After twenty minutes of my treatment she declared spontaneously "It's gone," and moved freely. The pain returned the next day, however.

An elderly Army major had cancer of one parotid gland with facial palsy on that side and a large fistula, and he complained of severe facial pain, only partly dulled by drugs. During a few minutes of my treatment he became completely painless, he said, for the first time in many weeks. He went to the hospital for radiotherapy weekly, and during that therapy the pain came back. It was relieved again by his visits to me after each such treatment for several weeks. He was temporarily much improved in spirits during this period. Then his condition deteriorated, he visited me no more, and died. His wife who had neuralgia accompanying erysipelas of the face, was made free of pain by a few minutes' treatment.

A woman complained of severe pain in one shoulder and arm, which was slightly swollen, a few years after mastectomy, and said the doctor's tablets once effective in relieving pain had now become ineffective. After fifteen minutes' treatment she declared herself painless. The relief lasted a day or two and was repeated similarly at intervals until she was re-admitted to hospital and I could no longer see her.

One of my employers in a factory where I worked hated dentists but complained of bad pain in the region of a decayed molar tooth. I told him it was unreasonable to ask me to deal with this; he should have it extracted. But he insisted I should try. After about ten

minutes of my efforts he declared himself painless and remained so for two days, when — again under protest — I repeated the treatment. The relief then lasted about twelve hours only. When it recurred again, my further attempt brought no relief. He went to the dentist and had it out.

A woman was said to be dying from cancer somewhere in the abdomen, in continuous pain, and lying on a water bed, not expected to get up again. After twenty minutes of my treatment she declared herself pain-free. Several days later, after repeated treatments, she got up and about the house and thought herself recovering. The improvement was maintained for several weeks and she was transformed from utter wretchedness to hopeful happiness. Then rather suddenly incontinence of urine and feces developed, she was confined to bed, and died soon after.

Though I began all this by following my Indian teachings carefully, spending some time each day charging myself with the hypothetical prana, I soon found that I had little time for these charging procedures, and that the treatments were quite as effective without them. Whenever I met with some obstinate condition that did not respond, I would perform these charging procedures again in the hope of making the treatment more effective. But I did not find it so. Thus I abandoned these time-consuming procedures.

Fortunately for my experiments, I had a patient at one time whose pain recurred with great regularity after each treatment. He said he had had one kidney removed for the stone years before, and that the surgeon now told him that the other kidney had developed a stone too, and that he was not prepared to operate on it. This patient complained of a continuous dull backache, worse on movement. He was a builder's decorator, and did ceilings. Every time he came to me he was made painless in about five minutes and remained so till he was whitewashing his ceilings next day.

This was one of the patients on whom I was able to test varieties of treatment procedure. Originally, I made passes with my hands over the patient without contact, somewhat as the mesmerists did. But I found I got the same results if I omitted the passes, as well as my teacher's special secret method, merely by effort of my will.

By that time my reading had revealed the very amazing history of the mesmerists, who obstinately refused, in many instances, to pay due attention to suggestion, and to its operation as a result of the patient's expectation. There is the story about the Marquis de Puy-

ségur,[1] to whom we owe the discovery of induced somnambulism, but who also mesmerized trees, and believed that people were relieved of their complaints when they stood under these trees. Then Benjamin Franklin devised the trick of merely saying to some people that certain trees had been mesmerized: they stood under those also and found them just as good as the ones the Marquis had mesmerized.

There is also the interesting record of Dr. A. A. Liébault[2] of Nancy, a general practitioner who gave up drugs in favor of mesmeric therapeutics. During many years of clinical experimentation in his practice, he eventually showed the animal magnetism of the mesmerists to be an illusion, and found that suggestion was the essence of the process. Many of these experiments were done in collaboration with Dr. H. Bernheim. Many years before that, however, Dr. Alexandre Bertrand, also in France, had already explained mesmeric phenomena by suggestion.

It appeared then, that I had somehow acquired a reputation for relieving pain, and that patients coming to me, whatever their stated beliefs (these did not seem to matter), were in circumstances arousing some degree of expectation of relief when in my presence. One might say that it was an implied suggestion, as I did not usually employ verbal suggestion of relief. If this were true, then my patient with backache attributed to his kidney stone should have become painless even if I omitted my effort of mind or will.

I tried it, having made sure in previous treatments to accustom him to get his relief when I was well outside his field of vision. He sat in an armchair and I stood four to six feet away behind him. He thus had no means of seeing my facial expression, e.g., to gauge what I was up to.

Without my mental effort he then obtained no pain relief whatever, even if I waited very much longer than the duration of a usual treatment. He became rather discouraged and said he felt the treatment was no use any more. After that, a brief spell of the usual mental effort brought the usual relief, however.

On the other hand, I found also that my effort of mind by itself was not enough: when I treated a patient in pain without his knowledge of my intention to help him, the treatment nearly always failed. It seemed that by themselves, neither the patient's expectation, nor my mental effort were adequately effective. Both together were needed for the high proportion of successes to which I was accustomed.

[1] G. Murphy, *Historical Introduction to Modern Psychology*, Harcourt, Brace and Co., New York, 1929.

[2] A. A. Liébault, *Therapeutique Suggestive*, Paris, 1891.

In the factory I saw many minor ailments. The great majority of headaches were relieved by this method, as also were sprained ankles, tenosynovitis (inflammation of a tendon-sheath), and some cases of dysmenorrhea. So-called sciatica proved a freakish thing: sometimes it responded in a spectacular way, sometimes not at all. Backaches were similar. Some of the most useful results were obtained with monarticular osteoarthritis (affecting one joint only). Some joints that had been painful for years were reported to have become painless, and this painlessness, if not immediately lasting, could be made so by a few repeated treatments, as far as I could judge over a period of several years.

My first failures in pain relief were disheartening to me, but gave me food for thought: The first headache that was unrelieved was odd in that the patient had had it regularly for about a year, every Monday morning upon awakening after his weekend in the country. It was not influenced by acetylsalicylic acid either. Another early failure was a woman complaining of continuous abdominal pain since three years, who had had repeated laparotomies, and who collapsed from time to time in the street with the cry: "Cut me open! I'm on fire!" When I had treated her, she declared her pain was worse.

Just then a doctor sent me to a patient of his, a wealthy old lady living in a private suite with full-time nursing in Harley Street. She told me she had arthritis in all her joints and had terrible pain on every movement. When I had treated her, and asked what the result was, she also said "worse," and I was puzzled as I left when her nurse told me that she moved her joints with remarkable freedom whenever she did not think she was being observed. These failures were followed by a failure with pain in cancer of the large bowel and one in a painful edematous arm (swollen with excess tissue fluid) following mastectomy, which were much more discouraging to me than the previous two. On the whole, the successes greatly outnumbered the failures, however, and I carried on.

At one time I felt that some type of laboratory investigation was needed, and began experiments with artificial pain. In spite of much enquiry and trial, I found no means of inducing a steady, unchanging pain with sufficient repeatable reliability to be useful. Ischemic pain (due to local anemia), when induced with practically complete vascular occlusion, was not susceptible to my attempts to relieve it. Other laboratory investigations also gave negative results, and have already been reported.[3]

[3] F. W. Knowles, "Some Investigations Into Psychic Healing," JOURNAL A.S.P.R., Vol. XLVIII, 1954. pp. 21-26.

It then happened that I acquired the means enabling me to complete my schooling with the matriculation and to study medicine, which I did in the hope of returning to these problems later. But as an assistant in a very busy general practice in London under the National Health Service, I found hardly any opportunity, and when I went to teach anatomy in one of the Australian Universities, I found myself too isolated from the clinical side of medicine to revive the old interests. For this reason I returned to India in 1953, and in general practice in remote villages I was again able to try psychic pain relief with results much as years before in England.

In the light of recent advances in parapsychology and of my own experience with psychic healing, which might be called a mesmeric type of psychotherapy, it appears now in retrospect that in the conflict of ideas during the last century, between mesmerists and their animal magnetism on the one hand, and suggestionists and hypnotists on the other, the errors were not confined to one side. The mesmerists claimed not only a satisfactory method of surgical anesthesia, but a therapy that included organic disease in its range of utility. The suggestionists were clever in exploding some superstitions that belief in animal magnetism had given rise to. Unluckily they were blind to the occurrence of phenomena that did not fit their own hypotheses, such as the induction of somnambulism at a distance without the patient's knowledge, and the occurrence of extrasensory perception, or telepathy. In the course of time, the suggestionists appeared to become more and more negative in their outlook and in their experience: soon they were unable to obtain surgical anesthesia except in a very small minority of people; soon they taught that their hypnotism could benefit only psychogenic disorders, or those organic ones in which there was a large psychogenic overlay. Somehow it then became obvious that their hypnotism was not much good in psychogenic disorders either, and it almost dropped out of medical practice.

In experimental parapsychology[4], it has been noticed that the ability to succeed, e.g., in extrasensory perception, is positively correlated with a belief in this possibility. Those who believe it impossible usually fail. I suspect there is a parapsychological factor in the induction of hypnosis also, in addition to suggestion, and though it has been found that a very few people can be deeply hypnotized by the verbal suggestion from a gramophone record, in the absence of any hypnotist exerting mysterious powers, nevertheless I have found in my own experience, and others have found also, that the ability to induce deep hypnosis in a high proportion of people is a precarious thing, and is

[4] J. B. Rhine, *The Reach of the Mind*, William Sloane Associates, Inc., New York, 1947.

very easily diminished if the operator adopts a critical attitude. To put it another way, enthusiastic confidence seems to be a factor favoring success.

It is easy to argue that if only credulous enthusiasts are good at all this, we can afford to ignore them. But it is not so simple. Perhaps my own experience will illustrate:

A few years ago, as a medical student, I tried to repeat James Esdaile's method of mesmerism, exactly as he described it,[5] to induce surgical anesthesia. I did not succeed in any degree whatever. However, I had then become very critical of hypnotic phenomena in general: I had successfully demonstrated the "cataleptic bridge" in the unhypnotized, as well as some common deceptions in tests for anesthesia. Years before, when I had a less destructively critical outlook, I had succeeded in obtaining deep hypnosis unaffected by the most painful stimuli I could produce without injury. Sheer gullibility is not the explanation.

It appears to me that in mesmerism, hypnosis, psychic healing, and the like, there is another factor of importance in addition to suggestion as generally understood. This other factor enhances the effect of ordinary verbal or implied suggestion and may occasionally be adequate in its absence, as, e.g., in somnambulism induced at a distance. It is a parapsychological factor, related probably to extrasensory perception on the part of the patient, or — less probably — to psychokinesis on the part of the operator. This idea is not altogether new: C. M. Barrows[6] described his practice of "silent suggestion," and explained it by telepathy sixty years ago. My hypothesis, in brief, is that a physician's thoughts can affect a patient by a parapsychological process.

According to this hypothesis, we might expect to observe that in addition to the patient's expectation of benefit, the doctor's, nurse's, physiotherapist's, or other attending person's attitude of mind can affect the patient and his disease. If a physician administers a treatment, e.g., a drug injection, with the firm conviction that he is putting a potent agent into action, and with the confident expectation of remarkable benefit to the patient, then that drug injection, whatever its intrinsic merits, is more likely to be helpful to the patient than if it had been administered with indifference or even the physician's thought (albeit well-concealed): "this stuff isn't the slightest use, but what else can I do." This hypothesis may illuminate certain

[5] J. Esdaile, *Mesmerism in India*, H. Ballière, London, 1845.

[6] C. M. Barrows, "Suggestion without Hypnotism," *Proc.* S.P.R., Vol. XII, 1896-97, pp. 21-44.

puzzling experiences in clinical medicine, viz., that certain physicians obtain good results with some favorite remedy of theirs, whilst others who try the same find it useless. One may observe this both in orthodox practice, and in those regrettable cases where regular medical men, unaware of possible mental factors, abandon scientific medicine and go over to homeopathy, osteopathy, and the like.

If we entertain the hypothesis that the physician's expectation profoundly affects the patient's welfare, we are faced with the need of enabling physicians to exert mental influence in this way. Experimental parapsychology has shown that those who do not believe a parapsychological process possible, tend to fail if they attempt it. How then may one acquire the necessary confidence on which results seem to depend? I have not enough experience to answer this question with assurance, but think that in my own case, I first obtained the necessary confidence by imagining that I possessed a potent secret method. When experience showed the secret part of it to be without intrinsic value, I had seen enough remarkable results to maintain confidence. From time to time individuals have asked me to instruct them to relieve pain as I did; this occurred on three occasions, and I explained the method I used. Two of these men appeared to be successful when they tried it, the third did not, but neither did he follow the simple instructions I gave him, for I found him appealing to a deity to cure his patient instead. All three had seen me at work in relieving pain; I suppose the demonstrations I gave them could have inspired the necessary confidence, when I assured them that I possessed no special supernatural gift.

Concerning the nature of the effort of mind or will that is required on the part of the physician, I have described this in detail on previous occasions[3,7], but should add that whatever concentration, meditation, or effort of will is used, it probably depends essentially on establishing in the physician's mind, a vivid expectation of benefit to the patient. Any method that establishes such an expectation in the physician's mind may be adequate, as far as I know. Possibly that expectation telepathically enhances the patient's own expectation to a level sufficient to secure benefit.

For some years I have attempted in vain to obtain the cooperation of members of the medical profession in order to explore the scope of psychic healing, and its nature, more adequately. This paper is largely based on my observations as a layman, and does not meet the requirements of clinical science. I should have preferred to keep

[7] K. Richmond, "Experiments in the Relief of Pain," *Journal* S.P.R., Vol. XXXIII, 1946, pp. 194-200.

these observations to myself and to await announcement of the findings from a properly designed research project in this field. As I see no prospect of such a project in the near future I now present this account in the hope that parapsychologists may find it of interest.

Dalhousie University,
Halifax, Nova Scotia.

The Society for Psychical Research Prize Essay

The excellent response to last year's essay competition, won by Mr. William E. Cox, Jr., has encouraged the donor to repeat his offer, and a prize of fifty pounds (or $140) will be awarded for an essay of less than three thousand words on one of the following topics:

(*a*) The best program of research for the first one hundred hours of work with the next successful card-guessing subject discovered: the design should be supported by argument.

