

## 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,











