JOURNAL OF SCIENTIFIC EXPLORATION

A Publication of the Society for Scientific Exploration

Volume 22, Number 1 **2008**

LIST OF ILLUSTRATIONS

Journal of Scientific Exploration, Vol. 22, No. 1, pp. 1–1, 2008 0892-3310/08

EDITORIAL

Henry Bauer is a very hard act to follow. So hard, that I shall not even try. It will take a team of editors to reproduce what Henry could achieve single-handedly. At my urging, and responding to the entreaties of several other officers, Henry graciously agreed to take on the job of Editor in the year 2000. As I recall, he agreed to take it on for only one year, but we have been able to prevail upon him to serve much longer than that. He has served as editor with energy, efficiency, good judgment, and good nature. I shall miss reading his entertaining and enlightening editorials in each issue of the *Journal*. Thank you, Henry, for your extraordinary service to the Society. I now look forward to receiving from you articles, essays, and perhaps an occasional Letter to the Editor.

There is no way that I could undertake to serve as editor *à la mode de* Henry Bauer! I can serve only as the first among equals. I am fortunate that seven capable scholars have agreed to serve as Associate Editors, including SSE Members Stephen Braude, York Dobyns, Bernard Haisch, Roger Nelson, Dean Radin, and Mark Rodeghier. I am particularly grateful that Bernie is willing to serve an Associate Editor, since he brings with him his eleven years of experience (from 1989 to 2000) as Editor-in-Chief. I saw the need for an Associate Editor from the medical profession, and I owe it to Wayne Jonas that he steered me in the direction of John Ives, who has kindly agreed to serve as an Associate Editor.

In order to make the job of Editor-in-Chief manageable to someone who does not have Henry's abilities and stamina, the editorial duties are being shared among us all. Our policy is that each submission is assigned to an Associate Editor, who retains responsibility for the submission until it is either rejected or accepted. (Thank you all, Associate Editors.) We are all fortunate that David Moncrief has agreed to continue his invaluable service as Book Review Editor. The book review section is a greatly appreciated part of the journal. (Thank you, David.)

Members and Subscribers should know and appreciate that the fine and timely production of this journal is possible due only to the skilled, efficient, and courteous service of the Allen Press staff–in particular of the Managing Editor Joy Richmond and the former Assistant Managing Editor Lindsey Buscher. (Thank you, Joy and Lindsey.)

As you will see, Volume 22, Issue 1, is a very special event. The entire issue is dedicated to the memory of Ian Stevenson, who was a member of the SSE family before there was even a society, and well before it was finally named. Ian was a member of the Founding Committee, which also included our long-time Secretary Laurence Fredrick, our long-time Treasurer and President Charlie Tolbert, and our long-serving Vice President Bob Jahn. The compilation of this issue has been in the hands of Emily Williams Kelly and Carlos S. Alvarado, to whom I am most grateful.

Serving as Editor of *JSE* is a big responsibility, of which I am keenly aware. It would be hard to improve on what Henry has done. But if you see ways to improve on what I am doing, please let me know.

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EDITORIAL

Theme and Variations: The Life and Work of Ian Stevenson

Emily Williams Kelly and Carlos S. Alvarado

Shortly after Ian Stevenson died on February 8, 2007, Henry Bauer asked us to guest-edit an issue of this *Journal* to be devoted to him – our mentor, colleague, and friend. Most readers of the *Journal* will probably agree that no one deserves such an honor more than Ian. He was a founding member of the Society for Scientific Exploration, but more importantly he exemplified the kind of scientist that the SSE was founded to encourage. For example, in 1957 he challenged a central tenet of psychoanalysis, then the predominant ideology in psychiatry, by publishing a paper in the *American Journal of Psychiatry* entitled ''Is the Human Personality More Plastic in Infancy and Childhood?'' He later said that, after this paper came out, a colleague in psychiatry asked him whether he could walk the streets unarmed (Stevenson, 1990, p. 8). Throughout his career he similarly challenged entrenched assumptions, not only in his extraordinarily well-reasoned and well-written publications but also in his painstaking investigations of empirical phenomena suggesting the need for a more comprehensive understanding of personality and consciousness, particularly the spontaneous phenomena of psychical research and, more specifically, phenomena related to the question of survival after death.

We are not at all certain that Ian himself would have welcomed the suggestion that an issue of this *Journal* be devoted to him. When he retired as Carlson Professor of Psychiatry and Director of the Division of Personality (now Perceptual) Studies at the University of Virginia, some of us told him that we wanted to hold a party to honor his nearly 50 years at the University. Ian's reply: ''Well, you can have a party if you'd like, but I won't be there.''

Regardless of whether Ian would have approved of an entire issue devoted to him – even in a journal that he helped found – we went ahead and gathered contributions from among his many students, colleagues, and friends. We begin with some longer pieces that focus on some (although by no means all) of the many aspects of Ian's life and work. Among these is a piece by Kerr White, Ian's older brother. The brothers clearly came from a remarkable family because, like Ian, Dr. White is a physician and prolific writer who tackled important issues that few others had the courage to take on – in his case, health services research, or the objective evaluation of medical treatments, a field that he was

instrumental in establishing. These longer pieces are followed by shorter reminiscences and comments from many people who worked with Ian during various periods of his career. At the end, we have reprinted an essay that Ian published 50 years ago in *Harper's Magazine* entitled "Scientists with Half-Closed Minds." Probably not many readers of this *Journal* will have encountered this essay before, but they will surely find it of great interest because – regrettably – it is as pertinent today as it was 50 years ago.

Readers will see that the contributions to this issue are all variations on a theme – that an extraordinary human being put all his immense capacities and energies to work on the most important question a person can ask: Who and what are we? We hope that, just as Ian personally inspired so many of us, this tribute to him might also inspire others to study his work and continue where he had to leave off. Because the number of his publications is so enormous, we have not included his bibliography here, but much of it is available at http://www.pflyceum.org/167.html

Reference

Stevenson, I. (1990). *Some of My Journeys in Medicine*. The 1989 Flora Levy Lecture in the Humanities. Lafayette: The University of Southwestern Louisiana. (Available online at http://www.healthsystem.virginia.edu/internet/personalitystudies/publicationslinks/some-of-my-journeys-in-medicine.pdf)

Journal of Scientific Exploration, Vol. 22, No. 1, pp. 11–17, 2008 0892-3310/08

ESSAY

Ian Stevenson: Recollections

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Memories are fallible, but fragments, however biased, may help to provide insights in our attempts to understand and remember one another. Ian Pretyman Stevenson was born in Montreal, Canada, on October 31, 1918, and died in Charlottesville, Virginia, on February 8, 2007. He (my brother—our different surnames is another story) and I as children, bundled in heavy winter gear, enthusiastically constructed snowmen in the chilly Ottawa winters and in summers fished for bass by a beautiful waterfall on the Rideau River that courses through Canada's capital city, where our parents lived. But all was not play; the serious business of childhood was education. Our mother, years ahead of her time, schooled us initially at home, using Montessori methods and equipment, before we began our formal education. Even that was ahead of its time. We started at the so-called ''Normal'' school—a government-sponsored experimental teachers' training institution. Although we both, I believe, enjoyed the experience and flourished, it was short-lived. When Ian was about five (and I two years older), his health became a major issue. I recall him having frequent bouts of bronchitis, for which our mother applied a hot, gooey, plaster-like substance labeled ''antiphilogistine''—larded onto huge planks of cotton batten—applied to the chest, front and back, and replenished every two days or so. I don't recall Ian having a chronic cough at this stage, but it was determined that the cold Canadian winter climate was too harsh for him. Our mother insisted that she, Ian, and I must de-camp for California. Our father's job as a political newspaper journalist for the *Toronto Star* made accompanying us impractical, he is said to have argued. In retrospect there is strong reason to believe that, after seven years, our parents' marriage was none too blissful.

In 1923, when Ian was five, we had the thrilling experience of our first train trip, the International Limited followed by the famous Santa Fe Chief, on our way from Ottawa, via Toronto and Chicago, to Los Angeles. Our father said good-bye as we boarded; it was a sad evening. One day Ian could not be found in our usual sleeping car quarters. Duly dispatched by our mother, I found him learning to play poker with our Pullman car porter! On arrival, we settled into

a pleasant bungalow that still exists at 2453 Beechwood Drive in Hollywood. Shortly thereafter our mother became deeply depressed and spent large parts of each day in bed. Somehow we got ourselves up, breakfasted, and off to school. First there was a public school where, among other experiences, we observed the first Zeppelin overhead and then a cow that was brought in on a flat-bed truck to demonstrate that milk did not originate in a bottle. Later we went to a private school, but we still faced a dismal home life. There was some relief afforded by, for example, watching a movie being made of Keystone Cops seeming to raid our neighbor's home as the husband fled before his wife, chasing him with a rolling pin held high. Ian and I became friends with the famed Peter the Hermit, who lived with his dog in the Hollywood Hills close behind us. During our frequent visits to Peter's cave-like abode, he treated us from large tubs of nuts, figs, apricots, and raisins. There were opportunities to meet movie stars like Mary Pickford (a fellow Canadian whom our father knew) and Douglas Fairbanks. We also were able to shake the hand (a.k.a. ''hook'') of the Pirate Captain in the first film made of Peter Pan. These outings added interest to a somewhat bleak period. Our father visited periodically as our mother slowly recovered from her depression. She believed she was helped by a circular gadget consisting of a bundle of multiple wires covered in velvet, worn around the waist, and plugged into an electric outlet. Although the treatment was accompanied by a faint rumbling sound, no one was electrocuted.

The most fascinating aspect of that two-year period was our mother's encounters with Richard and Isabella Ingalese. There is, indeed, some reason to believe that her desire to meet them may have been added to Ian's poor health as a reason for our sojourn in California. The Ingaleses wrote at least five books that our mother gave each of her children. In them they recounted the logic for their belief in reincarnation and the ubiquitous influence of karma as an explanation for the many diverse expressions of the human condition. They also spoke and wrote earnestly about a coming global ''cataclysm.'' These dire predictions seemed to cloud our mother's viewpoint so that she acquired a cache of gold coins that we helped her bury in our garden behind the bungalow. When Ian was seven and I was nine, we returned to Ottawa, probably at the insistence of our father, who refused to continue the expensive trips to California. Ian's health also may have improved a little. By this time, there seems little doubt that our mother was now well fixated on the Ingaleses' view of the cosmos and on the veracity of reincarnation.

Back in Ottawa we returned to the Normal school and lived for a time in an apartment, until about 1925, when we moved to a house, 1 Maple Lane in Rockcliffe, a suburb of Ottawa, and started attending the local four-room public school. Life was calm and relatively pleasant. Our twin siblings, born in 1926, had a ''nanny'' to help with their care, and there was a live-in cook. We organized a local baseball league, helped our father care for chickens and pigeons, and went fishing with him, while our mother was a full-time homemaker. Although she had not attended university, she read avidly and continued to

augment her substantial library of volumes on the occult, religion, and the then-expanding popular arena of theosophy—"a kind of potted Buddhism for Westerners," as Ian later described it. In 1929, the start of the Great Depression, our father was appointed the Canadian correspondent of the *Times* of London—the premiere journalism job in the country. With his increase in income and professional status, we moved to a larger home at 390 Lisgar Road in Rockcliffe. This substantial spread (now the home of Russia's ambassador to Canada) provided room for more chickens, a larger vegetable garden, and our mother's beautiful English herbaceous border, as well as a generous library for her rapidly enlarging book collection. Life was comfortable for several years, although our home felt the impact of the Depression as we housed two homeless men for over two years—one in the basement and a second in the attic.

Ian and I had many friends, built tree houses, trapped muskrats, set off loud explosions from gunpowder we manufactured, let our friends use (for a fee) the paper cup and string telephone system we assembled from a roof-top balcony, helped with feeding and cleaning the chicken house (our father was big on chickens), and completed elementary schooling. In addition, we established the World Travel Bureau, with printed stationery and a purpose-built display rack for travel brochures. To stock its shelves we visited hotels in Ottawa and Montreal with suitcases that we filled with substantial supplies of duplicate brochures from their diverse racks. Ian also sought material from travel companies. An enquiry to the Cunard Steamship Line resulted in an agent appearing at our front door asking to see Mr. Ian Stevenson about his plans for the S.S. Franconia's annual world cruise—only to be told by the maid that he had just gone out the back door on his roller skates! After reading about a yacht J. P. Morgan had purchased, Ian wrote the builder asking for details of other yachts they might build or had for sale; still another salesman called at our home. Our father then intervened by insisting that these enquiries cease. In later years Ian did own a more modest sailboat.

About 1931 Ian was off to England to attend an innovative "public" (i.e., private) school—Bryanston, in Dorsetshire. The school employed the newly established Dalton educational system of learning. It probably was our father who advocated an English education and our mother who pushed for the Dalton system, as she had done earlier for the Montessori one. So Ian was abroad for over 10 months each year. He developed a voracious appetite for history, reading widely and memorizing almost every historical date of any importance worldwide. During this time, in 1935, Ian started keeping track of all the books he read, and this continued until about three years prior to his death; there were 3535 entries. At the back of many books he read there were notes of dates, places, persons, events, and other things that he wished to remember, as well as annotated observations on errors of all kinds. His reading was eclectic, but when he was home they included works in our mother's library on, for example, comparative religions, theosophy, occultism, sex (on the top shelf!), contraception, herbalism, homeopathy, osteopathy, gardening, and medicine. Our father's

reading covered contemporary politics, biography, and history. Although he read multiple domestic and foreign newspapers and journals as well as two or three books a week, our father kept fewer volumes of his own and borrowed heavily from the Canadian Parliamentary Library. In addition to his day job with the *Times* of London, he was the Canadian correspondent for *The Economist, Boston Transcript, Baltimore Sun, Detroit Free Press, New York Times, The Scotsman,* and several Australian and South African papers. Clearly we had ample exposure to contemporary domestic and international news and literature. There was also a steady stream of politicians and fellow journalists—domestic and foreign—for meals or evening talks, some of whom we met and others we were told about. Lord Tweedsmuir (a.k.a. John Buchan, the novelist), then Governor-General of Canada, used to consult our father frequently and is reputed to have said about him that ''he knows everyone worth knowing in Canada and something 'bad' about each of them!'' Our father was an incorrigible gossip, and all his life Ian shunned and took a very dim view of this behavior.

Summers were occupied by weekday attendance at the Rockcliffe Vacation Club organized by our mother. Here we learned to swim, camp, construct handicrafts, box, wrestle, fire rifles, cook meals, and pursue physical exercises; these were thoroughly worthwhile experiences. Our young lives were further enriched by summer train trips our mother took us on to Montreal, Quebec City, Murray Bay, Niagara Falls, Toronto, and New York. On the latter trip we met her numerous well-heeled relatives in their Fifth Avenue apartments and nearby estates. At one abode we were shown the chair occupied by Colonel Edward House, long-time personal advisor to Woodrow Wilson. To us, however, it was just another chair! An elaborate British Commonwealth Conference in the summer of 1933 brought an array of fancy politicians and diplomats to Ottawa—many of whom our parents were expected to entertain. Ian and I were required to shake hands with several of these allegedly ''famous'' dinner guests. I don't recall their names, although Ian may well have since he had an almost flawless memory for places, events, dates, and names.

In 1937 Ian entered St. Andrews University in Scotland to major in history. I am uncertain why it was selected, bearing in mind that our father had attended Oxford University. It may have been because our father's sister, Aunt Mary, who was a physician, lived nearby in Alloa; Ian could have stayed with her during short vacations. It may also have been because she could obtain medical care for his increasingly bothersome bronchiectasis—a chronic, almost incurable, lung infection. In 1939 Ian returned home for the summer, but with World War II looming he switched to McGill University in Montreal, where he completed his bachelor's degree by continuing his preoccupation with history, in addition to the study of physics, chemistry, and biology. I think it was our mother's interest in medicine, possibly augmented by that of our father's sister in Scotland, that resulted in his enrollment in McGill's Faculty of Medicine in 1940. He progressed brilliantly and in 1943 graduated first in his class with a gold medal. An internship at the Royal Victoria Hospital in Montreal followed.

During some of this period, Ian and I both lived in a low-budget boarding home. Here the proprietor, Monsieur Mercier, served us dinner each evening that consisted of a long-running, permanent ''consommé'' soup spruced up daily with more water and a smattering of peas and carrots, plus an ''entré'' consisting of a couple of slender slices of tough meat, a boiled potato, an anemic carrot or two, and all topped off with a slice of limp, unadorned cake. This spartan fare was augmented by our weekly attendance each Sunday at a nearby pastry shop, where we over-indulged in calorie-rich edibles that we referred to as ''crud.''

Ian was ineligible for military service in World War II because of his poor health; it was sadly not improving. On the advice of his physician, who was also the medical school's dean, Ian moved in 1944 to the warmer and drier climate of Tucson, Arizona, where he worked as a resident physician in a community hospital. Following this phase of his training, he transferred to New Orleans and the famed Ochsner Clinic. Here he embarked on training in psychiatry while pursuing an interest in biochemistry and publishing a couple of papers dealing with studies of rats—a far cry from what was to come in later years. Eclectic interest in medicine's many facets was characteristic of Ian's approach to his chosen profession. During this period he completed a Freudian psychoanalysis; it was, he asserted, a pseudo-scientific attempt to understand the human condition, and he later denigrated it at every feasible opportunity. Armed intellectually with in-depth experiences of biochemistry, medicine, psychiatry, and psychoanalysis, to say nothing of his devotion to viewing new and old ideas through the prism of history, Ian obtained a Commonwealth Fund fellowship to Cornell University's medical school in New York City for further training with Harold Wolff and Stewart Wolf. Their landmark studies of so-called ''mind-body'' relationships, or psychosomatic medicine, were attracting increased professional and public attention. Ian studied the effect of emotional states on benign and dangerous cardiac arrhythmias. His numerous papers set new standards for cardiologists by following up on ideas introduced by one of Ian's heroes, Sir James Mackenzie, a general practitioner who became the pre-eminent cardiologist of his day.

Ian's earlier years in New Orleans had brought him joy as he courted his first wife Octavia, a pediatrician as well as a competent artist; they were married in 1948 when Ian moved to New York. By the end of his two-year fellowship there, Ian's many talents were recognized by colleagues back in New Orleans, and he joined Tulane University's Department of Psychiatry as an Assistant Professor. Here he continued his early start as an author of scientific articles with a continuing stream on diverse subjects. These included a classic piece in *Harper's Magazine* in which he promoted the urgent need to critically examine both new and old ideas—especially those that did not fit rigid contemporary scientific paradigms.[1] Ian wrote superbly; our mother's interests may have suggested much of the content, but our father's example provided the style. Ian and Octavia seemed to flourish in New Orleans, and his health problems

apparently abated slightly. His interest in the paranormal and in theosophy continued as a hobby rather than as a professional pursuit.

In 1957, at the age of 38, Ian moved to the University of Virginia's School of Medicine, where he was appointed the second chairman of its Department of Psychiatry. Here he thrived as he taught medical students, trained young psychiatrists, and cared for patients. Influenced by Aldous Huxley's experiences with hallucinogenic drugs, he conducted experiments on himself and others with LSD. For Ian, his academic life was focused fervently on expanding the frontiers of knowledge and wisdom and on understanding the human condition and its never-ending travails. Not for him were the small questions, the orthodox problems, or popular issues. Perhaps he was influenced by Alan Gregg, the famed vice-president of the Rockefeller Foundation, whose funding jump-started the biomedical era. Gregg used to remark repeatedly that ''if you are going to do research, you should tackle important problems. For me,'' he said, ''the most important problem is whether there is a life hereafter, but there aren't many people pursuing such matters!''

Encouraged and funded by Eileen Garrett, a famous and well-documented English medium, later based in New York, Ian traveled to India in 1961 to investigate his first case, that of a young child's account about an earlier life or of possible reincarnation. Chester Carlson—inventor of Xerox—provided funds for further research travels by Ian and eventually endowed a chair for him at the University of Virginia. This largesse enabled Ian to give up his departmental administrative duties and establish the Division of Personality Studies (currently labeled the Division of Perceptual Studies). Shunned by most of his academic colleagues but courageously supported by Dean Thomas Hunter—a believer in freedom of enquiry in Mr. Jefferson's ''academical village''—Ian persisted. Alas, his wife, Octavia, scorned his new pursuits; she would have no truck with them and spent much of her time alone in a cottage retreat they had bought outside Charlottesville. She had had diabetes since childhood, and it grew worse by the year. Ian took her to specialists near and far, and, in spite of Octavia's strong negative views about his work, Ian nursed her at home as she underwent a form of home kidney dialysis. She died in 1983, and he was greatly saddened. Ian always regretted that he never had children; Octavia's single pregnancy resulted in a stillbirth.

Ian's travails were eased beautifully in 1985 when he married Margaret Pertzoff, a professor of history at Randolph-Macon College. Although a skeptic when it came to belief in reincarnation, she has remained a steadfast supporter of his right to investigate the subject. Ian's international case-finding travels often kept them apart, but they also enjoyed the delightful home she made and they took many other memorable trips together.

For 45 years Ian and his colleagues, as well as a global cadre of ''scouts'' and interpreters, have amassed reports of almost 3,000 individuals who recounted memories of places, experiences, events, circumstances, and individuals for which, as he usually put it, ''reincarnation is not the only explanation we have,

but it is the best explanation we have for the strongest cases.'' Several documentary films, countless interviews, 15 books, and several hundred articles recount in exquisite detail his case reports, observations, experiences, and interpretations. The raw data are available at the University of Virginia for others to examine and draw their own conclusions. A long-time friend of Ian's, the late Dr. Harold Lief, once remarked that Ian's insights might one day be likened to those of Galileo. To Ian's ever-enduring disappointment, few in today's orthodox medical establishment would even examine his data, to say nothing of embracing his conclusions. As he wrote in *Harper's Magazine*, all those many years ago, there still are far too many Western ''Scientists with Half-Closed Minds.''

Note

[1] This article, ''Scientists with Half-Closed Minds,'' is reprinted later in this issue.

Journal of Scientific Exploration, Vol. 22, No. 1, pp. 18–35, 2008 0892-3310/08

ESSAY

Reflections on the Life and Work of Ian Stevenson

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Ian Stevenson's career must, I think, be adjudged one of the most remarkable of his time—indeed, from certain points of view, one of the most remarkable of any time. I would say this irrespective of whether posterity comes to remember him—and surely he will be remembered—as a trail-blazing scientist who opened up new problems or shed fresh light on old ones or accords him only a passing note in the chequered history of the relations between science and religion (as does Wallace, 2006: 35).

His widely ranging abilities could have brought him success in any one of a number of different fields. It is of interest—and of some significance—that although a good deal of his working life was devoted to medicine and its off-shoots, he began university studies as an historian and always retained a strong interest in history. Two extracts from his all-too-short autobiography tell us a lot about him.

The first is a comment on the effect on him of his historical studies: ''An awareness from my reading of history of the ephemeral nature of most concepts about the nature of things freed me to challenge received opinions in medicine. For me everything now believed by scientists is open to question, and I am always dismayed to find that many scientists accept current knowledge as forever fixed'' (Stevenson, 1990: 1). The second extract is a maxim of Thomas Jefferson, the founder of the University of Virginia, at which Stevenson spent so many years: ''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'' (Stevenson, 1990: 18).

The attitudes here expressed are reflected in his various changes in career direction. His early postgraduate work in biochemistry (1946–1947) left him dissatisfied with its prevalent part-by-part approach, and he turned, to an extent in consequence of his own clinical observations, to psychosomatic medicine, a more holistically oriented discipline which at the time seemed to be burgeoning strongly. But the field did not burgeon as expected, and Stevenson took up psychiatry as offering ''a better opportunity than internal medicine for the

further study of the effects of mental states on bodily ones'' (1990: 6). He underwent psychoanalytic training (1951–1958) but found that psychoanalysis, dominant though it then was, offered chiefly a new kind of reductionism, coupled with an uncritical adherence to the peculiar doctrines of its revered founder. In short, it had more of the attributes of a dogmatic religion than of an open-minded, fact-driven scientific quest for answers. More congenial to him was his partly concurrent work (1955–1963) on the therapeutic and biochemical properties and the phenomenological effects of psychedelic drugs, most notably mescaline and LSD. He thought that LSD in particular had therapeutic potential, and was disappointed when its use was suppressed by the authorities, remarking that ''Timothy Leary really wrecked things'' (Shroder, 1999: 102). And whereas the standard scientific view was that because a small amount of the drug acting on the brain can markedly alter one's state of consciousness, one's experiences must be entirely brain-generated, Stevenson felt that this supposition could not fully account for the extraordinary images and feelings released (Stevenson, 1990: 10).

In all this one can see at work both Stevenson's conviction that currently widespread scientific and medical assumptions should never be regarded as immutable verities and his view that truth supported by fact is to be pursued, however unpalatable the conclusions reached. But it was of course above all in his explorations in psychical research that these heterodox principles found their fullest expression. Stevenson's interest in this subject, which he attributed to his mother's influence, seems to have gone back to his boyhood, while his more serious involvement and earliest publications in the area date to the later 1950s, by which time he was already Professor and Head of the Department of Psychiatry in the Medical School at the University of Virginia, Charlottesville. I shall not attempt to sketch the combination of ability, determination, and deserved good fortune that led to his becoming within a further 10 years Carlson Professor of Psychiatry and Director of the Division of Parapsychology at the same institution.

Although Stevenson's career several times changed direction, the changes did not involve his simply abandoning one line of work and taking up another. For appreciable periods he worked and published in more than one field. The tenor of his mind was holistic; he sought to establish connections between subject areas rather than to operate within conventional boundaries. Nowhere is this more apparent than in his approach to psychical research, and no further illustration is required than the two monumental volumes of his *Reincarnation and Biology* (1997a).

The last 40 years or so of Stevenson's life were principally devoted to psychical research and to areas that he thought related to it. Although in this article I shall concentrate mostly on the work for which he is best known—his very extensive studies of children who ostensibly remember previous lives—it should not be forgotten that he did much else besides. He carried out, for instance, significant practical work on cases of spontaneous telepathic

experiences, especially veridical hallucinations (those curious examples of hallucinations that correspond to other events in ways that seem to elude ordinary explanation), on cases of near-death experiences, on examples of cryptomnesia, on cases of apparent xenoglossy, on ''maternal impressions,'' and on certain types of mediumistic communications. It is worth pausing to ask what qualities and what attitudes he brought to these undertakings or developed because of them.

Stevenson had, of course, a wide knowledge of medicine and its offshoots, between which and psychical research he found many links. He had read quite extensively in the philosophy of mind, was well informed about many different religions and religious sects and their cultural backgrounds, was a keen student of history, was fluent in French and German, and had a general curiosity about many other matters. If one visited him, as I from time to time did, in the apartments in Cambridge where, as a visiting scholar at Darwin College, he spent extended periods preparing the immense volumes just mentioned, one found that, although he was working prodigiously hard and organized his time meticulously, among the books on his shelves (he was a great book collector) he always had several that were unrelated or peripherally related to his main preoccupation, and were there simply for interest or relaxation.

It was indeed remarkable how relaxed he could be, and how much time he had for other people, especially when one considers what a driven person he was, the considerable quantity of his published work, and the potential importance, as he saw it, of the material he was tackling. His approach to the subject was very much fact-oriented. He was, of course, interested, as anyone would be, in possible explanations for the facts, but his prime aim was to collect and present carefully documented first-hand cases in such quantity as would permit interested persons to make up their own minds; and in this it cannot be denied that he succeeded. The cost of his success over the years—for cases were often to be found in rather remote parts of the world—was long periods of arduous and probably lonely travel, through territories in which roads were quite commonly poor, amenities few, and dangers sometimes quite serious. He does not say much about these hazards in his publications, though in conversation with him one occasionally got glimpses. I used sometimes to suggest to him, half jokingly, that he should unbend a little and write something about his more remarkable adventures.[1] He never did; but from Tom Shroder's book *Old Souls* (1999)—Shroder accompanied him on two of his last trips—one can gain some insight into the problems that he routinely faced and the calmness with which he faced them. Indeed, only someone with strong inner resources, considerable physical stamina, and great determination and perseverance could have done what Stevenson did.

Stevenson was at first (I think one may say with confidence) the only person in the world investigating these cases of ostensible reincarnation on anything like such a scale. But he was acutely aware that a case collection, however large, that is heavily dependent upon the judgement and reliability of a single person

can carry only rather limited weight, and he was correspondingly and rightly anxious to have his work independently repeated and if possible confirmed and enlarged by others. Sometime (I think) just before or just after the second edition of his *Twenty Cases Suggestive of Reincarnation* came out in 1974, he offered to fund me on a visit to India to reinvestigate his cases there with *carte blanche* to act as I thought best and reach whatever conclusions I thought fit. Circumstances did not permit me to take up this offer, but in recent decades a number of other persons (far more suited to the task than I) have carried out some fairly extensive replication studies (cf. Stevenson, 2001: 173–175).

Though Stevenson banged no drum for his own theories, preferring that his readers should make up their own minds on the basis of the evidence and the considerations advanced, he was always sensitive to the possibility that his views might be misrepresented and his findings exaggerated in the interests of sensationalistic journalism or television. He knew what a hindrance this could be to the scientific acceptance of his work. Media persons whom he did not fully trust (and these I am pretty sure constituted the majority) could get pretty short shrift if they approached him. He was apt to be uncharacteristically tetchy about, and I suspect sometimes with, such persons, and occasionally he overdid it and rather brusquely dismissed some individual who regarded himself as having a properly serious interest in the subject, thereby making an unnecessary enemy. But mostly he got it right, and on the whole the small and select group of writers and television program-makers with whom he was prepared to cooperate did well by him. Tom Shroder's book, in particular, presents a very fair picture of Stevenson's work and aims, together with a portrayal of the man himself, to which my instant response was: ''That is the Stevenson I know.'' Quite a few of today's psychical researchers could with advantage emulate Stevenson's cautious selectivity.

Stevenson's views on the aims and proper conduct of his investigations, involving as they did the intensive gathering and assessment of facts prior to any seeking of explanations, the avoidance of expressions of certainty, the careful investigation of possible pitfalls, and a positive aversion to premature popularization of his findings, might be characterized as austere. But his approach was not narrow. He disliked the term ''parapsychology,'' which ties the subject too closely to psychology whilst according it an inferior status, and he preferred to talk more generally of ''paranormal'' phenomena. He also disliked the increasing tendency of certain parapsychologists to speak as though their own laboratory- (and computer-) oriented approach was the only properly professional and scientific one and to dismiss somewhat contemptuously the field studies of spontaneously occurring phenomena which particularly interested him as merely ''anecdotal'' and fit only for that antiquated lumber-room with ''psychical research'' on its door.[2] Although Stevenson agreed that the experimental method had brought important successes, and although he went on to institute computer analyses of his own case collections, he became increasingly critical of the pretensions of parapsychology and to prefer the broadly based

Society for Scientific Exploration to most parapsychological organizations. He had named his research unit the ''Division of Personality Studies''[3] to allow it to stay close to medicine and biology, and he thought that most parapsychologists were too isolated. ''They were just talking to themselves and not talking to other scientists, and far too inattentive to the fact that the rest of the world wasn't listening to them. They were too locked into a rather narrow laboratory program'' (Stevenson, quoted in Shroder, 1999: 107). He might have added that in the last few years not many findings that are consistently replicable have emerged from this ''program'' and almost none that have fired the imagination of the outer world. He thought that parapsychologists in general suffer from an excessive tendency to imitate laboratory psychology, while laboratory psychologists themselves have for extended periods tried to imitate the physical sciences. Many parapsychologists are graduates of psychology departments and have been educated in the methods and concepts of laboratory psychology, particularly, nowadays, of cognitive psychology. But these concepts and methods, applied to parapsychology, far from helping to make it into a ''proper'' science like the ''hard'' sciences, may be quite inappropriate for the investigation of the most remarkable kinds of alleged paranormal phenomena, especially the ''spontaneous'' ones in which Stevenson's own interests particularly lay. As Stevenson points out (1990: 12–13; cf. 2001: 15–16), science can readily provide examples of important phenomena, for instance, the weather, volcanoes, earthquakes, fossils, meteorites, comets, sunspots, and stellar explosions, that simply cannot be generated or properly studied under laboratory conditions. Investigation of such phenomena requires its own special methods and often its own special concepts also.[4] Even more is this true of spontaneous paranormal phenomena (from the study of which, it should be noted, parapsychology was in considerable part originally derived). There is in most cases no guarantee that the rather modest statistical significances in, say, experiments on ESP or PK that once would not have been, but nowadays are, sufficient to excite the small world of laboratory parapsychology have any useful bearing upon full-blown examples of, say, veridical hallucinations, poltergeists, or children's apparent recollections of previous existences. And insofar as we have any ideas about the causal factors that may underlie such spontaneous cases, they would seem very difficult to effectively replicate in laboratory situations.

It is amusing to note that Stevenson had almost certainly more experience of work in the ''hard'' sciences than have most of the parapsychologists who sniff at the investigation of spontaneous cases as not proper science and that many of the features of his collections of cases of children who ostensibly remember past lives have been extensively replicated in widely separate parts of the world, and by the work of different investigators.

This would seem an appropriate point at which to offer a brief and necessarily inadequate discussion of Stevenson's own work in psychical research. As I mentioned before, I shall have to confine myself largely to his investigations of cases of ostensible reincarnation, especially in young children. An account of

these can conveniently be taken under two headings: (a) his approach to the investigations and (b) the data he collected and their interpretation.

The former of these need not detain us long. Stevenson's approach to the investigations reflected leading features of his character. He infused the whole enterprise with a moderation of tone that put facts before theories and caution before pronouncements. And he had a keen sense of what would constitute good evidence in such cases, and of the dangers to be guarded against, and to that end developed and refined semi-standardized methods and routines of case investigation and reporting that have been widely adopted, though sometimes modified, by others. They included, of course, detailed interviewing of the ostensibly reincarnated subject (the ''present personality'') and his or her family and extended family and of the family and friends of the ''past personality'' and a detailed search for and consideration of possible personal and geographical connections between the two families such as might have resulted in transmission of information between the two.[5]

The second heading, data and their interpretation, covers two main sets of issues, namely, what do we make of certain recurrent cross-cultural features to be found in Stevenson's data, and how, in those same data, may we best explain the present personality's often correct ''memories'' of the previous personality's life. I shall say something about each of these matters in turn.