(*b*) The best discussion of the logical and/or scientific implications of precognition, assuming this to be established statistically from the work of card-guessing subjects.

Entries should be submitted under a pseudonym together with a sealed envelope containing the entrant's actual name and address. They should be typewritten in double spacing, on one side of the paper only, and should be sent not later than October 1, 1956 to The Secretary, Society for Psychical Research, 31 Tavistock Square, London, W.C. 1, England. Entrants may submit more than one entry. The judges will be Dr. D J. West, London psychiatrist and Hon. Research Officer of the S.P.R., Professor H. H. Price, Wykeham Professor of Logic, Oxford University, and Mr. Denys Parsons, M.Sc., Cambridge University, and a Civil Servant in the Scientific Branch of the Patents Office, with assistance from Dr. S. G. Soal.

Paris Conference on "Psychology and Parapsychology"[1]

An international conference on "Psychology and Parapsychology" was held at the Abbey Royaumont (near Paris) from April 30 to May 4. The conference, organized by Mr. Robert Amadou (Paris) and Dr. Emilio Servadio (Rome) in cooperation with the Parapsychology Foundation of New York, brought together parapsychologists, psychologists, and anthropologists from several European countries and from the United States.

The Royaumont symposium was the fifth in a series of international meetings which began in 1953 with the First International Conference of Parapsychological Studies at Utrecht, The Netherlands. The Utrecht meeting was followed, in 1954, by two conferences at Saint Paul de Vence, France; these were devoted to the relations between parapsychology and philosophy, and parapsychology and unorthodox healing. A year ago, a Conference on Spontaneous Phenomena took place at Newnham College, Cambridge, England.

The symposium on "Psychology and Parapsychology" concludes the series of conferences envisaged at the Utrecht meeting. All five conferences have been generously supported by the Parapsychology Foundation.

The Conference opened with an address by Mr. Amadou, who expressed hope that the meeting would help toward "new strides in the knowledge of man." He said that "none of our approaches are truly sufficient, and an all-embracing effort is therefore necessary; distinctions between the various scientific disciplines must be regarded as arbitrary, provisional, and temporary—the need now is for unification and harmony."

Dr. Servadio recalled that last year's Cambridge Conference had illustrated the progress made by psychic researchers in their own field. However, he said, "parapsychology now demands increasingly the contributions of other disciplines, in order to fulfill its scientific potential."

The meeting received a message from Mrs. Eileen J. Garrett, President of the Parapsychology Foundation, which noted that the

[1] We are indebted to the Parapsychology Foundation for permission to summarize and quote from the Report of the Royaumont Conference in their *Newsletter* for June-July. For a full account of the Conference see the *Newsletter*.

conference provided "a bridge between the historical studies of psychical research, the more modern parapsychological approach, and the exact sciences." The message observed that parapsychology "moves at a frontier point where the natural sciences, medicine, and religion meet."

Current Status in Parapsychology

The first session, held on the afternoon of April 30, began with a report by Mr. Amadou on "Parapsychology Generally," which surveyed the current status of parapsychological studies.

Dr. S. David Kahn (New York) followed Mr. Amadou with a paper entitled, "The Enigma of Psi: A Challenge of Scientific Method."

Dr. Léon Delpech (France) then presented a "Preliminary Study of Attitudes toward Parapsychological Problems," outlining approaches ranging from resistance to credulity on the part of observers and students.

Dr. Donald J. West (London) presented a paper, "ESP and Mood," prepared by him in cooperation with Mr. G. W. Fisk (England). The experiment related had covered a period of about eight months, during which a series of three cards, containing various clock faces in random order, were exposed. Simultaneously, 160 subjects in various parts of the world were asked to guess the clock faces. The results indicated that "those subjects who were most frequently in a pleasurable mood tended to produce the largest positive scores."

At the conclusion of the day's meeting Dr. Roland Cahen-Salabeille (France) presented a paper concerning a dream of possibly telepathic content.

Psychoanalysis and Parapsychology

The symposium's sessions of the second day, May 1, began with a paper by Dr. Servadio which, under the title "Psychoanalysis and Parapsychology," offered a survey of psychoanalytical studies during the past three years. Dr. Servadio specifically mentioned "the closer study which has been made on the subject of complementarity" in the depth psychological inquiry into precognitive material.

Dr. Jan Ehrenwald (New York) then presented his paper, "Telepathic Leakage and Doctrinal Compliance," which concerned itself with the problem of "whether or not telepathic leakage or psi induction is capable of influencing the making of theories derived

from the practice of psychotherapy or psychoanalysis." As telepathic leakage Dr. Ehrenwald identified "a particular form of telepathy operating between therapist and patient, or between two or more persons in general."

The conference also considered the contribution of Dr. Jule Eisenbud (Denver). His paper, "Some Notes on Methodological Problems of 'Psychoanalytic Parapsychology,'" sought to define "logical and psychological rules according to which certain selected events are relateable in the absence of any demonstrable connection of a physical kind between them." The paper specifically concerned itself with the problem of selection and the problem of correspondence of events.

Also before the conference was a paper by Dr. C. A. Meier (Zurich) on "Projection, Transference, and the Subject-Object Relation in Psychology," which noted a "partnership" of analyst and patient, where certain significant efforts are dependent upon the emotional involvement of the analyst. "We cannot," Dr. Meier stated, "do without emotions in effecting transformations of personality."

Anthropology and Parapsychology

On May 2, proceedings were devoted to the subject of "Anthropology and Parapsychology," which also formed the theme of a paper submitted by Mr. Pierre Barrucand (France). The speaker stated that "the study of the ideological frame of reference of a culture, its foundation in religion and magic, might yield data which would be of interest when correlated with strictly parapsychological information." Mr. Barrucand observed that it is "probably not due to chance" that "the first hesitating stages of psychical research and the crash of evolutionist anthropology occurred at about the same time, near the turn of the century."

Dr. Ernesto de Martino (University of Rome) delivered a paper on "The History of Religion and Parapsychology," which traced religio-anthropological events in their relation to parapsychological studies. He pointed to parallels and contrasts in development, noting that, until recently, parapsychology and anthropology had failed to find a meeting-ground upon which joint research might be undertaken.

The meeting then heard a reading of a paper on "Geomancy, Clairvoyance, and Initiation," prepared by Dr. Jean Servier (France), who had been unable to attend the symposium. The paper reflected the view that, regardless of the "technical" means used in

cases of clairvoyance, "the only important thing is the presence of a condition which is always necessary and almost always sufficient, namely, the predisposition of the subject's mind to a nonsensory perception of time and space."

The subject "Psychology and Parapsychology: The Viewpoint of a Roman Catholic Theologian" was treated by Rev. Father Reginald Omez, O.P. (France), who stated that Roman Catholic theology, "in dealing with the miraculous, rules out the preternatural origin of a phenomenon, unless it is established that no plausible natural explanation of it may be found." Among "natural" explanations in this category may be found physical, psychological, as well as parapsychological factors, such as telepathy or suggestion. Father Omez said that Roman Catholic authorities are "eager to exhaust all possibilities of natural explanation, before accepting the hypothesis of miraculous divine intervention." He added that this policy should be "eminently encouraging" to parapsychology, as it tends to "urge scientists to explore the depth of psychic life that is as yet little known, but which may explain apparently paranormal events."

Psycho-Physiology and Parapsychology

The last full conference day, May 3, dealt with "Psycho-Physiology and Parapsychology." Dr. Alain Assailly (France) delivered a paper entitled, "On Parapsychological Influences of Middle-Aged Persons on Young Sensitives," which related a number of case histories based on first-hand investigations. Dr. Assailly noted that "attraction and fear" in an adolescent may create "a state of receptivity which probably favors the relationship under scrutiny."

In the absence of Dr. Jean Lhermitte (France), his paper on "Bilocation Phenomena in Neuropathology" was presented to the conference. Dr. Lhermitte, citing literature in the field of bilocation, or out-of-the-body experiences (sometimes also referred to as "traveling clairvoyance"), concluded that "the interesting fact is that certain subjects, whose behavior and ideas are outside the field of pathology, do experience things that correspond to states described by pseudo-mystics as well as those aspiring to a higher state."

Dr. Jean Bruno (France) spoke on the subject of "Yoga and Parapsychology," outlining certain parallels between Eastern and Western researches into alleged paranormal phenomena. He reviewed recent efforts to utilize Yoga techniques as part of Western inquiries, and pointed to the need of increased exchanges of information between researchers in the Far East and Europe.

Dr. Marcel Martiny (France) then presented his paper on "Parapsychology and the Different Planes of the Unconscious." The speaker outlined what he described as various layers of personality, including the subterranean forces of the instinctive personality. He referred to the "layer of the organic unconscious," which he divided into biological, motor, and sensory groups. Above this layer he placed a plane of "traits," and finally a layer "forged by reason." According to the speaker's thesis, "psi phenomena belong to the biological order, and this includes telepathy."

Discussions followed each paper read at the Conference. The organizers of the symposium noted its aims at the outset by stating that an exchange of views would be "of equal benefit to psychology and parapsychology."

The closing day, May 4, was devoted to the drafting of resolutions.

Resolutions

Among resolutions passed by the meeting was an appeal for "renewed efforts to seek the cooperation of physicists, with the intention of exploring all avenues that could lead to a wider understanding of problems which psychology, parapsychology, and physics may have in common."

Other resolutions reflected the following five points:

1. Endorsement of the activity envisaged by the International Committee for the Study of Spontaneous Paranormal Phenomena, organized at The Hague, Netherlands, by Mr. George Zorab.

2. Recognition of the work done in recent years in the field of psychiatric parapsychology, and an expression of confidence that such work will continue in the future.

3. Endorsement of a plan outlined by Dr. Ernesto de Martino, anthropologist and lecturer on the history of religion at the University of Rome, concerning the study of paranormal phenomena in Southern Italy.

4. Endorsement of a plan offered by Dr. Alain Assailly, Paris endocrinologist, regarding the study of possible parapsychological faculties in persons following frontal lobotomy.

5. Recording of the suggestion that a multi-lingual abstracting service, in the field of parapsychological and related literature, would facilitate international cooperation.

List of Delegates

France: Mr. Robert Amadou, Dr. Alain Assailly, Mr. Pierre Barrucand, Dr. Pierre Binois, Rev. Father Bruno de Jésus-Marie, Dr. Jean Bruno, Dr. Roland Cahen-Salabeille, Mr. Jean Cavozzi, Dr. Léon Delpech, Dr. Mircea Eliade, Dr. Jean Lhermitte, Dr. Marcel Martiny, Dr. Sacha Nacht, Rev. Father Reginald Omez, Mrs. Suzanne Pacaud, Dr. René Poirier, Dr. Jean Servier, Dr. Jean Vinchon.

United States: Dr. Jan Ehrenwald, Dr. Jule Eisenbud, Dr. S. David Kahn.

Italy: Dr. Ernesto de Martino, Dr. Emilio Servadio.

United Kingdom: Dr. Donald J. West.

Switzerland: Dr. C. A. Meier.

The Parapsychology Foundation was represented at the conference by Mr. Jean Andoire and Mr. Martin Ebon acting as observers. The International Committee for the Study of Spontaneous Paranormal Phenomena was similarly represented by Mr. George Zorab.

Reviews

THE SEARCH FOR BRIDEY MURPHY. By Morey Bernstein. Pp. 256. Doubleday and Company, Garden City, N. Y., 1956. $3.75.

The author of this interesting and well-written book is a Colorado business man educated at the University of Pennsylvania, where he graduated from the Wharton School of Finance. Its curriculum apparently did not include a course in abnormal psychology, for it was not until later that, after accidentally witnessing a demonstration of hypnosis, his disbelief in the reality of this psychological state gave way. This led him to read up on the subject and to experiment for himself with hypnosis.

Some time later two convincingly precognitive dreams of his own and some other paranormal occurrences which came directly to his notice broke down the complete skepticism which he had similarly entertained concerning reports of extrasensory perception. Once more, this led Bernstein to acquaint himself with the literature of the subject. He undertook experiments in extrasensory perception with hypnotized subjects, and eventually visited Rhine's Parapsychology Laboratory at Duke University.

Still later, a chance acquaintance who turned out to be, like Bernstein, interested in hypnotism and in ESP, but who happened to know something also of oriental religions and philosophies, brought up the idea of reincarnation. To Bernstein, it was as preposterous as had been earlier those of hypnosis and of ESP; but, once more, he read up on the new idea; learned that attempts, *prima facie* successful, had been made by some hypnotists to regress their entranced subjects to times anterior even to conception and seemingly relating to earlier lives on earth; and he then undertook a similar experiment with one of his subjects, Ruth Simmons, born in 1923 in Iowa, who when so regressed in deep hypnosis took on for the time being the personality of "Bridey Murphy," an Irish girl describing herself as having been born in Cork in 1798 and — to use her own word — "ditched," i.e., buried, in Belfast in 1864.

What particularly distinguishes the "Bridey Murphy" experiments from earlier similar ones is the persistent attempt Bernstein made to obtain from "Bridey," concerning her purported life in Ireland, as many details as possible of kinds perhaps capable of confirmation or refutation, but that could hardly plausibly be supposed ever to have become known in a normal manner either to Ruth Simmons or to

himself. The book records a fair number of details apparently such, and the verifications made of some of them up to the time the book went to press.

Now, if one does not accept the wholly materialistic conception of man, but believes at all that his mind or some parts of it may exist without occupying always a living body, then the question as to the existence of that "spiritual" part of man *before* the birth or conception of his body is on exactly the same theoretical footing as the question of its existence *after* the death of that body. The only difference between the two suppositions is the purely pragmatic one that a man's future commonly interests him more than his past, and the provincial one that among us the idea of a life after death is more familiar than that of a life or lives before birth. Once this is realized, it becomes clear that the question of an earlier life, like that of an after-life, is to be decided if at all only on the basis of such empirical evidence as may be or become available. The question then immediately arises as to *what sort of evidence* would be relevant to the question of whether pre-existence, or postexistence, is a fact.

The only answer to suggest itself is that the relevant evidence would have to be of the same kinds as, and comparable in weight even if not necessarily in abundance with, the evidence on which we now decide similar but more common questions. For example (with regard to pre-existence) the evidence on which is based our present belief that we were alive and conscious twenty years ago, or for that matter, last week; and (with regard to postexistence) the evidence on which we conclude that the person who has just telephoned us really is the childhood friend he says he is, of whom we have heard nothing at all for half a lifetime.

