

ESSAY

Half a Career with the Paranormal

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To begin with a definition, the word *paranormal* means communication without the currently recognized sensory processes; it may also refer to physical movements without the recognized physical processes. For centuries, phenomena now described as paranormal occurred and were described. Most historians of the subject agree, however, that systematic inquiries about such occurrences did not begin until 1882, when the Society for Psychical Research (SPR) was founded in London. Its founders openly stated their intention to investigate unusual phenomena.

I am a latecomer in this field, because my activity in it did not begin until I had already established myself in conventional psychiatry. I had had training in that specialty and in psychosomatic medicine. My research and training enabled me to advance in academic positions; in 1957 I was appointed professor and Chairman of the Department of Psychiatry at the University of Virginia.

How I reached that position requires a short digression. From birth on I suffered from repeated bouts of bronchitis and spent much time in bed. The illnesses held me back, but I read a great deal, and my succoring mother kept restoring my health. I have an unusually retentive memory, and in phases of good health I jumped ahead of my peers scholastically. Professors like superior students, and I became a favorite of some at McGill University. After I had recovered from several bouts of pneumonia, one of the professors advised me to leave the cold of Canada for the warmth of Arizona. While in Arizona, I somehow learned to improve my health. Thereafter, I resumed a normal upward path in training and academic placement.

On the way up I acquired some reputation as a maverick. This epithet seemed appropriate for someone who questioned the assumption, held then dogmatically by most psychiatrists, that human personality is more plastic in infancy and childhood than it is in later years (Stevenson, 1957). The publication of my challenge to this doctrine annoyed many of my colleagues in psychiatry and even enraged a few. For me, the reception of my article on this subject provided

useful training for responding to the rejection of my studies of paranormal phenomena.

About the time of my appointment to the University of Virginia I returned to an earlier interest. In childhood I was exposed to reports of paranormal phenomena through reading in my mother's extensive library about oriental religions and theosophy, the latter of which was a derivative of Buddhism and Hinduism. My training in medicine brought me some understanding of scientific methods, and I began to ask myself about the evidence for the unusual phenomena reported in the books I had read. It did not seem conclusive, but it also did not seem negligible. So I read more about psychical research, especially in the works of the founders of the SPR, such as Frederic Myers and Edmund Gurney, for whom I developed an abiding admiration. I also became acquainted with the leaders of the American Society for Psychical Research, which was a younger sister, so to speak, of the SPR. In this group C. J. Ducasse and Laura Dale especially earned my gratitude by showing me that skepticism about some evidence for paranormal phenomena did not exclude acceptance of other evidence.

I needed their guidance. My first publications in the field were book reviews, and one of the first of these almost exposed my inexperience publicly. I wrote a review of a book entitled *The Third Eye: The Autobiography of a Tibetan Lama*. Its author claimed to have been a Tibetan lama endowed with immense paranormal powers. I was taking him seriously until, just in time, I learned that the author of this book was an Englishman who had never been to Tibet, much less come from there. I modified my review (Stevenson, 1958).

Writing about a subject provides an excellent means of learning about it. Accordingly, I learned much by writing and then publishing in *Harper's Magazine* a review article about parapsychology entitled "The Uncomfortable Facts about Extrasensory Perception" (Stevenson, 1959). This earned the approval of Dr. J. B. Rhine, who was then director of a research laboratory at Duke University. (Rhine had renamed the field, or at least his substantial part of it, "parapsychology." Of this, he and his wife, Dr. Louisa Rhine, were undisputed sovereigns.)

In 1959 I visited the Rhines and their associates. After the conventional morning coffee with general conversation about parapsychology, Louisa Rhine led me into a side room for a private conversation. There she explained to me her belief that nothing substantial could ever be made of reports of individual cases. In her view, they were all worthless as scientific evidence. In my article in *Harper's Magazine* I had mentioned individual case reports and wrote that at least some of them deserved the attention of investigators. Louisa Rhine generously hoped to save me from futile endeavors. Her warning came too late. Some of the reports I read by the earlier psychical researchers of what were then called "spontaneous cases" had deeply impressed me. Despite her strictures about them, Louisa Rhine nevertheless studied spontaneous cases herself, but she did this almost exclusively only on the percipient's side of a case. The earlier investigators, however, had investigated both the senders (or agents) and the percipients (receivers) of the experiences. They noticed similar features in many

of the cases reported. Among these were a high incidence of sudden, often violent, death (or other serious crisis) in the agent and a familial or other emotional link between the two participants in a case.