Stevenson's collection of personally investigated cases rapidly became far larger than any that had preceded it, and through his own efforts and those of his associates its scope was extended across a considerable variety of cultures and of widely separated geographical locations. It soon became apparent that it presented features that to a greater or lesser extent crossed these cultural and geographical boundaries in somewhat noteworthy ways.[6] For example, most of the children involved began talking about their previous lives almost as soon as they could speak, and most (not all) lost these memories somewhere between the ages of five and eight; in a high percentage of cases (high, that is, in relation to the norms for their cultures) the previous personality had met a violent and frequently early death; cases were commonest in (but not confined to) regions in which reincarnation is widely believed in; ''announcing dreams,'' in which a mother-to-be has a dream in which she is told that her child will be the reincarnation of some lately deceased person, were to be found in most cultures and probably in all reincarnationist ones; and birthmark cases, in which the present personalities exhibited birthmarks or congenital deformities corresponding to ones borne by or injuries (frequently fatal ones) received by the supposed previous personalities and sometimes recollected by the present ones were similarly to be found in all cultures. In some cases Stevenson's own photographs and measurements of birthmarks matched autopsy sketches and measurements of the fatal wounds (Stevenson, 1997a: 349–508; 1997b: 43–54).

These data about recurrent cross-cultural features (there are other features I could have mentioned) present the theorist with something of a dilemma. It is easy enough at a certain level to think up everyday explanations for the recurrent

features (except, perhaps, for the last-mentioned). But when one comes to the details of the cases it becomes much harder. For instance, one might suppose that the prevalence of cases in which the past personality met an untoward and sticky end could be due to friends and relatives, who long to see him or her again, projecting their wishes upon some hapless but conveniently handy infant. However, what of cases—not hard to find—in which the ostensible rebirth takes place into a family having no knowledge of or connection with the previous personality? On the other hand, if one proposes instead that reincarnation can provide a unitary and more satisfying account of the recurrent cross-cultural features, one still has quite a bit of ad hoc hypothesis-patching ahead of one. For example, Stevenson, arguing from a reincarnationist viewpoint, has to engage in a fair amount of ingenious speculation to account for the apparent fact that certain present personalities may bear birthmarks corresponding to death-wounds that the previous personalities are unlikely to have known about and could never have seen (Stevenson, 1997a: 2078, 2083–2088). It could of course be (I do not say it is likely to be) that really impressive evidence of the kind we have next to discuss might, taken in conjunction with the data on recurrent cross-cultural features, tip the balance in favour of a reincarnationist interpretation.

This brings us to the second issue mentioned above, namely, how are we to interpret the correct ''memories''—including not just personal and factual memories, but ''behavioural'' and emotional ones too—of a previous life shown by so many of Stevenson's ''reincarnated'' children? Here I may remark at once that while I do not say (what Stevenson would have been the last person to say) that Stevenson never made mistakes in assessment or reached wrong conclusions, I do very much doubt that any simple ordinary explanation, or set of such explanations, will accommodate all or even many of the cases Stevenson thought worthy of publication. I shall spend a fair amount of rest of the article discussing this question.

Perhaps the first point that needs to be made in this connection is one made by Stevenson over 40 years ago in response to criticisms by Louisa Rhine (1966). Louisa Rhine had suggested that before claiming that the ''past personalities'' of his young subjects had been satisfactorily identified, Stevenson should utilize the techniques of statistical investigation proposed by Pratt and Birge (1948; cf. Pratt, 1969) to test the supposition that statements made by mediumistic ''communicators'' are more applicable to the individual supposedly communicating than to other individuals of similar age and the same background. Stevenson replied: ''I submit that in the cases with sufficient detail, the number and specificity of detail in the child's statements leaves no doubt that a particular person, and only that particular person, can be identified as the [previous] personality'' (1967: 150). With this statement, especially in view of subsequent developments and subsequently investigated cases, I find it impossible to disagree. It is not just that one is confronted with examples of children who can apparently correctly remember their alleged past-life names and the names of the towns in which they lived, and give details thereof, and perhaps recognize and name[7] various

relatives or neighbours, and who in an appreciable percentage of cases exhibit appropriate interests and personality characteristics, or birthmarks corresponding to the verified causes of death of the supposed past personality. Additionally, what Stevenson calls the ''specificity'' of the details correctly recalled, their peculiar appropriateness to just one person, can be quite startling. Consider the following three examples (a good many others could be cited).

1. Malik (Necip) Ünlütaşkiran (Stevenson, 1997a: 430–455; 1997b: 48–49) was born in Adana, Turkey, in 1951. When he became able to speak (relatively late), he began to insist that his name was really Necip (an insistence before which his parents eventually gave way) and that he had lived in Mersin, about 80 km away. He made a number of statements about this supposed previous life, but not until 1963 was it discovered, through a fortuitous meeting, that they coincided closely with the life of a man named Necip Budak who had lived in Mersin and been murdered there in 1951. Stevenson and a Turkish associate recorded 14 of these statements in 1963–1964. Eleven were correct, one partly correct, one unverified, and one incorrect. Among the correct statements were the forenames of his Mersin wife, mother, and one of his sons, that he was stabbed to death and had been very drunk at the time, and that, also when drunk, he had stabbed his wife in the leg. The latter was confirmed by the lady herself, who showed the scar on her thigh to a female member of Stevenson's party.

After the two families had met (in 1963), 12 correct recognitions by Necip of persons whom Necip Budak had known or of possessions he had owned were confirmed by the Budak family.

Both families denied any previous knowledge of the other, and Stevenson could not discover any plausible source of transmission of the information from one family to the other.

At birth Necip Ünlütaşkiran had had seven birthmarks, of which three were still visible when Stevenson examined him. Six of these corresponded in position with the knife wounds recorded in Necip Budak's autopsy report.

2. Swarnlata Mishra (Stevenson, 1974: 67–91), born in Madhya Pradesh, India, in 1948, exhibited from about the age of three and a half memories of an ostensible previous life as Biya from a family named Pathak in Katni, Madhya Pradesh.[8] When Swarnlata was 10, her father wrote down some of her statements, and the following year H. N. Banerjee, a parapsychologist from the University of Rajasthan, did the same. As a result of Swarnlata's statements, Banerjee was able to identify a family in Katni corresponding to Swarnlata's statements, and the two families shortly met up. Stevenson spent four days with them in 1961. He lists 18 statements, of which four were incorrect and the rest correct, made by Swarnlata before the two families met. After the meeting Swarnlata made six further correct statements and 25 correct recognitions of family members, including, despite attempts to mislead her, ''her'' (that is, Biya's) husband and two sons, other significant persons, and various items and places. Among her correct statements were the names of her former family,

Pathak, and of their home town, Katni; her own name, Biya; eight statements about the Pathak residence and household sufficiently specific to enable Banerjee to locate it; the name (slightly distorted) of the district of Katni in which that house lay; the fact that Biya had gold fillings in her front teeth; and the fact that her husband had taken 1200 rupees from a box in which Biya had kept money (this had been known only to Biya and to her still-living husband, Chintamini Pandey, who confessed it to one of his sons).

The Pathak family completely accepted Swarnlata as Biya, and affectionate relations developed between her and the ''brothers'' and ''sons'' of her supposed previous existence.

At no time did the Mishra and Pathak families live less than 100 miles from each other. Stevenson concluded that though it was possible that some degree of knowledge of the Pathaks by the Mishras could not be ruled out, there was no evidence for it, and that to account for the knowledge shown by Swarnlata, ''a rather widespread conspiracy'' between the Mishras, the Pathaks, the Pandeys, and others would have been required, and that there are very good reasons for doubting this occurred.

3. Indika Ishwara, one of a pair of male monozygous twins, was born in Sri Lanka in October 1972 (Stevenson, 1997a: 1970–2000; 1997b: 174–177). The family lived in a village near the town of Weligama. At about the age of three Indika began relating ostensible memories of previous incarnations. His memories concerned the life of a boy from the village of Balapitiya (which he named) about 45 km away (as the crow flies). Indika's father was sufficiently interested by his son's statements to ask a friend now living near Balapitiya to make enquiries there. This friend soon discovered a boy, Dharshana Samarasekera, who seemed to be unmistakably pinpointed by Indika's statements. Dharshana had died at the age of 11, probably of viral encephalitis, in January 1968. Dharshana's father was very interested by Indika's statements and in June 1976 paid several visits to his home. The result was that a report of the case appeared in a newspaper toward the end of June. This in turn led a Sinhalese associate of Stevenson's, Godwin Samararatne, to visit both the families involved and collect statements. He also arranged to be present when Indika was taken in early July 1976 to visit Dharshana's family for the first time. In December 1978, and again in November 1979, Stevenson, Samararatne, and another Sinhalese associate conducted extensive interviews with members of both the Ishwara and the Samarasekera families.

Indika is recorded as having made 36 statements about his alleged previous life before the two families had met. Of these, 31 were correct, two incorrect, two doubtful, and one unverified. Of the correct ones, some would have been applicable to many families in Sri Lanka, but others were quite specific. For instance, although he never gave the name Dharshana, he said that he had been called ''Baby Mahattaya'' (master or boss), that his older sister was named Malkanthie, that he had an uncle called Premasiri, that he had an uncle on his

father's side who was a *mudalili* (a successful businessman), that he had an aunt who used to cook chilies for him, that the family house was called "Buwan," and that he had "left his foot" (sc. his footprint in wet concrete) there.

When Indika was eventually taken to visit the Dharshana family (July 4, 1976), he seems to have spontaneously recognized several family members. Stevenson, however, had reservations about these recognitions, and also non-recognitions, because of the distractions occasioned by the considerable crowds of sensation-seekers who had come there out of curiosity. But on Indika's second visit, early in 1977, he was observed wandering around outside a neighbouring house in the Samarasekaras' compound, apparently looking for something on the wall of a concrete drain. He then pointed to the words "Dharshana 1965" that had been scratched into the concrete before it had set. No one in Dharshana's family knew about this writing.

The two families had not met prior to June 1976 and lived a long way from each other. Stevenson was unable to find any plausible lines of communication between them.

To return now to Stevenson's claim that identification of the designated past personality "is the least controversial and, in a way, the least important point we have to consider in weighing the best of the reincarnation cases" (1967: 151): It appears to me that even the three case accounts that I have hugely abridged from Stevenson's originals are sufficient to support this claim. Now quite obviously (as Stevenson further remarks) simply establishing the former existence of a person whose life is unmistakably indicated by the statements and activities of some present person does not tell us anything about the source of the present person's information. Before there can be any question of taking such evidence as even *prima facie* support for the hypothesis of reincarnation, a series of preliminary issues have to be addressed:

(a) Can this evidence be satisfactorily disposed of by any normal, as opposed to paranormal, hypothesis or hypotheses?

(b) If not, can it be accommodated by some hypothesis less than reincarnation, but still involving some supposed form of paranormal or hitherto unrecognised process?

(c) If these hypotheses fail, can we find any general scientific or philosophical grounds for dismissing the evidence for reincarnation?

These are large, difficult, and very complex matters concerning which I can offer only a few tentative comments.

(a) The idea of reincarnation provokes very varied, and quite often fairly strong, reactions in quite a few people. Among the greatest enthusiasts are sometimes Westerners converted to the notion rather than individuals from Eastern regions, with which the doctrine is popularly associated. Those who dislike and try to dismiss the idea include adherents of religions which reject it, philosophers and scientists with whose world view it is not consistent, and persons (such as myself) who rate their chances of obtaining a comfortable berth in the

next incarnation as slender and detest the thought of again jumping through hoops that they have jumped through many times before.

With so much emotion involved, it is hardly surprising that many of the arguments put forward in the hope of undermining the alleged evidence (and it is these arguments with which we are here concerned) are merely silly when applied to Stevenson's better cases.[9] Of course, in the abstract it is easy enough to think up possible normal explanations of apparent evidence for children's memories of past lives—fraud, prior contact or even conspiracy between the two families involved, motivated errors of memory by witnesses, contamination of testimony at the first meetings between the families, knowledge possibly originating from gossip or newspapers, cryptomnesia by the ostensibly reincarnated individual, culturally induced fantasy on the part of parents or child, attempts to exploit a case for gain or fame—and in a smallish number of cases such factors undoubtedly were, or very possibly could have been, at work. I cannot but feel, however, that were the proponents of these notions to try them out (as I have done) against a large number of Stevenson's detailed case reports, they would find the going decidedly difficult. Convincing oneself that one could in such terms dismiss a wide range of these reports would, it seems to me, require the sort of ingenuity and paranoid reasoning by means of which flat-earthers and conspiracy theorists manage to reconcile their favourite hypotheses with any twist that the facts may take or fail to take. It is remarkable, but perhaps not altogether surprising, that several of Stevenson's leading critics seem to have read rather few of his original reports and even rely upon second-hand and often hostile accounts of them. No one who has not carefully studied a good number of the original reports is qualified to offer comments and criticisms.

This is not to say that Stevenson did not from time to time assess a case too generously, or fail to search for flaws where flaws there might have been. He would probably in retrospect have said so himself; indeed, he sometimes did. But he quite certainly knew, or had learned, more about the evidential pitfalls confronting his kind of researches, and did more to spell them out and confront them, than anyone had before him, and no one who has investigated cases with him, as I occasionally did, would be likely to accuse him of credulity. His own findings tell heavily against the more obvious, and hence most frequently canvassed, counter-explanations. The proposal, for example, that children who ostensibly remember previous lives are merely enacting culturally driven fantasies is vulnerable to problems as to the source of the often quite specific ''memories'' produced by the present personality, to problems over the fact that cases have increasingly been discovered from cultures in which reincarnation is not a widespread religious belief, and to problems over the fact that even in cultures in which reincarnation is widely accepted parents have frequently, albeit unsuccessfully, tried to suppress their offspring's apparent memories of a previous existence. The common claim that witnesses' recollections of what the child or subject in question actually said or did are liable to become grossly warped by the passage of time and the pressure of suggestions is not so strongly

supported as some maintain by certain psychological experiments on eyewitness testimony. The conditions are not the same—the child's statements can be startlingly meaningful to the witnesses, and the child may tend to repeat them over and over again—and Stevenson's own investigations (Stevenson, 1968; cf. Keil & Stevenson, 1999; Schouten & Stevenson, 1998; Stevenson, 2001: 154–158) have suggested that, as with so-called ''flashbulb'' memories, witnesses may recall such statements very well even after considerable periods of time (this has been my experience also). The proposal that relevant information may have been accidentally or deliberately passed from one family to the other is in a substantial minority of cases rendered highly implausible by the considerable distances separating them and by the scarcity of potential connections between them. In rather over one percent of cases an independent investigator was able to record what the present personality had said about the life of his or her ostensible previous personality before the two families involved had been in contact. Uncongenial though I find the prospect of reincarnation, I can see nothing to be gained by taking refuge in criticisms, such as the above, of the evidence allegedly favouring it.[10]

(b) This brings us to the question of whether the alleged evidence for reincarnation can be accommodated by some form of hypothesis less or other than reincarnation, but still involving some supposed form of paranormal process. The obvious candidate here is telepathy between the present personality and still-living persons who knew the past personality. But as Stevenson points out (e.g., 1974: 343–348; 2001: 159–161), his child subjects rarely give any evidence of telepathic gifts apart from the past-life recollections that telepathy is supposed to explain. He also notes the intense identification with the past personality shown by so many of these subjects and the fact that present personalities not infrequently exhibit emotions, interests (including religious ones), skills, social attitudes, and character traits appropriate to the past ones. There are, he remarks, no grounds for supposing that telepathy can, in effect, ''reproduce . . . an entire personality transferred to another physical body'' (2001: 160). Hypotheses more exotic even than telepathy—for instance, possession, obsession—would be just as problematic as reincarnation.

(c) Are there then general scientific or philosophical reasons why we should reject any putative evidence for reincarnation *ab initio*? Such reasons have certainly been advanced. For instance, there is a great deal of evidence which suggests that events in one's mind are causally tied to events in one's brain and therefore cannot continue when one's brain has ceased to exist. These neuroscientific findings have in turn had considerable influence on the predominantly materialist tendencies of modern philosophy of mind. Other relevant strands in current philosophy of mind have to do with questions of personal identity. Some philosophers have claimed that one can only talk of identifying and reidentifying any object (including a human being) as the same individual again if one can, or could in principle, assign it a continuous spatiotemporal track between one's various encounters with it. Obviously, the argument goes, one

cannot do this in cases of alleged reincarnation, and so one can never justifiably identify the reincarnated with the original person. Other philosophers contend that one's ''self'' is a fiction, a narrative one tells about oneself, conditioned by the social context in which one lives. The author of the narrative, however, is regarded as somehow part of the story, its radiant point, and does not exist outside it.[11]

These are huge issues and cannot be tackled now, except insofar as they will shortly and briefly crop up in connection with Stevenson's own world-view. But there is a related issue that merits notice. What we have here are two bodies of evidence and theory (or at any rate speculation), the one to do with evidence for reincarnation, and the other to do with evidence for the dependence of mind on brain, which though clearly separable, are so related that *prima facie* (and I emphasize *prima facie*) they cannot both be simultaneously accepted. Kindred situations are not unknown elsewhere in science. For instance, at the turn of the 19th and 20th centuries there was a sharp division between geologists and astronomers, whose respective best estimates of the age of the earth and the age of the sun made the former considerably older than the latter. But one must not assume in such cases that the evidence presented by one or the other party (depending on which turned out to have been ''right'') must have been ''bad'' evidence. There are criteria for assessing evidence, in a given context, as ''good'' evidence or otherwise, and they do not include a necessary link to the side that in the end (if indeed there is an ''end'') proved to have been ''right.''[12] In the case of the relative ages of the earth and the sun the dispute was resolved by the introduction of a previously unknown or imperfectly understood factor, namely radioactivity. Perhaps a previously unknown factor may intervene in the reincarnation/brain function case currently under discussion. I cannot see this happening any time soon, but in the meanwhile, simply rejecting ostensible evidence for reincarnation as necessarily being bad evidence because apparently irreconcilable with presently accepted neuroscience amounts to ducking out of a genuine problem; namely, what are we to say about this in fact really rather impressive evidence, even if we reject the possibility that it indicates actual reincarnation? The distinguished philosopher Paul Edwards (1996: 256) has a really astounding proposal as to what a rational person should think about Stevenson's work. Is it more likely, he asks,

> that there are astral bodies, that they invade the wombs of prospective mothers, and that the children can remember events from a previous life although the brains of the previous persons have long been dead, or that Stevenson's children, their parents, or some of the other witnesses and informants are, intentionally or unintentionally, not telling the truth: that they are lying, or that their very fallible memories or powers of observations have led them to make false statements or bogus identifications?

The either-or dichotomy between the first half of this sentence, which sets forth speculations presumed to be Stevenson's, and the second, which propounds criticisms (favoured by Edwards) of Stevenson's evidence, is entirely false.

Stevenson devoted a considerable part of his life to locating and investigating evidence that was, so far as he could tell (and I think he was generally right), not vitiated by those sorts of evidential failures. With him it was always evidence before anything else. And these criticisms cannot cope with that evidence. Even if the speculations are misconceived (I am not here saying that they are or are not), the evidence still remains and requires a proper explanation.

It is time to draw a few threads together. Throughout his career in psychical research Stevenson had to contend with a *Zeitgeist*, more particularly an academic *Zeitgeist*, largely hostile to his work. It is a testimony to his qualities of mind and character that he was able to achieve as much, and gain as much recognition, as he did. He undoubtedly came to regard reincarnation as, at any rate, highly likely. To Tom Shroder he remarked (Shroder, 1999, p. 210) that he had gone through every other possible way of looking at the findings and by elimination had concluded that reincarnation must be the explanation. But he was well aware of the difficulties.

He knew, of course, about the copious data linking mental functioning to brain functioning, but he rejected mind-brain identity theories on fairly well-known grounds. Instead he adopted a view (similar to the positions of C. D. Broad, H. H. Price, and J. R. Smythies) according to which each person's mental phenomena inhabit a space of their own in which they (particularly mental images) have spatial extension. Each person possesses such a mental space, the contents of which can interact with his brain and also perhaps with the mental spaces of other persons. To this way of thinking, minds, being in mental space, can survive the dissolution of their corresponding brains in ordinary physical space (Stevenson, 1981; 1997a: 2070–2074; 2001: 223–227). I confess to some difficulty in making sense of such theories, but cannot pause to discuss them here.

To handle the question of how the memories, personality traits, etc., of a dying or deceased person can be transferred from the past personality to the present one, Stevenson (1997a: 2083–2092, 2103–2104; 1997b: 183–186) developed the notion of a ''psychophore'' (''soul-bearer''), a kind of vehicle which passes from the one to the other.[13] Of the nature of psychophores he confesses himself unable to offer any suggestions, although for their mode of action, particularly in producing birthmarks and congenital malformations corresponding to injuries inflicted on the previous personality, he has some ingenious analogies. On the whole, however, ''psychophore'' seems to be simply a dummy concept filling (pending further information) a vital gap in an explanatory system. The introduction of such dummy concepts may not always or often be illuminating, but it is hardly illegitimate. A parallel might be the status of the concept of a ''virus'' prior to 1943, when viruses were first observed by means of the electron microscope. For my part I cannot doubt that there is in Stevenson's best cases *some* connection between the past and the present personalities, not reducible to the lingering effects of the deeds of the past personalities, or to memories of those personalities still in the minds of living persons, or to records

of them still extant and available, or to ESP (telepathy, clairvoyance) directed on all or any of these. Whether this connection, which so far as the evidence goes is at best a rare and partial one, should be regarded as an occasional and so far inexplicable freak of nature, or whether it may signify a true and full ''personal identity'' between past and present personalities with all the implications that might carry, are further and far more difficult questions.

Without doubt Stevenson thought that the spread of a belief in reincarnation (always, of course, if supported by scientific evidence), though it might not make us more moral,[14] would add to the sum of human happiness by alleviating the fear of death. He seemed slightly puzzled by my reservations on the question. He also thought that experiences in past lives could have medical and psychiatric relevance to problems in present lives and (in a way one of his most remarkable achievements) had articles on this possibility published in medical journals. Nonetheless I should suppose that however important he found such considerations, the main reason for his interest in the evidence for reincarnation was a deep and abiding curiosity about the nature of the universe we live in and about whether or not it is, as Frederic Myers, and later on Einstein, put it, ultimately ''friendly'' toward us.

To return to Stevenson the man: It is hard to write about someone one counted as a friend for 40 years or more. He was a quiet but dryly humorous, gentle but firm individual, not greatly given to talking or writing about himself. He sometimes seemed to have Buddhistic sympathies—he told me, with a certain regret, that he had once been constrained to shoot a rattlesnake and considered this action ''unbuddhistic,'' and he mentions in his autobiography (Stevenson, 1990: 2) how he hated killing ''harmless rats'' for his early biochemical research. But I never discovered whether he had religious beliefs that could be put even approximately into any conventional category. He had, in his quiet way, a gift for friendship, and maintained contact over the years not just with fellow scholars and academics but with all kinds of people he had met and been helped by in the course of his extensive travels. To persons in distress or trouble of mind his calmness, patience, perceptiveness, common sense, and kindness could be an invaluable source of help and reassurance. On happier occasions his warm friendliness and range of unusual information made him the most pleasant of hosts and conversationalists.

His outward calm was indeed rarely disturbed, even in difficult or tricky situations. He had met with his share of personal sadness and professional difficulties, and was at times depressed that his immense labours had not received more recognition from his fellow scientists. Yet nonetheless he always impressed me as someone who was strengthened by an inner, perhaps not fully formulated, belief in his luck or destiny, a conviction that whatever problems he might encounter he would be allowed to continue his work. Continue it he did on a truly amazing scale. Its continuation has now passed into the hands of others, and we shall wait with interest to see how it develops.

Notes

[1] Stevenson once told me the number of convicted or known murderers he had met in the course of his investigations. I cannot remember the exact figure, but I think that at that time it was about eight.

[2] Irwin and Watt (2007: 2) talk of ''the days when such interests [sc. 'field studies'] were known as psychical research, the study of phenomena apparently mediated directly by the mind or 'soul.'" It was, however, made quite clear in the original list of objects of the Society for Psychical Research (1882) that neither this assumption, nor any other, as to the explanation of the phenomena under investigation was implied by membership of the Society.

[3] For the first several years of its existence, the research unit was called ''The Division of Parapsychology'' rather than the name that Stevenson preferred because certain authorities in the University of Virginia insisted on this, presumably to isolate it in the way Stevenson had explicitly wished to avoid. Under a more sympathetic chairman of the Department, the name Stevenson had intended was eventually adopted.

[4] The following remarks by Simon White, Director of the Max Planck Institute for Astrophysics, are relevant: ''Physics and astronomy make progress in very different ways. In physics, controlled experiments rule, but astronomers observe whatever nature shows them. I fear that blindly applying physics-style experimental design to astronomical projects risks costly failure, as well as undermining the methodological basis of astronomy and its attractiveness to young scientists and the public'' (*New Scientist,* July 14, 2007, p. 21).

[5] For succint accounts of Stevenson's methodology, see Matlock (1990, pp. 194–207, 216–238), Stevenson (2001, pp. 129–145), and Tucker (2005, pp. 17–29).

[6] Stevenson was not, nor would he claim to have been, the first to note some of these characteristics. Some small collections of cases recognizably similar to Stevenson's can be found in Fielding Hall (1898/1917: 290–308) and Delanne (1924/1927: 304–364). Delanne's cases run from about 1860 to the First World War and span several continents, including (somewhat unusually) Europe, but the evidential quality is quite variable. Stevenson (2003) includes several of them. Further references to early modern cases will be found in Matlock (1990: 192). The quite numerous reincarnationist beliefs of ''primitive'' societies recorded by early anthropologists and folklorists (see, e.g., Tylor, 1913, vol. 2: 1–6) contain occasional intimations of the recurrent characteristics we are about to discuss, but they say little about individual cases. So far as I know the earliest individual cases of a ''Stevensonian'' kind that have found their way into print are some of those culled by De Groot (1901: 143–155; cf. Paton, 1921: 26–27) from Chinese annals and other records of the 3rd to the 10th centuries A.D.

[7] It should be noted here that Stevenson was always rather cautious about evidence from ''recognitions'' by the present personality of the past personality's

relatives and friends. In too many (though not all) such cases the arrival of the present personality at the past personality's home drew crowds, the behaviour of which might have given clues to the present personality.

[8] Swarnlata also claimed some memories from a life in Assam intervening between her life as Biya and her present life, and from this intervening life remembered several songs in the Bengali language, which she would perform with accompanying dances.

[9] Edwards (1996) offers a valuable and often amusing selection of both bad arguments for and bad arguments against reincarnation.

[10] A useful survey of criticisms of Stevenson's work will be found in Matlock (1990: 239–255).

[11] One thinks of Emerson's lines: ''They reckon ill who leave me out / When me they flee I am the wings.''

[12] It is by no means unknown in science for bad or dubious evidence to be used to support the ''right'' side and good evidence the ''wrong.'' There are some interesting examples in Collins and Pinch (1993). Another interesting case is that of lunar craters. Throughout the 19th century and well into the 20th these were generally assumed by astronomers to be volcanic in origin, and it could not be said that the astronomers' evidence was ''bad.'' Their observations were careful, their drawings and photographs outstanding, and their comparisons with terrestrial features plausible. Only as knowledge of terrestrial meteorite impacts became greater and space vehicles discovered signs of such impacts throughout the solar system was the evidence for the impact theory recognized as being ''stronger'' than that for the volcanic theory.

[13] This notion resembles that of a ''psi-component'' introduced by C. D. Broad (1962: 414–430).

[14] Stevenson told Tom Shroder how an Indian swami deflated him when he expressed the hope that a belief in reincarnation might make people more moral. ''There was a long silence, a terrible silence, and finally he said, 'Well, that's very good, but here, reincarnation is a fact, and we have just as many scoundrels and thieves as you do in the West''' (Shroder, 1999: 94).

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Journal of Scientific Exploration, Vol. 22, No. 1, pp. 36–43, 2008 0892-3310/08

ESSAY

Ian Stevenson and Cases of the Reincarnation Type

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Ian Stevenson began studying children who claim to remember previous lives—an endeavor that will surely be remembered as the primary focus of his life's work—almost by accident. Enjoying a successful mainstream career with some 60 publications in the medical and psychiatric literature to his credit, he had become chairman of the Department of Psychiatry and Neurology at the University of Virginia in 1957. The following year, the American Society for Psychical Research announced a contest in honor of William James for the best essay on "the topic of paranormal mental phenomena and their relationship to the problem of survival of the human personality after bodily death." Ian, who had said when he interviewed for the chairman position that he had an interest in parapsychology, had been intrigued by the concept of reincarnation and in his readings had come across reports of individuals claiming to have memories of previous lives, or "apparent memories of former incarnations," as he called them. The reports came from a number of sources, such as books, magazines, and newspapers. Ian analyzed 44 of them as a group in a paper that won the contest and was subsequently published in 1960 (Stevenson, 1960a,b).

He was impressed with the similarities in cases from different countries and different kinds of sources. As he told Tom Shroder years later, "these forty-four cases, when you put them together, it just seemed inescapable to me that there must be something there . . . . I couldn't see how they could all be faked or they could all be a deception" (Shroder, 1999: 103). At the end of the paper, he wrote that more study of the reincarnation hypothesis was justified and he asked people who knew of additional cases of apparent past-life memories to contact him.

At the time, however, he was not planning to investigate cases himself; he was too busy running his department, treating patients, and conducting other research. After the paper was published in 1960, his plans changed when he received a telephone call from Eileen Garrett, the head of the Parapsychology

Foundation. She had learned of a case in India similar to the ones Ian had written about, and she offered to pay his expenses to investigate it. Ian accepted the offer, and by the time of the trip, he had heard about four or five cases in India and two in nearby Ceylon (now Sri Lanka).

Once he got to India, he was surprised at how easily cases could be found. He was there for four weeks and saw 25 cases. Likewise, he visited Ceylon for a week and found seven cases. He thus learned that children's claims of past-life memories were much more common than anyone had previously known.

Next came Chester Carlson. Carlson had invented the Xeroxing process that formed the basis for the Xerox Corporation. His wife Dorris had led him to an interest in parapsychology and in research regarding life after death, and after reading Ian's essay, he contacted Ian and offered financial support for studying new cases. Ian initially declined the offer because he was so involved in his other duties, but he eventually agreed to take a donation to purchase a tape recorder for the work.

Soon after returning from India and Ceylon, Ian was contacted by Louisa Rhine, whose husband, J. B. Rhine, was head of the Duke University parapsychology lab. She had received a letter from Alaska about a case involving past-life memories, and she forwarded it to Ian. He then went to Alaska and found a number of cases among the Tlingit tribes there.

Ian was soon hooked, and he began accepting funding support from Chester Carlson. Carlson's donations and eventual bequest made it possible for Ian to create essentially a new field of research. It was one dominated by cases of very young children, as the individuals who claimed memories of previous lives usually began doing so at a very early age, often when they were two or three years old. Many talked about the end of the life, which frequently had ended suddenly or violently. Some claimed to have been deceased family members, but others said they had been strangers in another location and often showed emotional longing for the previous family. Ian discovered that in such a case, people had often gone to the other location and found that someone whose life matched the details given by the child had in fact died.

In 1963 Ian took a sabbatical so that he could write the first of a number of books on the cases he had investigated. He completed the manuscript and returned to work. As the American Society for Psychical Research was preparing to publish it, a man who had assisted Ian as an interpreter in two or three of the cases in India was accused of fraud in other work of his. This caused concern that he had cheated in his interpreting work, and publication was delayed. Ian returned to India to reinvestigate those cases with another interpreter. He found that the interviews had been interpreted correctly, and the cases remained in the book.

Therefore, after some delay, *Twenty Cases Suggestive of Reincarnation* was published in 1966 (Stevenson, 1966/1974). The cases in it were ones for which Ian had made painstaking efforts to determine exactly what the various children had said and to verify how well the children's statements matched the lives of

the individuals they were thought to remember. The book consisted of detailed case reports that included lists of every person Ian had interviewed, along with lengthy tables in which each statement the child had made about a previous life was listed along with the informant for that statement and the person or persons who verified that it was correct for the life of the deceased individual. Ian presented the cases in an objective, evenhanded manner, discussing their weaknesses as well as their strengths.

Continuing the Work

Though *Twenty Cases* received a number of positive reviews—the *American Journal of Psychiatry* saying there were ''cases recorded in such full detail as to persuade the open mind that reincarnation is a tenable hypothesis to explain them'' (Laidlaw, 1967)—much of the scientific community ignored it. This did not deter Ian, and in 1967 he stepped down as chairman of the department to devote himself full time to the research. In so doing, he was allowed to set up a small research division, now known as the Division of Perceptual Studies, in which to study the cases and to conduct other work in parapsychology.

At that point, Ian had collected hundreds of cases, with his investigations taking him all over the world. *Twenty Cases* alone involved cases in India, Ceylon, Brazil, Alaska, and Lebanon. The cases were easiest to find in areas with a general belief in reincarnation, and with the help of assistants in those places, Ian went wherever he needed to go for the research, sometimes traveling over 50,000 miles a year. He published individual reports of some of the cases in journals and then began a book series of cases from particular areas. He titled it *Cases of the Reincarnation Type* and published four volumes through the University Press of Virginia from 1975 to 1983 (Stevenson, 1975, 1977a, 1980, 1983b). Of the first volume, the reviewer in *JAMA*, the *Journal of the American Medical Association,* stated: ''In regard to reincarnation he has painstakingly and unemotionally collected a detailed series of cases from India, cases in which the evidence is difficult to explain on any other grounds.'' The reviewer added: ''He has placed on record a large amount of data that cannot be ignored'' (King, 1975).