If the evidence we have either for pre-existence or for postexistence seems to be comparable in kind and in weight to that on which we do in fact decide the questions just mentioned, then the only rational ground there could be for declining to accept the conclusion at which it points would be availability of some other hypothesis that would account equally well or better for the seeming memories of an earlier life, or for communications seemingly from the dead.

In the case with which alone the book under review is concerned — that of *prima facie* regression under hypnosis to an earlier life — the obviously first alternative explanation of the purported memories is that, in order to comply with the hypnotist's regression command, the hypnotized subject unconsciously draws upon her powers of imagination, as writers of fiction, though consciously, draw upon theirs.

This supposition, however, does not by itself account for the fact that some of the recondite details purportedly remembered in the "Bridey" case and in some others turned out to correspond to very specific facts which neither the subject nor the hypnotist seemed at all likely ever to have known.

To account for these correspondences, the only orthodox hypothesis available is that of illusion of memory — the hypothesis, namely, that what the subject really recalled were images left in her subconscious mind not by the events themselves which they depicted, but by *descriptions* of those events accidentally heard or read, perhaps in childhood. That is, recalling the images but not the manner in which she had acquired them, the subject automatically took them for memories of the events themselves.

The plausibility, or lack of it, of this explanation turns on the probability or improbability that the subject, in the course of the particular life which has actually been hers, could have heard or read of the particular events and facts later verified.

If, after due investigation, the supposition that she did hear or read of them should appear wholly *ad hoc*, or too far-fetched to be credible, then, to account for the veridicality of the details seemingly remembered, there would remain the alternative possibility that, while in trance, the subject exercises an extraordinarily far-reaching power of paranormal retrocognition which at other times is inactive. Such a supposition, indeed, or that of similarly wide-ranging telepathy, has been advanced as a possible alternative explanation of the veridical details concerning the life of persons who had died, communicated by Mrs. Piper or by Mrs. Leonard or Mrs. Willett or other mediums, and which they could not have learned in any normal manner.

Although the book thus does not prove reincarnation nor comes near to doing so, nevertheless the hypothesis of genuine memories of earlier lives, which it entertains, cannot defensibly, any more than that of genuine communications from the deceased, be dismissed *a priori*. Not dogmatism pro or con, but only evidence, is of any value.

The chief ground on which it seems to this reviewer that Mr. Bernstein's book is open to criticism is the vastness of the powers for good, and the relative slightness of the powers for ill, which it rather confidently ascribes to hypnotism. On the other hand, the second part of the book, which deals with extrasensory perception, will undoubtedly bring the facts of this field to the attention of many persons previously unaware of them.

C. J. Ducasse

Brown University

THE UNKNOWN—IS IT NEARER? By E. J. Dingwall and John Langdon-Davies. Pp. 174. New American Library, New York, 1956. A Signet Key book. Thirty-five cents.

Dr. Dingwall is well known to readers of this JOURNAL as having been for many years research officer of the American and later of the British S.P.R.; as a widely experienced investigator of psychic phenomena; as well-acquainted with the artifices of magicians; and as author of a number of books, some of which have been reviewed in this JOURNAL. J. Langdon-Davies has written some twenty-five competent books on various subjects, including scientific ones, and is Science Correspondent of several newspapers. He has an exceptional gift for presenting technical matters in language understandable to lay readers.

The book on which these two distinguished authors have collaborated has a number of outstanding merits. The style is clear and forthright; the material is interesting and well-presented, and covers all the chief kinds of paranormal phenomena. Even above these virtues, however, stands the sober, informed, and objective manner of discussion of the questions considered — a manner free alike from the unscientific credulity prevalent among addicts of the marvelous, and from the incredulity, no less unscientific, of the too numerous scientists who, emulating the astronomers who once refused to look through Galileo's telescope, today sneer at psychical research while refusing to acquaint themselves with the facts it has established.

The authors throughout are careful not to jump to conclusions, whether *pro* or *contra*, that go beyond what the evidence available warrants; but they are equally scrupulous not to stop short of the conclusions — again whether *pro* or *contra* — which the evidence possessed rationally dictates. And not only do they thus demonstrate both true scientific caution and true scientific courage, but they take pains to instruct the reader concerning practical points in method of investigation, which, if heeded, can give scientific value to the layman's reports of such paranormal phenomena as he may have occasion to observe or to investigate.

In the Introduction, the authors give the reader a general idea of what the book is about by describing briefly some ten striking and well-authenticated cases of spontaneous paranormal phenomena, and two experimental cases, of clairvoyance and precognition. Chapter I points out that neither organized religion nor Spiritualism answers adequately the questions about ghosts, telepathy, hunches, precognition, life after death, etc., that often occur in the course of ordinary conversation; and that the societies for Psychical Research were or-

ganized for the purpose of inquiring into these subjects in a scientific manner. Psychical Research, or Parapsychology, is a young science as yet, whose present need is less for theories than for solid and numerous facts; and readers who chance to have or to hear of paranormal experiences may be able to contribute useful data if they report them in the manner described in Chapter 2, "Your Own 'Supernormal' Experiences," and at other places in the book.

The third chapter makes clear what "suggestion" is, and what, in *prima facie* paranormal experiences or in reports of such, can and cannot be accounted for as due to suggestion. Chapter 4 gives a brief account of the experimental methods by which extrasensory perception is investigated in laboratories; of the outstanding facts such investigation has established; and of the implications they have as to the credibility of well-authenticated spontaneous paranormal phenomena.

The fifth chapter considers automatic writing and other forms of automatism. The reader is cautioned at the outset that, in attempting to account for the content of such automatisms as he may himself have or observe, he should — as William of Occam counselled long ago — avoid elaborate explanations if some more economical one happens to be sufficient.

The sixth chapter examines telepathy and clairvoyance; the seventh, precognition; the eighth dowsing and so-called radiesthesia; the ninth, apparitions and haunted houses; the tenth, poltergeists; the eleventh, other paranormal physical phenomena; and the twelfth, mental mediums and the question of survival after death.

Throughout, emphasis is laid on the need to assess the trustworthiness and cautiousness of the persons who report having observed paranormal phenomena; and on the importance of distinguishing sharply between what has literally been observed, and what — perhaps without adequate warrant — the facts observed are assumed to mean.

This book is likely to make more impression on readers whose turn of mind is critical and whose point of view has been influenced by contemporary science, than any of the other books known to this reviewer, designed as introductions to the field of psychical research. The authors are to be congratulated on their having written so good a book; and the publishers on having made it available at a price modest enough to allow it the wide circulation it deserves.

C. J. Ducasse

Brown University

La Tour Saint-Jacques.

We have received the first three numbers of a new French magazine, *La Tour Saint-Jacques*. It will be published six times a year by Librairie Saint-Jacques-Saint-Germain, 53 rue Saint-Jacques, Paris (V). The Editor, Robert Amadou, is the author of two recent books: *La Parapsychologie*, a general survey of psychical research, and *La Science et Le Paranormal*, a report of three conferences (1) The First International Conference of Parapsychological Studies at the University of Utrecht in 1953; (2) An International Symposium on Philosophy and Parapsychology; and (3) The International Study Group on Unorthodox Healings, the latter two held at Le Piol, near Saint Paul de Vence, in the South of France in 1954. All three Conferences were made possible through the generous support of the Parapsychology Foundation of New York.

La Tour Saint-Jacques covers a wide field including papers on history, literature, philosophy, art, and magic. Of special interest to readers of this JOURNAL is that each number of *La Tour Saint-Jacques* contains a *Bulletin* devoted to articles and reviews on psychical research. In addition to original articles by French contributors the *Bulletin* offers translations of important papers on parapsychology originally published in other countries.

The leading paper in the first number (Nov.-Dec., 1955) by Dr. Emilio Servadio reports a dream incident of an apparently telepathic and precognitive nature, involving the analyst and a patient. It is comparable to many similar incidents that have been described in the psychoanalytic journals by members of the Medical Section of this Society and other analysts. Members who wish to be conversant with these interesting reports of paranormal phenomena in the psychoanalytic situation are referred to *Psychoanalysis and the Occult* edited by George Devereux and published in 1953.

The second number (Jan.-Feb., 1956) reviews the recent attack on parapsychology by Dr. G. R. Price in his article "Science and the Supernatural" in *Science* (August 26, 1955), with replies by Dr. S. G. Soal and Dr. J. B. Rhine in a later number (Jan. 6, 1956). A detailed account of this controversy over ESP was published in the *Journal of Parapsychology* for December, 1955.

The third *Bulletin* reviews two international conferences on parapsychology, one held in London in May, 1955, organized by the Ciba Foundation, the other on spontaneous phenomena at the University of Cambridge in July, 1955, organized by the Society for Psychical Research (London) and again generously supported by the Parapsychology Foundation.

This number also presents an abridged translation of Dr. Gardner Murphy's paper "Plans for Research on Spontaneous Cases" which first appeared in this JOURNAL in July, 1955.

Another paper in this number by Dr. Servadio is concerned with a psychoanalytic investigation of a spontaneous case that came to his notice. During the night of April 23, 1955, a girl of sixteen, here called Luisa (pseudonym), dreamed that the mother of her fiancé Guido (pseudonym) wore a curious silver ring. Marks resembling hieroglyphics were inscribed on the ring, which could open, as Luisa thought, and become a receptacle for perfume. On awakening, Luisa described her dream to her mother.

Several hours later she telephoned Guido and began to tell him of her dream. Guido, overwhelmed, replied he had returned from Milan where he had bought a silver ring for his mother. The ring, he added, opened on one of its surfaces; bizarre letters of unknown meaning were engraved there. Hearing this, Luisa dropped the telephone and frantically called her mother to testify that the same details had appeared in her dream.

Dr. Servadio was well acquainted with Luisa and Guido. All the circumstances of the incident were reported to him immediately after the telephone conversation. He took careful notes which Luisa and Guido verified as perfectly correct. In the opinion of Dr. Servadio there is little doubt that here we have a case of telepathy exceptionally well established.

A fourth paper in this number describes Mr. G. W. Lambert's theory that physical forces may initiate poltergeist phenomena (*Proc.* S.P.R., Vol. 38, 1955, pp. 49-71). The geographical distributions of the occurrences, the author points out, "favor" coastal regions.

It is obvious that *La Tour Saint-Jacques* performs a valuable service, not only to our French colleagues, but to psychical researchers in other countries, who might otherwise be unaware of current developments in the subject of particular interest to them.

L.W.A.

PRESENT-DAY PSYCHOLOGY. Edited by A. A. Roback. Pp. XIV+995. Philosophical Library, New York, 1955. $12.

Present-Day Psychology is a massive symposium embracing practically the whole range of psychology. The volume contains forty original chapters by experts in their various fields. The Editor is A. A. Roback and the book was published in 1955 by the Philosophical Library of New York.

Of particular interest to readers of this JOURNAL is the chapter "Introduction to Experimental Parapsychology" by J. B. Rhine. In his Editorial Note Dr. Roback writes: "No doubt there will be psychologists who will wonder why parapsychology should be treated as a branch in a volume of this sort . . . . It is not for us, however, to reject out of hand a body of data gathered in a number of institutions, simply because the conclusions might militate against orthodox psychology. Let us be skeptical, by all means, but let us not ignore. The psychologist must deal with every phase of human activity; and if psychical research is a human endeavor which has attracted at least some outstanding minds, then it is within our larger purview. We feel, at any rate, that students of psychology should have at least an idea as to what it is all about, so that they can take a stand toward it."

In his article Dr. Rhine surveys the early experimental work that led to the more systematic research in parapsychology at Duke University, beginning in 1930. He then traces the development of the research at Duke and elsewhere to the present time. "Above all in importance," Dr. Rhine says, "the discovery of psi must result eventually in the liberation of psychology from its present stifling bondage to physicalism . . ."

Notice to Members

THREE PAPERS ON THE SURVIVAL PROBLEM. Pamphlet Containing Three Articles by Gardner Murphy. Reprinted from the January, April, and October 1945 numbers of this JOURNAL. Edition of 300 copies. Pp. 90. $2.50.

Members who are interested in the question of the survival of human personality after death are reminded that Dr. Murphy's three papers on the subject, which originally appeared in the JOURNAL in 1945, are again available. Titles of the three articles are, "An Outline of Survival Evidence," "Difficulties Confronting the Survival Hypothesis," and "Field Theory and Survival."

HISTORY OF THE SOCIETY

The First American Society for Psychical Research was formed in 1885, in consequence of a visit by Sir W. F. Barrett to this country, and Prof. Simon Newcomb became its President. In 1887 the Society invited a man of signal ability, Richard Hodgson, A.M., LL.D., sometime Lecturer in the University of Cambridge, to become its Executive Secretary, and he accepted.

This organization later became a branch of the English Society under the very able guidance of Dr. Hodgson until his death in 1905. The American Society for Psychical Research was then re-established with James H. Hyslop, Ph.D., formerly Professor of Logic and Ethics in Columbia University, as its Secretary and Director.

THE ENDOWMENT

The American Society for Psychical Research, Inc., was originally incorporated under the Laws of New York in 1904 under the name of American Institute for Scientific Research, for the purpose of carrying on and endowing investigation in the fields of psychical research and psychotherapeutics. It is supported by contributions from its members and a small endowment fund. The income of the Society pays only for the publications and office expenses, but does not enable the Society to carry on its scientific investigations. A much greater fund is required before this work can be carried forward with the initiative and energy which its importance deserves.

The endowment funds are dedicated strictly to the uses set forth in the deed of gift and are under control of the Board of Trustees, the character and qualification of whom are safeguarded, as with other scientific institutions.

Moneys and property dedicated by will or gift to the purposes of the American Society for Psychical Research, Inc., whether to the uses of psychical research or psychotherapeutics, are earnestly solicited. The form which such dedication should take when made by will is indicated in the following:

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THE JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH

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THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH, Inc.
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PURPOSE AND SCOPE OF THE SOCIETY

1. The investigation of claims of telepathy, clairvoyance, veridical hallucinations and dreams, psychometry, precognition, dowsing, and other forms of supernormal cognition; of claims of supernormal physical phenomena, such as raps, telekinesis, materialization, levitation, fire-immunity, poltergeists; the study of automatic writing, trance speech, hypnotism, alterations of personality, and other subconscious processes; in short, all types of the phenomena called psychic, mediumistic, supernormal, parapsychological and metapsychic.