I decided to investigate cases that came to my attention and began to publish reports of them. At this time—the late 1950s—an earlier interest that I had in reincarnation revived, and I quickly learned that few cases suggestive of reincarnation had been investigated. One of the few exceptions was a report of four cases published by an Indian investigator in a French journal (Sunderlal, 1924). (I later learned that the author had first offered his report to an American journal, which had rejected it.) I thought that perhaps even uninvestigated cases would reveal some feature of interest. I therefore examined the published details of 44 reports of claims to remember a previous life. I had come across these in newspapers, magazines, and books. Most of these reports gave few details, and almost none offered any verified (or even verifiable) evidence. I winnowed the 44 cases by excluding those in which the subject and presumed deceased person were related or well acquainted and those in which the subject made six or fewer statements about the claimed past life. Of the remaining 28 cases, the age of first speaking about the previous life was known in 25. In 22 of these, the claimed memories had first been uttered when the subject was a child less than 10 years of age. This seemed worth wider attention. Accordingly, I published (in the *Journal of the American Society for Psychical Research*) a two-part article on these cases and recommended that more such children should be sought and their claims investigated (Stevenson, 1960a,b).

It never occurred to me then that I would be the person to initiate the investigations that I advocated. I was too busy: administering a department, caring for patients, and engaged in other research. My paper had, however, come to the attention of two persons whose interest and support it stimulated. They influenced my life profoundly.

The first of these persons, Eileen Garrett, was both a spiritualist medium and a remarkably successful entrepreneur. She had persuaded a wealthy donor to establish the Parapsychology Foundation, of which Eileen was the President. I first met her in about 1957 and, at the time, mentioned my interest in reincarnation. Early in 1961 she telephoned me and said that she had received a report of a child in India who claimed to remember a previous life. The child seemed to be like the ones I had mentioned in my article. Mrs. Garrett asked me whether I would be interested in going to India to investigate the child's claims. The Parapsychology Foundation would pay my expenses. I accepted her suggestion, with the understanding that I could only go to India during my vacation, in August. When August came, I went to India and spent four weeks there and then about a week in Ceylon (now Sri Lanka). Before leaving for Asia, I had some fragmentary information about three or four other cases in India and about two in Sri Lanka. This information did not prepare me, however, for the surprise of finding an abundance of cases in both countries. By the time I left Asia I had learned of no fewer than 25 cases in India and seven in Sri Lanka. In less than five

weeks, I could not adequately investigate all these cases and so selected a few to study carefully. I noted the locations and a few details about the other cases.

A second surprise for me during this first trip to India came when I learned that the cases consisted of much more than a child's claim to remember a previous life. The children also showed behavior that was unusual in their families and that, in those cases in which the claims were verified, matched the behavior of the deceased persons the children claimed to have been. My first journey to Asia therefore showed the need for more journeys.

This brings me to the second important reader of my 1960 article in the *Journal of the American Society for Psychical Research*. This was Chester F. Carlson, the inventor of xerography. He had trained as a scientist, and before his second marriage he believed, as most scientists did (and still do), that the mind is only a product of the brain and its properties entirely physical. His second wife, Dorris, had some capacity for extrasensory perception. She impressed her husband with her ability and also influenced him to support research into paranormal phenomena. Early in 1961 he offered funds for my research after I had already committed myself to going to India in August. I told him that I could not honestly accept additional funds at that time. (Before leaving for India I did nevertheless accept from him a few hundred dollars for a tape recorder.)