The data were in fact ignored by most in mainstream science, with some notable exceptions. One was Eugene Brody, the editor of the *Journal of Nervous and Mental Disease*. He published one of Ian's papers in 1977, ''The Explanatory Value of the Idea of Reincarnation'' (Stevenson, 1977b), that led to more than 1,000 requests for reprints from scientists all over the world (Stevenson, 1990b: 17). Later in 1977 Brody devoted most of one issue of the journal to Ian's work. He included a paper by Ian and commentaries from several others. One commentary was by psychiatrist Harold Lief, whose frequently quoted observation of Ian was that ''either he is making a colossal mistake, or he will be known . . . as 'the Galileo of the 20th century''' (Lief, 1977: 171).

One criticism of Ian's work was that the cases he was reporting had all occurred in places with a general belief in reincarnation. Thus it was thought that

families eager to find cases of rebirth were producing the cases, either purposely or accidentally. Ian addressed this in 1983 with a paper about a series of 79 American cases (Stevenson, 1983a). He compared them to cases from India and found that, though few of the American children had made verifiable statements about a previous life, they resembled the Indian cases in many ways, including the age at which the children first spoke about the previous life, the content of the statements they made, and their related behaviors. He noted that many of the cases had taken place in families without a belief in reincarnation.

Years later Ian published a book of cases from Europe (Stevenson, 2003). He had also planned one of American cases but did not complete it before his death. Nonetheless, he documented in the other publications that young children's claims to remember previous lives were not purely a cultural phenomenon, as they occurred in Western countries without a general belief in reincarnation and in families without such a belief.

Marked for Life

An aspect of the cases that interested Ian greatly was the frequent presence in the children of birthmarks and birth defects that appeared to match wounds, usually fatal ones, suffered by the deceased individuals whose lives they were said to remember. Ian worked on a book of such cases, but his wish for careful documentation combined with a growing collection of cases caused the book to take longer and longer to complete. (At one point, a letter was found at the Division in which Ian had written that he planned to publish a book of birthmark cases the following year—nearly 20 years before he actually completed the book.)

Finally, in 1997 Ian published *Reincarnation and Biology: A Contribution to the Etiology of Birthmarks and Birth Defects* (Stevenson, 1997a), a massive 2,268-page, two-volume work that included reports and photographs of 225 cases involving birthmarks or birth defects. Many of them were not the usual blemishes, either. They were often dramatic and sometimes bizarre lesions, such as malformed digits or missing limbs, misshapen heads, and odd markings. In all of the cases, the defects matched wounds suffered by the previous individual. Ian showed his customary determination in investigating the cases, getting autopsy or police reports when they were available, or eyewitness testimony of the corpse when they were not, to verify that the marks and defects he was seeing actually did match the wounds the previous person had received.

Ian wrote a synopsis, *Where Reincarnation and Biology Intersect*, that contained color photographs of some of the marks and defects along with far fewer pages than the two-volume set (Stevenson, 1997b). Cases with such lesions continued to be found, and several of us, led by Ian, later published a paper of additional birthmark/birth defect cases that included two American cases (Pasricha et al., 2005).

Behaving Strangely

Another area that interested Ian was the behavior of these children. He wrote a paper about phobias that many of the children showed, usually related to the mode of death from the life they claimed to remember (Stevenson, 1990a). He reported that 36% of the children in a series of 387 cases showed such fears. They occurred when the children were very young, sometimes before they had made their claims about the previous life. For example, he described a girl in Sri Lanka who as a baby resisted baths so much that three adults had to hold her down to give her one. By the age of six months, she also showed a marked phobia of buses and then later described the life of a girl in another village who had been walking along a narrow road between flooded paddy fields when she stepped back to avoid a bus going by, fell into the flood water, and drowned. He noted that the phobias tended to recede as the children stopped talking about the previous life but that this was not always the case.

Ian also wrote about the children's play (Stevenson, 2000c). He reported that in a series of 278 cases, almost a quarter of the children engaged in play seemingly related to the lives they described that was unusual in their families and had no known role model. This often involved the previous person's occupation, such as a boy who became so wrapped up in his play as a biscuit shopkeeper that he fell behind in school and a girl in India who described a life as a sweepress and who not only enjoyed sweeping but also happily cleaned up the stools of her younger brothers when they defecated in the house, undoubtedly to the surprise of her Brahmin parents.

Ian explored unusual behaviors in a series of Burmese children who reported lives as Japanese soldiers killed in Burma during World War II (Stevenson & Keil, 2005). Many of them showed behaviors that were unusual in Burma but typical of the Japanese. These included such items as wanting to wear Japanese attire—trousers, belt, and boots—rather than the Burmese *longyi*, wanting to eat raw or partially cooked fish instead of the spicy Burmese food, and personality features such as industriousness and, consistent with the occupying soldiers, cruelty and harshness. Ian thought this was one of the most important papers he had written for some years because it explored a possible third component in the development of personality, a theme he had addressed before (Stevenson, 1977b, 2000b). He pointed out that not all unusual behavior can be explained by genetics and environmental influences, alone or together, and suggested that some aspects of the deceased individuals' personalities had been transferred to the children in a way that could not be explained by conventional means.

Assessing the Work

When I met Ian in 1996, he was energetically continuing his work and directing the Division. Despite my lack of research experience, he was always supportive of my early efforts in the field, as he had been with many

others. We investigated three American cases together. He was unfailingly polite to the families in a very dignified way, but he also remained critical-minded. Of the three cases, he was quite impressed by one, and we published a report of it in the paper of birthmark cases noted above (Pasricha et al., 2005). He judged one of the other cases to be unsolved (that is, we did not find a deceased individual whose life matched the statements the child had made) and the third to be almost certainly the result of wishful thinking on the mother's part.

He demonstrated a similarly cautious attitude toward the overall phenomenon of young children's claims of past-life memories. He wrote that no single case offered evidence that compelled a belief in reincarnation, and he was adamant that the term "proof" not be used for the evidence he had accumulated or even hoped to find. Nonetheless, although he emphasized that other explanations were possible, he wrote that he considered reincarnation to be the best explanation for the stronger cases that he had investigated—including ones in which the two families involved were previously unknown to each other and for which a written record of the child's statements was made before they were verified and cases in which a medical record documented a close correspondence between a child's birthmarks or birth defects and wounds on the body of the previous individual (Stevenson, 2000a).

That assessment seems fair. I reviewed many of Ian's cases as I was preparing to write a book about the work, studying not just his lengthy case reports but his field notes as well. I could see the limitations of the cases—the way that memories could vary in some of them across witnesses and even across interviews with the same witness. I also saw, however, how resolute Ian was to establish the facts. Many of the files contained lists of questions that he sent his assistants after he had talked with families. These led to further work, sometimes to answer minor and seemingly unimportant details that were unclear. This happened repeatedly in some instances, and Ian himself interviewed some witnesses a number of times over a matter of years. He did all of this to make sure that he had determined as accurately as humanly possible what had happened in each case.

In the end, as he wrote, he produced data that allow those who find reincarnation a congenial concept to believe in it on the basis of evidence rather than purely on the basis of faith (Stevenson, 1980, 1990b). That group, however, was not the one he was most interested in reaching. He once said—with a smile—that he would die a failure because he had not achieved his primary goal of getting mainstream science to seriously consider reincarnation as a possibility. Such a goal, in retrospect, may have been quixotic, particularly to be attained in a relatively short time, but as with Galileo, science's ultimate judgment on his work may come long after the end of his life. The numerous researchers contributing to this issue who were inspired and supported by Ian, and who attempt to model their own efforts by the standard he set, also demonstrate that he did not die a failure.

Toward the End

Ian's passion for the work never abated. He was well into his eighties before he retired, and he might never have done so except for his wish to devote more time to his wife Margaret, whom he clearly adored. (In fact, he might never have taken a vacation either except for that wish.) He continued to write after retirement and even took one final ''final trip'' to India. Margaret said at one point that she did not mind his taking the research trips, but she wished he would stop referring to each one as his last. He was pleased that other researchers, with his encouragement, had become interested in the cases and had made significant contributions (e.g., Mills, Haraldsson, & Keil, 1994).

Ian's final paper was a wonderful summary of his career in parapsychology that he wrote for this journal (Stevenson, 2006). Even after he finished it, he continued to discuss papers he wanted to write but eventually lost the physical energy to complete. There were more books to be written as well, as a life of 88 years was not long enough to exhaust his productivity. Ian finished his last published paper with words that, though not referring specifically to his 40 years of research on children's past-life memories, might well have applied to them: ''Let no one think that I know the answer. I am still seeking.''

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Journal of Scientific Exploration, Vol. 22, No. 1, pp. 44–53, 2008 0892-3310/08

ESSAY

Ian Stevenson and the Modern Study of Spontaneous ESP Experiences

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In 1959, in his first paper about parapsychology, ''The Uncomfortable Facts about Extrasensory Perception,'' published in *Harper's Magazine*, Ian Stevenson referred both to spontaneous cases and to experiments as primary sources of evidence for the existence of ESP. He summarized aspects of the work of the Society for Psychical Research (SPR), as well as the work of J. B. Rhine and his associates at Duke University. In the same year Stevenson visited Rhine and had a conversation with his wife, Louisa E. Rhine, about spontaneous case research. As Stevenson wrote years later:

> 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. (Stevenson, 2006: 14)

One of us (CSA) remembers Stevenson saying some time in the 1980s that, while he listened respectfully to L. E. Rhine, his polite silence did not mean she had convinced him of her views. Stevenson instead became a specialist in the study of spontaneous cases. In this paper we will discuss his work with spontaneous ESP experiences, relying on his publications that appeared in print from 1960 on.[1]

Modern Spontaneous ESP Studies

By the time Stevenson came into parapsychology in the late 1950s the field had changed, largely due to the work of J. B. Rhine and his associates (Pratt et al.,

1940; J. B. Rhine, 1934, 1947). As Stevenson became familiar with the parapsychological scene, he quickly realized the importance of Rhine's work, using it to make several points. He wrote: ''Dr. Rhine's experiments . . . showed that extrasensory perception does not depend upon space. . . . Dr. Rhine's group seems to have demonstrated also that some persons have a capacity to influence physical objects without physical means'' (Stevenson, 1959: 23). To some extent Rhine's influence changed the field of parapsychology from one in which research was mainly concerned with qualitative studies of spontaneous cases and mediums into one in which the emphasis was on statistically evaluated laboratory tests. By 1959, when Stevenson's *Harper's Magazine* article was published, the Rhines had become the dominant force in American parapsychology, to the point that years later Stevenson characterized them as the ''undisputed sovereigns'' (2006:14) of parapsychology in that earlier time. Before the meeting in which L. E. Rhine attempted to steer Stevenson clear of spontaneous case research, the Rhines had plainly stated their position on the ''proper'' place of spontaneous phenomena in parapsychology. In their view, case studies could provide ideas for hypothesis-testing in the laboratory, but they were not useful as scientific evidence for the existence of psychic phenomena. Such evidence could be found only in experimental studies (J. B. Rhine, 1948; L. E. Rhine, 1949). Fortunately for parapsychology, and for those of us who have always felt that the Rhines were too extreme in their views, Stevenson went his own way, providing in later years a much-needed balance for the Duke group's narrow experimental program.[2]

Stevenson carefully studied the work of the early SPR, such as the classic studies of Gurney, Myers, and Podmore (1886) and Sidgwick et al. (1894). As he began to publish his ESP case studies, there were a few other people in the field with similar interests. L. E. Rhine (e.g., 1951, 1953) was already well known for her ESP case collection studies. Jan Ehrenwald (1955) had published psychoanalytic observations of ESP cases. Celia Green (1960) had conducted a survey, and Hornell Hart (1954) had analyzed published cases of veridical out-of-body experiences. In fact, in one of Stevenson's first papers on ESP experiences, he argued that interest in spontaneous cases had returned: ''Psychical research, after a period dominated by experimental work, has entered another phase of interest in spontaneous paranormal phenomena'' (Stevenson, 1961:98).[3] But even though he was not the only investigator publishing, Stevenson's studies of ESP experiences differed from those of others because of the way he combined attention to evidentiality, phenomenology, and psychological analysis.

Stevenson was well aware that using spontaneous cases as evidence required careful attention to detail and corroboration. In his 1968 Parapsychological Association Presidential Address, ''The Substantiality of Spontaneous Cases'' (1971a), Stevenson argued that because experimentation was constructed to test phenomena suggested by the spontaneous experiences that occurred in daily life, those experiences could not be set aside without leaving experimentation without its foundational basis. Experimental tasks were, after all, operationalisms of such ''naturally occurring phenomena'' as telepathic and precognitive dreams,

visions and impressions, apparitions, out-of-body experiences, poltergeists, and cases of the reincarnation type. Spontaneous cases, unlike the experimental data of the era, were qualitatively rich, providing deep descriptions of seemingly paranormal effects that surrounded vivid and dramatic events. Stevenson felt that the process by which ESP information was conveyed and interpreted was more directly visible in well-investigated spontaneous cases for a number of reasons, not the least of which was that it was more possible to identity the ESP ''moment'' in a spontaneous case than it was in an experiment.

In this Presidential Address, and in two guest editorials published in the *Journal of the American Society for Psychical Research*, ''Changing Fashions in the Study of Spontaneous Cases'' (1987a) and ''Why Investigate Spontaneous Cases?'' (1987b), Stevenson reiterated the need for thorough investigation. Cases needed to be both authentic (that is, the details of the case report needed to be investigated to the extent that the researcher was reasonably certain that the details occurred as the experiencer claimed) *and* evidential (that is, the researcher needed to be reasonably certain that the events described were mostly likely paranormal). Stevenson was very much aware of the known weakness of some spontaneous cases, from the fallibility of eyewitness testimony to experiencers' mal-observations and motivated errors. But he contended that such problems of authenticity were not insurmountable.

Stevenson's approach to case methodology was exemplified both by his more general ESP case research, which he abandoned in the 1970s, and by his work in cases of the reincarnation type. Collecting and authenticating cases could begin with surveys and questionnaires or with unsolicited case descriptions and correspondence, but the process required, insofar as was possible, face-to-face interviews and the independent corroboration of case details and a careful analysis of case features. Still, he was also conscious of the value of unauthenticated cases for their value as a point of comparison to authenticated cases, whether evidential or non-evidential. In actuality, from the spontaneous cases of his earlier period to the cases of the reincarnation type that occupied most of his later working life, his case files represent well a myriad of points along the authenticity and evidentiality continuum. That all the data have been preserved for future analysis is another of his unique contributions to the field.

Stevenson's Spontaneous ESP Studies

Stevenson's first published study focused on experiences connected with the sinking of the famous ship Titanic (Stevenson, 1960c; see also a later paper, Stevenson, 1965c).[4] He presented 12 cases in this paper, discussing their value as evidence for ESP. Drawing on their phenomenological details, Stevenson suggested that the hypothetical stimuli represented in the ESP experience ''may activate or push into consciousness images already present in the subconscious layers of the percipient's mind'' (p. 167). He argued that although personal attachments to persons who died on the Titanic and strong general emotions

associated with the perceptions existed in some cases, these variables were not sufficient or even necessary in accounting for the class of experiences because several cases lacked them.

In another paper Stevenson (1961) used a single case to illustrate the criteria for, and characteristics of, precognitive dreams. Mrs. Roger Fellom had a dream in September of 1958 about her daughter Vivian, then 20 months old. Mrs. Fellom wrote: ''I very vividly dreamt of entering Vivian's room and much to my shock found her sitting on the sill of the bay window, one leg atop, the other dangling over the edge. She was gaily babbling along, waving her little arms in total unconcern of any danger, when suddenly she started to lose her balance and was about to fall off the 3 foot drop'' (pp. 98–99).

About three weeks later, after Mrs. Fellom had been continuously checking on her daughter, she found Vivian in the position and place she had seen in her dream and was able to avoid a possible accident. Stevenson obtained more details in later correspondence, including the statement that Mrs. Fellom had mentioned the dream to her husband, and Mr. Fellom's corroboration of this statement. Stevenson also found that Mrs. Fellom had never had a similar dream prior to that one, nor had her daughter ever been on the window sill before. In addition, she claimed the ''dream was more real than reality'' (p. 101). Stevenson was careful to discuss the weaknesses of the case, as well as possible conventional explanations. For example, he noted the corroboration was not completely independent because Mr. Fellom testified to having heard the dream before the event took place, but he was not present when the event occurred. To obtain a better perspective of Stevenson's careful consideration of variables, it is worth quoting part of his conclusion:

> In considering this dream as a precognitive experience the principal competing explanation is inference. Nearly all children climb and Vivian had climbed a little before her mother had this dream. It seems very likely that Vivian would sooner or later have attempted the climb from her bed to the window sill. . . . That she had never actually done so before the dream favors precognition as an explanation; yet the likelihood, one might almost say the inevitability, of her eventually attempting such a climb makes inference distinctly possible. . . . Although I am inclined to think that the dream was a precognitive experience, we cannot completely exclude inference as an explanation for the dream. (p. 102)

Regarding the issue of the vividness of precognitive dreams, a topic to which he returned in later work, Stevenson argued that the issue deserved more careful investigation. He wrote: ''We need a more detailed and quantitative comparison of the frequency of this characteristic in ordinary and precognitive dreams'' (p. 103).

Stevenson (1963) reported a contemporary, apparently veridical dream about a plane crash experienced by a physician from Virginia, to whom he referred by the pseudonym of Dr. Rellum. In addition to the evidential aspects of the case, Stevenson returned to the issue of the importance of previous experiences and interests associated with veridical dreams. He found that Dr. Rellum had a lifelong interest in planes, having developed a deep interest in and a fear of

flying. Dr. Rellum had also witnessed three plane crashes in his lifetime. This experiential history, Stevenson speculated, may have created some associations that facilitated the expression of ESP when information received related to airplane crashes but not when other topics were involved. Stevenson wrote:

> What I am suggesting is that percipients in psychical experiences will be especially liable to arousal in connection with topics of importance to them. . . . Dr. Rellum had an affinity for airplanes and airplane accidents derived from his life-long interest in aircraft and his witnessing of three serious airplane accidents. We can say that he had become sensitized to matters connected with airplanes. . . . Dr. Rellum's subliminal mind could have been alerted to the occurrence of a crash and then accorded it the greater attention required for a more detailed perception. (p. 192)

In these speculations Stevenson was influenced by the writings of W. H. C. Tenhaeff, among others (see also a later paper, Heywood & Stevenson, 1966).

Stevenson was also interested in both the accurate and the inaccurate images in Dr. Rellum's veridical dream. Stevenson thought it was ''possible to suggest a source of each incorrect image of the dream in the percipient's earlier experiences with airplane crashes he had witnessed years before the dream'' (p. 202).

Another important but somewhat forgotten contribution in this era in parapsychology's history when there were many surveys of psychic experiences is a survey Stevenson and a colleague conducted with Indian school children (Prasad & Stevenson, 1968). In the survey, they reported some similarities with previous studies of Western groups, including the facts that dreams and impression cases were more frequent than hallucinatory experiences, a high proportion of cases were related to death and serious events such as accidents, and relatives of the experiencers were involved.

In another project Stevenson analyzed cases of precognition of disasters (Stevenson, 1970d), both in previously published reports and in cases he had collected. Stevenson noticed that in previous studies dream experiences were often described as vivid and realistic and that symbolic representation was not frequent. However, ''some precognitive dreamers have reported identifying symbols that are, for them, reliable indicators of future events'' (p. 199). Regarding the effect of interests and the personal significance of the event to experiencers, Stevenson argued that the percipient's interests were indeed an important factor in the manifestations of these dreams. Finally, he offered an analysis of the specific features of the 125 dreams he had collected (see Table 1).

Stevenson's most important work on spontaneous ESP experiences appeared in his book *Telepathic Impressions: A Review and Report of 35 New Cases* (1970c). In the volume he explored imageless experiences in which the person had thoughts, feelings, emotions, physical symptoms, or raw impulses to take action which corresponded to a relevant event taking place at a distance.

The book was divided into two parts. The first part consisted of an analysis of 160 published cases drawn mainly from psychical research publications. Working before computers were widely used to analyze this type of data, Stevenson carefully compiled the details of each case, presenting most of them

TABLE 1
Features of 125 Precognitive Dreams Analyzed by Stevenson (1970d)

Feature	Percent
Vivid or realistic	45
Dreams occur two or more times with little variation	14
Percipient tried to prevent event	34
Symbolism present	13.5
Event was about	
Dreamer	49
Close relatives	31
Distant relatives, friends, acquaintances	11
Strangers	9

in a 12-page table. The table included a case number, the place where it was published, the sex of agents and percipients, the relationship of percipient to agent, the condition of the agent, whether the agent was focusing on the percipient at the time of the experience, whether the percipient was alone or with others, the number of additional details, whether action was taken by the percipient, whether the agent was identified by the percipient, whether an emotion was felt by the percipient, and additional remarks.

Stevenson found that 62.5% of the cases were reported as involving individual members of immediate families. Furthermore, the agent was frequently dying (41%) or in a serious condition caused by illness or an accident (41%). A smaller number of cases were reported in non-serious circumstances (18%). Another interesting finding was that some of the percipients took action because of their impressions. In fact, Stevenson found a statistically significant relationship suggesting that the percipient took action more often in those cases in which the agent focused on the percipient, as compared to cases without such focusing. He believed these results supported the idea that the agent played an active role in these experiences.

In the second part of the book Stevenson reported on his investigation of 35 new cases. One of them was reported by Mrs. Joicey Acker Hurth, resident of Wisconsin, in a letter written in October of 1968. She had sent her 5-year-old daughter to the local theater where she was to meet her father and brother and watch a Walt Disney movie with them. Mrs. Hurth wrote that she was washing the dishes when: ''Quite suddenly while I held a plate on my hand an awesome feeling came over me. I dropped the plate, turned my eyes toward heaven and prayed aloud, 'Oh God, don't let her get killed!' For some unexplained reason I knew Joicey [the daughter] had been hit by a car or was going to be. I was quite conscious of her involvement in an accident'' (pp. 61–62).

Mrs. Hurth was so convinced by her impression that she phoned the theater and asked if her girl was hurt. The theater manager confirmed that her daughter had indeed been hit by a car, but that she was all right and that her father was with her. Joicey (the daughter) later said that when she was hit she called for her mother.

In discussing the old and new cases Stevenson again argued that well-investigated cases could provide evidence for the existence of ESP. He noticed two types of cases. Typical cases seemed to consist of (1) awareness that someone was in danger at a distance; (2) a feeling or emotion about that awareness; and (3) an impulse to take action to help the person related to the experience. Other cases were incomplete, containing one or more of these features, but not all of them.

Stevenson also noted that some cases included additional details or imagery that occurred after the initial impression and conveyed new information. He also argued that in some cases, experiencers' description of the process resembled the act of remembering things that one knows but has forgotten, such as names.

The varieties of ways in which ESP manifested, as well as the reasons for such variation, fascinated Stevenson. He wrote:

> As we have seen, some percipients experience an emotion, e.g., anxiety, or a physical symptom, e.g., pain, which resembles, or, as it were, copies the same condition in the agent. . . . Other percipients . . . initiate a response of their own, reacting, but not imitating, the condition of the agent. Thus we have percipients who are depressed and grieving over the death of the agent. And one percipient experienced joy in relation to her apparently paranormal awareness of her sister's death. . . . We also have percipients who may develop a physical illness, e.g., asthma, in response to a telepathically communicated stress in a loved agent. . . .
>
> Why extrasensory communications find their way into conscious and manifest expression in these different ways in different people we do not yet understand. The analysis of a large number of cases might well show that imagery, for example, develops more readily in percipients who are good visual imagers in other aspects of life and that perhaps physical symptoms develop more readily in persons liable to react with physical symptoms to other types of stress. And, as I have suggested earlier, the imitative type of expression of a telepathic communication may occur more frequently in those given otherwise to strong identifications with other persons. These are questions of great importance. Their solution will require the investigation of large numbers of cases and the alliance in one person, or in several, of the skills of the student of spontaneous cases, of the clinical psychologist, and perhaps of the experimental parapsychologist. (pp. 147–148)

Concluding Remarks

We chose to emphasize aspects of Stevenson's work with spontaneous ESP experiences because we feel that this part of his legacy to parapsychology is sometimes forgotten in the emphasis on his survival-related studies, particularly those with children who claim to remember previous lives. This may be the case in part because, with one exception (Stevenson, 1992), Stevenson dropped ESP experience research early on, moving in later years to focus instead almost exclusively on reincarnation and other topics with implications for the question of survival of bodily death (e.g., Greyson & Stevenson, 1980; Haraldsson & Stevenson, 1974; Stevenson, 1975, 1984b).

It goes without saying that Stevenson was an important figure—one may say a leader—in the study of spontaneous ESP between 1960 and 1970. He provided

an important balance in parapsychology against those who emphasized laboratory work to the exclusion of all else. In addition, he also countered workers in the field—such as J. B. and Louisa Rhine—who reduced the importance of work with spontaneous ESP experiences to mere hypothesis-generation for experimental work. Furthermore, Stevenson's interest in the imagery and prior experiences of percipients to some extent represents a link between modern parapsychology and the work of previous students of the qualitative aspects of ESP (e.g., Gurney, Myers, & Podmore, 1886; Warcollier, 1938).

We believe that Stevenson's work with ESP experiences is still relevant today, especially because it may guide a new generation to conduct process research on new cases. Much useful knowledge remains to be found using Stevenson's methods and those of the pioneers in the qualitative study of ESP.

Stevenson's body of research remains as a testament to the importance of the study of parapsychological phenomena as they occur in daily life. His work with ESP experiences—in keeping with his work on other topics—is an example of the profound influence a single committed individual can have on a field of study.

Notes

[1] Related to this work are Stevenson's discussions of the evidential value of spontaneous cases (e.g., Stevenson, 1971a). Throughout his career he both defended the potential evidentiality of spontaneous cases and outlined appropriate methodologies (e.g., Stevenson, 1962c, 1971b, 1987a,b). Some of these discussions centered on cases of the reincarnation type (Stevenson, 1966, 1975). Stevenson repeatedly defended the validity of spontaneous case research against a variety of critics, including Michael Scriven (Stevenson, 1962a,b), Louisa E. Rhine (Stevenson, 1970a,b), and David Marks (Stevenson, 1986). In addition, he developed rating scales to study spontaneous cases (Stevenson, 1965b; Stevenson, Palmer, & Stanford, 1977).

[2] This is not to say that Stevenson was against experiments or that he did not conduct experimental work. For example, he was involved in experiments with medium Hafstein Bjornsson (Haraldsson & Stevenson, 1974) and psychic Pavel Stepanek (Pratt et al., 1968). He also conducted studies of psychic photography (Stevenson & Pratt, 1968) and discussed both criticisms of (Stevenson, 1967) and testimony about (Stevenson, 1974) experiments.

[3] Other relevant work of the era includes Barker (1967); Dale, White, and Murphy (1962); Heywood (1955); Lambert (1961); Nicol and Nicol (1958); and Sannwald (1959). We are not including work with phenomena other than ESP experiences (e.g., Pratt & Roll, 1958).

[4] During the 1960s Stevenson also examined published cases of individuals who claimed to remember previous lives (Stevenson, 1960a,b) and was busy investigating new cases (Stevenson, 1966) of this type. He also published cases of apparitions (Stevenson, 1964, 1965a). Among these, Stevenson (1962d) examined the description of an apparition said to have been seen by John Donne. He argued in a later communication that the case was most likely a fabrication (Stevenson, 1984a) and that it had also been reported by Gurney, Myers, and Podmore (1886: vol. 1:lxxix, 394n). In fact, the case has a longer citation history preceding the work of Gurney et al. (e.g., Aubrey, 1857: 72–74; Ferriar, 1813: 63).

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Journal of Scientific Exploration, Vol. 22, No. 1, pp. 54–63, 2008 0892-3310/08

ESSAY

Ian Stevenson's Contributions to Near-Death Studies

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Most people familiar with the work of Ian Stevenson associate him with his groundbreaking investigations of children who claim to remember previous lives. But Stevenson was also a pioneer in the study of what have come to be called near-death experiences, or NDEs, the transcendent or mystical experiences that occur among many people who have come close to death or have even suffered an apparent clinical death during a severe illness or accident but who then recover or are resuscitated or escape serious injury.

Stevenson's Role in Fostering Near-Death Studies

The term ''near-death experience'' was coined by Raymond Moody in 1975. Yet in an article published 16 years earlier, Stevenson recounted the story of a clergyman who underwent an operation under general anesthesia and later reported in detail what happened while he was ostensibly fully anesthetized and unconscious, including the surgeon's leaving the operating room to get another instrument and details of conversations among the operating room staff (Stevenson, 1959: 21). He also noted in that article that nearness to death sometimes heightens an individual's apparent powers of extrasensory perception, and he related the story of an elderly woman whose family had gathered around her deathbed:

> Suddenly she seemed much more alert and the expression on her face changed to one of great pleasure and excitement. She raised herself slightly and said: ''Oh, Will, are you there?''—and fell back dead. (Stevenson, 1959: 22)

None of the family members present in this instance was named Will, nor could they identify any such relative, save a brother who lived in England. Not long afterwards they received word from England that the woman's brother Will had died two days before the woman's deathbed vision. Stevenson noted that this

case, though superficially impressive, illustrated a significant difficulty with spontaneous cases, namely that despite the event having been witnessed by several people, none of them had made a written account before receiving word of her brother's death.

Stevenson furthermore reported in medical journals, again several years before NDEs had a name, that he had amassed and was analyzing nearly 50 cases of persistent consciousness during close brushes with death, involving out-of-body experiences, unusual mental clarity, and ecstasy, and he urged his fellow physicians to inform themselves and to inquire about such experiences in their patients (Stevenson, 1971).

One of the earliest NDE accounts Stevenson collected was that of George Ritchie, a fellow psychiatrist at the University of Virginia, who had had an out-of-body experience with veridical perceptions after having been pronounced dead from double lobar pneumonia in the Army during World War II. With Stevenson's encouragement, Ritchie began lecturing publicly about his experience. In 1965 Ritchie's narration of his account to participants of a seminar at the University intrigued an undergraduate philosophy student named Raymond Moody, who recognized the similarity to Plato's story of Er in *The Republic* and other ancient Greek accounts. Ritchie alluded in his lecture to research by Stevenson, who was at the time Chairman of the Department of Psychiatry. Moody went on to teach philosophy and then graduate from medical school elsewhere, and during that time he collected enough similar cases to write his groundbreaking book *Life after Life* (1975), in which he coined the term ''near-death experience,'' bringing the phenomenon to public attention. When Moody returned to the University of Virginia the following year to begin his psychiatric residency training, Stevenson introduced him to donors interested in funding scientific research into this newly-described phenomenon. However, Moody disagreed with Stevenson's belief that NDEs, and particularly their implications for the question of postmortem survival, were amenable to scientific investigation, and he declined to participate in the research. He did, however, maintain a very cordial relationship with Stevenson, and turned over to him for study some of the voluminous correspondence he subsequently received from near-death experiencers who had read his book.

With that connection established, the University of Virginia started to become known as a center for near-death research. Even though NDEs were not Stevenson's primary focus, he recognized their importance and encouraged his students and junior colleagues to pursue their investigation of such events. Through his encouragement and mentorship, Stevenson played a vital role in furthering the near-death research of a number of scholars both at the University of Virginia and abroad, including Emily Cook (later Emily Kelly), Justine Owens, English psychiatrist Nicholas McClean-Rice, Australian sociologist Allan Kellehear, Indian psychologist Satwant Pasricha, and myself. Several of us received initial funding for our near-death studies from research grants that Stevenson obtained from various foundations, institutes, and donors.

My relationship with Stevenson began in 1972, when he offered me an elective opportunity at the University of Virginia during my senior year at another medical school. Largely on the basis of that relationship, I returned to Virginia for my psychiatric residency training and collaborated with Stevenson on following up on NDE accounts from Moody's correspondents and other individuals who subsequently learned of our interest in the phenomenon. Two decades later, I was able to return to Virginia thanks to a research grant we had obtained to collaborate on a study of long-term aftereffects of NDEs on a cardiac care unit. I was able to work for another decade with Stevenson at the University of Virginia before he finally succumbed last year to a life-long struggle with pulmonary disease.

Stevenson's Publications on Near-Death Experiences

Stevenson's publications on near-death experiences encompassed three overarching aspects of the phenomenon: its phenomenology, its implications for the mind-body and survival questions, and methodological advances in its investigation.

Phenomenology of Near-Death Experiences

Stevenson published several articles that delineated or expanded our understanding of the phenomenology of NDEs. He and I collaborated on an early paper published in a mainstream psychiatric journal describing the common features of 78 near-death experiences, based on firsthand written or tape-recorded accounts followed up with detailed questionnaires, personal interviews, and examination of medical records (Greyson & Stevenson, 1980). We found that these experiencers tended to report other transcendental experiences more often than did either the general population or psychiatric patients. Specific features of the NDEs did not conform to the pre-existing beliefs of the experiencers, but there were suggestions that cultural and psychological factors influenced experiencers' perceptions or reports of the NDE. We found that many important features of NDEs were more pronounced in experiencers who had *not* taken drugs or alcohol, controverting the brain dysfunction model of NDEs. While we did find some support for the role of NDEs as adaptive psychological responses to the imminent threat of death, apparent extrasensory and veridical out-of-body perceptions during the NDE seemed to require a different explanation. Finally, we found that the attitudinal and behavioral changes following NDEs were more pronounced and pervasive than those following other psychic experiences. We argued then, as Stevenson did in other publications, that studying NDEs was important not only to our understanding of the dying process, but also to the clinical care of terminally ill patients, grieving families, and suicidal patients.