2. The collection, classification, study and publication of reports dealing with the above phenomena. Readers are asked to report incidents and cases. Names must be given, but on request will be treated as confidential.

3. The maintenance of a library on psychical research and related subjects. Contributions of books and periodical files will be welcomed.

4. Cooperating in the above tasks with qualified individuals and groups who will report their work to the Society.

MEMBERSHIP IN THE SOCIETY

Members, who receive the Proceedings and the Journal, pay an annual fee of $10. (One may become a Life Member or endow a Memorial Membership on payment of $200.) *Fellows*, who receive all publications of the Society and are afforded special library privileges pay an annual fee of $25. (Life Fellowship, $500.) *Patrons and Founders*: For those who wish to make a still larger contribution to the Society's work, these classes are open at $1000 and $5000.

It is to be remembered that membership in a scientific society means more than merely a subscription to its publications. The work must be carried on largely through the income from membership fees. Therefore members, old and new, are urged to make their membership class as high as they feel they can. *If a comparatively small proportion of the present members went one class higher, the money available for research would be more than doubled.*

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VOLUME L OCTOBER 1956 Number 4

Fiftieth Anniversary Addresses

In this number of the JOURNAL we present the addresses delivered at the Fiftieth Anniversary Celebration of the Society held in the George Washington Hall of the National Republican Club for Women on Friday evening, March 2nd.

The speakers, under the chairmanship of the President, Dr. George H. Hyslop, paid tribute to Professor James Hervey Hyslop, Founder of the Society, for his untiring zeal and foresight in organizing the A.S.P.R. at a crucial period and insuring the development and continuity of psychical research in America.

Dr. James H. Hyslop and Psychical Research

WALDEMAR KAEMPFFERT

This occasion has for me both an historic and sentimental interest because of my relation to Dr. Hyslop. I doubt if the younger members here realize how important a factor he was in advancing the cause of what is now called parapsychology. He was a man of great intellectual power, great courage at a time when courage was needed. He had to endure the ridicule of the skeptics of his time. Nowadays much of the opposition that he had to face and overcome has disappeared. What Dr. Rhine has called extrasensory perception is now pretty well accepted. A professor in a university is no longer hounded as he was in Dr. Hyslop's day because he experimented with what are now called psi phenomena.

In those early days Dr. Hyslop, William James, and Richard Hodgson constituted the fighting front of parapsychology, which was then called psychic research. Three very able men, they were regarded as gullibles and yet I do not think that there was ever a man who succeeded in fooling any one of them. I know that Dr. Hyslop punctured every fraud that was perpetrated on him or was attempted. Hodgson was an expert when it came to trickery. He knew all the tricks that could be performed, had seen them performed, or could perform them himself if necessary. These men were the pioneers of what has today become a very important movement. It was part of the English movement. Dr. Hyslop and his group were closely allied with the Sidgwicks, Myers, and Podmore in England and on both sides of the Atlantic an effort was made to introduce scientific methods, scientific objectivity in the evaluating of the results of experiments in psychic research.

There is a paradox here. The methods that have been pursued by parapsychologists from Dr. Hyslop's day on are scientific. They are becoming more scientific because the controls are tighter. We have now quantitative measurements—at least for some things, which wasn't the case forty years ago. And yet the more scientific parapsychology apparently becomes, the more resentful are the physicists.

The chief reason for the resentment is that in parapsychology the laws of physics break down. Rarely can experiments be repeated. There is very little control over them in the sense that it is possible to predict what is going to happen in a given set of circumstances. It is an astonishing fact that the application of probability mathematics to extrasensory perception, for example, has aroused of late considerable objection, and yet without probability mathematics it is impossible to interpret the atom. In fact probability mathematics

has lately been questioned because it works in extrasensory perception, just as it works in life insurance and in atomic physics. The laws of nature have broken down. We now know that they are simply statements of statistical averages and yet the physicists object to the inapplicability of probability mathematics. Yet both physicists and parapsychologists are both trying to discover the great pattern of the universe, a pattern that includes both the heavens and the human mind.

Parapsychology has progressed since Hyslop's day as I have said. Its controls are better than they were. Much of it is accepted that was once rejected. There is no reason to fight for parapsychology in some of its phases as in Dr. Hyslop's day, yet the opposition is not dead. Those who accept probability mathematics but refuse to accept the facts of parapsychology, bring up a charge over half a century old, the charge that if parapsychologists are not actually frauds they have either been imposed upon or they are deceiving themselves. It is very difficult to believe that men like Sir J. J. Thomson, Linnaeus, Goethe, Mark Twain, Bergson, Swedenborg, a hundred others equally eminent, were fools or frauds. That argument, I think, must be dismissed. There were frauds in astronomy in its astrological days. There were frauds in chemistry in its alchemical days. Yet both astronomy and chemistry are now great sciences. We must expect some deception in parapsychology, but men like the late Dr. Hyslop, Hodgson, and Professor Rhine, are not easily fooled. In fact they are far more skeptical than their opponents.

The failure of psychic phenomena to obey the laws of nature, as I have said before, is not significant. I am not so sure that the scientific method, which has worked so well in physics, really holds good for every other branch of science. The sciences as a whole have been tyrannized by physics. There has been a passion for measurement. Yet we accept the facts of evolution without questioning natural selection. Yet few deductions can be made from experimental evolution. To be sure there are mutations, very slight ones, but no one has yet seen a reptile turn into a bird. We have merely observation, merely conclusions based upon observation. So it is in the atom. Observation must be supplemented by probability mathematics.

Strange to say, the physicists themselves have lost faith in their own objectivity. Since Werner Heisenberg announced what is called the Principle of Uncertainty, it is now known that the experimenter is part of the experiment. He cannot be left out of it. The result is that experimentation in some portions of atomic physics has lost some of its old merit and force. It is further to be noted that none

of the opponents of parapsychology have performed experiments themselves or taken the trouble to spend the years and years that men like Hyslop, Hodgson, Podmore, and the Sidgwicks spent in experimentation. Yet the opponents of parapsychology have the temerity to question the work of men of great ability, fine minds, and the highest objectivity.

The reason why I myself have always been interested in parapsychology is this: What Dr. Hyslop and his successors did and are doing goes far beyond extrasensory perception, far beyond psychokinesis. The fundamental question with which it is concerned goes very deep. Is the spiritual or the materialistic interpretation of the universe the true one? Philosophers and theologians have debated that question for centuries without result. We need a new approach. It may be that that new approach will come through parapsychology. The scientific method has been used in parapsychology. It must be used at this stage, but I wonder if it is the only method—the method of experimentation and measurement. There are flashes that come to every great scientific experimenter. Kekulé sits on top of a London bus. The picture of the benzene ring flashes upon him, the benzene ring being one of the fundamental conceptions of inorganic chemistry. There are dozens and dozens of these instances in scientific research. The explanation is that ideas have been incubating perhaps for years. Then something wells up out of the unconscious.

It is the unconscious that we are primarily concerned with in parapsychology and not ordinary sense perception. Here we open an entirely new field that has not yet been thoroughly explored. Parapsychology will have to devise its own method of exploration. For parapsychology deals primarily with the unconscious. And what do we mean by that? I don't know whether Eddington was a parapsychologist or not. He spoke of "mind-stuff." To him brains were just islands of this mind-stuff which have achieved consciousness.

If we accept this reasoning, parapsychology ought to become the most important of all sciences. What did Eddington mean? He meant that we can never know what the universe is through our knowledge of particles and fields of energy alone. Knowledge will come as a result of some direct spiritual experience. The fierce faith of the martyr willing to die at the stake, the sense of communicating with something outside of himself that every poet has, the exaltation that uplifts a mystic, the intuitions and the premonitions and the telepathic messages that are our common possession, the vivid dreams that come true—these are what Eddington meant by mind-stuff. And it is mind-stuff with which parapsychology is primarily concerned.

My Relation to James H. Hyslop as his Secretary

GERTRUDE OGDEN TUBBY

On a memorable Sunday morning in the season of 1906-07, at the morning service of the Montclair, N. J., Unitarian Church, I first heard a speaker who was to change the current of my life-work—Dr. James Hervey Hyslop. I eagerly took down every word, as he summarized the excellent evidences of identity he had received from both his wife and his father through Mrs. Piper in Boston under Richard Hodgson's direction. For nearly an hour he discussed the psychological and philosophical implications of such evidence and their impact on human life. When he had finished I was one of those who felt the compelling power and importance of such studies, and I took with me from the meeting an advance subscription blank for membership in the reconstructed American Society.

That spring I rounded out a five-year term as research and literary secretary for Dr. Edward Howard Griggs, who spoke at 210 words a minute, so my shorthand facility added to my Bachelor's Degree in Science from Smith College emboldened me to apply to Dr. Hyslop for a position. The following thirteen years were to open up wonderful vistas of knowledge and insight, under a fearless leader.

For years I had read avidly in physical and biological science and in the psychological volumes of William James, as well as in the pseudo-sciences and cults of "new" thought, "higher" thought, Christian Science, metaphysical and theosophical schools making sweeping claims that often conflicted. As I began my laboratory experience in psychic research under Hyslop, I could trace connecting threads of meaning in their diverse theories. From the start, Dr. Hyslop's comments on my queries showed a clear grasp of problem and a steady power of insight that bred confidence. He seemed very sure of what he did know, and equally sure that he didn't know what he didn't know.

In our thirteen years of association, I never knew my leader to have to backtrack on any positive statement he had made in his work, nor to accept statements by others with insufficient grounds of wisdom or experience. He was a merciless critic of both his own and others' work.

The first work I ever did with or for him was to take records of séances held one week in each of the spring months, beginning with March, 1907, with Mrs. Minnie Meserve Soule of Boston, whose ability had already been tested by Professor William James and Dr. Hyslop.

All Dr. Hyslop told me was that the medium was to be at the Irving Hotel, in New York, registered as "Mrs. Harbin" to protect her from any intrusions by New York clients, the hotel being party to the arrangement; and that to the sitter she was to be known as "Mrs. Chenoweth." He introduced me, of whom he knew only my home address and my credentials, as Miss Tubby. Mrs. Soule, being herself all wrapped up in aliases, supposed I was, also, my name being too odd to be true.

Having read two volumes by Thomson Jay Hudson ten years before, followed up with F. W. H. Myers' *Human Personality and Its Survival of Bodily Death* on its appearance in 1903, I had some idea of what was wanted in a record. Dr. Hyslop instructed me to make note of everything written or said by Mrs. Soule, as well as pauses or dramatic action, together with every word spoken by those present. I was to say nothing whatever to the medium about the communications received. The visitors would be admitted after the trance had come on, introduced under pseudonyms, and were not to speak except to acknowledge any correct information presented, or to so state if they did not understand what was given.

Thus the astuteness of the great pioneer was borne in upon me from the very start. I owe him a great debt of gratitude for setting this pattern of procedure followed closely thereafter in over 5,000 séances recorded by me. It has preserved many an important detail that seemed negligible at the time of its reception. Up to that time I had never attended any sitting with anyone.

Dr. Hyslop took the first two or three sessions for himself, when Hodgson, George Pelham, and members of the old communicators of the Imperator Group wrote automatically, after a brief introductory chat by Mrs. Soule's familiar young Comanche Indian guide Sunbeam, to get the trance well established.

I learned later on that some of the sitters were practically strangers while others were well known researchers such as Professor Newbold of Pennsylvania University, Dr. Weston D. Bayley of Philadelphia, head of the Philadelphia Section of the old American Branch of the S.P.R., Mrs. R. H. D. Ledyard, friend and supporter of Hodgson, Charles N. Jones, first President of our Board of Trustees, and a Mr. Smith whose name I recall because of an amusing little incident at the opening of the sitting. He had been introduced, after the trance had come on and he was ushered in silently from the hall by Dr. Hyslop, as "Mr. Smith, the sitter for the morning." Dr. Hyslop then left us three total strangers to one another to carry on while he went away to attend to other duties, as he frequently did, and Sunbeam said almost at once: "Of course lots of sitters are

introduced to me as Smith, Jones, or Brown, and I always take it for granted it's only a pseudonym, but you are *really* Mr. Smith, yourself." And he smilingly acknowledged the hit. One of the outstanding séances was that for Mr. Frederick Louis Thompson, who painted under the inspiration of R. Swain Gifford, who at this sitting gave excellent evidence of his identity. The case was published in our *Proceedings* Vol. III, Part 1, as "A Case of Veridical Hallucinations." Some of Mr. Thompson's paintings are now in the possession of our Society.

When we had two sittings in a day, after the morning one, Dr. Hyslop would take me to a very simple luncheon in a nearby restaurant and I was impressed with his economy in ordering. It was my first glimpse of that strong Scottish trait of character in him which contributed so largely to his success in building up from scratch, and almost single-handed, the considerable endowment he acquired for the Society before his death. We never wasted so much as a sheet of paper or a postage stamp. We traveled exclusively in public conveyances, never a cab. Our office was for many years Dr. Hyslop's own library, except for the mailing of publications which was done from the Tribune Building by a publisher who mailed the monthly JOURNAL and the one or more numbers of the annual *Proceedings*. We did not stint on printing bills, preferring humble housing in favor of the all-important output of scientific records. We then had no telephone. Everything was done to make possible a truly remarkable range of published detailed records and reports, the like of which has never been seen since Hyslop's day. It would be worth while to re-read them today.

My initial experience left me with a pretty well-grounded conviction that survival and communication after death was no myth, no subconscious theatrical wish fulfillment. I noted that the artful telepathic dodger would be hard put to explain how it happened that not one single trace of evidence from any past memory or history of mine leaked into those thirty-five sessions, despite the fact that I was the only visitor present every moment each time, with heart and mind full of wishful thinking to prompt it. It was Hyslop's judgment that far too much was apt to be attributed to telepathy, the *process* of which was, and still is, not yet understood.