When my first work in India showed the need for further journeys there, it occurred to me that I could make those journeys if I could reduce the time I was then giving to clinical practice. Chester Carlson made this possible with annual gifts to the University of Virginia. In 1964 he made a particularly large donation that became the "deposit," so to speak, for an endowed chair of which I was the first incumbent. It was, incidentally, one of the first such chairs at the University of Virginia. The funds of the endowed chair gave me more time for research, but the expenses of journeys to investigate cases still needed annual donations, which Chester Carlson also provided.

As a donor of funds for research, Chester Carlson was unusual, perhaps unique. He insisted on giving anonymously, but other donors have done this. Most donors, however, later remain detached from the details of the research they support. Chester Carlson, in contrast, followed the details of research—at least of what I was doing—with keen interest. He said he would like to observe some of my interviews, and he accompanied me on one of my field trips to Alaska, where I was studying cases among the Tlingit peoples. He sometimes asked questions, but was never obtrusive. He rarely made suggestions, but what he said always deserved attention. My friendship with him belongs among the most pleasant and also, as I shall explain, the most important of my memories.

The report of my first studies in Asia was in press when unexpectedly a man who had helped me with some cases was accused of cheating. Although the allegation applied to experiments with which I had nothing to do, suspicion spread to the work the accused man had done for me, and the editor stopped the printing of my report. I had had other interpreters besides the man accused of cheating, and, believing that the man had not cheated when working with me,

I proposed to return to India and study the cases anew. Yet this entailed great additional expense, and I asked Chester Carlson's advice. He encouraged me to return to India. I did this and, with new interpreters, showed the authenticity of the cases. The printing of my report was then resumed, and it was duly published as *Twenty Cases Suggestive of Reincarnation* (Stevenson 1966|1974a).

During the eight years of Chester Carlson's support of my research (1961-68), I was still not exclusively committed to the study of paranormal phenomena. My bibliography shows that my interest in psychiatry and psychosomatic medicine had not diminished. I had and still have a keen interest in the question of why a person develops one kind of illness instead of another kind. Papers touching on this subject could be published in conventional journals when studies of paranormal phenomena could not. In 1960 I published a book on interviewing (Stevenson, 1960|1971). A few years later I published another book, really a textbook, on psychiatric examinations (Stevenson, 1969).

In this period I widened my studies of paranormal phenomena beyond the children who claimed to remember past lives. For example, I investigated and published papers on apparitions, precognition, mediumship, and "psychic photography." In 1970 I published my first book on paranormal phenomena, one on what I called "telepathic impressions" (Stevenson, 1970). (This gave Dr. Louisa Rhine, who reviewed the book, an opportunity to belittle more publicly the study of spontaneous cases.) My most important accomplishment of this period, however, was the mentioned publication in 1966 of my book *Twenty Cases Suggestive of Reincarnation* (Stevenson, 1966|1974a). This presented reports of the cases with abundant details about the informants for each case and what they had said about the subjects' claims to have lived past lives.

In 1968 Chester Carlson died. I was just one of many persons who mourned his death as a personal loss. His friendship and that of his wife, Dorris, had enriched my life beyond measure. For me, however, his death also meant the end of his annual subsidies for my research. I remember thinking I would have to return to the other half of my career, the conventional one of research in psychiatry and psychosomatic medicine. Then, to the astonishment of a great many people, not least myself, we learned that Chester Carlson's will bequeathed to the University of Virginia a million dollars for my research on paranormal phenomena. Not surprisingly, this provoked a controversy among the University administrators. I learned afterwards that some adversaries of my research had said that I could take the million dollars with me if I would leave the University. (No one said this directly to me.) The President of the University (Edgar Shannon) had not long before publicly cited an oft-quoted statement of Thomas Jefferson, written in 1820 as he was in the process of founding the university. "This institution," Jefferson wrote, "will be based on the illimitable freedom of the human mind. For here we are not afraid to follow truth wherever it may lead, nor to tolerate any error so long as reason is left free to combat it" (Lipscomb & Bergh, 1903:303). Even the most obdurate opponents of my research did not dare act against Jefferson's precept. My supporters therefore prevailed, and the

University accepted Chester Carlson's bequest. For this, I owe much to President Edgar Shannon and also to Thomas Hunter, then Chancellor of Medical Affairs.