In the first systematic cross-cultural investigation of near-death experiences, Stevenson collaborated with Satwant Pasricha in a study comparing features

of NDEs in India with American cases (Pasricha & Stevenson, 1986). They interviewed Hindu-speaking experiencers or firsthand informants in northern India and compared their reports with those from the Greyson and Stevenson (1980) study described above. NDEs from both cultures shared many features in common, such as seeing deceased acquaintances and "beings of light" or religious figures. However, there were some apparently culture-bound differences. For example, NDEs in India did not typically include encounters with tunnels or lights. Near-death experiencers (NDErs) in India tended not to report viewing their physical body after they separated from it, as did Americans, but they sometimes reported residual marks on the physical body following the NDE, a feature not often seen in American cases. Indian NDErs commonly described being returned to life because a spiritual arbiter (Chitragupta), to whom they were led by messengers, determined that the wrong person had died by mistake. In contrast, American NDErs who described a reason for their return either choose to come back, often because of love for family, or were "sent back" for various reasons, although never because a mistake had been made. Stevenson concluded by arguing against a reductionist interpretation of NDEs as nothing but expressions of culture-bound beliefs, in that cultural variations in linguistic descriptions do not weigh for or against the reality of the phenomenon described.

Stevenson and Emily Cook later analyzed 122 experiences in which persons described involuntary recall of memories (Stevenson & Cook, 1995). Comparing NDEs that did and did not include involuntary memories, Stevenson concluded that these "life reviews" were more likely when the close brush with death was the result of a sudden, unexpected accident. The life review was more commonly described as anterograde (that is, from childhood to the present) rather than as retrograde, simultaneous, or randomly sequenced, and it was overwhelmingly described as very rapid. Despite the speed of the presentation of memories, the majority were also described as detailed, realistic, vivid, and encompassing the experiencer's entire life. A majority of experiencers also reported some form of judgment accompanying the life review, most often conducted by the NDErs themselves. Stevenson noted that this judgment that often accompanies a life review, by no means a pleasant experience, argued against the psychodynamic interpretation of involuntary memories as a pleasurable distraction producing a "euthanasia effect" in those about to die. Instead, he reported that the purpose most often ascribed to a life review by experiencers was to guide them in amending their behavior after their death.

Implications for the Survival Hypothesis

It was clear from the start that Stevenson's interest in NDEs stemmed from their implications for the relationship between mind and body and for the possibility of mind surviving bodily death, a fact that he never hid. As early as 1977, in a critical review of the history of research into the evidence for survival,

Stevenson included the (then) recent studies of persons near death who reported having seen their bodies from an out-of-body perspective (Stevenson, 1977). He noted that such cases had as yet contributed little to the evidence for survival, but he suggested that investigation of veridical out-of-body and extrasensory perception and heightened mental clarity during these experiences suggested that the mind-brain relationship was not as clear-cut as usually assumed.

Shortly thereafter he and I collaborated on an article that was published in *JAMA: The Journal of the American Medical Association,* entitled ''Near-Death Experiences: Relevance to the Question of Survival after Death'' (Stevenson & Greyson, 1979a). In this essay we noted academic near-death researchers' tendency to skirt the survival question and briefly reviewed the near-death literature to that point in time. We pointed out that NDE reports from differing cultures, while showing some influence of culture-bound expectations, also show remarkable uniformities suggestive of survival. These features included accurate out-of-body perceptions, exceptional mental clarity when apparently separated from the body, accurate perceptions of events going on at a considerable distance from the physical body, and encounters with deceased relatives and friends, some of whom the near-death experiencer could not have known had died. We suggested that if researchers refused to conjecture about the possibility of postmortem survival they would be unlikely to seek or recognize evidence bearing on that question.

It might be thought that the flagship journal of the American Medical Association would be an unusual venue for the publication of an article on the survival issue, and indeed that journal subsequently published a letter to the editor protesting that such a traditionally religious topic was out of place in a medical journal. In our response (Stevenson & Greyson, 1979b), we argued that publishing this article in a medical journal was justified by the frequent occurrence of NDEs in medical settings, where the clinical observations of physicians might contribute critically to our understanding of such experiences, and by the profound impact of NDEs on death attitudes, including but not limited to belief in postmortem survival, that affect how seriously ill patients approach their medical treatment and the prospect of death.

Stevenson repeatedly argued that features such as enhanced mental clarity when brains are dying challenge the monist view that minds and brain are identical (Stevenson, 1979) and that features such as visions of deceased persons, news of whose death had not reached the NDEr, cannot be regarded as hallucinations but must be taken seriously as bearing on the question of survival (Stevenson, 1981). In fact, Stevenson was so frustrated by the Western materialistic assumption that all unshared perceptions, such as NDErs' visions of the deceased, must be pathological hallucinations that he proposed a new term to avoid that dismissive label. While retaining the word ''hallucination'' for unshared perceptions that appear linked to mental disorder, he suggested the new term ''idiophany'' for unshared perceptions that show no association with mental disorder but in fact may convey veridical information. He argued that using a label

that did not connote psychopathology would not only encourage people to narrate their experiences but would also encourage psychiatrists to consider their therapeutic value and implications for the mind-body relationship (Stevenson, 1983).

Finally, Stevenson collaborated with Emily (Cook) Kelly and me in two articles confronting head-on the evidence that NDEs may contribute to the question of survival after death. The first of these articles, published in this journal, noted the neglect of the survival question by most near-death researchers, despite that being the major source of popular interest in the topic (Cook, Greyson, & Stevenson, 1998). We delineated three features of NDEs that provide indirect evidence supporting the survival hypothesis: paradoxically enhanced mental function during periods of brain impairment, seeing the physical body from an external visual perspective, and paranormal perceptions. Lucid mental processes under diminished physiological conditions that most neuroscientists regard as incompatible with complex cognitive activity suggest an independence of mind from brain under such circumstances, permitting consideration of the possibility that mental activity may persist after brain death (e.g., Owens, Cook, & Stevenson, 1990). Studies have documented the accuracy of NDErs' descriptions of their physical bodies as viewed from different spatial locations, again suggesting that the mind may be less tightly bound to the physical body under certain conditions (e.g., Sabom, 1982). And reviews have identified numerous accounts of NDErs' accurate reports of perceiving events at a distant location, many verified by independent witnesses, suggesting that NDEs are not entirely subjective in origin (e.g., Hart, 1954). We described the details of 14 NDEs that included all three of these features suggestive of survival.

In a second article on this topic, we noted that 92% of NDErs whose medical records documented their proximity to death reported enhanced mental functioning, including increased speed, logic, and clarity of thought; visual and auditory clarity; vividness of colors; and control of cognition; half of such NDErs reported looking down on their physical body from a different spatial position; and a small but significant number reported verifiable events outside the range of their physical senses (Kelly, Greyson, & Stevenson, 2000). While some of these features might be explainable by different mechanisms, the occurrence of all three features together in individual NDEs makes the survival hypothesis, which might explain all three, more credible. We described three additional, newer cases that included all three features and concluded that the evidence from NDEs was far from compelling a belief in survival, but strong enough to permit such a belief, particularly in the light of convergent evidence from other lines of research.

Refinements in Methodology

Another consistent thread throughout Stevenson's research into NDEs has been progressive refinement in research methodology. In the 1979 *JAMA* article

described above (Stevenson & Greyson, 1979a), he noted methodological pitfalls in near-death research up to that time, stemming from the confounding influences of cultural differences, physiological and psychological circumstances of a close brush with death, researchers' interviewing techniques, and inadequate medical records. He noted that much of the near-death literature rested on uncorroborated statements of patients about their closeness to death and that evaluation of many explanatory hypotheses required data regarding these patients' physiological conditions at the time of the near-death event.

A decade later, Stevenson and his colleagues published a comparative study of medical records of purported NDEs (Stevenson, Cook, & McClean-Rice, 1989). They were able to examine the medical records of 40 patients who described NDEs occurring in a medical setting recently enough to have had medical records preserved. These 40 near-death events included 29 illnesses, surgeries, or childbirths (72.5%), nine accidents (22.5%), and two intentional overdoses (5%). Stevenson and his two colleagues each independently rated each of the 40 records for evidence of life-threatening illness or injury, such as documentation of heart rate, respiration, blood pressure, and impaired consciousness. They categorized each record as documenting (1) no serious illness or injury, (2) serious but not life-threatening illness or injury, or (3) illness or injury so life-threatening as to be likely to end in death if not for medical intervention. Thirty-three of these patients (82.5%) claimed to have been dead or near death; 21 claimed to have been told that by medical personnel. However, in only 18 cases (45%) did the medical records contain documentation of life-threatening illnesses or injuries, such as cardiac arrest, anaphylactic shock, and severe head injuries, whereas 22 records (55%) did not. Stevenson and his colleagues considered that deficiencies in the medical records or the occurrence of the NDE prior to the onset of documented medical treatment may have accounted for some of this discrepancy between patients' reports and medical documentation of life-threatening status. However, they concluded that one's belief that one is dying, whether or not that is the case, may be an important precipitant of a near-death experience, and in fact they suggested that a better name for these events might be ''fear-death experiences.''

In a subsequent review of medical records comparing near-death experiences with such ''fear-death experiences,'' Stevenson and his colleagues compared the features of NDEs in persons who would have died without medical intervention and in persons who were not in such danger (although, again, most of them thought that they were) (Owens, Cook, & Stevenson, 1990). They found that persons who had actually been in danger of dying were more likely than the comparison group to report perceiving bright light and experiencing enhanced cognitive function. The two groups did not differ in reports of being in a tunnel, experiencing positive emotions, believing they had left the physical body, remembering past life events, or believing they had been near death. They concluded that the presence of many characteristic NDE features in persons who had not been near death supported a psychological interpretation of the

experience; the correlation of other features with documented proximity to death supported a physiological interpretation of the experience; and the increased incidence of enhanced mental activity in persons whose brain functions were most severely impaired supported a transcendental interpretation of the experience.

That comparative review of medical records of NDEs was published in *The Lancet,* the mainstream medical journal with the highest ''impact factor,'' a measure of its scientific importance. It predictably stimulated a number of letters to the editor. In a response to those letters, Stevenson and his colleagues pointed out several additional methodological considerations: the role of an investigator's attitudes in inhibiting experiencers from revealing their accounts; the distinction between mental clarity during the NDE itself and confusion regarding events preceding the NDE; the inadequacy of hypotheses that might explain selected features of an NDE but that are inconsistent with others; the danger of dismissing transcendental hypotheses simply because they are thought to contradict popular paradigms; and the fact that psychological, physiological, and transcendental explanations are not mutually exclusive (Owens, Cook, & Stevenson, 1991).

In addition to emphasizing the importance of medical documentation in NDE research, Stevenson argued for greater methodological rigor in the collection and analysis of cases. For example, in an article subsequent to Stevenson and Pasricha's (1986) cross-cultural study of NDEs in India and America, Susan Blackmore (1993) had reported that three of eight NDEs she had collected from India did in fact include a tunnel, a finding that she interpreted as supporting her model of NDEs as products of brain physiology. Stevenson and his colleagues responded with a methodological critique of Blackmore's survey (Kellehear et al., 1994). They noted that Blackmore had based her analysis on only eight experiences investigated solely through correspondence, whereas other studies with larger samples investigated through personal interviews had failed to find any evidence of tunnel experiences in India. Furthermore, they noted, none of Blackmore's correspondents had used the term ''tunnel'' spontaneously; three who had mentioned darkness accepted the term ''tunnel'' only after Blackmore had suggested it in leading questions. Moreover, none described a brightness in the center, a critical part of Blackmore's physiological hypothesis. Finally, Stevenson and his colleagues noted that Blackmore had collected her cases through advertisements in an Indian newspaper directed at a highly educated, English-speaking readership, atypical of the Indian population. They concluded that, rather than developing an explanatory model based on the data, this was an example of starting with a hypothesis and then ''torturing data until they give you the answer you need'' (Kellehear et al., 1994: 112).

In his final publication on near-death experiences, Stevenson collaborated with me and Geena Athappilly, then a medical student from another university doing a research elective at the University of Virginia, as I had done with Stevenson more than three decades earlier. This study addressed the degree to

which cultural expectations of the dying experience influence reports of NDEs. As his last publication on the topic, it is fitting both that it compared his earliest NDE cases, collected prior to Moody's coining of the term ''near-death experience,'' with our most recently collected NDEs and that it was published in the *Journal of Nervous and Mental Disease,* which had always been the mainstream medical journal most receptive to his work (Athappilly, Greyson, & Stevenson, 2006). Noting that Moody's 1975 description had come to define NDEs both among the academic community and in the popular imagination, we questioned the degree to which that model had shaped subsequent experiences, their recall and retelling, and the collection of their accounts. We compared the occurrence of the 15 characteristic features of Moody's model among 24 NDE accounts Stevenson had collected prior to 1975 and among 24 accounts collected in recent years, matched to Stevenson's pre-Moody cases on demographic, psychosocial, and physiological variables. With the single exception of the tunnel experience, which was more common among the recent cases, the two samples were indistinguishable, controverting the hypothesis that prevailing societal models substantially influence reports of NDEs.

Conclusion

Throughout his career, Stevenson's writings were permeated with an open-minded scientific attitude—the attitude of true skepticism rather than that of debunkers who try to pass themselves off as skeptics. In an early review of the field, he wrote:

> It is not helpful to declare that all near death experiences provide evidence of our survival after death; but neither is it helpful to categorize them all as merely ''toxic psychoses.'' I can feel dogmatic about only one conclusion in this controversy: that we still have a great deal to learn from the study of near death experiences. (Stevenson, 1980: 272)

After an additional decade and a half of research, an interview with Robert Kastenbaum, editor of *Omega*, revealed that Stevenson's conclusion, but not his skeptical attitude, had evolved:

> I believe that near-death experiences do not lend themselves to any single interpretation. They vary a great deal in the circumstances of their occurrence and in the content of the experience. I do believe, however, that a small number of them add to the evidence for mind/brain dualism, by which I mean that mind and brain, although interacting during life, are not identical and that minds may survive the death of the physical bodies with which they are associated. (Kastenbaum, 1994: 180)

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Journal of Scientific Exploration, Vol. 22, No. 1, pp. 64–72, 2008 0892-3310/08

ESSAY

Ian Stevenson's Contributions to the Study of Mediumship

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In 1965 Ian Stevenson wrote: ''Among all the cases which seem to provide impressive evidence of survival, a most important group consists of those in which a communicator appears whose existence neither medium nor sitters know anything about at the time of the manifestation'' (p. 65), a phenomenon he christened ''drop-in'' mediumistic communications (e.g., Stevenson, 1970: 53). He argues that if ''subsequent checking verifies the existence of a person and details corresponding with the communicator and his message ... [then] the explanation of the communication as resulting from telepathy between the medium and the sitters breaks down'' (Stevenson, 1965: 47). He goes on to discuss the difficulties in excluding the possibilities of latent subconscious memory (cryptomnesia) and fraud, but he also emphasizes the great importance, when these can be excluded, of *purpose* or *intent* that seems to lie behind such ''drop-in'' cases.

In the same paper, and again a few years later (1970), Stevenson writes that he is preparing a monograph reviewing some 60 cases of the ''drop-in'' type, mostly from the published literature but some of which had not yet been reported. This planned review of ''drop-in'' cases was never finished, probably because of his increasing commitment to the study of cases of the reincarnation type. He did, however, publish 10 papers on mediumship, eight of them dealing with cases of ''drop-in'' communications or communicators (Haraldsson & Stevenson, 1974, 1975a,b; Ravaldini, Biondi, & Stevenson, 1990; Stevenson, 1965, 1968, 1970, 1973, 1975; Stevenson & Beloff, 1980).

The "Drop-In" Cases of Abraham Florentine, Hans-Peter Pasona, and Robert Marie

Stevenson's first paper on mediumship deals with a well-known case, that of Abraham Florentine, which was first published by the highly regarded medium Stainton Moses in 1875. Stevenson (1965: 48) quotes from Moses' report of the case:

> In the month of August last [1874], I was staying with Dr. Speer of Shanklin, Isle of Wight. We had a number of sittings, and at one of them a spirit communicated, who gave his name as Abraham Florentine. He said that he had been concerned in the war of 1812,

and that he had lately entered spirit-life at Brooklyn, U. S. A., on August 5th, at the age of eighty-three years, one month, and seventeen days.

An entry in one of Moses' notebooks reveals that the actual date of the sitting had been September 1, 1874, and that Abraham Florentine had died after a painful illness. Inquiries in the United States led to the identification of Abraham Florentine and an interview with his widow. She verified all the details, except she said that her husband had died at the age of 83 and 27 days (not one month and 17 days, as reported in the communication). Much later, in 1921, Dr. E. J. Dingwall made further inquiries in the United States and found obituaries of Abraham Florentine in the *Brooklyn Daily Eagle* and *New York Daily Tribune,* both of which stated that Abraham Florentine, a veteran of the war of 1812, had died on August 5 after a painful illness at the age of 83 years, one month, and 17 days. Steamers to Europe sailed two days after publication of the obituaries, and so there would have been time for someone concerned to see a copy before the sitting took place on September 1. ''The case obviously seemed best explained as an instance of cryptomnesia, the similar error about Florentine's age in the obituaries and in the communication being regarded as almost conclusive evidence that the communication itself had derived somehow from the obituaries'' (Stevenson, 1965: 51).

Stevenson undertook new inquiries more than 40 years later and learned that records of veterans of the United States Armed Services, as well as records of the Greenwood Cemetery in Brooklyn, showed that the age of Abraham Florentine given by the communicator and the obituaries was in fact most likely right, and the widow wrong. Stevenson acknowledged that these new inquiries had not changed the status of the case markedly: Whereas before it ''was properly regarded as *almost certainly* one of cryptomnesia,'' its status now was ''*possibly* a case of cryptomnesia, but by no means definitely so'' (p. 53). Stevenson was ''inclined to regard the case as still best explained by cryptomnesia'' (p. 53), and he gives four reasons for that conclusion, one of which was that there was no *purpose* behind the communication—a feature, he notes, often characteristic of cases of cryptomnesia and fraud. Nevertheless, he argues that the case illustrates an important lesson, namely, that ''every case, no matter how old, still deserves attention to its details,'' since these might ''modify our interpretation of the case'' (p. 54).

Stevenson's next contribution to the question of ''drop-ins'' concerns a case that was brought to his attention in 1964 while he was on a sabbatical in Zürich, Switzerland. The medium, Frau Schütz, was a non-professional trance medium who gave regular sittings to a small private circle in Zürich. At such a sitting in 1962 there appeared ''a little lad.'' He gave his name as Hans-Peter Pasona (apparently an Indian surname), said that he had dark hair and brown eyes, died (of something to do with the appendix and ''an unusual illness with a lot of fever'') in a children's hospital ''a long time'' ago, lived in the seventh district in Zürich, and had two living brothers. He also said that his father had something to do with tea, and he said ''give my love to my mother.''

In the telephone directory for Zürich there was a family of Passanah. They were of Portuguese (not Indian) origin, but they had lived in India and were British citizens. They had three sons, all born in India. After the birth of the youngest child, they moved in 1931 to Zürich, where they lived in the seventh district and where the father engaged in the business of importing wares from India, probably including tea. The youngest son, who had indeed had dark hair and brown eyes, died in 1932. Although the boy was named Robert, not Hans Peter, nearly all the other details were accurate. Stevenson concluded that ''although the communicator gave rather scanty information about himself, he did provide enough information to establish his identity clearly. There is only one family of the name of Passanah in Zürich and they had lost one of three brothers. Nearly all the communicated details fitted their circumstances very well'' (Stevenson, 1970: 60).

With his usual thoroughness Stevenson examined possible normal sources of information available to the medium: the telephone book, newspaper obituaries, hospital records, and the gravestone. After a detailed discussion of the pros and cons of the case, Stevenson argues that he considers fraud and cryptomnesia unlikely but not firmly excluded and that ''the communication contained information paranormally derived.'' Motivational factors in the case made him ''favor the idea that a real discarnate communicator influenced the communication,'' and he concludes that cases of this kind seem ''of very great potential importance for advancing the evidence for survival after death'' (p. 64).

In 1973 Stevenson published ''A Communicator of the 'Drop In' Type in France: The Case of Robert Marie.'' In the introduction he mentions that it was perhaps a slight defect in the Passanah case that the medium and the sitters lived in the same city as the communicator and ''that the information given by the communicator referred only to events that had occurred prior to his death'' (p. 47). In contrast, in the case of Robert Marie the medium lived and worked in Paris, whereas the communicator had lived in a town far away from Paris, and the communicator referred to several events that took place after his death, such as the death of his son. This is a lengthy paper in which no stone is left unturned in attempting to verify the 14 different statements made by the communicator. It is beyond the scope of this paper to review the details of this investigation, but suffice it to say that many of the statements were verified, although there was some uncertainty about which of two brothers the communicator was, especially because both had been killed in World War I. Stevenson concludes that ''perhaps the communications would make most sense if we suppose that *both* brothers were attempting to communicate during the sittings'' (p. 74).

The Icelandic Medium Hafsteinn Björnsson

Now on a more personal note: My association with Ian Stevenson started in the fall of 1970, when I entered into a year-long internship program in clinical psychology in the Department of Psychiatry at the University of Virginia.

At my request I was allowed to spend a part of that year in the Division of Parapsychology (now the Division of Perceptual Studies). I soon learned of Stevenson's keen interest in mediumship, and we discussed my observations of mediums in Iceland and books I had read about them. The most prominent medium at that time was Hafsteinn Björnsson (1918–1977), about whom books had been written (in Icelandic) with interesting accounts of individual cases, including drop-in communicators and one impressive case of xenoglossy. For example, Elinborg Larusdottir (1946) had written a few books consisting of reports obtained from sitters whom she had interviewed. She usually included in these reports only details about which all the interviewed sitters agreed, and she often cited published written documents, including affidavits from sitters affirming that her reports accorded with their memories.

Some years earlier I had attended a few sittings with mediums in Iceland, although not with Hafsteinn (in Iceland we always refer to each other by the personal name). During a visit to Iceland in the spring of 1972, however, I spent a month studying Hafsteinn's mediumship as an observer at his sittings. He was cooperative at all times and agreed that I could attend every sitting that he held during that month and audio-record them.

Hafsteinn's abilities as a medium had already attracted much attention in Iceland, and there was always a long waiting list for his sittings. During the day he was employed full time collecting bills for the State Radio. In the late afternoon and early evening of every working day, and sometimes also on weekends, he usually held two séances (each for six to seven sitters). Easter fell during my time with him, and he visited the eastern part of Iceland, where he held many séances during five days there. I attended 48 sittings and gathered valuable observations about how he operated.

Hafsteinn was primarily a trance medium with regular controls who would describe the departed at the séance and bring messages from them, often giving their full names so that they were easily identified. One of his controls in particular, Runki (see below), described deceased persons, often associated with or related to each other in some way, who he said were present with the various sitters. Twenty to 30 persons or more might thus be described or identified at each séance.

At practically every séance, most of which lasted up to an hour and a half, a few direct communicators (that is, those who seemed to speak directly through Hafsteinn rather than through a control) also appeared. Usually these direct communications were of brief duration, but long enough for the communicators to reveal their identities, express some message, and exchange a few sentences. They would also sometimes send greetings to people not present but with whom one of the sitters had a relationship. The direct communicators nearly always came to a particular sitter and addressed him or her. The sitters were often deeply impressed by the characteristic features, manner of speech, and behavior expressed by the direct communicators.

I did not make a systematic analysis of the frequency of direct communicators, but in my notes from a séance on April 14, 1972, I recorded nine direct communicators,

most of them seemingly recognized by the sitters. As I recall now, this might have been a rather typical séance. I remember in particular how relatively frequent were manifestations of direct communicators who had suffered a violent death.

Hafsteinn was also able to describe while in a waking state discarnate entities which he claimed to see around people. He would sometimes have public meetings, usually in a cinema or assembly hall, held in semi-darkness, at which up to a few hundred people were present. At these ''clairvoyant (non-trance) meetings'' he would describe 80 to over 100 deceased persons, usually describing one group at a time of people related or known to each other or from the same location, giving their names, relations to each other, appearance, and sometimes short messages from them, usually of a rather general kind, such as greetings saying that they were with the deceased and observing their life from the ''other side.'' Often Hafsteinn would invite the audience to ask questions, which he would apparently relay to the deceased entities and which might bring out further details about the deceased persons.

When Stevenson learned about the reports of Hafsteinn's mediumistic abilities, he decided that a serious investigation should be conducted. He thus visited Iceland in 1971 and 1972, and I then had the opportunity to investigate thoroughly with him two cases of ''drop-in'' communicators (Haraldsson & Stevenson, 1975a,b). For me it was a great learning experience to work with Stevenson, to plan the research we did together, including an experiment we designed and carried out (Haraldsson & Stevenson, 1974, described below), and to get acquainted with the thoroughness of his inquiries, his skillful art of interviewing witnesses, and his interest in improving methods for the study of mediums (Stevenson, 1968). This experience proved to be of great value for me later during my studies of Indian religious figures and psychics (Haraldsson, 1987) and in my investigations of children in Sri Lanka and Lebanon who were claiming past-life memories (Haraldsson, 1991a; Haraldsson & Abu-Izzeddin, 2004).

The Case of Runolfur Runolfsson

The more impressive of the two cases that Stevenson and I investigated together was that of Runolfur Runolfsson, often referred to as the Runki case (Runki was the nickname by which Runolfur came to be known). The case had some unusual features. The term ''drop-in'' derives from the fact that many such communicators just ''drop in,'' and then after registering, so to speak, they just as quickly ''drop out,'' giving little or no reason for appearing. For example, the communicator Abraham Florentine never gave any reason for appearing in England, after having died in Brooklyn several weeks earlier. Robert Passanah also gave no reason for his appearance, although Stevenson speculated that he appeared to reassure his grieving mother of his survival at a time when the removal of his tombstone in the cemetery had renewed her sorrow. Robert Passanah appeared at only one séance, Abraham Florentine at two, and Robert Marie appeared about four times. The case of Runki differs widely from these cases, as will become clear in the following paragraphs.

In 1937, quite early in Hafsteinn's mediumship, there manifested, sometimes forcefully, a disturbing communicator who used rude language and would not reveal his identity. When asked what he wanted, he replied, ''I am looking for my leg. I want to have my leg.'' This continued at many sittings until a new sitter, Ludvig Gudmundsson, unknown to the medium, joined the group. Ludvig was a fish merchant who lived in Reykjavik but who owned a fish-processing factory and a house in Sandgerdi, a village some 36 miles from Reykjavik. The disturbing communicator expressed pleasure in meeting Ludvig, and after a few sittings he said that his leg was in Ludwig's house in Sandgerdi. Somewhat later he finally revealed his identity, saying that Runolfur Runolfsson had been his name and that he had drowned in 1879 when walking drunk along the seashore to his home in bad weather. He had stopped to drink, had fallen asleep, and had been taken out by the tide. His body later washed up to the shore, ''where dogs and ravens came and tore me to pieces.'' Three months after his disappearance ''his bones were found dismembered,'' according to the record book of the clergyman of his parish. These bodily remains were buried in January 1880. Records from his parish church confirmed his identity and burial.

To make a long story short, in the end a long thigh bone (femur) was found between the inner and outer walls of Ludvig Gudmundsson's house in Sandgerdi. This happened in 1940, 60 years after Runolfur's death. Although it is known that he was a very tall man, it is impossible now to know for sure whose femur was found. The femur was given a burial in the churchyard of Runolfur's old parish. Runki expressed his gratitude at the next séance. He then became Hafsteinn's main control and was particularly liked by the sitters for his jovial nature and vivid, forceful personality. The question of cryptomnesia seemed remote. Runki's death had occurred almost 60 years before the relevant sittings, and his statements could not be verified by any *one source*, but rather by three different sources: the parish records, at that time kept in the National Archives; an obscure manuscript which was not published until long after the relevant sittings took place; and information obtained from Runki's grandson.

Runki's case illustrates another feature that Stevenson noted frequently in his reports of various kinds of cases. In five of the six individual cases of ''drop-in'' communicators that Stevenson published, the communicator had suffered a violent death through war, accident, murder, or drowning, whereas only one had died naturally. This observation immediately brings to mind the high frequency of violent deaths in cases of the reincarnation type (Stevenson, 2001: 165), among Hafsteinn's direct communicators (as I mentioned above), and in apparitions of the dead (Stevenson, 1982: 346–347; in a large representative sample in Iceland, the figure was 30%; see Haraldsson, 1991b).

Experiments with Hafsteinn Björnsson

In August 1972 Hafsteinn visited New York. That visit provided Ian and me with an opportunity to conduct an experiment in which we could control Hafsteinn's contact and interaction with sitters. The experiment was conducted

on the premises of the American Society for Psychical Research, and its purpose was to test the paranormality of Hafsteinn's clairvoyant (non-trance) public sittings (Haraldsson & Stevenson, 1974).

Ten Icelandic persons living in New York and unknown to Hafsteinn took part as sitters. For the experiment Hafsteinn and I sat at the end of an oblong room. Stevenson brought the sitters into the room at the opposite end, one at a time and in an order known only to Stevenson. A curtain across the room prevented both Hafsteinn and me from seeing the sitters or Stevenson. The sitters had earplugs in their ears, and they also wore earphones in which stereophonic music was played loudly while Hafsteinn made his reading for each of them. The sitters were thus visually and acoustically isolated from Hafsteinn, and their identities were unknown to Hafsteinn and me. In his readings, which usually lasted about 10 minutes, Hafsteinn would describe the deceased persons that he perceived with each sitter. The readings were tape-recorded. There was a short break between individual sitters.

The recordings were later transcribed by a person who had not been present for the readings and who was also ignorant of the identity of the participants. The order of the readings was randomized and each marked alphabetically so that it was impossible to know whether a particular reading was from the early or later part of the experiment.

A few days later each sitter was given a copy of all 10 readings and was asked to read them carefully, to identify which reading the sitter thought was intended for him/her, and to explain the choice by describing the correct details and the identities of persons he recognized in the reading. Sitters were also asked to rank the other nine readings. Often the transcripts were also later taken to a close relative of the sitter, usually his parents, since many of the sitters were rather young. These relatives sometimes gave important additional information and served as a check of the sitters' statements about their relations to the persons identified in the readings.

After all this was completed, Stevenson and I exchanged our lists of the choice of readings and the order of participation of the sitters in the experiment. The results were significant ($p < .01$): Four of the sitters had selected the reading which Hafsteinn had given for them, and two sitters gave the report of their own readings a second rank. This experiment established under controlled conditions the paranormality of Hafsteinn's mediumship, at least in this experiment. Further experiments with Hafsteinn that I conducted with other co-experimenters, however, were, with one exception, nonsignificant (Haraldsson, Pratt, & Kristjansson, 1978).

Studies of Xenoglossy

Stevenson defined xenoglossy as the ability to speak a foreign language not normally learned. He was greatly interested in such cases and published two monographs on this rare phenomena: *Xenoglossy: A Review and Report of a Case* (Stevenson, 1974) and *Unlearned Language: New Studies in Xenoglossy* (Stevenson, 1984). In his writings on xenoglossy and on cases of the rein-

carnation type, Stevenson often emphasized the basic distinction between information, or ''knowing that,'' and skill, or ''knowing how.'' The former could be communicated—normally or paranormally—whereas the latter could not. ''Knowing how'' had to be practiced and learned and could thus not be explained by telepathy or any form of psi. Moreover, ''if we can further exclude the possibility that the person concerned [learned] that language earlier in his life, it follows that it was learned by some other personality manifesting through him. That other personality could be a previous incarnation of the persons concerned or it could be a discarnate personality temporarily manifesting through a living subject—possessing the subject, we might say'' (Stevenson, 1984: 160–161).

Cases of xenoglossy, particularly of responsive xenoglossy, were hence of great potential importance as evidence pointing toward survival. One of the reasons for Stevenson's interest in Hafsteinn's mediumship was a case of xenoglossy that had been published by Larusdottir (1970). Professor Sven Fredriksen of Denmark, visiting in Iceland, attended a séance with Hafsteinn in 1966 and conversed for a short time in the Eskimo (Inuit) language with a communicator purporting to be an Eskimo shaman whom Fredriksen had known in his childhood and youth in Greenland, where he learned the local language. Because of its potential importance, Stevenson encouraged me to investigate this case further, provided financial support for this purpose, and offered suggestions for the investigation. One of the sitters had written a description of the incident the evening it occurred, and I was also able to interview two additional witnesses. One of them was a physician and old friend of mine; he had taken part in the sitting and listened to an exchange between Fredriksen and the communicator in a language unknown to him. The other was the person in whose home Professor Fredriksen had stayed while in Reykjavik. Both of them confirmed that Professor Fredriksen had said that he had conversed in the Eskimo language with the communicator and that he was deeply moved by it. Unfortunately, no tape recording or concurrent notes exist of this exchange, and Professor Fredriksen had died by the time my investigation took place. Although the case thus did not warrant a full report, Stevenson described it briefly in his first book on xenoglossy (1974: 8).

Stevenson and I also made attempts to elicit instances of xenoglossy. During my month of sittings with Hafsteinn in 1972, I brought to his séances some 15 persons, one at a time, who spoke rare languages. I observed a few brief instances of words spoken in German, Danish, and Norwegian, but none in these more distant languages. Hafsteinn had received only minimal schooling and knew no foreign language, but he is almost certain to have picked up some words in German, Danish, and Norwegian, all of which are related to Icelandic. These attempts to elicit instances of xenoglossy were unsuccessful, underlining the great rarity of the phenomena.

Stevenson's primary interest concerned the question of possible survival after death and the question of pre-existence. He searched for empirical evidence of phenomena which were generally exceedingly rare and hard to find. He pursued

his search with a commitment and a stamina that often left astounded those of us who had the privilege of working with him.