For several years I reported on special cases for the Society in New York—Miss Etta deCamp, medium of "The Return of Frank R. Stockton," and Gwendolyn Kelly Hack (later author of *The Millesimo Mysteries*)—and a medium in Kansas City, Kansas, where I was visiting, brought to Dr. Hyslop's attention by a member out there. Meantime I refused the secretarial position because he said it would involve some bookkeeping in which I felt I had had

too little experience. After a year abroad to study I returned to find that I could still have the job and that the bookkeeping was a simple matter of daily entries of receipts and expenses. I became Dr. Hyslop's secretary on a half-weekly basis, for lack of funds. This gave me the advantage of his instructive commentary on widely varied matters of correspondence and also an opportunity to read in his well-stocked library.

Of course as time went on I was permitted to handle some of the routine correspondence, and I had the copying of interesting and important séance reports to do. Dr. Hyslop was taking three days a week of work in Boston with Mrs. Soule by this time and did his own typing on a little traveling Blickensdorfer machine with indelible purple ink, using but two or three fingers for the many thousands of pages he covered. He worked literally from morning to night, seldom leaving his desk even in the evenings when reading and book reviews were done. His family saw too little of him but he seemed to feel driven to "keep everlastingly at it." "I know it's killing" he said to me one evening when I had helped catch up with a mass of work after he had been away on a necessary trip, and by 11 P.M. I felt all in. Two days before I had worked all night until 5:30 A.M. to get the annual bills out.

No regrets: I would do it over again to have the same great leader back.

I believe it was in 1915 that he notified me that the Board of Trustees had authorized him to denominate me Assistant Secretary of the Society—I still have the notice in my archives—a promotion I valued especially because it aided me in the lecture field which I had entered in 1912. Dr. Hyslop rarely gave praise, but I learned from his friend, Miss Lilian Whiting of Boston, actively interested in psychic matters, that he had advised her to hear one of my lectures, as he thought she would find it worth her while. But for her, I should never have known that he had any opinion whatever of my lecturing. How he was able to form one I had no idea.

If he seldom praised, neither did he find fault. I was given a free hand to execute any job assigned me in the best way I could. One year we issued a series of five leaflets containing instruction and information for correspondents. I asked if I might shape up a sixth, to inform inquirers how to conduct a scientific mediumistic séance. When I submitted it to him he changed but two words or phrases in the twelve sections on the page, an endorsement still treasured in memory. I later preserved them in a chapter in my book, *Psychics and Mediums.*

Owing to his illness in March, 1914, I had been given the privilege of substituting for Dr. Hyslop at two weeks' work with Mrs. Soule. When I turned in my records and enthusiastic comment, he asked me to write them up for use some time in the JOURNAL. Months after I turned them in he looked up as I sat writing at my green felt-topped kitchen table at right angles to his huge desk, and remarked, "I suppose you're wondering what has become of your three Chenoweth sittings. I've been holding them for the January JOURNAL. I always want something especially good for that number." I was fit to burst with pride and joy. They appeared in January, 1920. My only regret was that I hadn't annotated them sooner.

Once only did I see Dr. Hyslop thoroughly angry, and justifiably so. A wealthy woman, an old supporter of Hodgson's regime, had joined our Board of Trustees and was promising to do great things for the Society. She was trying to ingratiate herself with all hands, but also was determined to run everything to suit herself. To satisfy her curiosity, she one day sent a young man from her lawyer's office to inspect the Society's books. Dr. Hyslop himself was very fortunately at home and answered the door bell. I heard him say quietly but with finality, "You go back to her and tell her that whenever the Board of Trustees wish the Society's accounts inspected and authorize an inspection it will be made, and not at the order of any one member."

He came back to the library, his face white with anger, opened and closed the deep desk drawer where the account books were always kept, exclaiming in a loud tone for him, "Sugar beets! Inspect the books—makes me so mad, Sugar beets! Thinks she can take the whole organization into her own hands and do as she pleases about things. She'll soon find out what the Board has to say about that." She did. They removed her very shortly. Our Treasurer, Hon. Lawson Purdy, concurred in that action.

Sugar beets, by the way, was a substitute for swearing in the family of Robert Hyslop, whose boys had adopted this cover-up expression to give vent to unbearable feelings. Swearing was strictly forbidden at any time, as Dr. Hyslop had told me before this occurrence, with a twinkle in his eye.

His sense of humor was keen and often exercised. One day he told me a story then quite new to me, which I have heard elsewhere since, and I dare say you have too. The local Maine resident dressed up in his good togs heading for the railway station is accosted by a passing neighbor: "Where be you agoin' Si?" "Aw, I'm goin' down t'Portland to git drunk. Gawsh, how I do dread it!" This coming from such a source doubly amused me and my hearty laughter

evidently registered well with my Chief. From then on he used to bring out from his wallet small snips from the Boston papers of the early half of the week to regale me and others with on his return. He thoroughly enjoyed a good laugh. An added fillip was given to it by the fact that he, an expert in the terminologies of epistemology, epiphenomenalism and obstruse philosophy in general, found time and zest for homespun humor.

In his final illness, in one of our last conversations, after he had suggested that I should be the next Secretary of the Society and spoke of other serious matters of concern to him he said, "I look to see an increase in endowment after my death. It may stir up some interest." And I replied, "I suppose you'll just go around seeing what's in people's wills and then knock the right ones on the head!" "Yes," said he with a grim twist of humor, "I suppose I'll have that to do, too." What his going meant to this Society I know only too well.

From the very morning of his passing he has continued to keep in touch, evidentially, and I am still kept busy by the untiring, inspiring leader who said the only thing to do was "to keep everlastingly at it"—James Hervey Hyslop.

Science, Scientists, and Psychical Research

C. J. DUCASSE

The late Professor James H. Hyslop, in the 27th chapter of the last of his books, *Contact with the Other World,* discusses the relation between Spiritualism, Religion, Science, and Psychical Research. He finds certain faults with Spiritualism, and points out that it is possible to charge the representatives of Religion with the opposite faults. Then he turns to Science and writes: "Science, content, without thorough inquiry, to confine its investigations to the physical world in which it has achieved so much, will not open its eyes to anomalies in the realm of mind and nature, and so degenerates into a dogmatism exactly like that of theology" (p. 425).

These words were written by Hyslop in 1919. The fact to which they point is what I shall start from; and the question I propose to consider is: What accounts for the unscientific attitude with which even now, 37 years later, the majority of scientists continue to meet well-authenticated reports of phenomena of the kinds investigated by the societies for psychical research?

The scientific attitude, as scientists and philosophers alike rightly proclaim, is characterized by unswerving and painstaking dedication to the discovery of truth; it is open-minded in the sense of free alike from adverse and from favorable prejudices; and it welcomes facts as such, no matter whether they confirm or invalidate the assumptions or theories on which they have bearing. In short, disinterested curiosity—the passion to know the truth—is the one scientific passion. It is a stern censor, which rules out of scientific judgments factors such as emotion, dogmatism, hopes or fears, and wishful belief or disbelief—factors which so generally vitiate the judgments of ordinary men.

Such is the scientific attitude. It is altogether admirable, and the command over the forces of nature, which adherence to it and to the methods it dictates has put into the hands of man, testifies to the fruitfulness of that attitude.

But the fact that, in so far as it has actually been that of scientists, they have accomplished wonders; and that these wonders have given magical prestige to the very words, Science, and Scientist—this fact does not at all guarantee that, whenever a man who is by profession a scientist speaks, what he says is always one of the fruits of the scientific attitude. For, like other men, scientists usually have the usual human frailties, even if they park some of them outside the doors of their laboratories. Inside the door, of course, they either live up to the demands of the scientific attitude, or they achieve nothing. But, outside, they are as prone as other men to pride of profession or of office; and the prestige with which the name, Scientist, endows them in the public eye easily provides for them an irresistible temptation to pontificate concerning all sorts of questions which fall outside their professional competence, but about which naive outsiders nevertheless respectfully ask them to speak because they are known as Scientists, and Scientists, by definition, are persons who know!

The oracular role which this flattering deference invites them to play, of course caters to the vanity of which they are no more free than other men, and which then almost fatally leads them to assume that—except when speaking to a fellow scientist on scientific matters —their utterances have high authority. For the idea which a person harbors of himself is largely determined by the picture of him which other persons hold out to him.

Now, that pleasing though mainly subconscious picture of himself as an oracle is what is outraged when outsiders venture to call to the attention of a scientist certain facts, such as those psychical research investigates, which seem to clash with some of the principles

of his science, but which he ignores. It is on such occasions that the admirable scientific attitude I have described easily deserts him. On such occasions, as the late Dr. Walter Franklin Prince charged, proved, and illustrated in his book, *The Enchanted Boundary*, by quoting the words of some twenty scientists, from Faraday, Tyndall and Huxley to less eminent ones—on such occasions the outraged scientist is prone to become unscientifically emotional, obscurantistic, inaccurate, illogical, evasive, dogmatic, and even personally abusive. Mention of this last—abusiveness—brings to mind an anecdote often quoted in textbooks of Logic in the chapter on Fallacies. It is that once an eminent counsel had been asked by the lawyer for the defense in a law-suit for advice as to how to conduct the presentation in court of his client's case. After examining the facts, the distinguished counsel advised: "Your client has no case. Abuse the plaintiff's attorney!"

My remarks up to this point have concerned only the psychological factors which account for the abandonment of the scientific attitude by so many scientists when their attention is invited to the existing evidence, experimental and other, that paranormal phenomena of various kinds really occur. But something must now be said also about the source of the quite honest and firm conviction of many of them that, in the light of modern scientific knowledge, those phenomena cannot possibly be real, but must be mere semblances, delusions, or frauds.

Let us note first that, when a scientist declares that something, which belongs to the field of his scientific competence, is *possible*, there is no mystery as to the basis of his assertion. It rests either on the fact that he or some other scientist has actually done or observed the thing concerned; or else that that thing is anyway not incompatible with anything which science has so far established.

Again, when a scientist declares something to be *impossible by certain means* and *under certain conditions*, the basis of his assertion is likewise not mysterious. It is that he or some other scientist has actually tried to cause that thing in that manner under those conditions, but that it did not in fact then occur; or else that he already has observed what does occur when the procedure stated is employed under the conditions stated; and that what does then occur is *not* the particular thing in view but something different.

On the other hand, when a scientist declares something to be impossible, *period;* that is, impossible not as in the case just considered, *by certain means under certain conditions*, but impossible *unconditionally;* then it is a mystery indeed how he can possibly know this. And, in fact, he does not know it but, when he asserts

it, he is only dogmatizing even if unawares. The history of science is strewn with the corpses of absolute impossibilities rashly proclaimed at various times.

But what then accounts for the scientist's nevertheless quite sincere conviction that certain things are impossible absolutely?

The answer, I think, lies in the fact that, all unconsciously, he has made a metaphysical creed out of what actually is only a description of the particular field of scientific inquiry he has elected as his own.

Of course, he will indignantly deny that he, a Scientist, has any truck with that vain and vaporous thing called Metaphysics, which he is more than glad to leave to philosophers and other unscientific thinkers. But, as one philosopher has pointedly observed, a person's repudiation and scorn of Metaphysics does not at all insure that he does not himself harbor unawares a metaphysical creed—in which case he is the more helplessly a prisoner of it that he does not suspect the existence of that mental prison and cannot recognize its walls.

That this is actually his predicament will become evident if we now consider on the one hand what that despised thing is, which philosophers call Metaphysics; and on the other, what is the field of inquiry which the Natural Sciences have chosen as their own.

Metaphysicians, of course, can justly be charged with many sins, and the spectacle of them has led one philosopher to the facetious definition that Metaphysics is the systematic abuse of an elaborate technical terminology invented especially for the purpose! But in fact Metaphysics, or more particularly in the present connection the branch of it called Ontology, concerns itself with the question as to the nature of Reality as distinguished from mere appearance, semblance, unimportance, or nonexistence. And a metaphysical creed is a conviction which, if put into words, takes the form: "To be real is to have such and such characteristics." Hence, to have a metaphysical creed is to proceed in all one's activities and judgments, and whether consciously or automatically, under the assumption that to be real is to have certain characteristics—the particular ones, namely, which differentiate one's conception of the nature of reality from other conceptions of it.

What, on the other hand, is the field of inquiry which the Natural Sciences have chosen as the one they undertake to explore? Before answering this question, it is necessary for us to be quite clear that, in the phrase "the Natural Sciences," the word "Natural" is not used as opposite of "Supernatural," but is only the customary name by which the physical, chemical, and biological sciences are distinguished from other groups of sciences—for example, from the Formal Sciences, namely Mathematics and Logic.

This being understood, it becomes clear that the field of inquiry the Natural Sciences have chosen as their own consists of the things and events we all can perceive by our senses—solids, liquids, gases, vegetable organisms, animal bodies. These things, their behavior, their minute constituents and hidden processes, are the whole of what the Natural Sciences study. And the comprehensive common name of that entire object of study is "the material world."

The material world, of course, is highly important to us; and study of it by scientific methods has yielded a vast amount of knowledge of it and of its laws. And this in turn has put into our hands a corresponding amount of control over its processes. The scientists who have devoted themselves to this great and difficult task can justly be proud of what they have achieved. But the material world is not the whole of the world, nor is it the only part of it capable of being investigated in a scientific manner.

Now, however, let us recall the question which led us to the remarks just made. It was: What accounts for the sincere conviction, so widespread among natural-scientists, that the phenomena in which psychical research is interested are absolutely impossible? Those remarks, I believe, will now make clear both the meaning and the truth of the answer to this question which I offered. That answer may now be restated as follows: The only reason why natural-scientists regard the phenomena in view as absolutely impossible is that, unconsciously, they have made a metaphysical creed—a doctrine as to the nature of *all* of Reality—out of what in fact is only the description of the particular *part* of Reality they undertake to explore, namely, the material world. That is, they have, uncritically and gratuitously, committed themselves to the particular metaphysical creed that to be real is to be some material event, process, or thing. And obviously, if one thus proceeds from the start and all along on the arbitrary metaphysical assumption that nothing is real unless it is some process or part of the world perceivable by the senses, then necessarily thoughts, feelings, mental images, volitions, and all the other psychological events, none of which is directly so perceivable but perceivable only by introspection, are automatically conceived as unreal; that is, as mere appearances, incapable of doing or of accounting for anything.