Even before Chester Carlson's death, I had decided that I wanted to devote full time to research on paranormal phenomena, particularly those suggesting life after death. In 1967 I resigned as Chairman of the Department of Psychiatry after negotiating the establishment of a small Division within the Department. I did not wish the word "parapsychology" in the title of the new Division, because I thought that would imply and even facilitate a separation from psychiatry and medicine. That, however, was exactly what my successor as Chairman seemed to wish—an insulating distance between our research and respectability. (Later, under a friendlier administration, I readily obtained authorization to change the Division's name to the one I had earlier wished: Division of Personality Studies.)

During the 1960s and through most of the 1970s, I worked alone at the University of Virginia. When I was in Asia, I had some excellent interpreters assisting me, but they all had regular occupations to which they returned as soon as I left. We needed more continuity. Chester Carlson's bequest and some funds from other donors made it possible for me to engage a Research Assistant and to support other investigators.

The first of the other investigators was Gaither Pratt. He had for many years been a close associate of J. B. Rhine, but when Rhine retired from Duke University and established a private foundation (to which he took the funds then held by his laboratory), Pratt had no place in the foundation. At this point (1964) Chester Carlson offered to fund Pratt if we could find a place for him at the University of Virginia. I welcomed this proposal, but had to use all my diplomatic skill to persuade the Dean of the Medical School to agree with me. With some reluctance he did so, remarking as he did, that "This is something that we cannot keep private."

For five years after Chester Carlson's death, Dorris Carlson gave the Division annual donations. This enabled us to continue supporting Gaither Pratt and two other able parapsychologists, Rex Stanford and John Palmer. The publications of these three researchers, then and later, provided an important chapter in the history of parapsychology. When, in 1973, Dorris Carlson withdrew her support, I was obliged to encourage my colleagues to find other positions.

Later, our fortunes revived, and in one way or another I could afford to have colleagues again. Bruce Greyson, Satwant Pasricha, Emily Kelly, and Antonia Mills came to me and in one or another way moved from being assistants to become independent investigators. More recently Jim Tucker joined our group and has already shown himself a prolific and highly competent investigator and author. I should here also mention Erlendur Haraldsson of the University of Iceland and Jiirgen Keil of the University of Tasmania. They maintained their local academic positions, but received funding from our Division that enabled them both to work independently and to collaborate with me on some joint projects. Walker Cowen, founder and Director of the University of Virginia Press (to give its current name), became my publisher from 1970 until his death in 1987. He enabled me to put into print a substantial number of case reports that would otherwise still remain in

typescript on my shelves. He acknowledged that my books "are for the future." Unfortunately, he died before the future he expected had come, and his successor had a different opinion of what that future should be. I had to find a new publisher; but fortune favored me again and led me first to Praeger Scientific Publishers and then to Robbie Franklin of McFarland and Company.

Some of my later books were reviewed in general scientific journals, but most were not. Along the way, I have learned much about the power of book-review editors and that of editors also. For example, in 2000 I sent a review paper about the children who claim to remember past lives to David Horrobin, the editor of *Medical Hypotheses*. He had founded this journal to provide a publication for deviant ideas and research on unconventional topics. It had referees, and he sent my paper to several of them. Then he wrote to me, saying he could not find anyone who would take my paper seriously, but he was going to publish it anyway, which he did (Stevenson, 2000).

I believe I am best known for my studies of children who claim to remember past lives. I cannot object to that, but I hope other investigators will continue some of the other approaches to the evidence for life after death that I explored. Here I am thinking of cases of responsive xenoglossy (unlearned language) about which I published two books (Stevenson, 1974b, 1984) and the combination-lock test (Stevenson, 1968) and "drop-in" communicators (Stevenson, 1973). Fortunately, my successors are not bound by my ideas. Emily Kelly's ongoing studies of mediumship show her independence.