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Journal of Scientific Exploration, Vol. 22, No. 1, pp. 73–80, 2008 0892-3310/08

ESSAY

Where Science and Religion Intersect: The Work of Ian Stevenson

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For most people, the word ''fundamentalism'' mainly evokes images of Islamic terrorists or Christian mega-churches and revivalist tents; but in the battle for people's hearts and minds, scientific fundamentalists have become equally as vocal and strident—and perhaps even more arrogant—in promoting their beliefs and assumptions about the nature of reality. As a result, the polarization between the two sides seems to be steadily deepening.

The well-known Templeton Prize—officially called the ''Templeton Prize for Progress Toward Research or Discoveries about Spiritual Realities''—is described on its web site as an award meant to recognize and encourage those who, ''particularly through scientific research, serve to supplement the wonderful ancient scriptures and traditions of all the world's religions.'' The goal, apparently, is to try to break down the traditional antagonism between science and religion, and many Templeton Prize winners have been scientists who have written about the implications of modern science for a spiritual understanding of the universe.

For several years, beginning in 2000, we nominated Ian Stevenson for the Templeton Prize, believing that no one exemplifies better than he what we understood to be one of the Templeton Foundation's primary purposes—to bring the empirical methods of science to bear on fundamental spiritual questions. Regrettably, he was never given the award. Nevertheless, we remain convinced that no one in modern times has done more than he to lead the way toward finding a scientifically defensible yet humanly meaningful middle path between the fundamentalisms that threaten to engulf us. In what follows we amplify this statement using a slightly modified version of what we sent to the Templeton Foundation in support of Ian's nomination.

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Since the beginnings of the scientific revolution over three centuries ago, the relationship between religion and science has been complicated, tumultuous, and often acrimonious. Although scientific and religious views managed to co-exist harmoniously well into the 19th century (as the examples of scientists such as Newton and Darwin show), the famous confrontation between Professor Huxley and Bishop Wilberforce in 1860 exemplified the modern trend, with religious-minded persons increasingly on the defensive and scientifically oriented persons on the offensive. In the last 150 years this gap has further widened, as the physical sciences have made unprecedented advances while religion has been forced to retreat even further from the position of itself offering definitive knowledge about the nature of the universe we live in. Here at the dawn of the 21st century, we see an alarming bifurcation in modern society, one that began in the West but has spread with science and technology into much of the rest of the world. On the one hand, the world view that now prevails among intellectual leaders and permeates all levels of society is that of a mechanistic and materialistic universe in which mind, consciousness, human personality, and the spiritual values by which many people try to live are, in the final analysis, merely byproducts of physical and biological processes. On the other hand, an increasing number of people clearly feel that this materialistic world view cannot account for a wide variety of important human experiences, and that it fails to satisfy their hunger for a sense of meaning and dignity in human life. Over the past several decades, proliferating New Age fads, the explosion of interest in alternative and complementary medical treatments, and the rapid growth of fundamentalist religious movements have all attested to a deepening dissatisfaction with the currently dominant materialistic world view. It has become increasingly obvious to some people, therefore, that our most urgent need is for systematic efforts to bridge the gap between scientific and religious views of the nature of the universe and especially to reconcile modern society's respect for empirical science with the widely felt sense that our lives somehow transcend the boundaries of our current spatiotemporal existence.

We are nominating Ian Stevenson for the Templeton Prize because we believe that his whole career exemplifies such an attempt, one which has systematically brought the powerful epistemological tools of science to bear on large questions about the origins, nature, and destiny of human personality. Since the beginning of his professional career in the 1940s, there have been two primary themes running throughout his research and publications. First, his encyclopedic knowledge of the history of science, religion, and ideas has instilled in him an acute awareness of the dangerous tendency shared by most of us, including scientists, to adopt fixed theoretical systems and thus to resist examining new ideas. As he put it: ''We all tend to organize our experiences in various explanatory schemata which give us the impression (or illusion) of understanding the world around us. Any new idea impinges on the existing schemata and may by its simple strangeness arouse anxiety'' (Stevenson, 1965: 55–56). One important theme in his thinking, therefore, has been that progress in any area of human thought

requires us to resist complacency and to incessantly question or probe deeper into all received views, systems, dogma, or authority. An implied corollary of this theme has been that progress in both science and religion requires us to recover ''the wisdom that, as T. S. Eliot told us, we have lost in knowledge'' (Stevenson, 1990: 2).

The second theme, fundamental to Dr. Stevenson's own research efforts, has been to examine, empirically and in depth, the nature of human personality. In opposition to the reductive views prevailing in modern science, medicine, psychology, and psychiatry—that human personality and mind are simply byproducts of the brain and body—he has consistently struggled, beginning with his work as a physician in the area of psychosomatic medicine, to fathom this vital but difficult subject in all of its complexities. Beginning in the earliest years of his career, for example, he published numerous papers arguing—and demonstrating—that disease and healing involve the whole person and not just the breakdown or the treatment of separate parts (see, e.g., Stevenson, 1948, 1949; for later statements, see Stevenson, 1984a, 1985) and that individual differences and character derive from more than just genetic makeup and the influences of early childhood environment (e.g., Stevenson, 1977; see also Stevenson, 2000). In these respects, it has since become evident that he was far ahead of his time.

Because of the volume and quantity of this early work, he was already a distinguished scientist, professor, and head of the Department of Psychiatry at the University of Virginia when in 1967 he was appointed to a chair at the University endowed by Chester Carlson (inventor of the Xerox process) specifically to enable him to conduct full-time empirical research on the even more contentious subject of the possibility of human survival of bodily death. All the major religions view human personality as something that transcends the biological organism, and as William James emphasized in the *Varieties of Religious Experience* (1902/1958), this view is rooted not in the dogmas and systems of religion but in individual human experience. Nevertheless, as science and religion have diverged over the past 150 years, there has been remarkably little effort to apply the methods of science to questions and experiences that religion has long attempted to address, and ''knowledge'' and ''faith'' have increasingly been seen as entirely separate domains. Dr. Stevenson is in fact one of the extremely few individuals in the past century who have attempted to bridge this gap between scientific methods and knowledge and religious experience and faith by directly examining and strengthening the many and varied kinds of empirical evidence we actually have for the survival of human personality after death. By far the largest proportion of his subsequent scientific output revolves around this central topic, which obviously carries him far outside the reductionist mainstream of contemporary science. As a result, he has so far suffered undeserved neglect and even a certain amount of ridicule from people who ought to know better. But we assert categorically—and his publications abundantly demonstrate—that his work involves real science and deserves to be more widely known and studied.

The centerpiece of Dr. Stevenson's many scientific contributions to this area consists of a sustained, determined, and disciplined effort to locate and study "cases of the reincarnation type." The sheer volume of this entirely original work, most of which had to be carried out under very difficult and sometimes even physically dangerous conditions, is simply staggering. Even so staunch a critic of the paranormal as Carl Sagan expressed admiration for this body of work (Sagan, 1996: 302). Dr. Stevenson's *magnum opus* along these lines, *Reincarnation and Biology,* which he published in 1997 at the age of almost 80, is a two-volume, 2268-page work that should be of particular interest to biomedical scientists since it concentrates on cases in which the children display birthmarks or birth defects, often of extremely unusual form, that correspond to wounds or injuries that killed the person whose life the child claims to remember. Over 200 such cases, investigated by Dr. Stevenson, are reported in these volumes in meticulous detail (Stevenson, 1997a; see also its synopsis, Stevenson, 1997b).

Although Dr. Stevenson originated and is probably best known for the work just described, he has sought throughout his career to identify and pursue any and all kinds of empirical data that could shed new light on the survival question. Thus he has made unique and important new observations on topics as diverse as near-death experiences, deathbed visions, apparitional phenomena, telepathic impressions, poltergeist cases, and trance mediumship. He is also alone in having carried out and published intensive studies of so-called "xenoglossy" cases, cases in which the subject appears capable of fluent and productive use of a language that he or she did not learn normally. The best such case, published in *Unlearned Language* (Stevenson, 1984b), involved a secondary personality in a young Indian woman; this personality spoke and wrote fluently an archaic form of Bengali appropriate to the life she claimed to have led some 150 or so years earlier, and she also provided certain factual details which Dr. Stevenson was subsequently able to verify, but only by means of an extremely laborious investigation of obscure historical records.

Clearly this brief account can serve only to sketch the overall direction and character of Dr. Stevenson's scientific contributions. But what does it all mean and how does it relate to the Templeton Prize? At the very least this work adds important new information to an already large body of experimental and field studies which collectively establish beyond any reasonable doubt the existence of "paranormal" human capacities that in principle cannot be reconciled with any of the currently orthodox physicalist/reductionist theories of the mind-brain relation. A scientific world view in which these pernicious doctrines have been decisively overthrown is already much more friendly to spiritual matters, so that even on these narrow grounds Dr. Stevenson's work would merit the Templeton Foundation's recognition and support.

Even more importantly, in our opinion, he has significantly enriched and strengthened the already substantial empirical evidence *directly* supporting the possibility that some aspect of human personality does in fact survive bodily

death. This possibility, as we said earlier, lies near the core of virtually all of the world's great religious traditions, and its scientific investigation by all available means formed the central purpose of the extraordinarily distinguished group of people who founded the Society for Psychical Research (SPR) in England in 1882. We believe that future generations will come to see Ian Stevenson as one of the great scientific pioneers of our age, following in the direct line from that group and coming on down to the present through such eminent American counterparts as William James and Gardner Murphy. Dr. Stevenson's efforts can in fact be viewed appropriately as delivering one major installment on a research program explicitly proposed by James, initially in *The Varieties of Religious Experience* (1902/1958) and even more clearly a few years later, near the end of his own extraordinary life, in *A Pluralistic Universe* (1909/1971), his last completed work. As James himself put it: ''Let empiricism once become associated with religion, as hitherto, through some strange misunderstanding, it has become associated with irreligion, and I believe that a new era of religion as well as philosophy will be ready to begin'' (p. 270). Or, as James's colleague and friend F. W. H. Myers put it: ''Religion, in its most permanent sense, is the adjustment of our emotions to the structure of the universe; and what we now most need is to discover what that structure is'' (Myers, 1893/1961: 46).

Ian Stevenson's entire career exemplifies that spirit and testifies to its fruitfulness. His efforts seem especially important in this age when the traditional belief in survival after death—and more broadly the belief in human personality as something transcending the physical organism—seems to have been badly eroded by the advance of scientific materialism. Most educated persons today—including highly educated religious persons—erroneously believe that such traditional concepts have little or no empirical support, and, perhaps for this reason, many of the more liberal or educated religious leaders have downplayed or even discarded the concept of survival as central to a religious view of the universe, emphasizing instead the social and moral importance of a religious life and perspective. Without some concept that human life transcends this finite material existence, however, the foundations of religion as a spiritual and moral force are gravely and unnecessarily weakened. Scientific research such as Dr. Stevenson's, which specifically addresses deep questions about the nature and postmortem destiny of human personality, can and should have a powerful impact on the religious beliefs and spiritual well-being of many people, particularly if the research becomes more widely known. Although not lending itself to affiliation with any *particular* religion, his work is rooted in the firm conviction that there is no irreconcilable antipathy between science and religion and that open-minded empirical inquiry is leading us inexorably to an expanded conception of the nature of human personality, one which is compatible with many aspects of traditional religious beliefs. Similar views, including an explicit endorsement of the importance of survival research, were expressed by the distinguished British biologist and former Templeton Prize winner Sir Alister Hardy in his book *The Divine Flame* (1966).

Like others who have attempted to apply the methods of science to traditionally religious questions, Dr. Stevenson has encountered much misunderstanding, resistance, and even hostility, both from scientists and from religious persons who, from their very different perspectives, too often assume that all such questions have already been answered. Although a few other psychiatrists, psychologists, and anthropologists—most of them inspired by Dr. Stevenson—are attempting to carry on research on the question of survival in the same spirit of methodological rigor and intellectual honesty, efforts such as his to strengthen both religion and science by bringing them to bear on each other have a lamentably precarious existence in today's society. There is a circular relationship between the low levels of funding currently available for such research, the unwillingness of most mainstream journals to publish it, the widespread lack of understanding of its purposes and methods, and near-universal ignorance about the scope, depth, and quality of the data already available. Recognition of Dr. Stevenson's extraordinary accomplishments by the Templeton Foundation would help immensely in promoting more widespread awareness of the religious, scientific, and human implications of this work. Dr. Stevenson's humility notwithstanding, we hope that you will agree with the views expressed here and that you will find it appropriate and timely to honor him with the recognition he so richly deserves.

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With this statement, we hoped to inform the panel of judges for the Templeton Prize about the nature and importance not just of Ian's work, but, by extension, that of psychical research in general. But we fear that this attempt fell on deaf ears at the Templeton Foundation, just as attempts to educate the larger scientific community and general public about the goals and methods of psychical research have similarly so far failed. Those goals and methods were described succinctly over a century ago by Myers: ''The method which our race has found most effective in acquiring knowledge is . . . the method of modern Science. . . . This method has never yet been applied to the all-important problem of the existence, the powers, the destiny of the human soul'' (Myers, 1903, vol. 1: 1).

Ian, we repeat, exemplified to a degree unparalleled in modern times this thorough-going empiricism in relation to matters of religion. As the most visible organization today ostensibly promoting the reconciliation of science and religion, the Templeton Foundation ought to have honored him, because he, like his distinguished SPR predecessors, pursued psychical research out of a conviction that the gap between spiritual faith and scientific knowledge would only widen without empirical support for the fundamental idea that human personality somehow transcends the physical organism. Ian's was not the currently popular ''thin'' path of reconciliation between science and religion, which seeks common ground in recondite ''anthropic principles'' and the like, while scarcely mentioning fundamental elements of traditional belief, such as the existence within us of something like a soul, something capable of surviving

bodily death. This strategy may avoid or at least minimize overt conflict with present-day mainstream materialist science, but it does so at the cost of marginalizing and trivializing the world's great wisdom traditions. Ian's work, in stark contrast, goes straight at controversial subjects lying at the heart of all religions, and our human hearts as well, and in a manner that shows all the tenacity, discipline, rigor, and open-mindedness that is characteristic of the highest scientific achievements.

Ian himself did not say much about the spiritual implications of his work (one exception is Stevenson, 1969, especially pp. 27–33). He rarely wrote in this vein because he believed strongly that the primary role of the scientist is to provide evidence relevant to the question, while it remains the duty of each individual person to examine that evidence and draw his or her own conclusions. Ian frequently noted that when people asked him about his beliefs,

> I decline to answer this question because my beliefs should make no difference to anyone asking such a question. As Leonardo da Vinci said, ''Whoever in discussion adduces authority uses not intellect but rather memory.'' Everyone should examine the evidence and judge it for himself. (Stevenson, 1990: 21)

Furthermore, as a scientist in a society in which it is ''subversive'' for a scientist to talk about a soul that may survive death (Stevenson, 1990: 22),[1] Ian held firmly to the belief that his first and most important task was neither to preach nor to speculate, but rather to amass empirical data that can undermine the assumptions that currently make such talk subversive. The evidence that Ian collected, investigated, and presented as suggestive of survival after death speaks volumes for the religious and spiritual implications, without his having to spell these out explicitly. If his work is not a prime example of science working effectively in service of the truly fundamental interests of religion—and strengthening both science and religion in the process—then we don't know what else is or could be. Even though he has gone, and will now never be recognized with a Templeton Prize, we remain hopeful that the larger view of what he tried to do—and did—will become evident to future generations.

Note

[1] Nevertheless, Ian was never shy about engaging in subversive talk. Early in his career he cautioned that ''I do not believe science has heard the last of the mind or the soul'' (1954: 34).

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Journal of Scientific Exploration, Vol. 22, No. 1, pp. 83–84, 2008 0892-3310/08

Professor Ian Stevenson – Some Personal Reminiscences

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My first encounter with Ian Stevenson arose when I wrote a letter to *Journal of the Amercan Society for Psychical Research* in 1971 expressing disgust that in a lengthy review article on the subject of animal experiments in parapsychology – mostly involving electric shocks to mice – the ethics of inflicting pain on animals in the interests of intellectual curiosity was not questioned. This viewpoint was not well received by the people to whom it was directed, but Ian Stevenson wrote me a letter thanking me for raising the issue; he said that in reading about these experiments he had been excited by their implications (for precognition) to the point where he had failed to take account of the distress caused to the animals. Whatever view people may take of the issue, that the President of the Parapsychological Association (PA) should take the trouble to write such a letter to one of its most marginal adherents speaks volumes in terms of his integrity and other human qualities.

The following year the PA held its conference in Edinburgh, and I met Ian for the first time, finding him as likable as I had imagined him to be. He had a reserve that some people might have found chilling; it reminded me of what Sibelius said about his own great music, that others may offer wine, but he gives you a glass of pure water. Behind Ian's reserve there was charm, humour, and kindness.

We kept up an occasional correspondence over the years, consisting mostly of my asking and receiving advice, and on Ian's visits to London we would sometimes meet. In 1982, at the joint conference of the Society for Psychical Research with the PA in Cambridge, I gave a paper on the subject of Ossowiecki's retrocognitive clairvoyance, and I learned later that Ian had a strong interest in Ossowiecki. In 1994 he wrote about the desirability of having a translation made of Ossowiecki's autobiography, and this idea surfaced again in 1998. Out of this came *A World in a Grain of Sand* (Barrington et al., 2005), co-authored by Ian, Zofia Weaver, and myself. As a collaborator Ian was entirely responsive to any representations made to him about his own contribution, and he was meticulous in scrutinising ours. We both felt it was an honour to work with him.

Recently I discovered *Old Souls* (Shroder, 1999), a book by the journalist Tom Shroder, who in the late 1990s accompanied Ian on his reincarnation

research travels in Lebanon and India, writing his own account of these enterprises. Only after reading Shroder's descriptions of hours spent in extreme discomfort on dirt track roads, of arduous journeys undertaken in the hope of interviewing a witness or obtaining a document or examining a birthmark, of weeks spent surrounded by squalor, and sometimes by hostile crowds, and in dire conditions, all borne with stoic heroism despite his advanced age and imperfect health – only then did I realise the courageous and unstinting dedication that went into Ian's collection of reincarnation-type case records.

People are sometimes asked: Who, among the illustrious of this world, would you like to have met? My answer is Ian Stevenson, and I did have that privilege.

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Journal of Scientific Exploration, Vol. 22, No. 1, pp. 85–86, 2008 0892-3310/08

Ian Stevenson: A Recollection and Tribute

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I had the pleasure of debating the issues concerning survival with Ian Stevenson over many years, both in person and in print. And there were quite a few issues on which we didn't see eye to eye. But what made those disagreements possible, and what allowed them to be as focused and substantive as they were, was the indispensable and monumental body of work that Ian had already produced and continued to produce.

Our debates tended to center around the interpretation of the survival evidence. The vast majority of modern cases were investigated and discussed either by Ian or by those who adopted his protocols, his terminology, and many of his philosophical and methodological assumptions. As in any area of empirical inquiry, all those matters are open to scrutiny and possible revision or abandonment. Ian understood that, and in our discussions he always displayed a commendable willingness to reflect critically on his own approach (and of course on mine as well, about which he had plenty of thoughtful things to say).

Quite apart from our disagreements about how best to interpret the survival data, we were in complete accord over the importance to parapsychology of spontaneous cases. Indeed, Ian's clear-headed and sensible advocacy of non-experimental evidence impressed and influenced me greatly during my early years in parapsychology. In fact, I found his 1968 essay "The Substantiality of Spontaneous Cases" (Stevenson, 1971) to be especially helpful. Moreover, since Ian and I were both members of an academic establishment in which intellectual freedom is often trumpeted but seldom practiced, I understood first-hand the sorts of pressures and criticisms that Ian had been confronting for many years. And I have no doubt that he handled them, not simply tenaciously, but with more grace and dignity than I'd ever been able to muster.

I should add that my talks with Ian were not confined to the topic of survival, or even to parapsychology. I first met Ian when he and Jule Eisenbud came to hear me give a piano recital at the 1978 PA conference in St. Louis, and in our conversations thereafter we usually found time to discuss some mutual interests about music and the arts. On those matters, incidentally, our opinions were likely to converge. And I was usually glad we could end our discussions by setting

aside our differences over the survival evidence to share our similar assessments of, say, Schubert and Brahms.

With Ian's passing, parapsychology has lost one of its most important and inspiring figures. Fortunately, he has left behind a formidable legacy of theoretical and empirical studies whose riches, although already appreciated, are far from exhausted. In fact, just as the work of F. W. H. Myers (whom Ian admired greatly) is appreciated more now than during Myers's life, I expect that Ian's research will also grow in stature for many years. And I sincerely hope that it will eventually be recognized as essential reading not simply in parapsychology, but in an increasingly mature and well-rounded behavioral science.

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Journal of Scientific Exploration, Vol. 22, No. 1, pp. 87–92, 2008 0892-3310/08

Ian Stevenson and His Impact on Foreign Shores

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This will be a very personal contribution. Although I am a great admirer of Ian's work, I am not a professional parapsychologist, so other people can write with greater authority about his scientific contributions. However, Ian's achievements lay not only in the corpus of his written works but also in the influence he had on colleagues whom he exhorted to take an interest in the subject from other fields. So while his supreme scientific accomplishment was to pioneer and set the standards for an entirely new type of scientific methodology and to establish a school which now continues this line, he also made a vital contribution by his interactions with individuals beyond the shores of parapsychology itself. The effects of this may be harder to assess because they are indirect, but I believe they are also an important part of his legacy.

Another of Ian's characteristics was his strong connection with the U.K. He studied at St. Andrews in Scotland, he spent much time, including several sabbatical periods, at Darwin College in Cambridge, and he had close links with–and made an important contribution to–the Society for Psychical Research (SPR) in London. He therefore had many friends on these (literally) foreign shores. The title of this contribution may therefore be understood in two different ways. Several of his U.K. friends have contributed to this volume, and I am proud to be among them.

In order to explain why Ian had such an important effect on my life, I need to recount briefly my own involvement in psychical research. Although my interest in the subject goes back to my schooldays, it only became a passion when, as an undergraduate in 1968, I went up to Trinity College, Cambridge, where I read mathematics and joined the Cambridge University Society for Psychical Research (CUSPR). It was through the CUSPR that I met Tony Cornell, who became my first mentor in the subject, and as a result I spent far more time in this period reading about psychical research and doing experiments with the CUSPR than I did studying mathematics.

At the end of my undergraduate studies, I had to choose between doing a Ph.D. in physics or parapsychology. I think my primary interest was in parapsychology, but my dream was to produce a theory of physics which accommodated psi, and I realized that I could hardly attain this unless I first mastered physics. Also, there was little prospect of getting a job in parapsychology at the time, and I was wisely advised by Donald West (whom I also met through the

CUSPR) that I might benefit the field more in the long run if I first established myself in a more conventional discipline. In 1972 I therefore chose to do a Ph.D. in cosmology, and I was fortunate that my supervisor was Stephen Hawking, who was already producing exciting new ideas in physics.

After completing my Ph.D.–studying the first second of the Universe–I became a Research Fellow at Trinity College, and this allowed me to continue my activities in psychical research, albeit at a rather modest level. It was during this period that I first met Ian Stevenson. In 1981 he was spending a sabbatical year at Cambridge, and I recall being introduced to him by Donald West at a dinner in Darwin College. I knew all about Ian's work, of course, so it was a tremendous thrill to meet him, and I was looking forward to discussing several aspects of his research. To my surprise, however, he seemed much more interested in talking about my own experiments with the CUSPR. He was particularly interested in two of them, which I will now briefly describe.

The first experiment, carried out with Tony Cornell in 1969, involved an attempt to detect the telepathic transmission of emotion using hypnotized subjects and psychogalvanic skin response (Carr & Cornell, 1970). The agent was an excellent hypnotic subject (called Alison) who had been trained to experience intense 20-second bursts of emotion (happiness or hate) on a prearranged signal from the hypnotist (Tony himself). The rapport was clearly good since Tony and Alison later married! The periods of emotion during each 10-minute experiment were chosen randomly, and the idea was to examine whether the psychogalvanometer trace of the percipient showed unusual activity during Alison's arousal period; this experiment might thus be regarded as a forerunner of modern DMILS experiments. Ian was interested in this work because he was very aware of the important role of emotion in spontaneous psi–a point stressed, for example, in his book *Telepathic Impressions* (Stevenson, 1970).

The second experiment, conducted while I was a Fellow at Trinity in 1978, was an investigation of the relative roles of telepathy and clairvoyance in ESP. This involved using colour-blind agents and Ishihara card targets. The cards displayed the numbers 1 to 5, but two of them would be read as different numbers by the colour-blind agents, and so one could tell whether the percipient was picking up the information directly (via clairvoyance) or through the mind of the agent (via telepathy). The experiment had given interesting results, and these had been presented at the SPR conference in Cambridge in 1978 but had not been published.

Ian asked to see the reports of these experiments and evidently read them very meticulously, because some weeks later he returned them to me, with many pages of detailed comments. Even more significantly, he invited me to visit his group at the Division of Personality Studies in Charlottesville with the specific intention of preparing reports for publication. I was delighted to accept this invitation.

My month in Charlottesville in July 1982 was a wonderful experience. I remember staying in the laboratory at the back of the Division, and this became

a haven where I could focus on parapsychology undistracted. (There is also an astronomy group at Charlottesville, but I deliberately made no contact with them–partly because I wanted to focus on parapsychology, but also perhaps because I was not keen to advertise my unconventional interests to fellow astronomers!) Not only did I get a chance to browse through the extensive collection of books in the Division's library–including Ian's own works–but I also met his charming young colleagues Emily Kelly and Satwant Pasricha, and enjoyed many stimulating discussions with them. Another interesting visitor during my stay was Carlos Alvarado, who came for a job interview. All three became good friends and remain so to this day.

Ian was a wonderful host, and I can vividly recall trips with him to the Blue Ridge Mountains and the home of Thomas Jefferson. (My attempt to hide from astronomers was not entirely successful, because I recall a social event at which Ian introduced me to some of the ones from the University of Virginia who played a prominent role in the founding of the Society for Scientific Exploration.) I also managed to complete my reports. The Ishihara card paper was published in the *Journal of the Society for Psychical Research* the following year (Carr, 1983), in the same issue as Ian's Myers Memorial Lecture to the SPR (Stevenson, 1983). His modesty is typified by a remark he made in a letter to me around that time: ''Readers looking up your article as an original contribution may come across mine in the same issue.'' Such praise was unwarranted but very encouraging. Regrettably, the earlier paper by Tony Cornell and myself on ESP and emotion never appeared (the experiments were deemed unpublishable because they were already a decade old), but I did give a lecture on the topic to the SPR, and follow-up experiments were carried out by the CUSPR and reported at the SPR centenary conference in 1982 (CUSPR, 1983).

Over the following years I met Ian on many occasions during his visits to Darwin, when he was writing *Reincarnation and Biology* (Stevenson, 1997). He was a very hospitable man, and he often used to invite friends back to his flat in Cosin Court. He also had strong connections with the SPR–having joined the Society in 1961 and contributed to its *Journal* since 1964–and this gave me further opportunities to interact with him since I was on the SPR Council. He gave frequent lectures to the Society, and he also participated in some of the Study Days which I organized. (Recently a set of CDs with his 12 talks to the SPR has become available; these include four on near-death experiences, three on survival, three on the decline effect, and two on maternal impressions.)

In 1988 Ian was elected President of the SPR. He was not the first American to have achieved this distinction–most recently before Ian, J. B. Rhine had served as President in 1980 (followed by Louisa Rhine when he died in office)–but previous American Presidents had only been figure-heads. Ian, by contrast, was able to be a very active President since he was spending a sabbatical year in the U.K. The SPR was encountering financial problems at the time (it still is), and I vividly recall his launching an initiative to persuade members to donate money at the Annual General Meeting in 1989. He asked for

a show of hands from people willing to contribute £100 per year for the next seven years, and this direct (if rather un-British) approach proved remarkably effective!

His SPR Presidential Address focused on the decline of major paranormal phenomena in the West (Stevenson, 1990). He speculated that this might be partly because psi is inhibited by the scepticism born of the philosophical materialism so prevalent in industrially developed countries. After his Presidency, Ian became a Vice-President and continued to give the SPR much support. His standing within the Society was recognized when he became the first recipient of the prestigious Myers medal in 1995.

My other connection with Ian was through my role as Secretary to the Perrott-Warrick Fund, which is administered by Trinity College and was set up from bequests from Frank Perrott in 1937 and Frederick Warrick in 1956 for ''the investigation of mental or physical phenomena which seem prima facie to suggest the existence of supernormal powers of cognition or action in human beings in their present life, or the persistence of the human mind after bodily death.'' This, of course, precisely describes Ian's own research remit–indeed, as Emily Kelly (2007) points out, theirs is the only survival-focused, university-based research group in the world. The fund usually supports only U.K.-based research, but I am pleased to record that we did occasionally pay for summer students to codify Ian's collection of near-death experiences and reincarnation-type cases.

In 2000 I organized a Perrott-Warrick-hosted interdisciplinary conference at Trinity College entitled ''Rational Perspectives on the Paranormal,'' involving a select group of active parapsychologists, informed sceptics, and interested scientists from other fields. I was very keen for Ian to present his work on birth defects and biological markers in cases of the reincarnation type and was delighted when he accepted. With a sprinkling of Nobel Laureates, Fellows of the Royal Society, and Knights of the Realm, as well as coverage in *Physics World* (a prestigious science journal), the event certainly had a positive impact, and I believe he judged it a success. A full report of this meeting later appeared in the *JSE* (Carr, 2002).

This was the last time I met Ian in person, but my interactions with him continued through correspondence and e-mail. During my Presidency of the SPR in 2000–2004, I often received good advice from him, and he would occasionally comment on my Presidential Notes in the *Paranormal Review*. He also took some interest in my theoretical attempt to link psi and physics. This involves the notion that psi needs some sort of higher-dimensional space, which I relate to the higher-dimensional space currently invoked by theoretical physics (Carr, in press). I believe this sort of approach appealed to Ian because it complied with Myers's ideas of a metetherial space, and Ian felt that souls needed somewhere to reside between incarnations (Stevenson, 1974). Indeed he explored this theme in some depth in his Presidential Address to the Parapsychological Association in 1980 (Stevenson, 1981).

I have stressed the U.K. connection in this contribution, and so I would like to end by linking Ian's work to that of the three founding fathers of psychical research in Britain: Frederic Myers, Edmund Gurney, and Henry Sidgwick. At the SPR centenary conference, held in conjunction with the Parapsychological Association at Trinity in 1982, Ian organized a session on the historical roots of psychical research entitled ''Then and Now,'' and he was intimately aware of the links. However, it seems to me that no one in our age has better embodied the aspirations of these three colossuses than Ian himself.

In his perspective on human personality and the science-religion interface, he was undoubtedly the intellectual descendant of Myers (Kelly, 2007), for perhaps the unifying theme of all Ian's work was the inadequacy of the reductionist materialistic world view. This supposes that there is no more to the Universe than matter and that all aspects of human personality can be explained in terms of brain function. Reincarnation studies do not provide the only evidence for this–Ian also studied such diverse topics as near-death experiences, deathbed visions, apparitional phenomena, telepathic impressions, poltergeist cases, and trance mediumship–but they perhaps provide the *best* evidence. (Of course, this evidence was not available at the time of Myers, who seems to have been rather opposed to the notion of reincarnation.)

The link with Gurney is equally striking because Gurney was the SPR founder who most ardently stressed the central role of spontaneous cases and the importance of human testimony. Ian's meticulousness in assessing relevant evidence and his remarkable energy in traveling all over the world to interview witnesses at first hand was the very epitome of Gurney's approach. Throughout his career he strove to identify all kinds of evidence that could shed light on the survival question, but the emphasis was always on *empirical* data.

Ian emulated Sidgwick because he understood the importance of obtaining the support and respect of influential people from other areas of science. This is why he put so much effort into encouraging scientists in other fields (such as myself) to get interested and involved in the field. I believe this is one of the reasons he shifted his allegiance from the Parapsychological Association to the SSE. He felt that the former was too narrowly focused and that more effort to make contact with mainstream science was necessary if the field was to advance significantly. I agree with this exhortation, and indeed it was at his instigation that I joined the SSE myself. That is why I have strived to be an emissary for the subject in the domain of physics–because I believe that parapsychology will not have come of age until it has a theoretical basis which accords with physics.

For Ian Stevenson to have embodied so effectively the aspirations of the three founders of the field is no mean epitaph, and doubtless those he inspired by his example and scholarship will carry on the flame. He may sometimes have been frustrated at how difficult it was to attract the interest of people from other shores, but, in my case at least, he was successful, and I'm grateful for the passion he instilled in me for psychical research.

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Journal of Scientific Exploration, Vol. 22, No. 1, pp. 93–94, 2008 0892-3310/08

Ian Stevenson: Gentleman and Scholar

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As many will no doubt come to realize, the science of parapsychology has sustained a grievous loss in the death of Dr. Ian Stevenson. Wearing our ''administrative hats,'' both the Parapsychology Foundation's President, Eileen Coly, and I as the PF's Executive Director acknowledge on behalf of our organization the immense vacuum created at his passing. Since the PF's inception at the hands of Eileen J. Garrett in l951, we have been privileged to work closely with those striving to understand the complexities of our field. It is by no means hyperbole to state that in our experience over the years working with parapsychologists the world over, we have seldom been graced with the opportunity to work with someone of his academic acumen and overall patrician gentlemanly demeanor.

The PF remains justifiably proud of its early support for Dr. Stevenson's research in l961 when at the request of Garrett he took a trip to India and Sri Lanka to investigate reincarnation memories in children, a trip which spawned a lifetime of research. Dr. Stevenson graciously acknowledged this support in the ''Remembrances'' section of the Helix reprint of Garrett's autobiography, *Adventures in the Supernormal*. Referring to Garrett, he stated: ''Later I obtained much other funding; but I remain deeply indebted to Eileen for encouraging me and for making possible my first endeavors to study the children who claim to remember previous lives. Without her I could not have even started.'' The PF remains most gratified that in this manner we contributed to Dr. Stevenson's research efforts, which culminated in such a prodigious amount of valuable work.