It is, of course, perfectly legitimate and proper to push as far as it is successful the attempt to account in purely material terms for all material events, including the activities of human bodies. But at the many points in, for example, human voluntary acts, at which no material event is observable that would account for those acts, there is no rational justification at all for insisting willfully that their

causes *must,* somehow, anyhow, be material events; so that when, for example, I wrote the present words, my thoughts and my desire to formulate them in writing could not possibly have been what caused the writing of those words. What accounts for but does not justify that insistence is only the pious but quite arbitrary metaphysical creed, uncritically adopted and cherished by most natural-scientists, that only what is material is real; and therefore that not only the vast majority of material events, but all of them—absolutely all without exception—must have purely material causes.

In conclusion, the substance of my remarks may be put both summarily and picturesquely in the apt words used by Professor C. D. Broad in the preface to his Tarner Lectures at Cambridge University in 1923. What he said was that the scientists who regard the phenomena investigated by psychical researchers as impossible seem to him to confuse the Author of Nature with the Editor of the scientific periodical, *Nature;* or at any rate they seem to suppose that there can be no productions of the former which would not be accepted for publication by the latter!

Integration of Experimental Research with Investigations of Spontaneous Cases

GARDNER MURPHY

I am especially grateful to Miss Tubby for bringing back some very warm and rich moments in those last years of Dr. James Hyslop's life, in which, despite problems of health and problems of ridicule from his colleagues, he always had the courage, the generosity, the patience to deal with young and confused people who were trying to latch on to the meaning of psychical research. I was very grateful to you, Miss Tubby, for making real to me again that vivid evening in which Dr. Hyslop walked along the Charles River with me, telling me some of the mistakes that all psychic researchers made, all of which I have made of course many times in spite of the warning. He maintained a sort of perspective on the vast reaches of knowledge, of understanding of human nature that might perhaps be ultimately conquered if, instead of fragmentation and bickering, we might try to see the human sciences as a whole and psychical research in all its many facets as a unified expression of the science of man.

As a matter of fact, it is this theme of the unity and the interdependence of the different aspects of psychical research that I thought I would like to treat for a few moments in memory of Dr.

James H. Hyslop, a man who found time and patience to delve into every claim, to maintain openmindedness and criticism, balance and poise in such an amazing range of phenomena, always with a sense that there was a field that was bigger than any person and any particular finding. As a matter of fact, if I think of the men with whom he had been associated, if I think of Richard Hodgson, William James, of Walter Franklin Prince, of Elwood Worcester, I think of this kind of catholicity and universality as defining the psychical research which Dr. Hyslop wanted to encourage. And I think in the same spirit of those two sensitives long since passed on, Mrs. Piper and Mrs. Soule, who played such a very large part in the education of Dr. Hyslop with regard to the nature of the phenomena, and the fact that he always saw more in the investigations than the testing of some narrow corners, some specific hypothesis. He incurred the wrath of his colleagues when he put in the details which so many people thought could be left out; he wanted the factual picture of all the interrelations of psychical research phenomena to be made clear. As a matter of fact it is this conception of the unity of our subject-matter that is the hardest for us to spell out adequately and live up to most confidently. It is so easy to dart off into a special technique or special hypothesis and make this slant or bias everything that one does.

I thought I might illustrate this point particularly with reference to the endless argumentation as to whether we ought to put our primary emphasis upon spontaneous cases or upon laboratory experiments. On the one hand, we seem, at times, to make spontaneous paranormal contact with our environment; there are sudden flashes of insight into the future, especially in the case of those special sensitives whose unusual gifts we investigate in the hope of seeing more deeply into the process. In the spontaneous cases we take life, so to speak, as it is *thrown at us*. On the other hand, there is the method of the laboratory, which has been the method of the last three and a half centuries, since the work of Galileo, and has been the orderly framework within which modern science has taken shape. You can always get the argument as to whether spontaneous or experimental data should come first. Should the raw data, as nature gives them to us, come first, or should the refinements, the controls, the abstractions which are the nature of science come first? We can get lost in this argument. We can get lost in the same argument, for example, as between clinical medicine and experimental medicine. We can get frightfully lost in psychical research by looking at spontaneous cases as something self-sufficient and independent of laboratory studies, or on the other hand by looking at laboratory studies.

with their refinements and measures, as the be-all and end-all of psychical research. It seems to me that we are beginning to learn the hard way, as Dr. Hyslop could have taught us if we had had our ears open. We can learn to see that there is not only mutual support between the study of life material and the study of laboratory material, but that actually neither one could function very far, even keep its head out of water, without the other.

I would like to bring out, in particular, some of the struggles that characterize the last few decades to understand the nature of the telepathic interchange. Nothing is easier than to demand that we go beyond the sheer collection of facts and set up a systematic hypothesis about telepathy and test it out. Nothing is easier than to accuse us of running around grabbing facts under every stone and lacking that broad vision which characterizes real progress in science. Yet there is no way in which the laboratory alone can give us workable hypotheses. The data are too specialized, too fragmented, too unrepresentative of human nature. We have learned that the hard way. Having been at this thing for decades, we know that most of the working hypotheses that are really worth while in the study of telepathy come from life situations. This would apply whether you think of the trivial interchanges between friends or the more profoundly stirring phenomena of apparitions of the dying, shared by loved ones of the dying person. Regardless of the quarter in which we look, we find that it is from the spontaneous cases that the laboratory hypotheses are chiefly derived. On the other hand, the close, sensitive observations of persons in laboratory settings, noting the kind of motivation that drives them, the kind of performances of which they are capable, the ways in which they get excited or bored, eager to cooperate or coldly hostile, revealing the real tissue of the contact between the subject and the experimenter, yields hypotheses which can be checked back against spontaneous cases. We say a great deal about how the laboratory may test hypotheses given us by spontaneous cases, but how often it is remembered that the laboratory, with its more definite control and its more systematic thinking, may often offer hypotheses that prove useful in the study of spontaneous cases? I believe in point of fact that this conception of a unified science towards which we all look as an ultimate fulfillment of our groping efforts of today lies in the recognition that every phase of our work throws light on every other phase. Dr. Hart said a moment ago: "We need each other." I don't believe that there is any moment more appropriate to remind us of this unity of psychical research than the moment of remembering a great leader like Dr. James H. Hyslop. One might have used one's time to point out the need to combine the biological approach, the psychological ap-

proach, and the sociological approach; or, to put it another way, the need to integrate the study of the tissues of which we are made, the study of our personal motives and attitudes, and the study of the cultural wholes of which each individual is an expression. In utilizing all these approaches psychical research, I believe, will make good only insofar as there is a tolerance, a warm sensitivity to the diversity of viewpoints which one may legitimately use.

Hand in hand with this tolerance goes, of course, a need for diversity in the interpretation of complicated and obscure data. As Professor Ducasse very well pointed out, there is an enormous amount that masquerades as science which is at best metaphysics. We cannot achieve the status of science by standardizing our viewpoints, by grouping people together in terms of what they believe, with intolerance of the disbelieving. It may be a very long time before all men and women of good will and broad reasonableness will agree upon the meaning of the data or even upon the best methods of prosecuting the investigation. There is room, however, for all sorts of people who believe in the enormous importance of understanding more deeply the nature of human personality and the nature of the social tie which holds that person together.

I was thinking a minute ago how curious it was that a certain fact hadn't been mentioned this evening. I will bring it in even if this may seem a strange moment for it: The two great efforts, I believe, of the last hundred years, to shatter the limitation of our understanding of human beings are psychoanalysis and psychical research, both of which are trying to understand the unconscious nature of human needs, attitudes, and interpersonal relations. This is the hundredth anniversary of the birth of Freud and the fiftieth anniversary of the American Society for Psychical Research. We may be grateful to realize that today we are beginning to accept as a matter of course the thought that there is always *"more beyond"*—the thought that there is always a *bigger human nature than we can see*: the fulfillment of our being lies not simply in our own individual existence, but in the ties, the conscious and the unconscious bonds, which connect us with other human beings in the attempt to develop upon this planet some sort of decent integrity and unity of human life. I would say then that if we are so foolish as to congratulate ourselves on fifty years of the American Society for Psychical Research we might correct this by saying that just as Newton was happy to have collected a few pebbles on the infinite seashore, so we might be grateful that since the time of Dr. James H. Hyslop we have begun to make a few tiny beginnings here and there on what will ultimately be a unified science of human beings and their interdependence upon one another.

An Exploratory Analysis for Displacement in PK

CARROLL B. NASH

In PK tests with dice, other than tests solely of PK-placement, the target is dual as it consists not only of a particular target face but also of a particular target position. The target face is one of the six faces of the die, and the target position is customarily the top one of the three possible positions of the target face on the die—top, side and bottom. In distinction to displacement to the target of the wrong trial (inter-trial displacement), displacement may be entirely within the same trial. Such intra-trial displacement in PK could consist either of response to the target face in a nontarget position, i.e., on the side or bottom of the die, or of response to a nontarget face, i.e., the opposite or adjacent faces, in the target or top position. Mitchell and Fisk (2) obtained a negative deviation of hits on the opposite face and a positive deviation of hits on the adjacent faces (and target face) and interpreted this as a tendency for the target face to avoid coming to rest in contact with the throwing surface. There is, however, no prima-facie evidence of whether deviations of hits on opposite and adjacent faces of the die are the result of response to those faces in the top position of the resulting die or whether they result, respectively, from response to the target face in the bottom and side positions. The results of the present experiment will be shown to support the former of these two interpretations.

In distinction to intra-trial displacement which is spatial, inter-trial displacement may be, and in some cases must be, temporal. For example, forward displacement in PK must be temporal where the target for the succeeding trial is not determined until the current trial is completed. In ESP, inter-trial displacement consists of displacement to a wrong card, and intra-trial displacement consists of displacement to a wrong symbol on the target card as in clock card tests (1).

Displacement can be determined in PK tests only when the upper faces of all of the dice are recorded after coming to rest. Such experiments have been few and the present experiment was one of the first of such to be conducted. Although this experiment was performed for other purposes, the method of recording the data made it possible to analyze for displacement. The experiment was conducted in the spring of 1948 with twenty students at Washington College as subjects. Mr. Edmund H. Bray, Jr., then a senior at the college, was the experimenter. By pulling a cord attached to a container, the subject simultaneously released a set of three red dice and a set of three white dice. The dice rolled down an inclined,

corrugated board into a tray, the dice container, inclined board and tray being partitioned so that the two sets of dice were continuously separated. The subject was instructed to attempt to score high with one of the two trios of dice and not to concern himself with the score of the other, the subject selecting the trio of dice with which he wished to attempt to score above chance as well as the side of the apparatus to be occupied by the chosen trio.

The upper faces of the two trios of dice were recorded by the experimenter after each trial and the subject checked the count. Twenty-four falls of the two trios of dice constituted a set and six sets composed a page, each subject completing a page at one sitting for each of three series. By selecting a card from a pack of six numbered cards shuffled at the start by the experimenter, the subject selected a different target face of the die for each set. The targets were unknown to the experimenter until the end of each sitting.

The experiment consisted of three series, each being completed for all of the subjects before the succeeding series was begun. In the first series each of the twenty subjects was tested separately, and in the last two series the subjects were tested in pairs. In the second of the three series both members of the pair sought the same target face with the same trio of dice, while in the third series each member of the pair sought a target face unknown to his partner and used the color of dice and the side of the apparatus not used by his partner. As the data of the third series were analyzed separately for each member of the pair, they are equivalent to twenty pages.

Following is a summary of the data taking.

3 red and 3 white dice per fall
24 falls per set
6 sets per page
1 page per sitting
1 sitting per subject per series
3 series in all
20 subjects used in every series

Series 1: Subjects tested separately
20 pages of data
8640 dice meant for targets
8640 dice without targets

Series 2: Subjects tested in pairs
Both for same target and same color dice
10 pages of data
4320 dice meant for targets
4320 dice without targets

Series 3: Subjects tested in pairs
For secret target and own color dice
10 pages of data
8640 dice meant for targets

The scores and deviations of target hits are as follows:

	Selected Dice				Disregarded Dice
	Series 1	Series 2	Series 3	Total	(Series 1 and 2)
hits	1457	678	1418	3553	2119
dev.	+17	—42	—22	—47	—41

The deviations are not significant, and there are no significant differences between the scores of the selected and disregarded dice or among the scores of the three series. As far as the original analysis of the experiment is concerned, the results are explainable by the chance hypothesis.

It was decided at the outset of the analysis for displacement to ignore the distinctions between the series and the distinction between selected and disregarded dice. In this procedure all Series 3 dice are scored twice (once for each subject), since it was believed that this would yield the maximum amount of information. The supposition is that each subject may have influenced the trio of dice selected by his partner. As a result, all displacement effects are probably slightly overestimated in significance.

The analysis for intra-trial displacement consisted of a study of opposite displacement and of adjacent displacement. Opposite displacement could result either from response to the opposite face in the target or top position or from response to the target face in the opposite or bottom position, and adjacent displacement could result either from response to adjacent faces in the target position or from response to the target face in an adjacent or side position. Analysis for inter-trial displacement consisted of a study of backward displacement to the target of the immediately preceding set (—1 displacement) and of forward displacement to the target of the immediately succeeding set (+1 displacement). While the —1 displacement could be either spatial or temporal, the +1 displacement could only be temporal as the target for the succeeding set was not determined until after the current set was completed. The first set of the page was not analyzed for —1 displacement nor the last set for +1 displacement, as such displacements would be to the target of a different subject.

The data were analyzed both with the individual die fall and with the three-dice trial as the experimental unit. For the former the results are as follows:

	target hits	opposite displacement	adjacent displacement	—1 displacement	+1 displacement
hits	7138	7085	28,997	5911	6113
dev.	—62	—115	+177	—89	+113

With the individual die fall as the experimental unit, the displacements are not significant.

With the three-dice trial as the experimental unit, the number of analysands is greater. With respect to target hits, the individual die fall is either a target hit or it is not, but a three-dice trial may be a triple target hit, a double target hit, a single target hit or a zero target hit. Similarly, with respect to inter-trial (—1 and +1) displacement and opposite displacement, a three-dice trial may be a triple displacement, a double displacement, a single displacement or a zero displacement.