In 1980 I met yet another man who greatly influenced my life. A colleague at the University of Virginia introduced me to Peter Sturrock, who explained to me his idea for what became the Society for Scientific Exploration. He invited me to join the Founding Committee, and I did so enthusiastically. The Society's meetings and its *Journal of Scientific Exploration* provide a forum where research on paranormal phenomena can be presented to other scientists without obstruction or derision. The Society also welcomes presentations of research on many other phenomena neglected by most scientists. The founders of the Society believed, and I think they and their successors still believe, that the very existence of the Society challenges other scientific societies to liberalize their policies toward unconventional ideas and investigations. This has not yet happened.

Yet we must persist. I think we should do so uncomplainingly. I am myself weary of reading lamentations about Galileo, Wegener, Jenner, and numerous other scientists whose contemporaries at first rejected their novel ideas. We cannot expect all skeptics of new ideas to surrender as a whole, collapsing simultaneously like the walls of Jericho. Each of us must contend for our own new ideas. We are blessed that we can at least expose them to some other scientists through the opportunities afforded by the Society for Scientific Exploration.

The Society for Scientific Exploration offered me the first opportunities to report adequately two of my most significant investigations. I refer, first, to the birthmarks and birth defects that occur frequently in children who remember past lives, and, second, to what I believe are important residues of unusual

behavior derived from past lives. Informants drew my attention to these two features of the cases as early as my first journey to Asia in 1961, and I find it now a source of chagrin that I did not publish full details of the birthmarks and birth defects until 1997 (Stevenson, 1997a,b).

Some readers of my publications may regard my monograph *Reincarnation and Biology* as my *Meisterwerk*. With regard to mere bulk (2 volumes, 2268 pages) no one would disagree. I hope, however, that the work is more than a compilation. It includes reports of cases and additional details about cases that I had not previously published. The chapter on twins (one or both of whom claim to remember a past life) may be one of the most important of all my publications.

As for the behavioral residues of past lives, I have repeatedly drawn attention to their importance as a third component to the development of human personality, the other two being genes and the environment after conception (Stevenson, 1977,2000). In a paper recently published (with Jiirgen Keil) I have recurred to this important feature, which is well exemplified in the cases of children of Myanmar who remember previous lives as Japanese soldiers killed during World War II (Stevenson & Keil, 2005).

We often cannot identify important aspects of events as such when they happen. My second marriage provides a significant example of this. In 1985 I married Margaret Pertzoff, who was then a professor of history at Randolph-Macon Woman's College. She was and remains an avowed skeptic of paranormal phenomena. She did not conceal her stance on the subject, but never allowed it to interfere with the happiness she brought me with our marriage. Her benevolent silences sometimes provided a valuable check on what might have otherwise become unwarranted enthusiasm on my part.

In 1997-98 I committed myself to a project that seemed foolhardy, but also had the possibility of making my research better known to the general public. I agreed to a writer's request to accompany me on field trips in Asia. He would "look over my shoulder" as I conducted my interviews for the cases. He was to pay his own expenses and afterwards would be free to write about his experiences without censorship by me. This turned out well. The writer was Tom Shroder, who is now a senior editor with *The Washington Post*. Tom was a companionable traveler, and he endured well the frequent roughness of journeys in Lebanon and India. The book he wrote is entitled *Old Souls: The Scientific Evidence for Past Lives* (Shroder, 1999). It seems fair to me and, more importantly, fair to the children who claim to remember past lives. The book has indeed made better known the cases of these children.

My physical journeys are now over, at least for this life. Nonetheless, I do not regard the time I devoted to psychiatry and psychosomatic medicine as ill-spent. On the contrary, I think that it gave me a helpful preparation for whatever I have later accomplished in studying paranormal phenomena.

We all die of some affliction. What determines the nature of that affliction? I believe the search for the answer may lead us to think that the nature of our illnesses may derive at least in part from previous lives. The cases of children who claim to

remember previous lives and who have related birthmarks and birth defects suggest this; some such children have related internal diseases. My own physical condition, defects of my bronchial tubes (from early childhood on) of which I have written separately (Stevenson, 1952a,b), has given me a personal interest in this important question. Let no one think that I know the answer. I am still seeking.

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