Over time we continued to have a much-valued association with him, which included his publishing articles in PF publications and participating in our International Conference series with continued grant support. But now, switching our ''hats'' to our personal recollections, we remember with gratitude his warm good counsel and support as we sought to administer and guide the Foundation through the turbulence caused by the death of Garrett in l970, and also the kindness he showed us as we personally came to grips with the loss of our mother and grandmother.

Perhaps at first blush thought to be difficult to approach, Dr. Stevenson was in truth an extremely kind man and consummate gentleman. He will always hold a revered spot in the hearts of many, myself included, for I am sure there were

many acts of kindness – such as what I experienced that I hold most dear – that will stand testament to the man. He had just published his first book on the reincarnation research, *Twenty Cases Suggestive of Reincarnation*, and had come to visit Garrett at our offices, then at 29 W. 57th Street in Manhattan, with his book proudly in hand and with gratitude for the Foundation's initial support of his studies. I was a young girl of 16 who had started her apprenticeship by doing odd jobs at the Foundation, drawn within the orbit of Eileen Garrett, who, as many will attest, was hard to resist. After a brief meeting with her in her tastefully appointed office, and fully expecting a private *tete-a-tete* luncheon allowing for the opportunity to discuss at length various research projects and future directions for more advanced study, he was ushered out of her ''inner sanctum'' and introduced to me. Garrett, with her arms waving expansively, stated: ''Now, Ian, my granddaughter Lisette will be happy to take you to lunch!'' thus rendering him unceremoniously saddled with a somewhat clueless young teen. Now others in that same situation might well have declined the invitation for understandable reasons; but not so Dr. Stevenson. He gallantly and with good humor shared an elegant formal luncheon at Mrs. Garrett's favorite ''of-the-moment'' French restaurant, Le Baroque, all the while patiently introducing me to the concept of reincarnation and the ramifications for its continued study for humankind. How I would appreciate having the opportunity for that private tutorial to be replayed so that I could experience it as an adult! Not only did he gamely escort me to the restaurant and back to the office, but I was so impressed to receive by mail a few days later a copy of his book inscribed to me personally – a habit he continued, sending me all his subsequent titles and usually with an inscription alluding to our first meeting. He watched me grow up literally and figuratively within the Foundation and was a quiet and ever available resource that both my mother and I were free to draw on for support. He inspired many students – myself included – to investigate more closely the mysteries raised by psychic functioning.

Parapsychology and the PF family are greatly diminished by his loss.

Journal of Scientific Exploration, Vol. 22, No. 1, pp. 95–99, 2008 0892-3310/08

The Quest for Acceptance

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Ian Stevenson's work came to my attention in the early 1970s. During my years as a graduate student at Berkeley in the 1960s, I had developed a strong interest in the approach to meditation through yoga, but had not fully come to grips with the issue of reincarnation so central to Hindu philosophy. I was unsure about the subject and maintained a natural degree of skepticism (which extended to some of the other more mystical aspects of yoga), but took a conscious attitude of wait-and-see, a form of suspending disbelief. Berkeley in that period was a center of interest in Eastern religion, partly under the influence of psychedelic agents that were popular at the time, and the *Tibetan Book of the Dead* was featured reading. For such an unfathomable subject, I was intrigued by Ian's scientific approach and arranged to meet him on a trip to Charlottesville. By then I was on the faculty in the Section of Biochemistry of Cornell University in Ithaca, New York.

My first impression of Ian was how dignified and serious he was for someone working on such an unorthodox subject: well-coiffed, meticulously dressed in suit and tie—elegant from head to toe (of his wing-tipped shoes). We exchanged occasional letters over the following years and met once in Ithaca when he was traveling in the region. We began a period of serious collaboration when Ian suggested that I look at material related to sickle cell anemia with respect to reincarnation in equatorial Africa. For many years I had been actively investigating the molecular basis of this genetic disease and with my group at Cornell had recently established the structure of the complex 14-stranded helical cables of the mutant form of hemoglobin responsible for distorting red blood cells into their characteristic sickle shape. I was fascinated by the idea of pursuing how this molecular disease could elicit cultural responses in traditional African societies and followed up on Ian's suggestion to learn more about "repeater children." My reading kindled serious interest, and in the 1980s we made two trips together to Nigeria, with additional stops on the way in Senegal and the Côte d'Ivoire on the second trip.[1] I made a subsequent trip to Africa alone that included a stop in Senegal, where I investigated the case of Tadé Sarr (for a report of this case, see Stevenson, 1997a: vol. 2, pp. 1644–1645).

Our investigations revealed that West African societies had integrated

reincarnation as an active element in their lives, even to a greater degree than some societies traditionally identified with a strong belief in reincarnation such as, for example, the Hindus. Among the Igbos of southeastern Nigeria, parents generally consulted an oracle shortly after the birth of each child to identify which deceased ancestor had reincarnated. Even more interesting was the practice of marking the cadaver of deceased children in order to ascertain if the next child born to the same parents carried the mark, which would be taken as evidence for the reincarnation of a ''repeater child.'' It was surprising to discover that traditional societies were conducting virtual ''experiments'' in reincarnation across a wide swath of West Africa. The idea that this practice might provide unique opportunities for reincarnation research heightened our interest. At the time of our travels, Ian's research was focused to a large extent on birthmarks and birth defects related to purported instances of reincarnation, and our investigations in Africa provided a number of interesting new cases, which he summarized principally in *Reincarnation and Biology* (Stevenson, 1997a), particularly in Volume 2. These major tomes follow upon his many earlier published works covering cases from various parts of the world, most of them based mainly on verbal testimony, but some of which also included birthmarks and birth defects.

Whether or not the cases could be used to substantiate the existence of reincarnation, I found it intriguing from an anthropological point of view that such practices could be so widespread. By adopting this anthropological perspective, I was able to present the essence of the cases of repeater children in West Africa (along with other features of sickle cell anemia, including its molecular basis) without crossing the line into parapsychology, and my book *The Sickled Cell: From Myths to Molecules* was published by Harvard University Press in 1986. We observed the consequences of marking children by amputating the last bone of the left little finger among the Igbos, as well as several other distinctive forms of marking which were observed by us or reported by other witnesses. For example, my own investigations among the Serer ethnic group in Senegal along the coast south of Dakar documented the practice of cutting a notch in an ear (see the case of Tadé Sarr in Stevenson, 1997a: vol. 2, pp. 1644–1645).

These and other birth defects that we studied are far more specific and atypical than the forms that have been reported in the medical literature and are difficult to explain by any conventional biological arguments. In some extreme cases discussed in Chapter 20 of *Reincarnation and Biology*, more extensive birth defects were observed on children that corresponded to amputations allegedly carried out on the cadaver of a deceased child. The unforgettable encounter with Cordelia Ekouroume, who lacked portions of many fingers and toes, is documented there (1997a: vol. 2, pp. 1634–1640). These cases bear some resemblance to others from different parts of the world described in Chapter 17, but the African cases have a systematic quality that challenges explanations based on conventional biological mechanisms.

As I look back on these cases and the many others that are thoroughly documented in the two volumes of *Reincarnation and Biology*, I am again struck by the challenges they present for mainstream developmental biology. I also recall the great hope that Ian placed in the publication of these volumes (which he considered to be his masterwork) to attract the attention of establishment scientists. I remember only too well the disappointment that he expressed in our final meetings in Paris when the volumes had been largely ignored. I had moved to Geneva in 1986, but we continued to meet regularly when Ian was in Paris, where he came to conduct bibliographic research at the *Bibliothèque national*.

I often reflected on why his findings remained so far outside the pale of establishment science, generating the sense of frustration that Ian increasingly experienced over the years as he realized that scientific recognition of his work was not forthcoming. Ian tended to blame the scientific community for faint-heartedness, but in the many discussions during our travels and other meetings, I tried to use my knowledge of the scientific community in which I lived and my sympathy and interest for his research to formulate the reasons for the enormous gap—a veritable Grand Canyon—that separated his research and establishment science.

My view was that orthodox science had no way of dealing with his findings, because they could not be connected with the large body of scientific knowledge. Without a new cosmology or theoretical biology that could accommodate the concept of reincarnation in some form, no field of scientific deliberation could seriously enter into studies of the subject. I strongly felt that if reincarnation were to emerge as a natural phenomenon finding its place in our understanding of the Universe, it would not be in opposition to traditional science, but as a complementary feature, an additional perspective on the nature of being. It seemed to me that reincarnation would have an impact on scientific thinking only if it could be integrated into existing biological concepts, with, for example, reincarnating birth defects viewed as an extreme manifestation of psychosomatic medicine.

Ian did express similar views in *Reincarnation and Biology* (1997a) and its synopsis *Where Reincarnation and Biology Intersect* (1997b). After drawing attention, in Chapter 26 in particular, to the inadequacy of genetics, Darwinian natural selection, and environmental influences to explain all aspects of human personality, he went on to emphasize that ‘‘I do not propose reincarnation as replacing these factors. I regard it as a third factor that may fill some of the gaps in the knowledge we presently have about human personality and ... about the human body also’’ (1997b: 180–181). In addition, in Chapters 2 and 3 he described various other psychosomatic phenomena that seem related to birthmarks and birth defects in cases of the reincarnation type. Nevertheless, he made little progress toward providing a theory that can link these cases and current scientific knowledge. In particular, his attempt to present a unifying concept in this book by introducing the ‘‘psychophore’’ as the vehicle for reincarnation was of only of marginal value in providing a bridge to conventional science.

I have no illusions that more emphasis on an integrative approach would necessarily have led to better acceptance, particularly since I have only some tentative hypotheses on how such integration might be achieved. Reincarnation from any perspective is a difficult concept, and even the Buddha himself left us utterances on the subject at various times in his life that were not always consistent. Attempting such a global synthesis would inevitably carry problems of its own, as can be seen in the wake of Rupert Sheldrake's *A New Science of Life* (1981). This work advanced the hypothesis that the specific size and shape of living organisms are determined by ''morphogenic fields'' that are molded by the form and behavior of past organisms of the same species through direct connection (''morphic resonance'') across both space and time. Ian sent me this book when it was published, as he viewed it as indirect support for his work, but its general impact on the scientific community was totally negative. The book did obtain a review in *Nature*, but it was roundly criticized under the title ''A book for burning?'' Sheldrake's position was to replace mechanisms of biology, rather than seeking concepts that could extend the principles of biology in new directions. Body plan development has benefitted from spectacular progress in recent years due to the understanding of homeobox genes, and no serious approach to morphogenesis can ignore these findings. In particular, with respect to birth defects involving fingers or toes, it would be important to compare the anomalies reported for transgenic mice carrying altered *HoxD* genes (e.g., Kmita et al., 2002).

Where reincarnation is concerned, the subject is also confronted with additional barriers related to the conflicts it generates with established religions and the validation it provides to quack ''past-life'' readers. The history of a subject without these handicaps, the alleged ''memory of water'' in an article published in *Nature* (in 1988) from the laboratory of Jacques Benveniste in France, demonstrated clearly that an exceptional hypothesis must bring exceptionally strong data in order to be supported. Since the data were not credible, the hypothesis was ridiculed.

It may well be said that Ian was ahead of his time, but the question now is ''will his time ever come?'' Traditional societies that are propitious for reincarnation research are modernizing rapidly in many parts of the world. The repeater-children phenomenon has continued in some forms among the Igbo, according to a report by Nzewi (2001), but for how long?

In conclusion, some 25 years ago I thought Ian's work had the potential of triggering a major change in the scientific landscape. Such an upheaval has not happened, but perhaps his work will be rediscovered and placed in a new light at a later date. In any case, his lifetime of serious study of reincarnation demonstrates that intelligence, insight, meticulous work, and persistence are not sufficient to bring so hostile a subject in from the cold. It is not enough to demonstrate that reincarnation can exist: A new theoretical framework is also needed to show that reincarnation is possible.

Note

[1] For information on the belief in reincarnation in West Africa and the associated cases that Stevenson and I investigated, see Edelstein and Stevenson (1983) and Stevenson (1985, 1986, 1997a: chap. 20).

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Journal of Scientific Exploration, Vol. 22, No. 1, pp. 102–103, 2008 0892-3310/08

Remembering My Teacher

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When Ian passed away in February 2007, traditional-format obituaries honoring him, and his unique contribution to scientific understanding, were published in several major newspapers and elsewhere. A Google search will readily produce the most salient, historical specifics of Ian's life and career from those several pieces. The fact that his passing was so prestigiously noted speaks volumes about the recognition his research has attracted from so many widespread audiences. Any dedicated reader of this journal needs no introduction to Ian or the focus of his work. What follows has been written almost entirely from a very different perspective than the above-noted obituaries; rather it is my best effort at a fitting eulogy for the most important teacher I've ever had.

I have made only one pilgrimage in my life. The year was 1991 when, in the middle of a very busy engineering project, I stole the last two days of a work-week to travel to and attend what was left of that year's SSE meeting (my first) in Charlottesville, Virginia. My sole focus was to hear a scheduled address there by Ian. Over the previous several years, I had become a devoted fan of his research, and at long last had the opportunity personally to see and hear the man behind it.

It is very difficult to describe clearly and effectively the life-changing impact that the implications of his research have had on me. In the simplest terms, he gave me my first true ''handle'' on life, a tangible grasp on the intangible. ''Handles'' of that quality are not trivial things to engineers like me. It is little wonder that his work is often referred to as the ''gold standard'' of reincarnation research. Even in what is, for most people, the esoteric landscape of philosophy, here was something to really hold onto.

I am acutely aware that each of us must find his own path through the dogmas of organized religion, the abstractions of formal philosophy, and the wonderland of current physics and cosmology, but here, in the midst of it all, was a handle, the rational appeal of the concept of reincarnation, backed up by a repeating phenomenon ''from the mouths of babes.'' But it is a handle available only to those willing to listen, and willing to actively reinforce what they learn from Ian with the supporting perceptions of other researchers.

Initially, Ian was not easy to get close to. By the time I had belatedly discovered him, he was already attracting more attention, both positive and negative, than he could personally handle with his unusually busy schedule.

Still, he responded politely by mail, but with regrets that the one-on-one meeting which I had proposed would not be possible. I still smile when I am reminded of his statement therein: that if he accepted all such invitations, there would be no time left for the research that attracted his admirers in the first place. However, I am nothing if not persistent, and eventually I received his invitation to the SSE meeting to hear his address.

Nor, as our relationship grew and developed, did we agree on everything, especially the potential benefits of hypnotic past-life regression (and the stature of one of its leading proponents); but I believe that our related disagreement was primarily a matter of individual perception. Is hypnotic past-life regression a reasonably reliable research tool? Per Ian's perception: Certainly not! – and I agree (generally). However, as a reasonably effective therapeutic tool, and from my engineer's perspective, ''If it works (and it really seems to, much of the time), use it!'' We didn't have to fully understand fire in order to use it to great effect – although, as my Dad taught me, ''Fire is a good servant, but a cruel master.''

Ian, rest in peace, old friend and revered teacher. You were granted the time to make your message abundantly clear. It is up to others to listen and understand. It was an honor to know you, and I look forward to our next meeting.

Journal of Scientific Exploration, Vol. 22, No. 1, pp. 104–106, 2008 0892-3310/08

Comments on Ian Stevenson, M.D., Director of the Division of Personality Studies and Pioneer of Reincarnation Research

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It is an honor to have been invited to write some comments on the life and work of the late Ian Stevenson, just as it has been a honor to have become familiar with the man and his immense, careful, and well-written work on the important issues of survival of bodily death in general (e.g., Stevenson, 1972, 1977b, 1982) and reincarnation in particular (e.g., Stevenson, 1966, 1974, 1975, 1997, 2000b, 2001). These are not topics that are at the forefront of Western thought and education, yet Ian Stevenson spent what he describes as ''half a career in the paranormal'' (Stevenson, 2006) meticulously investigating, and writing wonderfully clear, well-reasoned, and articulate studies of these phenomena. To me it seems that this work represents more than half a career – it represents a monumental achievement of bringing these topics into the arena of scientific investigation and placing them within the context of understanding the mechanisms that underlie the psychology of human beings. To Ian these studies seemed a logical progression from his research as a psychiatrist with a particular interest in psychosomatic illness. I find it fascinating that in his last article (Stevenson, 2006) he links his own chronic bronchial maladies to the study of psychosomatic illness, and then connects these personal concerns and professional issues with his *magnum opus* on birthmarks and birth defects and their relation to trauma or cause of death in a previous life (Stevenson, 1997). How fortunate that his sickly youth was spent reading the works of theosophists that his mother made available to him; one is left wondering what malady he may have carried from a previous life and whether his long life despite his bronchial condition will render him less impacted by such health concerns in a subsequent life.

However that may play itself out, I am very grateful that I had the good fortune of meeting Ian Stevenson and becoming aware of his work and participating in it. Our meeting took place in Vancouver in about 1984 when he was returning from a study of cases of the reincarnation type among the Gitksan Indians of northwest British Columbia in Canada. While in Vancouver, Ian was hoping to find some researchers who would be interested in carrying on his investigation into reincarnation in BC, and he enquired at the Department of Anthropology of the University of British Columbia about whether there were any faculty members or graduate students so inclined. He was given two names,

one of which was mine. We met, and I was thereby introduced first to this kindly gentleman and then to his impressive research, which I had previously not known about at all. In my Ph.D. thesis I had noted the importance of reincarnation in the Beaver Indian world view, and to learn whether reincarnation was part of the experience of other native groups, I examined the 10 different culture areas in North America, choosing from each the group that had the best documentation about their spiritual views. I was curious because rebirth was one of the essential elements of Beaver Indian/Dune-za philosophy and experience, and yet my graduate studies in Anthropology had not prepared me to expect it as a part of Beaver Indian understanding of personality. Learning of Dr. Stevenson's careful case-by-case research, I was delighted to add such an investigation to the research project that I was planning to carry out during the summer of 1984 with the Beaver Indians, and to accept Dr. Stevenson's proposal that I undertake similar research with the Gitksan First Nation peoples, with whom I had not previously worked.

I was impressed by how much the Beaver Indians had to tell me when I pursued the topic with them *à la* Stevenson, and even more impressed that a week's work with the Gitksan produced information about 33 fascinating cases of reincarnation replete with the characteristics that Stevenson had found in cases throughout the world: birthmarks relating to wounds or markings; recognitions and statements from small children made from the point of view of the previous person; similarities of temperament, skills, and talents; and philias and phobias explicable on the basis of the previous life but not the current one. It was a real pleasure to write up this research (Mills, 1988), following the admirable example of Ian's measured evaluation of the features of cases. You can imagine my delight when, after that, Ian asked me if I would be interested in undertaking a replication study of his research in India. The answer was a definite "Yes." At about the same time Dr. Erlendur Haraldsson undertook a similar study in Sri Lanka, and Dr. Jürgen Keil conducted one in Turkey. We wrote our respective articles, and then I was delighted when Ian offered me a research assistant position at the Division of Personality Studies (DOPS), combined with a lectureship in the Anthropology Department at the University of Virginia.

Moving to Charlottesville and into an office at the Division of which Ian was the Director, I was ever more impressed by his complete integrity, his perseverance and dedication to documenting and presenting the data, and his thoughtful gentleness. The files contained the data from the cases, but so did Ian's mind. He meticulously presented data on so many aspects of the cases; for example, his assessment of gender dysphoria (e.g., Stevenson, 1977a: 317–318, 1977c, 2000a: 654–655) introduced a fascinating explication of gender orientation at odds with physiology, a topic explored more recently by a number of indigenous and non-indigenous North American scholars. It was a real pleasure to attend the weekly Tuesday luncheon meetings in which Ian or another scholar would present their current research, with Ian always acting as the genteel and insightful host. When Ian eventually retired from his position as Director of DOPS, the University of Virginia Medical School newspaper noted that it took at least three people to replace him: Dr. Bruce Greyson, as the foremost

researcher of near-death experiences; Dr. Emily Williams Kelly, in a variety of topics related to the question or survival after death, as well as a fellow appreciator of F. W. H. Myers; and Dr. Jim Tucker, a child psychiatrist who continues the investigation of children said to remember previous lives. Indeed, in large part because of Ian Stevenson the Division has attracted a larger cohort of researchers, including Dr. Carlos Alvarado, Dr. Nancy Zingrone, Dr. Edward F. Kelly, Dr. Michael Grosso, and Dr. Ross Dunseath, and it has thus become one of the important focal points for parapsychological research and intellectual interchange.

The legacy of Ian Stevenson to the understanding of the dynamics of psychology is huge and not yet fully realized. What he has afforded the intellectual, academic, and professional psychiatric world will slowly and inevitably unfold and unfurl and reach out beyond academe to influence the understanding of personality by the larger culture and public. That is already happening not only through the book of Tom Schroder (1999) but also through the networks and web sites of lay people like Carol Bowman, who was influenced by the work of Ian Stevenson in her personal understanding of the experiences of her own children. I am confident that his work will permeate more deeply into the views of Western lay people as well as psychiatric and psychological practice. I hope that his vision of the role of reincarnation in understanding personality will inspire more research carried out with the care that he exercised in his investigations, and that the legacy of his work will lead to new understandings.

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Journal of Scientific Exploration, Vol. 22, No. 1, pp. 107–109, 2008 0892-3310/08

Ian Stevenson: Reminiscences and Observations

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If the reader will forgive a tired cliché, Ian Stevenson and I go way back. In 1971 Ian gave me my first job in parapsychology, a Research Associate position at what was then called the Division of Parapsychology at the University of Virginia Medical Center. In addition to Ian, my colleagues were Rex Stanford, later to become a major player in parapsychology, and J. G. Pratt, who was already a distinguished parapsychologist based on his pioneering ESP research with J. B. Rhine. Although the mission of the Division was research on the question of survival of death, neither Stanford, Pratt, nor I were primarily engaged in survival research during our years there. Although Ian later tightened the reins, he deserves great credit for allowing a broader research agenda in order to support parapsychologists who otherwise had little or no opportunity to continue in the field.

The focus of my research at the Division was out-of-body experiences (OBEs). Although this topic can be related to survival, my experiments were not designed to assess the externalization hypothesis but rather to explore the psychological correlates of the experience and its relation to ESP. In fact, while I was there I published a paper based on the premise that nothing leaves the body during an OBE (Palmer, 1978), although my theory did not preclude externalization outright.

One of the survival-related tasks to which I was assigned was screening mediums for possible extended research at the Division. I was to go to various places and pose as an ordinary visitor, not revealing that I was a researcher. One of my targets was a Spiritualist camp in Pennsylvania. Unfortunately, I was by far the youngest visitor there and I suspect that aroused suspicion; at any rate, they were very guarded with me and I got no useful information. I was also sent to Chicago and London. The Chicago trip yielded one medium who impressed me (Deon Frye), and we subsequently invited her to Virginia for a taped session. We did not find good evidence for ESP, but we noticed a marked change in her face when she entered trance. I was also a co-author with Ian of a paper describing a rating scale to be used for testing the ''authenticity'' of spontaneous case reports (Stevenson et al., 1977). Authenticity is ''the degree to which the information recorded about a case corresponds to the case as it actually happened'' (Stevenson et al., 1977: 274).

After I left the Division, most of my contacts with Ian concerned the American Society for Psychical Research. Both of us were members of their Board and also past presidents. During the 1990s, we participated in an unsuccessful attempt to change the leadership and direction of the organization. The latter stages of this effort involved a lawsuit, which Ian largely paid for out of his own pocket.

Ian projected a calm, stately, gentlemanly demeanor that one is tempted to compare to the stereotype of the British upper class during the late 19th and early 20th centuries. This of course was the heyday of the British Society for Psychical Research (SPR) and of prominent psychical researchers such as F. W. H. Myers, Edmund Gurney, and Eleanor Sidgwick. Ian saw himself as very much in the mold of these early pioneers, especially in the importance they placed on the survival question and their appeal to well-documented spontaneous cases as evidence for survival. Thus, this perception was also the reality; during his professional career, Ian was the paramount personification of the classical SPR tradition, more so than the SPR itself at this time.

Although Ian delved into many areas of spontaneous case research (Alvarado et al., 2007), his primary research interest was reincarnation or, as he more objectively defined it, research on children who remember previous lives. He was careful to qualify his findings as only ''suggestive'' of reincarnation (e.g., Stevenson, 1974), although he would often make strong arguments in favor of the reincarnation interpretation of individual cases. His methods were in the best tradition of the SPR, featuring extensive documentation of the validity of the child's statements and an exhaustive analysis of alternate interpretations. During his career he exhibited the patience to collect over 2500 cases (Stevenson, 2001), which have provided fertile ground for correlational research to uncover patterns in his sample. Ian did some analyses of this type himself (e.g., Stevenson, 2001), and this research hopefully will be carried on by his successors.

In my opinion, Ian's most important intellectual contribution to survival research was the emphasis he placed on evidence for survival other than statements of fact (e.g., verbal memories in reincarnation cases). Because such classic examples of ESP are frequently demonstrated in non-survival contexts, the so-called super-psi hypothesis (ESP from sources other than the deceased) provides a very parsimonious alternate explanation. However, in his reincarnation cases Ian reported other kinds of evidence that have much fewer analogues in non-survival contexts. The most noteworthy of these are the demonstrations of precocious skills (e.g., playing musical instruments) and the presence of birthmarks (Stevenson, 1997). Although this type of evidence does not conclusively prove survival, it does increase the likelihood that we do survive death, and that is the most we can expect scientific research on the topic to achieve. Still, that is no mean achievement and a legacy of which any investigator or scholar could be proud.

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Journal of Scientific Exploration, Vol. 22, No. 1, pp. 110–114, 2008 0892-3310/08

Dr. Ian Stevenson: A Multifaceted Personality

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My association with Dr. Ian Stevenson goes back to October 1973, and I have known him ever since, as a mentor, a guide, a friend, a scientist, and above all a good human being.

My First Meeting with Dr. Stevenson

During the silver-jubilee conference of the Indian Psychiatric Society that was held in Chandigarh in December 1972, a senior Indian psychiatrist with whom Dr. Stevenson had corresponded told me about Dr. Stevenson's work on reincarnation. Because of my background in science, I was quite skeptical about such cases, about which I had read only in the newspapers. Subsequently, however, during another meeting in 1973, the same professor informed me that Dr. Stevenson was coming to India and that I could meet him if I wished.

I was employed in Chandigarh (about 250 kilometers north of Delhi) and came to Delhi to meet Dr. Stevenson for the first time in October 1973. He was staying at Hotel Janpath, where he often stayed when he came to Delhi. Dr. Stevenson was not in his room, and the late Dr. Jamuna Prasad (a close associate of Dr. Stevenson) was waiting for him in the lounge. I thus met Dr. Prasad for the first time, and since I had never met Dr. Stevenson before, Dr. Prasad offered to introduce me to him. Finally we all met, had lunch, and immediately went to see a few cases of the reincarnation type known to Dr. Stevenson. These were the cases of Gopal Gupta (Stevenson, 1975: 70–106) and Pushpa (Pasricha & Stevenson, 1977). He took me to these cases to introduce me to ''actual'' cases, and he allowed me to interview the family of the previous personality for the case of Pushpa and the mother of Gopal Gupta. Since this was the first time I was meeting and interviewing informants for cases, I first acted as an interpreter for Dr. Stevenson and then asked my own questions, for which he gave me a free hand. That was a good learning experience for me in understanding his research. Since I had not participated in the investigation of these two cases from the beginning, I could not yet form a judgment about the cases, but I was definitely impressed by his approach to investigating them.

After the initial two-day meeting with Dr. Stevenson, and before I returned to Chandigarh, he suggested that I study a few cases with one of his associates, but independently of him, which I did in December 1973. I went with Dr. L. P. Mehrotra, an associate of Dr. Prasad. During this trip, we faced many

difficulties, mainly because we had fewer hours for work and long, sometimes even arduous, journeys. We had to walk for several kilometers across fields to the interior of villages and had frequent taxi breakdowns, all of which resulted in frustration as we got little or no information by the end of the day. (The road conditions have tremendously improved since then.) In spite of the hardships, however, I began to like the work after I had investigated a few cases, particularly that of Manju Sharma (Pasricha, 1990/2006), who was about four years old at that time. I found quite a few strong features in her case and decided to investigate it further. That was, in a way, a turning point for me in deciding to take up full-time research of reincarnation-type cases.

My Subsequent Years of Research and Associated Memories of Dr. Stevenson

In October 1974 Dr. Stevenson returned to India, bringing with him a student from the USA, John Russell, and he invited me to join them for the entire trip in India of about four weeks, an invitation I accepted. We were also joined by Dr. Mehrotra for this trip. We investigated quite a few new cases and interviewed informants on both sides of the cases, that of the subject (the child who claimed to remember a previous life) and that of the previous personality (the deceased person whose life the subject claimed to remember). This trip gave me good hands-on experience, and also a long period of close interaction with Dr. Stevenson both as a scientist-teacher and as a person. My experience in the field work and in getting to know Dr. Stevenson personally eventually helped me to decide on my future career in this field. During the first part of this trip, Dr. Stevenson, Dr. Mehrotra, John Russell, and I all went to see a case, the informants for whom lived in the interior of a village. We went as far as the cab could take us and then came to a point from where one had to either walk or go by bicycle. Dr. Stevenson ''ordered'' John Russell and me to stay by the cab; perhaps out of concern he did not want to subject us to unnecessary hardship or discomfort at the beginning of the trip. He hired two bicycles for himself and Dr. Mehrotra, and they went on to the village to interview the informants. When they returned, both looked tired, but I could see a glitter of success in Dr. Stevenson's eyes. Throughout the trip (and my subsequent trips with him), he treated us all with equal affection and respect; I observed no hierarchical division in his dealings with us.

During both of my initial trips with him, I found in Dr. Stevenson an excellent teacher, a scrupulous researcher, and a kind person who preferred to work diligently rather than talk about his work. He was extremely polite, humble, and unassuming; I did not realize that he occupied such a high position at the University of Virginia until I went there in 1976.

Apart from his personal attributes, I found his techniques highly scientific and the investigations quite interesting and challenging – so much so that I gave up my job at Chandigarh to delve deeper into the subject. At the end of our 1974 trip, he had encouraged me to enroll for the Ph.D. program, which I did. I had had good training with him and was convinced about the authenticity of some of

the cases that we had investigated during the 1974 trip, as well as some that I had investigated in December 1973 without him. Between 1974 and 1979, therefore, I began investigating cases independently to replicate his research; these cases formed the basis for my Ph.D. dissertation. Since it was a replication study of Dr. Stevenson's work, he did not participate in the investigation of most of my cases. He was, however, concerned both about my personal safety during field trips and about reliability in the data collection. With both of these aspects of the investigation in mind, he asked Dr. Prasad to arrange for some of his colleagues to accompany me on field trips. As a good scientist, he did not want to influence my investigations in any way, but as a responsible and good human being he did not want to put me in any danger. I was both impressed by his scientific fervor and touched by his genuine human concern for my safety. After I submitted my dissertation, I collaborated with Dr. Stevenson almost until the end of his life. His academic assistance and generous financial support for my research, through the University of Virginia, helped tremendously toward my personal and professional growth, for which I shall always remember him with gratitude.

When I began the investigations, most of our cases came from the villages, and I was new to the village settings and customs. Sometime informants would come to us and offer to give information about the subject or the previous personality. When we asked about their relationship with the person concerned, they would say something like: ''I am his uncle,'' or ''I am his brother.'' On further probing, we would learn that they were not related by blood but that they belonged to the same village and were, say, like a brother of the subject's father or were a close friend of the subject's previous personality. This was not to deceive us; it is a common practice in Indian villages for friends to introduce themselves in terms such as ''brother'' or ''sister''. Over the years we learned quite a few such expressions together. Dr. Stevenson was always eager to learn new things and put them into practice, mostly seriously, but sometimes jovially. For example, once when we were visiting Dr. Prasad, Dr. Stevenson asked him to arrange for one or two of his colleagues to accompany me when Dr. Stevenson or his associates from the USA were not available to tour with me. He told Dr. Prasad: ''You see, I am her academic uncle, and we should take responsibility to help her in data collection.'' In showing a genuine concern for me, he aptly applied his recently acquired knowledge about Indian relationships. Over the years as we worked together, I found myself growing closer to my ''academic uncle,'' and I must confess that I became much more close to him than even my own father or uncles.

I remember that sometimes during our long taxi rides he used to tell me, ''I would not mind sleeping on a bed of nails if I could get a perfect, or near perfect, case,'' and I had seen him several times putting up with physical discomfort for the sake of getting information about the cases. On several occasions, to conserve time and energy from long taxi rides, we would spend a night in the public guest houses, which did not even have adequate basic amenities. I do not think a professor from the USA (or any country for that matter) would have liked to stay there more than once; but Dr. Stevenson's quest was different, and this

research seemed to be his mission in life. I never heard him complaining about any discomforts.

I have had the good fortune of closely working with him for nearly three decades; he came to India (often on his way to or from Burma and Sri Lanka), sometimes twice a year, until October 2002. He worked tirelessly on these trips for nearly 16–18 hours a day and seven days a week. After we returned in the late evenings from the field work, I would suddenly wake up in the night and hear non-stop the sound of his typewriter in the next or opposite room in the hotel. He would work on the notes until late at night and would be ready the next morning with a long list of questions for our informants. I think his training in psychiatry and his personal attributes enabled him to win over the confidence of most informants. He was gentle in his approach and extremely careful in formulating or asking questions. Earlier in his career he had written a book on psychiatric interviewing (Stevenson, 1960/1971), and he applied those techniques while investigating cases of the reincarnation type, as well as other cases involving paranormal experiences.

I learned a great deal from him during our travels. During train journeys and taxi rides, he would bring a huge bag full of articles or books to read and update himself with the latest developments. In the mornings when the light was sufficient he would read something new, and while returning from the trip late in the evenings, when he could not read, he would share his knowledge with me and other colleagues with us. In spite of being a giant scholar himself, he was always ready to learn from others irrespective of their age. He would not hesitate to ask when he wanted to learn or correct his knowledge about India; and when he wanted to share with me information about other countries or when I wanted to learn new things, he would explain in detail and very patiently.