Because a die has more than one adjacent face, there are two alternatives for the analysis of adjacent displacement with the multiple-dice trial as the experimental unit. Multiple adjacent displacement may be taken as response to multiples of the same adjacent face, or it may be taken as response to multiples of adjacent faces, per se. For example, if the target face is 1, in which case the adjacent faces are 2, 3, 4 and 5, two 2's in the top position would constitute a double of the same adjacent face, while a 2 and a 3 in the top position would constitute a double of different adjacent faces. If the adjacent displacement results from response to the target face on the side of the die, there would be no distinction between multiples (doubles and triples) of the same adjacent face and multiples of different adjacent faces. On the other hand, if the adjacent displacement results from response to adjacent faces on the top of the die, there may be a difference in response to multiples of the same adjacent face and to multiples of different adjacent faces. Because it is more discriminating, multiples of the same adjacent face was used as the index of multiple adjacent displacement. On this basis a trial producing only one adjacent face or two or three unlike adjacent faces constitutes a single adjacent displacement. A trial producing three adjacent faces, only two of which are alike, constitutes both a double and a single adjacent displacement and is counted in both categories. The results of the analysis of the 14,000 three-dice trials are presented in Table 1.

Without correction, the chi square of opposite displacement is significant and the critical ratios of all of the double displacements are significant or marginally significant (Table 1). However, as analysis was made of ten factors (target hits, opposite displacement, adjacent displacement, —1 displacement and +1 displacement, both with the individual die fall and with the three-dice trial as the experimental unit), the p's listed in Table 1 should be multiplied by a

TABLE 1

TARGET HITS

Factor	Observed frequency	Deviation	CR*	Chi square	p with 3° freedom
triples	62	—4⅔	0.57		
doubles	991	—9	0.29		
singles	4970	—30	0.42		
zeros	8377	+43⅔	0.48		
				0.82	.92

INTRA-TRIAL DISPLACEMENT

Opposite Displacement

triples	54	—12⅔	1.55		
doubles	906	—94	2.97		
singles	5111	+111	1.57		
zeros	8329	—4⅓	0.01		
				13.71	.003

Adjacent Displacement

triples	278	+11⅓	0.69		
doubles	4140	+140	2.21		
singles	11897	—103	0.94		
zeros	559	+25⅔	1.11		
				7.50	.06

INTER-TRIAL DISPLACEMENT

—1 Displacement

triples	57	+1⅑	0.19		
doubles	771	—62⅓	2.16		
singles	4198	+31⅓	0.49		
zeros	6974	+29⅝	0.36		
				5.40	.15

+1 Displacement

triples	49	—6⅝	0.88		
doubles	904	+70⅔	2.45		
singles	4158	—8⅔	0.13		
zeros	6889	—55⅛	0.67		
				7.23	.06

* These critical ratios are the square roots of the corresponding chi-square entries, and are intended principally to show the degree of interest associated with the double displacements.

correction factor of ten.[1] When properly corrected, none of the chi squares or critical ratios are significant. However, to a rough approximation the five chi-square analyses of Table 1 are independent and, because of this, although their probabilities cannot properly be combined, the over-all significance is higher than is indicated by any one of the corrected p's of the separate analyses. In any case, significance is not expected in exploratory analyses that have large correction factors. The analysis fulfilled its objective in suggesting that displacement occurs in PK and that the trial of multiple dice rather than the individual die fall may be the effective unit. It serves its purpose in indicating the desirability of similar analysis of other PK experiments in which the upper faces of the dice were recorded after they had come to rest.

The results of the experiment also suggest that in it the adjacent displacement consisted of response to adjacent faces in the target or top position rather than of response to the target face in a nontarget position, i.e., on the side of the die. This is shown by the following comparison of the trials that produced a multiple of only like adjacent faces with the trials that produced a multiple of only unlike adjacent faces.[2]

	obs. freq.	dev.	CR
multiples of only like adjacents	1997	$+130\frac{1}{3}$	3.23
multiples of only unlike adjacents	6347	-53	0.89
		CR diff. =	2.70

Response to the target face on the side of the die would have caused multiples of only like adjacent faces and multiples of only unlike adjacent faces to have deviations of the same sign and not to have deviations of opposite sign that are shown above to have occurred. On the other hand, response to like adjacent faces in the target position would have caused multiples of only like adjacent faces and multiples of only unlike adjacent faces to have deviations of opposite sign as occurred. Although the results of the experiment suggest that, in it, the intra-trial displacement was to nontarget faces rather than to nontarget positions, they do not necessarily indicate this to have been the case in the experiment of Mitchell and Fisk (2) and, in this respect, experiments may differ.

[1] Also, because of the double analysis of Series 3, all critical ratio and chi-square values of Table 1 must be presumed to be slightly too large.

[2] A multiple of only like adjacent faces contains no unlike adjacent faces and a multiple of only unlike adjacent faces contains no like adjacent faces.

REFERENCES

1. Fisk, G. W., and Mitchell, A. M. J. "ESP experiments with Clock cards; a new technique with differential scoring," *Journal* S.P.R., Vol. 37 (1953), pp. 1-14.
2. Mitchell, A. M. J., and Fisk, G. W. "The application of differential scoring methods to PK tests," *Journal* S.P.R., Vol. 37 (1953), pp. 45-61.

Parapsychology Laboratory
St. Joseph's College,
Philadelphia, Pennsylvania

Research Note

In an attempt to test the hypothesis that the emotional relationship between friends may facilitate telepathic communication between them, under conditions in which one friend can save the other from stress by the transmission of a telepathic signal, in 1952 an experiment was performed at the rooms of the Society by S David Kahn, M.D. and Ulric Neisser in which forty subjects from a college in New York City were tested for the presence of telepathic transmission. In each trial the percipient had to guess whether the agent, 35 feet away in another room, faced a red light or a white light. If the percipient guessed right, his throwing a switch exempted the agent from an electric shock; if he guessed wrong, the agent received the shock. No positive results were achieved, either in direct hits or in terms of decline effects.

Spontaneous Cases

On March 2, 1956, Mrs. Alma Dettinger interviewed Dr. Gardner Murphy and Professor C. J. Ducasse on her program "Other People's Business," Station WQXR. The interview centered about the problem of spontaneous psychical experiences and listeners were asked to send in accounts of recent and clear-cut instances.

Among the letters received was the following, from Mrs. F. A. Taylor,[1] of Albany, New York.

(Undated—received March, 1956)

Dear Sirs:

Because of your comment on Alma Dettinger's program that you are interested in collecting individual experiences which may, or may not, have some psychical significance, I am enclosing a brief outline of two recent experiences of my own.

No. 1. Last July [1955] we drove West from Albany to Laramie, Wyoming. On the night before our arrival in Laramie we stayed in a motel. During the night I had a dream—a dream of incredible confusion centering around me. Eventually in the dream I started sobbing. I awakened and awoke my husband with my sobbing which continued after I woke up. I had no explanation for it, nor had I ever done such a thing before.

The next day we arrived in Laramie. I was handed a telegram saying that my father had been struck by a car on a highway in front of his home and killed (this was in Florida). It was nothing I could have anticipated. The confusion down there was considerable. My father lived alone near St. Petersburg. I was on the road and could not be reached. I am the oldest child and had the information regarding relatives, etc., which they needed. Somehow or other I have the feeling that all the confusion and panic came through to me along with the grief.

No. 2. Two years ago we made a trip through the Southern states during incredible heat. One day as we were driving along and passing one dreary motel after another, I rather facetiously said to my husband that motel owners should study psychology and build a motel that one could not pass up because it would appear to offer a real respite from the heat. I then described such a motel. Everything around us was hot, dry, and shimmering with heat. The motel I "saw" was as follows:

It would stand free and hospitable—would have a lake of lovely blue water in front of it, with flowers, and ducks swimming on the water. Some time later we drove over the crest of a hill—and there before us was the motel I had described. It

[1] Mrs. Taylor asks that pseudonyms be used in referring to herself and her family. The real names are on file at the Society.

somewhat resembled a Southern colonial house. There was a pond in front with ducks swimming on it.

The interesting fact was that we were so astonished that we did *not* turn in!

I might add that I had never been over that road before and had never seen a motel such as I had described.

Sincerely yours,

BARBARA TAYLOR
(Mrs. F. A. Taylor)

Mrs. Taylor was kind enough to fill out and return to us a questionnaire we had submitted to her:

[The dream of "confusion" occurred last July, 1955. Could you give the exact date.] Night of July 16th-17th.

[Did you note the time when you woke up sobbing?] No, to my regret since. But I had no idea at the time that I might feel this had significance.

[Where were you on the night of this occurrence?] In Caspar, Wyoming, at a motel.

[What was the exact date and time of your father's fatal accident? Did he die instantly? If not, how long after the accident?] Approximately between 5:30 P.M. and 6:30 P.M., between St. Petersburg and Tampa, Florida. It is reported that he was killed instantly—at least he never regained consciousness.

[Did you make any written record—diary notes, etc.—of your experience at the time?] I mentioned the experience in letters to several people and have written them to ask whether they saved the letters. They apparently were not kept.

[After receiving the telegram announcing your father's death, did you go down to Florida where you at first-hand saw the confusion, etc.?] No. I learned of it through correspondence with relatives.

[Any comments you would care to make about your health, mood, whether fatigued or not, etc., at the time of the experience would be greatly appreciated; also any comments about your relationship with your father.] Because I had had a minor operation in June, we had driven from Albany to Laramie by slow stages, sight-seeing en route. We were due in Laramie on July 17th. On July 16th we found ourselves near enough to Laramie to take a side trip to Caspar and still arrive in Laramie on the 17th. The weather was hot. I had not entirely recovered, but was in good enough health to have camped out at four or five places during the trip. On this evening, however, we stayed in a motel. I was undoubtedly fatigued, but not more so

than on any other evening of the trip when I was en route. I did not have an especially close bond with my father. I had been with him for four months five years ago during the prolonged illness and death from cancer of my mother. My father had spent December, 1954 and January, 1955 with us in Albany. He lived alone seven miles out of Tampa and I tried to keep in close touch with him.

Mr. F. A. Taylor sent us the following statement, dated April 19, 1956:

> This is to advise you that Mrs. Taylor woke me with her crying. She informed me that she had had a terrible dream—that she was crying hard in her sleep and had wakened up crying.
>
> F. A. TAYLOR

In addition to filling out the questionnaire, Mrs. Taylor wrote us as follows:

April 21, 1956

Dear Mrs. Dale:

I am enclosing a letter just received from my sister-in-law in answer to my questions. After receiving your letter, I decided to try to fill in the record of events for my own knowledge, because it is evident that my dream did not take place at the time of my father's death, but considerably later.[2] Since it was a dream of confusion—excessive confusion and demands—I have felt that if it related to the events of July 16th-17th at all, it was the family distress and problems that had come through to me. My dream took place late at night or during the early morning hours—until now, I have never attempted to find out exactly how these hours were spent by my brother and his wife in Florida, who had the unfortunate problem of coping with the unexpected tragedy, and who were least informed concerning relatives' names and addresses, etc. Accordingly, after receiving your letter, I wrote asking for an account of what transpired, how late they were up, when they tried to reach relatives, etc. I find, as you will see in the attached letter, something I had not known before—that they were up most of the night. Thus my dream could easily have taken place at the time they were engaged in meeting the problems of the situation. I am the oldest child, it was I who had most of the information that they needed, and I was unavailable.

Sincerely yours,
BARBARA TAYLOR

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[2] Mrs. Taylor's father was killed between 5:30 and 6:30 P.M., Florida time. This was between 2:30 and 3:30 P.M., Wyoming time. Mrs. Taylor's dream occurred much later than this.

The letter received by Mrs. Taylor from her sister-in-law in St. Petersburg, dated April 14, 1956, is too long to quote in full, but the following excerpts will verify the fact that there was a great deal of confusion following the accident:

> . . . We dressed and went to the hospital and sat there until around 10 P.M. We did not know what funeral home to have Dad taken to (there were two with the same name within a couple of blocks of each other) so we had to go to both to find the one Mother was buried from. It was only then that we went to Dad's home . . . We tried to find information that the funeral parlor wanted, but could not. We did not know Grandma's maiden name nor Aunt Lilly's name. John was debating whether to send you word at all as you had been so sick, but decided to, for what else could he do? So we sent the telegram . . .
>
> You mentioned the dream to me over the phone, I believe, as I know you did not write me about it. I recollect the crying part of it . . . We were wishing there was a way to contact you that night about Grandma's maiden name, etc., as it was daybreak before we gave up the search and still had not found it. . . .

It thus seems clear that Mrs. Taylor was needed in her father's home to provide information that the other members of the family did not have, and that a state of confusion prevailed in the household at the time of the dream.

Mr. Taylor also sent us a statement, dated April 19, 1956, corroborating Mrs. Taylor's second experience:

> Mrs. Taylor facetiously described what she imagined would be an enticing and desirable motel (the temperature was 106-108 degrees). Some few hours later we passed a motel that contained the salient features of the one she had described.
>
> F. A. TAYLOR

It may be of some interest to note that Mr. Taylor says it was "some few hours" after his wife described the motel that they passed the one that apparently fulfilled her description. This would seem to rule out the possibility that she might have seen, as they drove along, a billboard depicting the motel, noted it subconsciously, and that this had given rise to the description. If it was several hours before they reached the motel, they must have driven close to a hundred miles, and it seems unlikely that a motel would have an advertising billboard so far from its actual site.

L. A. DALE

Another Veridically Significant Dream

The following is an account of a recent additional case in the series of Mrs. Dommeyer's veridically significant dreams reported in the July 1955 issue of the A.S.P.R. JOURNAL. It is the first since then to have apparently resulted from a deliberate attempt unsuspected by Mrs. Dommeyer to induce it.