As a co-author I also benefited a great deal from him. He was a perfectionist in every sense of the term and always did a flawless job, beginning with data collection, to searching for relevant literature, to writing and revising articles and books until he was convinced that there were no plausible errors left.

Eventually, he wanted to concentrate more on his writings and spend less time on field investigations. From time to time he appointed others at the Division of Personality Studies at the University of Virginia to come to India to work with me and thus help him reduce his visits. But I think he was addicted to investigating cases, and he was always keen to learn about better or stronger cases. Beginning in the late 1980s, he would tell me, ‘‘This is my last trip, unless we find a case of the century.’’ In spite of saying this repeatedly, he returned several times. In the mid-1990s he made a few trips to investigate or to help me in the evaluation of cases of birthmarks and birth defects in the subjects. His last few trips lasted for one to two weeks as he was becoming physically weak (mentally he was as agile as ever). His trip of October 2002 unfortunately turned out to be really his last trip to India. However, we continued to communicate with each other whenever something about the cases or related matters came up.

Now a few more lines about my observation of his performance in different roles. I feel proud – and I think I am correct in saying – that of all his associates

I had the opportunity to spend the most time with him on field investigations. Hence I can claim to have known him well, both as a scientist and as a human being. I have enumerated some of his qualities as a scientist, and I am sure his other colleagues too will highlight, and perhaps better than me, his scientific fervor and achievements. Therefore I will focus now on his other qualities.

Apart from being a scientist and a scholar, he had a charismatic personality and the qualities of a saint. He was kind, generous, and affectionate, which is why everyone who came in contact with him thought that Stevenson belonged to him/her. His kindness was not restricted to human beings only; he was equally sensitive toward animals. I had often seen him walking with full enthusiasm and a smile on his face alongside the Tongas (horse carts) or bullock carts, even on uneven pavements and for long distances, so as not to put undue strain on the animals. A few times he even pushed the bullock carts when they were stuck in the mud.

I can go on writing about him but space limits have to be kept in mind. My long association with him brings many pleasant memories and a sense of pride that I knew and worked with such a great person so closely. It also gives me lot of pain to accept that he is no more. But such persons never die, and he has become immortal through his monumental contributions to the field of parapsychology in general and reincarnation in particular.

To sum up, I suppose his research was his mission and humanity was his religion; he was an ocean of humanity. His qualities are best described by the words beginning with the letter "**C**." He was a scientist and an exemplary human being who had the **courage** to venture into an unconventional field. He had the **curiosity** and **competence** of a first-rate scientist and could **communicate** his ideas and theories with utmost **clarity**. He had a **charismatic** personality of substance and bore a **character** that endeared him to all his associates and whoever interacted with him. He was **compassionate,** and he had the **conviction** and **confidence** in himself and his work that led him to stand out among conventional as well as conservative scientists. In his death, I have lost a mentor, a guide, and a friend. My only regret is that in spite of having done pioneer work in an unconventional area, he has not received the kind of recognition he deserved.

Our true tribute to him would be to carry forward the research that he so painstakingly made scientifically credible with the hope that generous grants would be bestowed for us to continue his work.

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Journal of Scientific Exploration, Vol. 22, No. 1, pp. 115–116, 2008 0892-3310/08

A Good Question

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Washington Post
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How do you thank someone for providing the adventure of a lifetime?

After a long career of keeping a careful distance from the press, Ian Stevenson invited me to come along with him on two of his last journeys in search of children who talked about previous lives. Without caveat or precondition, he met me in Paris for a flight to Beirut, where we spent a month being driven at high speeds on narrow twisting roads in the Druze-inhabited hills that arced around the city. Half a year later, we flew into the blinding chaos of Delhi, then by train, plane, and Maruti microbus we criss-crossed half the continent. Together we endured 14-hour days bouncing down rutted farm roads into the outback, a broken axle away from serious trouble. We confronted undisguised hostility, and humbling hospitality. We went hungry, and we feasted. We pursued dead ends, and stumbled into gold mines of first-hand testimony.

In his 80th year, twice my age, he out-walked, out-talked, and generally outlasted me. There was never a question of starting late or stopping early – or sometimes even for lunch. At the end of grueling days when I only wanted to unfold myself from the car and crawl into bed, Ian emerged ramrod sraight and declined the elevator.

''I'll think I'll walk up the stairs instead.''

He never once complained of fatigue, hunger, or physical discomfort of any kind, even after nights where I heard him up half the night with the deep hacking cough of someone who'd battled respiratory disease his entire life.

After nearly 40 years and countless field trips far more difficult than these, he approached his work with the same methodical deliberation he'd shown from the start, his briefcase filling steadily with notes and forms that would wind up in the cabinets of his Charlottesville office, along with the hundreds of thousands of pages that documented his life's work, work that would earn him a measure of fame in certain circles, but skepticism and scorn from many of his scientific colleagues.

While I worried about out-of-control drivers swerving around water buffalo into our path, bandits, angry villagers, contaminated food, Ian was fearless, except for one thing.

''Why do mainstream scientists refuse to accept the evidence we have for reincarnation?'' he asked me late one night after a particularly long journey into the hinterlands.

Here's what I believe: Neither self-delusion, intentional fraud, peer pressure,

nor coincidence could explain how the children Ian investigated could have known all that they knew about strangers who'd died before they were born. But neither Ian nor anyone else had a shred of evidence for some process that *could* explain it.

Maybe in ten years, maybe in a hundred, some data stream from a super collider will reveal the secret connection between consciousness and quantum reality, or the fallacy of the arrow of time. Maybe the testimonies of Ian's children are the leading edge of a new paradigm, points of light peeking from the firmament of a new dimension.

But whatever the truth turns out to be, Ian's work, those countless files filled to overflowing with the passionate precision of his research . . . well, they are something. They are really something.

Journal of Scientific Exploration, Vol. 22, No. 1, pp. 117–119, 2008 0892-3310/08

The Fight for the Truth

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Our times are witness to a massive conflict between reductionist scientism and a resurgence of irrational religious fanaticisms. In the resulting smoke and flames the quest for truth falters. By scientism I mean the claims made by scientists, such as Richard Dawkins (2006), to know things they cannot possibly know—in particular, that God does not exist and that the mind is identical with the brain. Avrum Stroll (2006) has demonstrated that it is logically impossible for humans to know whether God exists or not. As for the mind-brain Identity Theory, this is most certainly untrue, as it violates Leibniz's Law of the Identity of Indiscernibles (see Smythies, 1994a, 1994b, for details). Scientism is also picky about which scientific facts it accepts. Its adherents pour scorn on parapsychologists as pseudo-scientists. However, these people have very rarely studied the work of parapsychologists and base their opinions purely on dogma. Their own metaphysical theory of mind-brain Identity does not allow for any such facts as the parapsychologists report, and so, like Galileo's Pope, the skeptics deny the experimental facts on a purely *a priori* basis. Likewise, neuroscience is currently infected with scientism. Almost all contemporary neuroscientists believe, as an article of faith, that neuroscience has demonstrated the truth of the mind-brain Identity Theory. But, of course, it has done no such thing (Smythies, 1994a). As Ayer (1951) pointed out many years ago, the information about all and every detail of how the brain works as an electro-chemical machine is irrelevant to the quite separate question of how ALL this activity is related to the phenomenal events that we experience in conscious awareness. As he put it, if one is trying to build a bridge across a river, it does not do merely to raise one of its banks.

Fortunately, if we reject Identity Theory, we are no longer left with Cartesian Dualism (with its many defects) as the only alternative explanatory theory of mind-brain, or consciousness-brain, relations. A rival theory—extended

materialism—has been developed over the years by C. D. Broad (1923), Bertrand Russell (1946: 45, 581–593), H. H. Price (1953), Lord Brain (1960), and myself (Smythies, 1994b). This theory allows for the existence of the facts reported by parapsychologists. It also allows that humans may have immortal souls after all, along the lines suggested by Hindu and Buddhist psychology. Included amongst these alleged facts are the reports of reincarnation that Ian Stevenson devoted much of his life to studying. His meticulous work accumulated a wealth of evidence that supports the reality of this phenomenon. Ian's work was rejected by the Establishment of the practitioners of scientism (masquerading, in this respect, as scientists). They *knew* for *a priori* reasons that reincarnation was impossible, so they did not bother to read Ian's books. In doing this, they overlooked the fact that Identity Theory is riddled with errors and cannot, as detailed above, be true. This scientism represents not only an example of the resistance to paradigm change at any cost that has afflicted science throughout its existence (*vide* the ferocious fight against the germ theory of disease, tectonic plates, *Helicobacter pylori* as the cause of stomach ulcers, and many others), but is also an example of ideological imperialism ("How could ancient [and pathetic] Hindu psychology be right and our own glittering science be wrong!").

Scientism leads to nihilism, with which Western culture, in its politics, art and literature, philosophy and everyday life, is currently awash. The main hatred of fundamentalist Islam for things Western is directed, not at the Christian religion, which Islam respects, but towards our sickly, and to them evil and disgusting, culture. In their eyes, humans in the West have turned themselves into things without any purpose in the world other than mere biological activity. Scientism, without any evidence, denies the role for individual humans as travelers in eternity that is central to Islamic belief. Certainly one can make a case that many Western intellectuals, such as Marx, Nietzsche, Heidegger, Sartre, and Derrida, were merely so many termites gnawing at the foundations of civilization. It is up to us to do what we can to stop the rot.

Ian Stevenson has, for over half a century, been a leader in the uphill struggle to establish real science in the area of the mind and mind-brain relations in the face of bitter, and often envenomed, resistance by the Establishment. Richard Dawkins (2006) makes great play in his recent book *The God Delusion* of what he calls the appalling propensity of religious people to base their ideas on dogma rather than on the evidence. Well, many do, but this reads strange in a book bursting at the seams with its own dogmas, its uncritical acceptance of metaphysical theories such as Identity Theory, its refusal to take note of the relevant evidence from parapsychology, and its special pleading with regard to the views of Darwin and Wallace on genocide (see Smythies, 2007, for details). For example, Dawkins states that he knows that this life is the only life we have. How could he possibly *know* that? In these discussions I am reminded of some tadpoles in a muddy pond complaining that they cannot understand the special theory of relativity.

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Journal of Scientific Exploration, Vol. 22, No. 1, pp. 120–124, 2008 0892-3310/08

Ian Stevenson: A Man from Whom We Should Learn

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Ian Stevenson was one of the most extraordinary individuals, in the best sense of that word, whom I have had the pleasure of knowing. I had the privilege of working at his center for roughly five years (1968–1973) and of knowing him for many more years. I welcome the opportunity to share some impressions of this remarkable man because he is someone whose work we should continue to study and from whose example we should continue to learn.

One way in which Stevenson was extraordinary, although not unique in parapsychology, was his breadth and depth of scholarship, both in parapsychology and outside it, and in the application of those findings and insights to his own research in parapsychology. Based on what he read in contemporary parapsychology journals, he sometimes was dismayed about the apparent ignorance, by too many contemporary parapsychologists, of the contributions to this field by historical figures. He felt that this was unfortunate for the advancement of the field. Of course, he was correct, but I would toss in the caveat that historical myopia (including prejudice against old literature as somehow deficient simply because it is old) often seems also to characterize work in other fields of science, with comparably ill consequences. Parapsychology probably is far from unique in this regard, but Stevenson's concern merits continued attention.

There is another way in which Stevenson was outstanding among contemporary investigators. Based on both his writings and my many interactions with him over the years, I gained the impression that he saw his research mission fundamentally as dedicated to illuminating the nature of the person (as in the concept of ''personality''), rather than toward illuminating the nature of mysterious powers (i.e., of psi events), although he surely felt that understanding the latter had its own importance. (I recognize and Stevenson surely did, too, that illuminating the nature of psi function conceivably might illuminate the ultimate nature of the person, but I suggest that his interest in the nature of the person was less focused on this abstract kind of issue than on something more germane to everyday functioning; see comments that follow.) Stevenson contributed in a variety of ways, and very well, to domains related to spontaneous cases of possible psi communication (e.g., a very important work, ''Telepathic impressions: A review and report of thirty-five new cases'' [Stevenson, 1970]), but if we stop there in thinking of him, we will have stopped very far short of the mark. Of course, there is his monumental work on evidence of possible previous lifetimes (i.e., ''reincarnation cases''). His selection of reincarnation as a research

problem might well have been driven fundamentally by its implications, if true, for the nature and development of the individual personality and for that personality's functioning in everyday life. He surely was interested in the abstract question of ''survival,'' but he, I suspect, pursued the reincarnation quest because of its potential to illuminate what happens in the here and now—perhaps to a much greater degree than anything that might come out of other areas of survival research. I suspect that he, as someone with a passion for psychiatry, found this possibility substantially more engaging than simply the relevance of reincarnation evidence to the ''survival question'' in the abstract (i.e., ''Do we survive death at all?''). None of this is to say that he was uninterested in the ''survival'' question generally, for there is much evidence of such interest on his part—he proposed, for example, tests in which a person would set the combination to a padlock and then after death try to communicate (say, to a medium or in a dream) a mnemonic that would reveal the combination (Stevenson, 1968).

In an early publication on the reincarnation cases (Stevenson, 1960, Part II of a winning essay in a contest in honor of William James), he opined that the study of alleged memories of a previous lifetime might provide a more plausible means of demonstrating survival than could be had through the study of mediumistic communication. This, he suggested, was because the study of possible reincarnation cases involves trying to find evidence that someone presently living had lived before (and died), whereas in efforts to prove survival via mediumistic communications one must try to find evidence that someone who has died is in some sense still living (see Stevenson, 1960: 117, his concluding statement). In his many years of later work on possible reincarnation cases, he was interested not merely in cognitive content (i.e., possible ''memories'') related to a putative previous lifetime but also, very importantly, in affective, behavioral, and even presently visible physical influences of the supposed previous lifetime (e.g., birthmarks or deformities). Dispositional psychological/behavioral evidence, rather than just supposed cognitive-perceptual memories, presumably held special interest for Stevenson because behavioral/affective inclinations, especially those that manifest in everyday life over a long term, may be less likely to be the result of ordinary psi communication. Of course, they also have special relevance to the development of a view of personality that is informed by survival research, something in which Stevenson, as a psychiatrist, certainly had a strong interest. For him, survival research potentially held real meaning for understanding human personality in the present time. I suggest that it was this broader, closer-to-life picture of the meaning of survival that fired in Stevenson a special enthusiasm and resulted in many years of research on this topic. This was true even though he never claimed that he had anything like definitively shown that the cases he studied were due to reincarnation, although he pointed to a variety of lines of what he regarded as convergent evidence favoring the reincarnation hypothesis over alternative ones. Nonetheless, he always discussed a wide variety of alternate interpretations of the data and frankly, but humbly, noted that in his view the reincarnation inter-

pretation was more plausible for some cases. This brings me to the discussion of Stevenson's character, which is, in many respects, the most important aspect of my commentary.

There are many things that Stevenson's life and work can convey to us that should be take-home messages when we think of and honor him and his work. Please permit me to mention a few, not in any special order:

1. He was an individual of outstanding intellectual honesty, which he greatly valued in others too. Although he personally favored certain interpretations of his data, he did not, as I just noted, overstate his preferred interpretation by claiming that he had ruled out beyond all reasonable doubt any of the many alternate hypotheses that he discussed. Nor did he state or imply that those who disagreed with him were foolish or intellectually dishonest. I suggest that this fairness of spirit and conservatism of statement went a long way toward his being able to present his findings and preferred hypotheses even in a variety of nonparapsychological outlets. I many times saw him look for—and report when he found it—evidence that went contrary to what seemed to be his personal inclinations. In subsequent discussion of it he did not "gild the lily."
2. I worked at his center for five years, and I can say honestly that in addition to his intellectual honesty, he was very honest personally. He greatly valued truthfulness and was not afraid to tell the truth, even when telling it might not be easy.
3. He was uncommonly bold in the areas he was willing personally to investigate and in the audiences to whom he was willing to present his evidence. For example, it requires outstanding courage to claim, especially in certain nonparapsychological publications or meetings, to have evidence suggesting memories of past lifetimes or other kinds of evidence suggesting reincarnation, and Stevenson had all the courage needed to present his evidence and ideas, usually to a scientific audience, but sometimes to a broader one. He had great curiosity about strange events and did not hesitate to study them. Dramatic incidents or reports that some parapsychologists might hesitate to investigate seemed deeply exciting to him, and *not* investigating them—or, at least, not encouraging others to do so—would have been unthinkable to him.
4. Stevenson presumably would have scored very high on the personality trait of conscientiousness. He very much desired to do tasks carefully, fully, and properly, and he would recruit others in areas that went beyond his knowledge or skills. Most of my work at the University of Virginia was on my own projects, but I collaborated with him on several projects and heard him report his research on many occasions. Based on these experiences, I never had reason to believe that he would even think of doing less than his best on any task, be it planning, investigating, or reporting. I always was confident in working with him that he would, to

the best of his ability, carefully monitor both himself and anyone else involved with the project.

5. A word that could accurately characterize Stevenson is *professionalism.* He exemplified all the qualities of professionalism, including conscientiousness (already noted), high standards of professional ethics, and proper social conduct, including fairness and kindness in his dealings with people. He was at one time much involved with developing ethics guidelines for the Parapsychological Association, but, most importantly, he himself exemplified those high standards of professional conduct. He was always very careful in his thinking, writing, speaking, and acting, and he taught his co-workers the importance of these virtues. He often gave practical advice. For example, he noted the importance of ''always leaving a paper trail'' to document what one did and did not do or say, not leaving things to the vagaries of memory or bias. He sometimes dispensed sage advice on grant getting. He clearly felt a sense of responsibility to himself, to his colleagues, and to his professional disciplines.
6. He may be deemed a superb role model for persons planning to do work in unorthodox areas such as parapsychology, because *before* he became deeply involved with parapsychology he had already earned a high regard for himself in psychiatry, his profession of training and practice. He had, for example, published a well-respected textbook on psychiatric interviewing (Stevenson, 1960/1971), and he was at one time Chair of the Department of Psychiatry at the University of Virginia School of Medicine. It is extremely wise to establish a very solid reputation in a socio-scientifically more acceptable and traditional area before embarking on parapsychological research and publication. There are reasons for this that are beyond the scope of the present discussion, but there have been other examples of this in the field, both historically and today. What is more, the knowledge derived from other scientific fields can enhance one's later psi research. Stevenson's ability to present his work in non-parapsychological journals and meetings undoubtedly was enhanced by his solid reputation in psychiatry and his holding a tenured position in that field.
7. Last, but not least, please do not forget that Stevenson also was able to continue with his work in unorthodox domains because (a) he had a home for it at a respected university and medical school and (b) the funding for it was largely raised by him. I believe he was successful in the latter regard at least in part because of being in such a setting, because of his personal character and accomplishments in psychiatry and in parapsychology, and because he cultivated support by being unafraid to address bold, but potentially very important issues. I must say also that he knew much about how to treat people, including potential donors. That included cultivating donors by being respectful, truthful, and honest in his dealings with them. He also had many traditional grant-getting skills, and that is something

that can be fostered by contact with grant-getting colleagues, usually after having established a home in a grant-getting institution.

Stevenson was an individual who stood out in parapsychology on account of his boldness, courage, and—even by that field's standards—unorthodox hypotheses. He also stood out because of his success in gaining support. He exemplified in his personal and professional life many qualities that are fundamental and important in any field of science. In my view, he exemplified a professional high road that we too seldom see in any field. By his everyday words and deeds he showed that intellectual boldness can successfully be melded with professional ethics, scientific carefulness, and social responsibility in public discourse. In remembering him, we would do very well to emulate his professionalism, conscientiousness, hard work, and honesty.

It is sad to say goodbye to such an individual, but it is gratifying and edifying to contemplate his example.

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Journal of Scientific Exploration, Vol. 22, No. 1, pp. 125–127, 2008 0892-3310/08

Ian Stevenson and the Society for Scientific Exploration

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The key role that Ian Stevenson played in the Society for Scientific Exploration came about only as a result of events in two faraway cities: Montreal in Canada and Cambridge in England.

The Cambridge connection dates from 1954 to 1955, when I was a research fellow at St. John's College. There were two or three visiting scholars that year, and one of them was a Professor of Philosophy from the University of Virginia. His name was David Yalden-Thomson, born in Scotland and a fiercely proud Scotsman all of his life. David returned to Charlottesville in the summer of 1955, and I was scheduled to move to Stanford in the fall, so David invited me to visit him in Charlottesville after I was settled in California.

I do not remember exactly when my first visit occurred, but I do remember going to a horse race (in a party that included the English philosopher John Wisdom, who was a horse-racing fanatic) and climbing a small mountain called Old Rag (in a party that included David's future wife Barbara). At that time, I was working in plasma physics rather than astrophysics, so my first contact with the University was with the Physics Department. Some years later, in the 1970s, I met Larry Fredrick while he was Secretary of the American Astronomical Society (AAS). Charlie Tolbert was also an office holder in the AAS, responsible for organizing the Shapley Lecture Series. So during my fairly frequent visits to Charlottesville to visit David and Barbara, I had the privilege of developing a friendship with Larry and Charlie.

Robert Jahn, then Dean of the School of Engineering and Applied Sciences at Princeton University, came to spend a few months of sabbatical leave at Stanford in the spring of 1978. In view of my interest in the UFO problem and the great difficulty I had experienced in getting my articles on that subject reviewed – let alone published – by a scientific journal, I had begun to meditate the need for a new scientific society, and its journal, that would be open to the discussion and publication of work on topics considered improper – or even heretical – by the scientific establishment. Meeting Bob, and learning that he was facing a similar dilemma, had provided the necessary impetus to proceed with such an initiative.

Plans for what became the Society for Scientific Exploration were therefore

beginning to take shape around 1980, and I already had the generous support of Larry and Charlie at the University of Virginia. However, it was shaping up to be a society heavily weighted by physicists and astronomers, and that would have been unduly restrictive from both an intellectual and a sociological point of view. This is where Ian Stevenson came upon the scene, but that happened only because of events in Montreal.

Ian and David were both alumni of McGill University, to which they were both very attached and of which they were both very proud. This common background led to a strong social connection between David and Ian that developed over the years into a strong and affectionate bond. As a result of this connection, David and Barbara were very familiar with Ian's bold but meticulous research into evidence ''suggestive of reincarnation'' (to use Ian's cautious terminology). Hence, when during a visit to Earlysville (David and Barbara's new home near Charlottesville) I discussed with them the evolving plans for a new society, David told me about Ian and his remarkable research. I was of course most anxious to meet him.

In due course, David telephoned Ian to ask if he would be willing to meet an ''astrophysicist from Stanford University,'' and Ian replied that that would be ''quite agreeable.'' In preparation for the dinner party at which we were to meet, Barbara studied Ian's recently published book, *Twenty Cases Suggestive of Reincarnation*, to ensure that we had good discussions during dinner and over brandy (but no cigars!).

We did raise the idea of a new society at that time, but I made arrangements to visit Ian in his office a few days later. It was on that occasion that I informed him of my own ''anomalous'' research interests and those of Bob Jahn, and of my contacts with Larry and Charlie, and where it all seemed to be heading. Ian was very knowledgeable about experimental parapsychology, the area of Bob Jahn's research. Moreover, Ian had of course experienced his own difficulties in getting the results of his research, in either parapsychology or reincarnation studies, published in mainstream scientific and medical journals. He therefore immediately sympathized with the need for a forum that would be more open to the presentation and discussion of the results of research outside the scientific mainstream, a forum that would be free from the constraints of conventional scientific orthodoxy. By the end of our meeting, Ian had agreed to become a member of the Founding Committee of the new society, for which we did not yet have a name.

The addition of Ian to the Founding Committee added an important new dimension to the intellectual scope of the incipient society. The Committee had a big complement drawn from the physical sciences (Bob Jahn from Aeronautical Engineering; George Abell, Bart Bok, Tommy Gold, and myself from Astronomy and Astrophysics; and George Siscoe, Bill Thompson, and Jim Trefil from Physics). We also had representation from Philosophy (Bob Creegan), Psychology (Roger Shepard), and Statistics (Persi Diaconis). However, the only research being carried out by these members was in the experimental para-

psychology and UFO areas. Ian's groundbreaking research into the possible survival of physical death represented a crucial expansion of the intellectual scope of the planned society.

The Society's Dinsdale award was initiated in 1992, and Ian was from the beginning an obvious candidate worthy of that award. However, Ian served on the Council from 1989 until 1997, and the Council properly considered it inappropriate to give the award to a current member of the Council. Eventually the Society was able to bestow the award on Ian, in 1998 after he had stepped down from being a councilor.

All who came into contact with Ian were the richer for the experience. He was unfailingly polite and attentive in one-on-one conversations, and he was unfailingly attentive and wise in Council meetings. He was truly ''a gentleman and a scholar.'' Despite – or perhaps as a result of – his difficulties with and rebuffs from the mainstream scientific and medical communities, Ian had developed a remarkable equanimity and seemed to rise above the friction that he encountered. It would have been difficult for anyone else actually to emulate Ian, but those of us who encountered similar difficulties were able to learn from him.

The Society for Scientific Exploration owes a great debt to Ian Stevenson.

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Ian Stevenson's Early Years in Charlottesville

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My first association with Ian Stevenson occurred when I was completing my second-year studies in medical school at the University of Virginia. Ian had moved to Charlottesville from New Orleans the year before and had taught ''Introduction to Psychiatry'' to the second-year students. I needed a job for the summer and thought it would be interesting to work in his research laboratory, where he and his Ph.D. assistant were investigating the biological/chemical causes of schizophrenia. So one spring day I waylaid Dr. Stevenson in front of the old Medical School on his return from Chancellor's Drug Store, where he had just had lunch. I introduced myself and told him that I wanted to work in his laboratory during the summer. I could see that he was taken aback by my confident assurance that he would hire me on the spot. In his careful, investigative style he asked me about my qualifications, and I told him that prior to medical school I had worked in a laboratory for Drs. Parson and Crispell, both of whom he knew. The result was that I spent that summer in Ian's laboratory looking at rats' brains which had been subjected to substances obtained from patients who were schizophrenic, as well as the brains of other rats who were the controls. The control rats had been subjected to substances obtained from members of the professional staff. (One can, of course, question the latter's suitability as ''normal controls,'' but nevertheless . . .)

Several years later I worked with Ian as a resident in psychiatry. During our first year the residents rotated night and week-end duties, and when Ian was the Attending he met with the resident on duty early Sunday morning and discussed his/her work with one of the patients. Woe be to the resident who had not boned up on the history of that patient, including illnesses in his/her life and family, childhood traumas, and the state of his/her physical health, as well as the psychological reasons for his/her admission to the hospital. Ian was an internist first and a psychiatrist second. I valued those sessions with him and always came away more enlightened and challenged and determined to work even harder.

Our paths diverged when I went into child psychiatry and Ian went into full-time study of reincarnation. Later I was delighted when he married Margaret Pertzoff, whom I had known earlier but had not seen in years as our paths had diverged. They gave much happiness to each other over the years.

It was very sad watching Ian's lung condition progress until he was bed-ridden and shortly thereafter died. Throughout that time he was cheerful when I visited, asking about me and my family, and he always urged me to ''come back soon.''

Among the many qualities that I admired in Ian, and hope to emulate, was his pursuit of the truth to the end of his life.

Journal of Scientific Exploration, Vol. 22, No. 1, pp. 129–130, 2008 0892-3310/08

Tribute to a Remarkable Scholar

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I have known and greatly respected Ian Stevenson and his works over many years. Although my direct contacts with him have been less frequent than I should have liked, he was always generous with advice and donations of inscribed copies of his works, the last of which arrived with a faltering inscription very shortly before his death. It was for me a happy circumstance that I was able to support an invitation to him from Darwin College, Cambridge, to become a visiting scholar and dine with us during the times he spent in England. He was immensely well read, aware of academic concerns in many different spheres of humanities and science, and over the years he became a much appreciated visitor. During his final illness, I received various anxious inquiries about him from the college.

Ian took a keen interest in my attempt to distribute to a Cambridge population a version of the Society for Psychical Research's (SPR) Census of Hallucinations question, which confirmed the impression that vivid visionary experiences are as prevalent today as they were in the late 19th century. He was plainly disappointed when I found it impractical to obtain further information and confirmation about one particularly interesting veridical vision. He was indefatigable in the pursuit of evidence that might confirm or refute a paranormal interpretation. While obviously pleased when mundane explanations could be ruled out, he was meticulous in recording the data, whatever the outcome. This was a valuable feature of his studies of cases suggestive of reincarnation. He never shunned consideration of points that invite scepticism, such as the culture-bound features of many cases, the claims to have had a much higher status in a previous life, and the theoretical confusion between a moment of fresh incarnation and the ''possession'' of an already developing personality.

Ian had an abiding concern with mind-body relationships and took a particular interest in such phenomena as the somatic expressions of emotion (including hypnotically induced skin eruptions), ''recovered'' memories, cryptomnesia, ''multiple personality,'' and near-death experiences. These topics featured prominently in the early publications of the SPR, but, being less available to the investigatory techniques of contemporary experimental psychology, are somewhat neglected today. Ian has done more than anyone to remedy this.

Never frightened to think beyond currently fashionable scientific theories (such as the supposedly illusory nature of the will and the mind), he saw the far-reaching implications of his discovery of the inheritance of physical blemishes in some reincarnation cases. If true, this would suggest the development of physical as well as mental characteristics being derived from individuals other than parents. The difficulties of investigating such cases, particularly in view of the impossibility of observing or measuring the wounds or abnormalities of the deceased, would have deterred many researchers, but Ian pursued this research with his customary tenacity and, in the face of much derisory scepticism, has published some challenging evidence. Whether this work will be followed up and prove to be, as he believed, his most important contribution remains to be seen.

Although at one time I thought otherwise, I now share Ian's belief in the importance of looking beyond effects that can be conveniently reproduced in laboratory experiments. He was out of sympathy with J. B. Rhine's policy of neglecting field research, especially when particular lines of experimentation were yielding insubstantial results and contributing little to an understanding of the phenomena. When Rhine retired from Duke University and could find no place in his new unit for my friend the late Gaither Pratt, a senior experimental researcher with a wider outlook and experience than most, Ian found a place for him at the University of Virginia.

Ian's powerful intellect, academic accomplishments, and enormous drive enabled him to become a well-respected figure in university establishments, attract fund-givers, and provide facilities for some who might otherwise be lost to the field. He wanted and strived for improved acceptance of researches of the paranormal in mainstream academic publications, arguing that it was unhelpful to have parapsychological reports buried in specialist journals unread by those in the academic establishment. In my opinion, one of his greatest legacies is that his Division of Perceptual Studies, situated in a fine university, is providing places for a new generation of productive workers to carry on where he was finally forced to leave off.

Journal of Scientific Exploration, Vol. 22, No. 1, pp. 100–101, 2008 0892-3310/08

Ian Stevenson: Founder of the Scientific Investigation of Human Reincarnation

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One may confidently expect that, in years to come, Ian Stevenson will be recognized as one of the most illustrious personalities in the history of Mr. Jefferson's university. He has earned this distinction through laying the foundation for the scientific study of human reincarnation. How long it will be before this discipline, as well as the fact of at least occasional human reincarnation, becomes widely accepted with a soundly established methodology, we do not know, but there can be no reasonable doubt that this acceptance will occur, with recognition of Ian Stevenson's seminal role herein. It was he who pioneered virtually all investigative methods in this new science, and, with an unmatched knowledge of the pertinent facts, he identified many of the questions still to be answered.

Most important among future areas for research is likely to be the frequency of the occurrence of reincarnation: Is human reincarnation almost universal, as accepted in Buddhism and Hinduism, ceasing only with final deliverance, with "enlightenment," the cessation of "samsara"? If not, how might it depend on variables such as age at death, mode of death, culture, geographical location, beliefs, or personal effort? Further, is there any reasonable indication from memories of other lives for reincarnation in other locations than earth? And what evidence, if any, may be explored to account for the discrepancy between numbers of deceased humans and new births, especially the historically recent explosive increase in numbers of humans so that almost as many humans are alive now than ever lived before? Exploration of these and related questions will continue for a long time to come. One important aid herein is likely to be the investigation of cases involving birthmarks and birth defects, as also pioneered by Ian Stevenson. In addition, the condition of the personality between incarnations, the effect of a past life and its various circumstances on the new personality, and a host of additional subjects worthy of detailed research will presumably be similarly investigated.

Ian Stevenson has made seminal contributions to most of the above questions,

but he shied away from speculation and the seemingly outlandish. This certainly was wise, since it will require steadfast clinging to fact and shunning of speculation to retain scientific integrity. Almost certainly it was this consideration that prompted him to refuse to assert that his research had documented the existence of reincarnation as an at least occasional occurrence, and to refuse saying that he himself believed so. Rather, he acknowledged no more than that the evidence amassed by him and others was ''suggestive'' of reincarnation. This was the one issue regarding which I fundamentally disagreed with him: True, certainty always eludes humans except as established per definition, and so we cannot be certain that reincarnation ever occurs; but neither can we be certain of Newton's laws or relativity theory or Darwinian evolution. Yet the statistical probability that reincarnation does in fact occur, at least occasionally, is so overwhelming, established by thousands of already documented cases of remembered lives, and strongly buttressed by the incidence of birthmarks in conjunction with many of his well-documented cases, that cumulatively the supporting evidence is not inferior to that for most if not all branches of science, whether physics, cosmology, or Darwinian evolution.

Indeed, all human knowledge is burdened with a degree of uncertainty, but in the hard sciences we are accustomed to accepting odds once they go into the millions and billions, let alone astronomically large numbers, without saying that such and such evidence is ''suggestive of,'' say, relativity theory or the Big Bang. And there is no logical reason to act otherwise in regard to the evidence for reincarnation, simply because reincarnation counters age-old Western religious beliefs and cannot be reduced to mathematical formulae with testable numerical predictions. I argued many times with Ian Stevenson that, as a result of his undue reticence, readers of his publications are led to believe that he himself harbored genuine doubts about the results of his own research, thereby inviting the doubt of others and preventing that research from being widely accepted.

No, contrary to such an appearance, Ian Stevenson's pioneering work has laid as secure a foundation for human reincarnation as may be claimed by almost every other well-recognized science. Although a flawlessly proven case does not exist, his documentation has statistically proven, to stupendous odds and beyond any reasonable doubt, that at least some humans have been reincarnated, his own refusal to make such a claim notwithstanding.

Most importantly, also, Ian Stevenson has inspired many of the present highly gifted and dedicated reincarnation researchers to continue his work and to expand the structure of reincarnation science, so that it will live on and gradually win over the universal acceptance that I believe to be inevitable. Thereby Ian Stevenson's place as one of the great personalities in the history of the University of Virginia will be secured, and while we have reason to mourn his departure, we have much more reason yet to celebrate his wonderful life and achievements.

Journal of Scientific Exploration, Vol. 22, No. 1, pp. 81–82, 2008 0892-3310/08

The Gentle American Doctor

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Every time I talk to people who were interviewed by Dr. Stevenson, they always ask the same questions: ''Where is the American Doctor? When is he coming back to Lebanon? Is he still doing research on reincarnation?''

Dr. Stevenson had a way of putting children and their parents at ease. He always brought gifts to the children that he interviewed, and his gentle, non-invasive style won their confidence. As a result they told him about many private incidents connected to their previous lives. Some even consulted him on medical issues! Whenever we returned to a house for a follow-up interview, Dr. Stevenson was always welcomed heartily.

I first met Dr. Ian Stevenson in 1976, having just graduated from the American University of Beirut (AUB) with a Business Administration degree. Dr. Sami Makarem, a professor at AUB, asked me if I would be interested to work with Dr. Stevenson, who was working on cases of reincarnation in Lebanon and elsewhere. I started as an interpreter and then became his research assistant. My duties were to conduct and translate field interviews from Arabic to English, prepare reports, and research new cases for him. It was through him that I met Dr. Emily Williams Kelly, who also came to Lebanon to investigate and follow up on Dr. Stevenson's cases.

In the summer of 1980, Dr. Stevenson asked me to come to Charlottesville to assist him in organizing data for his research work at the University of Virginia. I worked on assigning computer codes to selected Lebanese cases on reincarnation.

In 1997, after an absence from Lebanon of 16 years, Dr. Stevenson asked me to assist him again in his research on reincarnation in Lebanon. He arrived in the company of Tom Shroder, an award-winning journalist and later the author of *Old Souls* (Shroder, 1999). During that memorable visit, I arranged for Dr. Stevenson to deliver a presentation and lead a discussion on reincarnation at AUB. The hall was full, and everyone commented on his vast knowledge and dignity.

It was also through Dr. Stevenson that I met Dr. Erlendur Haraldsson, who was interested in conducting psychological tests and research on Lebanese children who remember previous lives. It was thus that I became a co-author with Dr. Haraldsson of a paper on three cases of reincarnation in Lebanon (Haraldsson & Abu-Izzeddin, 2004).

Dr. Stevenson was a patient man. He would wait for me to translate, in

particular after long, and sometimes emotional, responses from his subjects. Dr. Stevenson was also a meticulous person. We carried all kinds of forms with us on our trips, including ''preliminary information forms about reincarnation cases,'' ''birthmarks forms,'' ''follow-up interviews,'' and our daily expense record forms.

I always received autographed copies of his new published books. On January 23, 2001, he sent me his book *Children Who Remember Previous Lives*, with this note: ''For Majd with thanks for all your excellent help over many years and love, from Ian Stevenson.''

What would one of Dr. Stevenson's subjects say about him today? To find out I visited Itidal Abul-Hisn, who as a child remembered a previous life (see Stevenson, 1997: vol. 2, p. 1900). She is one of many cases of the reincarnation type that Dr. Stevenson had investigated in Lebanon. She still lives in the same apartment in Beirut where Dr. Stevenson, Tom Shroder, and I had a follow-up interview with her in 1997. She was moved by the news of his death and said:

Dr. Stevenson had good manners. He always asked me how I was doing. If I got uneasy while I was speaking about my previous life, he would stop and ask me if I want to rest for a while before I continue. He would also ask me if I want to continue talking, stop or change the subject. All his questions were asked politely and nicely. He always thanked us for the time we gave him.

Itidal was silent for awhile, and then with a smile said:

I remember when Dr. Stevenson came to interview me for the second time. He gave me a strawberry-fragranced cologne bottle. I still remember that bottle. He also gave my mother a pistachio-colored scarf. Dr. Stevenson visited us four times. I am really sorry that he passed away.

In a note to Dr. Stevenson's friends and family, I wrote that, for the Druze, reincarnation is more than a field of study, it is a firm belief. With that belief it becomes a little easier to accept the death of those we love. It does not, however, diminish the sense of loss.

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Journal of Scientific Exploration, Vol. 22, No. 1, pp. 132–140, 2008 0892-3310/08

REPRINTED ESSAY

Scientists with Half-closed Minds[1]

IAN STEVENSON

A surprising number are scared to death of new ideas. They have attacked major discoveries without even glancing at the evidence. And their distrust of unconventional experiments may now be hampering scientific progress.

A Dutchman living in the East Indies once tried to tell a native of Java that in his country the water sometimes becomes so hard you can walk on it. The Javan was immediately convulsed with laughter, and the Dutchman could make no progress with his explanation.

We find this an amusing story, but it would be even funnier if it did not really refer to us all. Ordinarily our reaction to new ideas does not harm us or others. But when we make the discovery of new facts and new concepts our business, then incredulity can prove costly. When humans become scientists they continue to experience some of the less rational qualities of being human. And with this part of them they can get in each other's way, and in the way of progress.

Pierre Gassendi, for example, made notable contributions to seventeenth-century physics. He devised the first atomic theory of matter since Democritus, and his works strongly influenced Newton. Yet when in 1627 someone reported the fall of a meteorite in Provence, Gassendi explained it as due to some unidentified volcanic eruption. This attitude toward meteorites was shared by nearly all astronomers and many other leading scientists for the next century and a half.

Some insisted that the stones had been picked up somewhere and carried by the wind; others accused those who claimed to have seen the stones fall of lying. In the late eighteenth century the great Antoine Lavoisier, himself a radical innovator in chemistry, rejected accounts of meteorites as the products of malobservation. Stones could not fall out of the sky, he declared, because none were there. Finally, in April 1803, a shower of small meteorites on L'Aigle, France, persuaded the astronomers to change their attitudes.

In the same way the first reports of hypnotism—or mesmerism, as it was called in the eighteenth and early nineteenth century—evoked many denials that the reported phenomena had ever occurred. In London, Dr. John Elliotson was driven from the chair of medicine at University College for endorsing and

promoting the study of hypnotism. The early accounts of surgical operations conducted under hypnosis encountered extraordinarily irrational opposition. Dr. James Esdaile reported from India in the 1840s the successful completion of over a thousand operations (one-third of them major operations) with the patients hypnotized and a death rate of only 6 per cent during or after the operations. Although this occurred before asepsis when almost 30 per cent of other surgeons' patients died, Esdaile had great difficulty in getting his work even published, much less accepted. His scientific critics alleged that he had bribed his patients to sham insensibility. According to one account ''it was because they were hardened impostors that they let their legs be cut off and large tumors be cut out without showing any sign even of discomfort.'' In their opposition to hypnotism many of the most creative scientists of the period forgot the rules of their own calling. Lord Kelvin announced that ''one-half of hypnotism is imposture and the rest bad observation.''

Similar prejudices met Harvey's discovery of the circulation of the blood, Pasteur's work on microbes, and Semmelweis' discovery that physicians themselves spread the infection of puerperal or childbed fever from one mother to another. To the list of scientists who have suffered from the incredulity of their colleagues we can add Darwin, the several discoverers of anesthesia, and Freud.

Early in the nineteenth century a tragic example of resistance delayed the introduction of a life-saving medical treatment. An English physician, O'Shaughnessy, discovered evidence that patients with cholera died not of the infection directly, but of the depletion of salt and water carried off in the diarrhea. Another physician, Dr. Thomas Latta of Leith, boldly acted on these observations and snatched from the grave a number of patients desperately ill with cholera to whom he gave infusions of salt and water. He reported his almost miraculous success; a few other physicians tried and confirmed the value of his treatment; but still not enough interest could be aroused to promote the treatment further. Almost one hundred years later, twentieth-century physicians rediscovered it.

Contempt Prior to Examination

A common and astonishing feature of the opposition to scientific advance is the certainty with which it is offered. For the moment, and sometimes for years, the doubter forgets that he could be wrong. At the first demonstration of Edison's phonograph before the Paris Academy of Sciences, all the scientists present declared that it was impossible to reproduce the human voice by means of a metal disc. One man proposed to throttle the demonstrator. ''Wretch!'' said he. ''Do you suppose that we are fools to be duped by a ventriloquist?''

Resistance to the new can reach into the highest places. We owe to Francis Bacon much of the foundation of scientific method. He said: ''We have set it down as a law to ourselves that we have to examine things to the bottom; and not to receive upon credit or reject upon improbabilities, until these have passed

a due examination.'' Yet Bacon could not believe that the Earth goes around the Sun. Galileo, who could not persuade fellow astronomers to look into his telescope, could not himself accept Kepler's evidence that the planets move in ellipses. Nor could he believe that witches suffered from mental illness, a view beginning to gain acceptance in his day.

Professor P. G. Tait, a contemporary and colleague of Lord Kelvin, made contributions to physics hardly less important than those of Kelvin. But when the news of the discovery of the telephone reached him, he said, ''It is all humbug, for such a discovery is impossible.'' Another interesting conversation occurred between Sir William Hamilton and Sir George Airy, justly celebrated mathematicians of the nineteenth century. Hamilton had just published his discovery of quaternions and was explaining it to Airy. Airy said, ''I cannot see it at all.'' Hamilton replied, ''I have been investigating the matter for many months and I am certain of its truth.'' ''Oh,'' rejoined Airy, ''I have been thinking it over for the last two or three minutes and there is nothing to it.''

Many great ideas have, to be sure, won rather easy acceptance. Einstein had his difficulties, but they did not include stupid hostility from fellow scientists. Still such hostility should not occur at all among scientists. For it was science that once fought religion for freedom of inquiry and belief. In its original victories—and some of its more recent ones too—science defeated attempts to censor ideas. The principle of expanding knowledge replaced that of closed revelation. What had seemed to be a body of established facts was challenged and succeeded by a new body of facts based on observation rather than on reason and authority. But in the process a confusion arose between science and that body of newly discovered facts.

As science has progressed, more and more facts have become established with reasonable certainty—with enough certainty unfortunately to stimulate the illusion that science is chiefly a body of knowledge. The current body of scientific knowledge differs markedly from that of the seventeenth century, and the comparison shows the transience of our concepts. Yet we frequently overlook this and identify science with current knowledge. Those who forget that science is fundamentally a method and not a collection of facts will righteously challenge new concepts which seem to question old facts.

Organized scientific activity as we know it goes back less than five hundred years. And during this time it has occupied the interest and attention of only a few people. I am not referring to the millions it has affected, but to the few thinkers who have affected the millions. These people had first to struggle with themselves to believe that things could be other than they appeared to be. When someone asked Einstein how he came to discover relativity, he replied: ''By challenging an axiom.''

To accomplish anything worthwhile in science (and in nearly everything else), one has first to persuade oneself that things may be different from what they seem. This is the most difficult step to take and we should not be surprised if those who have walked furthest have frequently slipped. A scientist is—perhaps

fortunately—only capable of scientific thought for a small portion of his time. At other times he usually allows his wishes, fears, and habits to shape his convictions. The wish not to believe can influence as strongly as the wish to believe. Most of us most of the time practice Paley's recipe for obstruction: ''There is a principle, proof against all argument, a bar against all progress . . . which if persisted in cannot but keep the mind in everlasting ignorance—and that is, contempt prior to examination.''

Scientists may also become seduced by their own attainments and acquire the conviction that success in one matter makes them authorities in all. James Clerk Maxwell's genius achieved an advance in the theory of electromagnetism from which came radio, television, and radar. His imagination shattered previously impenetrable theoretical barriers. Yet today he would surely blush crimson to read what he said to the British Association in 1879: ''Atoms are the foundation stones of the material universe, unbroken and unworn. They continue to this day as they were created, perfect in number and measure and weight.''

Pasteur struggled as much as any important scientist against the uninformed opposition of orthodoxy. After he attained recognition and at the height of his fame, he addressed a distinguished group of scientists and gratuitously included in his speech an announcement that scientific methods would never be used successfully in the study of the emotions. Yet already living at the time of his speech were the two persons who later established the scientific study of the emotions—Ivan Pavlov and Walter B. Cannon.

Like lesser human beings, scientists have a proprietary affection for their own contributions. Having given the best of their lives, as many have, to new observations and concepts, they may defend these as devotedly as those who give their lives to material possessions. And this kind of psychological investment can carry the investor into the most ridiculous positions. About fifty years ago, for instance, a curious exchange took place between the great anthropologist Malinowski and Dr. Ernest Jones, one of Freud's most devout followers and his biographer.

Jones subscribed wholeheartedly to Freud's statement about the universality of little boys' attachment to their mothers, which he called the Oedipus complex. This occurred often enough in nineteenth-century Vienna, and Freud declared it an invariable feature of human development When Malinowski studied the Trobriand Islanders in the South Pacific he found that their children were reared by their mothers and uncles and had little or no contact with their biological fathers. The domestic relations and psychological development of the Trobrianders differed considerably from those reported by Freud for Viennese families. Malinowski published his observations, but they did not convince Jones. From his office in London he insisted that Freud was right and urbanely reprimanded Malinowski for faulty observations. To this Malinowski patiently replied that he was compelled to accept the evidence of his own senses rather than statements made by those who had never visited the Trobriand Islands.

The tendency to erect ''systems''—which are then marketed as a whole—affects particularly the less mature sciences of medicine and psychology.

In these subjects we have had a succession of intellectual edifices originally made available only in their entirety. It is as if one cannot rent a room or even a suite in a new building, but must lease the whole or not enter. Starting with a substantial contribution to medicine the authors of such systems expand their theories to include ambitious explanations of matters far beyond the original validated observations. And after the first pioneer, later and usually lesser contributors to the system add further accretions of mingled fact and theory. Consequently systems of this kind—like homeopathy, phrenology, psychoanalysis, and conditioned reflexology (the last dominant for years in Russia)—eventually contain almost inextricable mixtures of sense and nonsense. They capture fervid adherents, and it may take a generation or several for those who preserve some objectivity to succeed in salvaging the best in them while discarding the dross.

Many such systems repeat the same story almost tediously. A few brilliant observations encounter fierce opposition from entrenched authorities. Despite this the new ideas slowly acquire adherents. Gradually opposition to much of the original propositions crumbles. But in the meantime the avant-garde of the enlightened have stiffened their doctrines into a sectarian orthodoxy. Instead of befriending further advances, they frequently attack and deride them. Certainly not all early adherents to a new discovery do this, but those who do not often find that loyalty to a group requires loyalty to a set of ideas which conflicts with dispassionate examination of later ideas and observations.

Harmful Incredulity

Rigid systems and their fanatical devotees have driven many scientists into the camp of the too incredulous. The querulous ‘‘schools’’ of psychiatry have by their own extravagance delayed the acceptance of the best in psychiatry by other physicians and laymen. However, physicians of all kinds are particularly guilty of failing to keep up with advances in their own specialty. This comes about because medicine is, to be frank, a trade as well as a science. Most medical students go into the practice of medicine, not research, and we all know worthy physicians who devotedly practice the medicine taught them twenty-five years ago, apparently uninfluenced by the events of intervening years. Yet these same men conscientiously trade in their old automobiles for new ones every two or three years.

Theoretically, physicians should have no more difficulty than, say, chemists or physicists in changing their habits to accommodate new advances. But to accomplish this, medical schools must change their principles in selecting students and try, first, to attract flexible minds into medicine, and, second, to avoid doing anything that will harden these minds against new ideas. Happily, medical educators have already recognized the need for this. When medical science moved slowly a man could write the same prescriptions for thirty or more years and still not fall far behind the times. The increasing pace of medical discovery has made such physicians not only foolish, but positively harmful.

Whitehead's comment that "knowledge keeps like fish" applies to medicine as much as any subject.

However, research scientists, too, are bound by harmful incredulity, although it is harder to determine to exactly what extent. In some ways scientists today have more protection against uninformed authoritarian opposition than their predecessors. For one thing there are more scientists and they are constantly testing each other's work so that confirmation, revision, or rejection of new observations and concepts can come rather rapidly. Communications between scientists have improved, and many journals now spread new data and new theories quickly across the world. Thus many scientists and not merely a handful judge the work of a fellow scientist.

On the other hand, the vastness of our scientific activity tells us nothing about the number of genuinely open minds occupied with it. A few years ago, Dr. Lucien Warner surveyed a number of psychologists on extrasensory perception. He asked what they thought about the existence of extrasensory perception and how they had reached their conclusions. All who replied had convictions, but less than 20 per cent said they had studied the original reports of the work on this subject. Seventeen per cent had reached their opinions on the basis of hearsay. Twenty per cent had made up their minds entirely on a priori grounds.

One can only respect the candor of persons who have registered themselves as scientists and yet make public declaration of the fact that they can decide on a matter of extraordinary importance without examining the relevant published work. Perhaps parapsychology provides a special case and scientists do not feel so free to make up their minds on other matters. Certainly the implacable opposition parapsychology encounters among some scientists illustrates again the relationship between the heat of antagonism and the possible threat to established convictions from the new data or ideas. For the data of parapsychology portend, I believe, a conceptual revolution which will make the Copernican revolution seem trivial in comparison.

We may tell ourselves that this incredulity has no effect on creative achievement but I personally do not believe it. I am convinced that deep conservatism strongly influences the approach of many scientists to new ideas. I have tested this frequently by throwing out into a group of them some new idea, especially one whose acceptance would fracture favorite concepts. Almost invariably they attack it like a school of piranhas. By the time it reaches the bottom of the discussion they have stripped off its flesh.

My friends are not ordinarily destructive people. They do not injure people, only ideas. And I think this behavior has to do with a mistaken concept of the role of scientist. Certainly the role includes skepticism and tough-mindedness, but these alone are not enough. The best part of science derives from the imagination and creativity which contribute to it no less than to the arts. A scientist should examine an idea as an artist might look at a delicately enameled vase—in many different lights and positions so as to bring out all its beauty and value.

Scientists frequently pride themselves on not being gullible. Sometimes they do not seem to realize that they cannot be incredulous about new ideas without at the same time being excessively credulous about old ones. Between the merits of accepting too much and not enough of what is new there is perhaps little to choose, but surely that little favors a receptivity to the new since we already know so little.

I believe our conservatism has infected the financial support of scientists. Although a lot of money flows toward scientific research we do not know how much runs in well-cut gorges and how much can irrigate new ground. But the system of project grants for research is a symptom. Nearly all the funds poured into research by foundations and the federal government reach scientists *after* they have submitted a project to a committee. Since a scientist must gain the approval of the committee for his project, he may not resist the temptation to design his project along the lines most likely to harmonize with the convictions of the committee. The committee in turn must account to a board of trustees or to Congress or the public for the success of the research it has supported. Who can blame the members if they behave like bankers and venture their money more readily on ''good risks'' than on ''wild ideas''?

Once he has his money, the scientist feels committed to the project he has outlined. If he makes some interesting but unexpected discovery or observation, he cannot easily abandon his main object to pursue a new line. Nearly every year he must submit an account of progress to the committee. I have heard a number of scientists tell, half laughing, half crying, how they adjusted their applications or reports, or, worse still, adjusted their scientific projects, to the real or apparent expectations of a granting committee.

It matters little that often the scientist's fears are unjustified or exaggerated. Certainly most scientific members of committees evaluating projects consciously wish to give the working scientists the greatest possible freedom. Still possession of the power to make decisions can eventually persuade anyone that he also has the proper knowledge to do so. The fault, I think. lies in the system, but wherever the fault, I believe that our scientists and the tellers of their money can easily become mutually involved in timid projects which always succeed but never advance.

It is difficult to pin down instances of the withering effect of incredulity on budding ideas. Prejudice can rationalize itself as caution or be easily disguised by other appearances. A surveying committee may conceal from the applicant, and even from itself, the real reasons for turning down a request for funds. Yet there are grounds for believing that research in psychiatry in this country has become excessively influenced by the theories of psychoanalysis. I know of two first-rate investigators who have had great difficulty in obtaining support for their projects because (so the available evidence strongly suggests) their ideas run counter to psychoanalytic concepts. One eminent psychiatrist, much experienced in such matters, told me in discussing one of these cases that it is now extremely difficult to obtain support for psychiatric research projects which

are not psychoanalytical in orientation. (He was referring to psychological and psychotherapeutic projects, not biochemical or neurophysiological ones.)

Another leading American psychiatrist recently published a vigorous protest in one of our professional journals against the centralization of psychiatric research and its control by committees which permit a few persons to swing enormous funds toward a few favorite or fashionable themes of research. The one-sidedness of our approach evokes both horror and amusement in our European colleagues who have managed to preserve a better balance in planning psychiatric research. This state of affairs does not reflect adversely on the merits of psychoanalysis, only on those who insist that its assumptions must be the point of departure for all psychiatric research.

Freedom to Act Foolish

Defenders of grants for project research claim that they permit scientists to get funds long before they are sufficiently well known to receive support for themselves. This supposes that we can support scientists directly only when they have already become well known—certainly a most unsatisfactory criterion of worth and one more likely to lead to a search for publicity than for truth.

A second symptom of harmful conservatism is the figures published by the National Science Foundation on the distribution of funds for research. In the years between 1940 and 1954—a period in which sums allocated for research skyrocketed—the funds available for basic research (*i.e.*, research not bound to any immediate application) increased ten times. But in this same period the *percentage* of funds allocated for basic, as opposed to applied, research decreased by half.

Moreover, applied research has become increasingly important in the universities which have traditionally remained free to support new ideas and their testing. Recently, in order to maintain themselves against rising costs (or so they rationalize, perhaps), universities have accepted more and more contracts for applied research. According to a report prepared by Dr. Vannevar Bush in 1945, basic research received 70 per cent of all the funds devoted to research by universities before World War II. This contrasts sadly with a recent estimate derived from the report of the National Science Foundation that basic research now accounts for only 35 per cent of universities' research funds.

One remedy would be to give more money directly to scientists for themselves, rather than for special projects. The federal government has already begun this on a small scale, although we apparently lag far behind the Russians. Such a system would have its weaknesses in this country, as it undoubtedly has in Russia. Its mistakes would be more obvious and perhaps more wasteful than those of the present system. But if we had more failures, we might also have more new knowledge. Certainly we will have no new knowledge at all unless we continue to foster ideas which shake present beliefs. Prophets have warned us. John Dewey told us "every great advance of science has issued from a new

audacity of imagination.'' And Whitehead wrote that ''every great idea sounds like nonsense when first propounded.''

During the planning of the Rockefeller Institute for Medical Research, someone asked Dr. Simon Flexner, who was one of the chief architects of modern medicine: ''Are you going to allow your men to make fools of themselves at your Institute?'' As it turned out the Rockefeller Institute made many more discoveries than fools, but the freedom to make a fool surely contributed to its extraordinary success. Scientists at the Rockefeller Institute were given full support to pursue their own work in their own ways. Unfortunately, this system had almost no imitators (except in Russia) and even the Rockefeller Institute departed in later years from its original principle. Today we badly need not only new institutes of the kind it was, but new freedom to pursue strange ideas. And scientists themselves must encourage each other to think brazenly and experiment boldly.

When I read about the now-primitive treatments practiced by our predecessors in medicine a hundred years ago, I cannot refrain from smiling at some of their fatuous remedies. My smile includes a little pity for them because they knew so little and some pleasure for us because we have come so far. Then I hope that a hundred years from now, some medical descendant will read our books with similar pleasure for similar reasons. If he does, this supposes that we in our time have remained humble about our knowledge and receptive to the new ideas which will furnish the justification for his pity. May it not be said of us: ''No man having drunk old wine, straightway desireth new; for he saith 'The old is better'.''

Note

[1] Stevenson, I. (1958). Scientists with Half-closed Minds. *Harper's Magazine, 217*, 64–71.

Journal of Scientific Exploration, Vol. 22, No. 1, p. 131, 2008 0892-3310/08

An Ian Stevenson Remembrance

RAY WESTPHAL
Devon, PA

A dozen or so years ago, I decided to ''get serious'' about the many psi experiences that I have had during my life. So I documented the most memorable ones, included a resume, and sent these materials to many individuals whom I had identified through my research of the paranormal.

Two responses were all that I received: One of these was from Dr. Ian Stevenson. His letter was specific to my particular experiences, and his advice was practical and unassuming. He identified an organization within an hour-and-a-half drive from my home which he felt might be helpful to me. I followed up on his recommendation and made several visits to this organization, which I found informative and interesting, further stoking the fire in me to learn more about what others were doing to unravel these mysteries.

Dr. Stevenson closed his initial response to me with an invitation to meet with him should my travels take me to the Charlottesville area. A year or two later I called him, reminded him of his invitation, and set up a meeting. He was very hospitable and warm in making the arrangements. We met, and I met his very learned and erudite staff as well, a most enjoyable experience.

In the years that followed, we met several times, and my respect for Ian grew more and more, and not only for Ian the scientist, but also for Ian the man: his dignified yet totally gracious manner, his good-humoredness and wit, and his youthful approach to life—tennis, anyone?—and trips to remote areas of India!

In time, I thought of Ian as a friend and hoped he felt the same toward me. This led me to asking him a question which long since had been on my mind, and as I sat in his office on one of my visits, I simply blurted out: ''Ian, do you believe in reincarnation?''

After a significant pause, he turned to me and in very measured words said: ''The physical marks present strong evidence.''

Then silence.

Journal of Scientific Exploration, Vol. 22, No. 4, 557–560, 2008 0892-3310/08

LETTERS TO THE EDITOR

Editor: Commendations to you, Dr. Sturrock, and your Associate Editors for the tributes to Ian Stevenson, MD. I thoroughly enjoyed the articles and remembrances from those who wrote of their associations with Dr. Stevenson and his many contributions to so many disciplines.

I wish to provide a few comments to underline what others have written about Dr. Stevenson: a *scholar* and a *gentleman*.

During 1980–1981, he and I exchanged a half dozen letters, with reports and some audiotape recordings. I had referred a family to him, and he had referred a family to me, for the investigation of POLs (Possible Other Lifetimes). His intelligence, and experience, were on a higher level than mine; yet, he treated me as a peer.

I was busy as a faculty member and Director of Counseling and Testing, University of Wyoming. Also, I was attempting to establish, on campus, the Rocky Mountain UFO Conference (1980–2000). Yet, his schedule of activities, writing, and travel gave me a different perspective of stamina and commitment.

Although his writing indicated his skepticism about the use of hypnotic procedure to investigate ''cases of reincarnation type'', he encouraged me in my hypnosis sessions with a family and their possible earlier lifetimes together. Also, he tolerated my report of ''psychological resonance'' (channeling information about the possible family interactions). Although the channeled information may have added ''flavor'', it probably did not add ''substance'' to the previous information that he had gathered so skillfully.

I treasured the opportunity to exchange correspondence with Ian. I assume that he continues his spiritual path in service to Humanity & Creation.

May we all share more Love & Light.

R. Leo Sprinkle
Laramie, WY

Editor: While Bill Bergston's survey of the membership is useful in canvassing the views of SSE members on ''scientific anomalies'', one aspect caught my attention, which I will use to make an important point, without any criticism of the survey and its worthwhile objectives. In the questionnaire, the scale of responses to ''scientific anomalies'' (phenomena such as UFOs, out of body

experiences, etc.) was graded by the degree of skepticism the item engenders in the reader. I had submitted to *JSE* last year a paper on ''dowsing the vocabulary'', in which I reported on practical experiments with a method of dowsing that evinces subconscious responses to phenomena in divination mode. It gives quantitative values for our emotive and practical vocabulary, which span a wide range of vital energies when expressed on the Bovis Scale. I can already imagine a significant proportion of readers putting on their skeptical hat at the mention of ''dowsing'' and ''Bovis'', but without having tried the method themselves, I ask them to bear with me for a few more minutes. For example, when I hold in mind the emotion ''Skepticism'', I receive readings that are substantially below the levels registered for practical words, such as ''assess'', ''consider'', or ''evaluate'', and of course much lower than for emotional words, such as ''joy'', ''love'', ''family'', ''friendship'', etc. One conclusion is that at the time of Galileo and later, ''proto-scientists'' had to invent a vocabulary that falls within what I have called (Caddy 2007) ''the band of rationality'', excluding emotive words, in order that the logical consequences of initial assumptions and observations can be followed without the distraction of either ''doubt'' or ''inspiration''. From practical experience, I have noted that paranormal or ''anomalous phenomena'' are difficult to induce while negative emotions dominate the mind; perhaps because these create a ''morphic field'' such as those suggested by Rupert Sheldrake? My working hypothesis is that an observer should avoid skepticism when evaluating a ''scientific anomaly'', since the skeptical mind filters out phenomena that contradict its accepted axioms. It seems better to phrase questions alternatively, such as asking whether the evidence for the phenomenon in question is adequate, incomplete, or lacking. Even in the circumstances that the last response is chosen, this does not preclude an improvement in methodology or new evidence from easily reversing an initial opinion.

A Post script: My paper on dowsing the vocabulary was rejected for publication in the *JSE*; apparently on the grounds that dowsed responses are subjective. The referee/editor suggested that the paper would have been suitable for publication if I had been attached to an electroencephalogram, and if it had been written by the professional neurologist making observations from electronic instruments on his dowsing ''guinea pig''. I mention this to make the point that ''anomalies research'' is heuristic; often the only way to gain partial verification is to present a method for testing by a wider audience. At present however, the only way to read my paper is to contact me at jfcaddy@yahoo.co.uk, and I will send a copy.

JOHN CADDY
Latina, Italy

Editor: Comment on Dieter Gernert, ''How to Reject Any Manuscript,'' *JSE*, Vol. 22, No. 2, 2008, p. 233–243.

There is very little that is really new in this interesting and provocative paper. It is well known that on rare occasions even Nobel class work has been rejected by one or more sets of referees. A fellow in Spain named Juan Miguel Campanario has written about this subject. He often refers to the *Citation Classic Commentaries* that were published in *Current Contents,* which demonstrated that on occasion these highly cited papers were rejected even by journals as respected as *Nature*. Wolfgang Glanzel and I published a paper in *The Scientist* about the myth of delayed recognition: Glanzel W. and Garfield E., ''The Myth of Delayed Recognition–Citation analysis demonstrates that premature discovery, while rare, does occur: Nearly all significant research is normally cited soon after publication'', *The Scientist* 18(11): 8–8 June 7, 2004. Original article in *The Scientist* <http://www.the-scientist.com/article/display/14757/>

Quite frankly when you consider the tens of millions of papers and books that have been published, it is surprising to me that it is so rare that such paradigm breaking papers are delayed or rejected. One wonders what Gernert would consider an acceptable level of rejection considering the huge volume of publication. Indeed many people would argue that rejection rates should be even higher. I am glad he agrees that peer review does serve a useful purpose if properly administered. I've had a lot of positive experiences with the system and a few bad ones. The worst two cases involved papers that were actually requested of me by the editors of the *New England Journal of Medicine* (*NEJM*) and *Science*, respectively.

In the case of *NEJM*, the then editor, who is justifiably a highly respected editor and scientist (Arnold Relman), after making me go through several revisions of my manuscript, refused to publish it because it would be ''unseemly'' for a paper published in *NEJM* to show how much higher *NEJM* ranked as compared with the other journals in the study. After delaying my paper for almost two years, he turned it down but within a few months it was accepted by Edward Huth, the editor of the *Annals of Internal Medicine*.

The second paper was requested by Daniel E. Koshland when he was editor of *Science*. It took me almost two years to write what I thought would be my magnum opus for *Science*, since I had published two core papers there in 1955 and 1964,1 which are both highly cited. By the time I sent in the ''Synoptic history of the Science Citation Index'' manuscript, Dan had retired from *Science*. His successor Floyd Bloom, a highly respected neuropharmacologist, refused to publish the manuscript after delaying it for six months or more. The extensive revisions he requested would have delayed the paper another year. Shortly thereafter, I was asked to speak in Copenhagen and my ''talk'' was published in an established European journal of library science. The full text is available under the title ''From Citation Indexes to Informetrics: Is the tail wagging the dog?'' *Libri*, 48(2), p. 67–80, June 1998. Based on oral presentation–Center for Informetric Studies, Royal School of Librarianship, Copenhagen, December 15, 1997.

<http://www.garfield.library.upenn.edu/papers/libriv48(2)p6780y1998.pdf/> The original title was ''A Synoptic History of the Science Citation Index''. That it has been cited only 26 times in ten years tells you something about the importance of where you publish. Had it appeared in *Science* or some other leading journal I have no doubt that it would have been more widely read and cited.

EUGENE GARFIELD

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Garfield, E. ''Citation Indexes for Science: A New Dimension in Documentation through Association of Ideas.'' *Science*, 122(3159), p. 108–11, July 1955; and Garfield, E. ''*Science Citation Index*–A New Dimension in Indexing.'' *Science*, 144(3619) p. 649–54, 1964. See also: Garfield, E. ''Citation Indexing for studying science,'' *Nature*, 227 (5259) p. 669–671, 1970.

ERRATUM

We regret that, by mistake, the Reviewer's comments on ''Memory and Precognition'', by Jon Taylor, were included as an Appendix to that article (Issue 21.3, pp. 553–571). These comments referred to an earlier version of the manuscript, and they had already been addressed in the version that was published.