The new case is interesting also in that, although the dream did contain the symbol which over many years has invariably been followed shortly by receipt of otherwise unexpected money, and the dream was so followed also in this case, nevertheless, for the reasons mentioned in the statements by Mr. and Mrs. Dommeyer, it did not occur to them, at the time she described the dream to him, to interpret the presence in it of the symbol as predictive. In this as in most of the earlier cases in the series, telepathy would account for the occurrence of the dream. The facts are as follows:

STATEMENT BY C. J. DUCASSE

On July 23, 1956, I had to go to New York. That morning, before leaving Providence, the idea occurred to me to repeat my earlier experiment of sending Mrs. Dommeyer a dollar bill. Accordingly, I enclosed one in a plain envelope addressed to her, and put the envelope in my pocket. I had not mentioned to anybody my intention to do this, and I was equally careful not to mention to anyone that I had done it. That night in New York, as I left a friend's apartment at 10:10 P.M., I noticed a mail box at the corner of 16th Street and 7th Avenue. This reminded me that the envelope containing the dollar was still in my pocket, and I dropped it in the mail box there and then, recording the fact in my pocket note book. On July 27, I received a letter from Professor Dommeyer, stating that Mrs. Dommeyer had told him at breakfast on July 26 that she had, during the night of July 25-26, had a dream; that it contained the usual symbol; and that an envelope containing a dollar arrived by the noon mail, some four hours after the dream had been described at breakfast. Until I received Professor Dommeyer's letter on July 27, nobody but myself knew that I had sent that dollar or that I had planned to do so.

STATEMENT BY F. C. DOMMEYER[1]

On July 26, I got up at 6:00 A.M. to do some reading. About 8:15 A.M., my wife called me in for breakfast. She told me that she had a dream that the cats had messed up "the coop," i.e., she saw excre-

[1] This statement is a verbatim quotation from a letter I sent to Professor C. J. Ducasse on July 26, 1956. The letter was written between 1:00 and 2:00 P.M. on the 26th, less than an hour after the money had come to my attention.

ment in her dream. She said to me that she supposed she had had this dream because of her recent worry over the three cats we own. Actually, we now no longer let the cats sleep inside at night, but put them out in this shed-like room off the kitchen. We made this change because we had found them messing up our front room. My wife's explanation that the dream was a result of her concern over the cats was thus a very natural one. She did not think to take it as a sign. Neither did I. One may wonder why we did not take it as a sign after our past experiences with this kind of dream. All I can say is that we did not. I wanted to get out to the study again to finish my work and had other things on my mind. Also, we have had no dream of this kind occur or received any money for a long time; it must be quite a few months now since the last episode. For whatever the reason, all I can say is that it occurred to neither of us to interpret this dream as a sign of the coming of money, and we did not; hence, no post card, recording the dream at once as before, was sent. It was not a matter of carelessness. Had I thought of the dream as a sign-dream, I would have sent the card.

When I came home at 1:15 P.M. from my teaching, I looked at the mail. I found an envelope postmarked 1:00 A.M., July 24, N. Y. 1, N. Y., along with other mail. To my complete surprise, there was a dollar in the envelope in a piece of folded paper. My wife had opened the envelope before I had got home and already knew of the presence of the dollar when I looked in the letter. I turned to my wife (momentarily forgetting her statement about the dream made earlier) and said: "Well, here's a test dollar and you had no dream." She then said, "But I did have a dream and I told you about it." Of course, I remembered at once then that she had had the dream and had told me about it.

I then turned to Carl, our oldest boy, age 12, and asked, "Did you hear your mother make that statement about the dream around breakfast time?" He remembered it and restated accurately what I remembered my wife to have said at around 8:15 A.M.[1]

STATEMENT OF MRS. DOMMEYER

I awoke in the morning with disgust, for in my dream I had just finished cleaning the "Coop," a room our cats live in and sometimes misuse. I have cleaned up after the cats many times actually, and thought my dream was a result of my concern for this problem with the cats. It did not occur to me that it was a "sign" dream. Neither did it occur to my husband when I told him of it.

In the noon mail, a letter addressed to me, arrived with a dollar bill in it. I realized then that this was connected with my dream.

MARIAM DOMMEYER

[1] [Professor Dommeyer has kindly sent us Carl's statement to this effect. —Ed.]

Reviews

EXPLORING THE SUPERNATURAL: The Weird in Canadian Folklore. With 23 illustrations. By R. S. Lambert. Pp. 198. McClelland & Stewart, Toronto, 1955. $3.75.

This is a book worth adding to any psychical research library. It is by a man whose ability to write readably has not been dulled by prolonged and often disappointing research into source material. Also it is a book definitely enlarging our knowledge of psychic happenings in a land of which Rupert Brooke once said. "Canada? Canada has no ghosts."

Mr. Lambert himself seems to feel that Canada is a bit short in what, since Myers' day, has been called "visual veridical hallucinations." It may not be as short as he thinks, for I myself know several well-authenticated Canadian ghost stories not to be found in his book. But his book does have an abundance of other and most varied psychic occurrences, dating back to the coming of the first white men to Canada.

Those white men, according to the authors of *The Jesuit Relations*, found that the "medicine men" of the Red Indians, particularly in their Shaking Tent performances, possessed powers not equalled by the best cabinet mediums of today. These powers the Jesuit Fathers themselves investigated to the best of their ability. "Father Paul Lejeune," Mr. Lambert feels, "may be considered the first psychical researcher in Canada." The Jesuits found much they could not explain away, and not surprisingly credited the unexplained remainder to the Devil. For that matter, there still are some who see something diabolical in all psychic happenings. To this day the Shaking Tent persists, if only in the wilder parts of the Canadian West and North West. To this day, too, many Shaking Tent phenomena defy solution.

"For over three centuries," Mr. Lambert notes, "white men of all kinds — soldiers, missionaries, traders, and travelers — pried into the secret of the Shaking Tent. Some scoffed, or pronounced it a trick. Others admitted themselves completely baffled. No one, however, whether skeptic or believer, could explain satisfactorily how the phenomena were produced — the quakings and shudderings of the stout poles and their coverings; the weird lights that sparkled above the tent top; the eerie wailing voices that seemed to come from far and descend from the sky in a rushing wind upon the medicine man within; and, lastly, the oracular forecasts of the future that he uttered in his trance."

Not only prophecy, but some of the most debated phenomena of today, such as levitation, fire walking, and other fire tests — seem to have been commonplace among the Indian medicine men studied by the Jesuits and their successors. As to the early white settlers of Quebec, they had their witch hunts precisely as Massachusetts settlers had theirs. Also, beginning with the Indians themselves, Canada would almost seem to have been infested by that peculiarly obnoxious visitant, the poltergeist.

Perhaps because of a poltergeist experience in his own family, Mr. Lambert deals in most detail with these noisy, trouble-making, destructive visitants, which in one Nova Scotia case — "The Fire-Spook of Caledonia Mills" — started many fires and brought our own Dr. Walter F. Prince to the scene, at the request of the *Halifax Herald*. Dr. Prince's report to the *Herald* was not altogether to Mr. Lambert's liking, as Dr. Prince put the blame, not on any ghostly visitant, but on Mary Ellen, an adopted daughter of the tormented family:

"It was a case of obsession. The little girl was under the influence of some volition not her own . . . Or, as one might put it in more familiar language, Mary Ellen 'was possessed by an evil spirit.' "

Some years earlier Dr. Prince had analyzed another Nova Scotia haunting, with which Mr. Lambert deals in a chapter entitled "The Amherst Mystery." At that time another Prince — both Princes were friends of mine, but not related to each other — Dr. Morton Prince, the eminent Boston neurologist, had told me in answer to a question:

"He might as well save himself the trouble of dealing with the Nova Scotia material. He's sure to find the poltergeist in some hysterical girl subject to attacks of dissociation."

Which is precisely what Dr. Walter F. Prince did find, another case of split personality. Actually, however, in such cases there is no need of suggesting possible possession by an evil spirit, as Dr. Prince did suggest in the Caledonia Mills case.

A clue to the true explanation of most, if not all, poltergeist cases, as it seems to me, is to be found in recent discoveries of psychopathology regarding dissociation of personality so severe that it leads to the formation of a new personality with complete forgetfulness — total amnesia — of past events. The one thus affected may actually flee to some remote part of the country, taking a new name and perhaps a new occupation. This really is more than a bodily flight. It also is a psychological flight from some environmental condition that has become unbearable.

Or, if the unfortunate is so constituted that complete dissociation is impossible, some minor disease symptom may develop leading to a change in living conditions. Invariably, or almost invariably, that change is one enabling the individual to get something he or she much wants, or to get away from something detested.

It is indeed significant that poltergeists are most numerous in homes where one of the family — whether adolescent child or adult of immature psychological development — is unhappy either because of a sordid material environment or because, rightly or wrongly, he or she feels neglected and unloved. Then we have an inner emotional storm that culminates, not in a completely new personality with resultant bodily flight, or of some disease symptoms calling for special care and sympathy, but of a poltergeist outbreak.

In other words, most poltergeist outbreaks, in my opinion, call not so much for a psychical researcher, as for the study of the suspected central party by a qualified medical psychologist.

This, of course, is only one man's opinion. But it is an opinion to which I have long inclined. Mr. Lambert's book, with its unusually detailed accounts of the surroundings and personnel of poltergeist hauntings in Canada, inclines me more strongly to this opinion. In any case, as said at the outset, his is a well written book and one well worth having.

H. ADDINGTON BRUCE

SOME ASPECTS OF THE CONFLICT BETWEEN SCIENCE AND RELIGION: The Eddington Memorial Lecture, 1955. By H. H. Price, Pp. 54. Cambridge University Press. 3*s*. 6*d*.

In this lecture, Professor Price argues that there is genuinely a conflict between the essential contentions of religion and those of modern natural science; that a difference between them as to the nature and destiny of human personality is at the foot of the conflict; that the materialistic conception of the human personality adopted by contemporary biological science is based on *prima facie* impressive experimental evidence; that the facts psychical research has established give some support to the religious conception of man's personality, as against the biological conception; but that this support is for the most part indirect rather than direct, in the sense that what those facts show is not that the religious conception is correct, but only that the biological conception is to some extent somehow incorrect, since, if it were wholly correct, the facts which psychical research has proved to occur ought not to occur at all.

Thus, although they do not prove that the religious conception of man's personality is right, they anyway undermine the materialistic conception opposed to it, and warrant us in concluding that, contrary to what is today commonly assumed in scientific circles, it is not obvious nor certain that the religious conception is false.

C. J. DUCASSE

Brown University

THE SEARCH FOR BRIDEY MURPHY. By Morey Bernstein. With a new chapter by William J. Barker. Pp. 324. Pocket Books, Inc., New York, 1956. Cardinal Edition. Fifty cents.

The first edition of this book, which was published in January, 1956, and which has since then had ten printings, and editions in six foreign languages, was reviewed in the July 1956 issue of this Journal. The present new, paper-back edition contains an additional chapter, "The Case for Bridey in Ireland" by William J. Barker, of the staff of the Denver Post, who was sent to Ireland for three weeks by its editor "to conduct the only really intensive hunt for 'Bridey evidence' which has been made up to the present."

He points out that the purported debunkings of the Bridey affair, which have been published in various magazines and newspapers, are patently in most cases wishfully motivated and therefore warped by the fact that the idea of Reincarnation is both religiously and scientifically unorthodox at present in the Western world; and he states that he, being a reporter, has on the contrary "striven to be objective — to write honestly and without bias," finding "neither for nor against Bridey," but "leaving the decision up to the reader" (p. 268).

During his investigation in Ireland, it rapidly became evident to him that "three years, not three weeks, would be required to carry the assignment through to a point where it could be called final and conclusive."

The various points of the Bridey story, and of the would-be debunking of it, looked into and commented upon by Barker, cannot be detailed here, but only his statement be quoted that "Bridey's 'autobiography' stands up fantastically well in the light of such hard-to-obtain facts as I did accumulate" (p. 271), and that "conviction strengthens in her story, the more you explore it" (p. 287).

Of course, accuracy of such recondite facts in the story as are accurate, does not prove that memory of a previous incarnation is their source. Paranormal retrocognition would be another possible

explanation. So would that which Spiritualists would urge, namely, temporary possession of the hynotized subject by the surviving spirit of a deceased Bridey Murphy, as in the famous "Watseka Wonder" case. And another possible explanation would be that favored by currently orthodox psychology, namely, that such facts of the story as are correct were accidently learned in childhood, forgotten, and brought back to consciousness by hypnosis.

At all events, the *accuracy* of such recondite facts as happen to be accurate is the only thing in the affair that is in need of being explained; for the mere invention of a character, and of a life past, present, or future for it, by a hypnotized subject in compliance with the hypnotist's suggestions, is a commonplace occurrence, in itself no more significant than is the invention by a novelist of the lives of the characters in his novels.

To this reviewer, the most interesting aspect of the whole Bridey affair is the vast amount of irrelevant doctrinaire and emotional gibberish — some of it purporting to be scientific — which the book has brought forth from a variety of quarters.

C. J. DUCASSE

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C. J. DUCASSE

HISTORY OF THE SOCIETY

The First American Society for Psychical Research was formed in 1885, in consequence of a visit by Sir W. F. Barrett to this country, and Prof. Simon Newcomb became its President. In 1887 the Society invited a man of signal ability, Richard Hodgson, A.M., LL.D., sometime Lecturer in the University of Cambridge, to become its Executive Secretary, and he accepted.

This organization later became a branch of the English Society under the very able guidance of Dr. Hodgson until his death in 1905. The American Society for Psychical Research was then re-established with James H. Hyslop, Ph.D., formerly Professor of Logic and Ethics in Columbia University, as its Secretary and Director.

THE ENDOWMENT

The American Society for Psychical Research, Inc., was originally incorporated under the Laws of New York in 1904 under the name of American Institute for Scientific Research, for the purpose of carrying on and endowing investigation in the fields of psychical research and psychotherapeutics. It is supported by contributions from its members and a small endowment fund. The income of the Society pays only for the publications and office expenses, but does not enable the Society to carry on its scientific investigations. A much greater fund is required before this work can be carried forward with the initiative and energy which its importance deserves.

The endowment funds are dedicated strictly to the uses set forth in the deed of gift and are under control of the Board of Trustees, the character and qualification of whom are safeguarded, as with other scientific institutions.

Moneys and property dedicated by will or gift to the purposes of the American Society for Psychical Research, Inc., whether to the uses of psychical research or psychotherapeutics, are earnestly solicited. The form which such dedication should take when made by will is indicated in the following:

"I give, devise and bequeath to the American Society for Psychical Research, Inc., a corporation organization under the laws of the State of New York, the sum of ..... dollars (or if the bequest is real estate, or other specific items of property, these should be sufficiently described for identification), in trust for the corporate purposes of said Society."

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