

# **BOULDERS IN THE STREAM: THE LINEAGE AND FOUNDING OF THE SOCIETY FOR THE ANTHROPOLOGY OF CONSCIOUSNESS**

**By Stephan A. Schwartz**

## **ABSTRACT**

*The founding of what has become the Society for the Anthropology of Consciousness (SAC) can only be understood properly in the cultural context of its heritage, and the world in which it came to life. The author is the last living founder from the original group. This paper is an historical narrative tracing the intellectual lineage of the SAC and how it came to be. It focuses particularly on seminal events at the 1974 annual meetings of the American Anthropology Association in Mexico City. At those meetings a paradigm within anthropology begun decades before was shaken, and the process was painful. The immediate cause were a series of symposia: The Rhine Swanton Symposium on Parapsychology and Anthropology, Session 703, and several symposia on the challenge to anthropology represented by Carlos Castaneda and his writings. Through a series of best selling books, including the publication of his dissertation for the anthropology department of UCLA, Castaneda, attacked the way a critical part of anthropology was conducted. The argument in his own narrative was that one could not understand the Shamanic world view, without becoming a shaman. No informant could ever convey this, because so much of it was experiential. And it could not be properly known unless one entered with sincerity into the experience, as a participant not just an observer. Implicit in this was the worldview that non-technological cultures can be as insightful as their technological counterparts; albeit in different areas of human functioning. Two insights central to this thesis are particularly relevant to SAC: an aspect of human consciousness exists independent of time and space susceptible to volitional control; and, there is an interconnection between all life forms which must be understood if the universal impulse humans feel towards the spiritual component of their lives is to properly mature. The SAC can be seen in pure Kuhnian terms as one response to the reassessment that Castaneda forced on anthropology.*

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## **PART I THE LINEAGE**

J.R. Swanton was one of those very few scholars who in their own lifetime become icons within their disciplines. A special collection of essays discussing his influence on all phases of anthropology was published by the Smithsonian on his fortieth anniversary at the Institution.

After a career of impeccable orthodoxy Swanton came out of retirement in 1952 to drop a bombshell -- an open letter to the field. "A significant revolution which concerns us all is taking place quietly but surely in a related branch of science," Swanton said, and then stated, "It is not being met (by anthropology) in an honest, a truly scientific manner."<sup>1</sup>

He argued earnestly for a study of the psychic by all anthropologists, of whatever subspecialty, in the process describing how he himself had come to a point where "the thunderbolt has fallen."<sup>2</sup> For Swanton, a clear choice now faced the discipline he loved. "Adhesion to current orthodoxy is always more profitable than dissent but the future belongs to dissenters. Prejudice and cowardice in the presence of the *status quo* are the twin enemies of progress at all times and (especially) of that 'dispassionate method' in which science consists."<sup>3</sup>

To risk anything less than open-minded exploration of the psychic, in Swanton's mind, was to make science "a set of dogmas which the 'faithful must accept or be damned.'"<sup>4</sup> Swanton died seven years later, his challenge ignored. Good manners and respect for his position precluded most open criticism but anthropology's general response was to act as though the letter had never been written.

Clarence Wolsey Weiant, however, was one colleague who did hear what Swanton was saying. Weiant was an unusual anthropologist; to begin with it was his third professional career. First, he had become a Doctor of Chiropractic Medicine, and would continue throughout his professional life to practice. As a clinician, he was an early proponent of what today we call Alternative Medicine. His second career was in parapsychology where he had worked with Hereward Carrington on

thought-photography experiments. He had reached his thirties and was already a tenured professor of Chiropractic before he even had gotten interested in archaeology. And he was 41 the summer of his Columbia University doctoral fieldwork at Trés Zapotes in Mexico, as a Mathew Stirling protégé on a joint Smithsonian-National Geographic sponsored dig that ran during the winter of 1938, and the spring of 1939.

The site was a two mile long area named for a nearby village, and it would produce one of the most famous single objects ever to come out of Meso-American archaeology: the *Cabezo Colosal*, a giant enigmatic stone head six feet high and 18 feet in diameter, weighing 10 tons and carved from a single piece of basalt. Less dramatic, but equally important, were the discoveries of what became known as Stela C, and a laughing figurine that established the site as meeting point for Aztec, Mayan, and Olmec cultures. It was an extraordinary run of finds; the kind that make a career. The official Tres Zapotes publications issued by the Smithsonian make no mention of anything out of the ordinary in the discovery of these artifacts, but most of these finds would not have been made had Weiant not used a local shaman whose gifts included what would now be called Remote Viewing\* to find them.

Faced with a short digging season because of anticipated heavy rains, and the time-consuming need to clear jungle before actual archaeological work could begin, Weiant had been in a difficult position as 1938 was ending. His initial efforts had failed, and he was beginning to face the possibility that his doctoral fieldwork would not be successful. One afternoon, after one particularly depressing failure, a dignified 80 year old Indian workman, Emilio Tegoma, whom Weiant knew only as the oldest man on the site, saw how disappointed he was and came up to him.<sup>5</sup> Tegoma told Weiant that if he would dig where he told him to he would find what he sought. Instead of dismissing the man as a crank, Weiant, perhaps the only man then in archaeology capable of fully understanding what was being offered, who was already experienced in parapsychological research, agreed. The results were immediately spectacular, even though there was nothing on the surface to suggest what lay beneath; and Weiant's work so meticulous that both Stirling and Columbia's William Duncan Strong recommended that Weiant's

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\* Remote Viewing is the human ability, mechanism unknown, by which individuals describe persons, places or events from which they are shielded by reason of time or space. There is an extensive corpus of research literature on the subject.

dissertation be published as a special Smithsonian bulletin,<sup>6</sup> a rare honor.

Weiant's success convinced him that both shamanism and psychic functioning were areas that should be of intense to anthropologists. When he got Swanton's letter in had been more than a decade since his experiences in Tres Zapotes, but those memories were still powerful, as easily evoked by Swanton's letter. But Weiant also saw that even a man of Swanton's stature could not get an objective hearing, and that convinced him to remain silent about his own interests. He could not forget the challenge though and, seven years later, he decided to submit a formal paper on the subject to the 1959 AAA program committee.

His request, addressed to Dr. Ignacio Bernac, director of the National Museum of Anthropology and History, who was serving as AAA program chairman for 1959 brought a quick and enthusiastic response: "I am delighted that you plan to present a paper on *The Present Status of Parapsychology and Its Implications for Anthropology*. We are looking forward to seeing you at the December Meeting."<sup>7</sup>

When Weiant arrived at the Culture and Personality session, where his paper was the last of seven scheduled for that afternoon, the signs continued to be auspicious. As the day drew on, people drifted in until the room was full and they were standing along the walls. But, to Weiant, as important as the crowd size was the presence of the one person in the room whom he recognized, although her being there also made him nervous. From the dozens of papers being presented at other sessions that same afternoon, Margaret Mead had decided to come and hear Weiant's – the only one in the entire conference to deal with the psychic.

"I had no idea what stand she would take, but I had observed her in other sessions. Whenever a controversy arose, she always seemed [to be] able to make a statement that made any further discussion unnecessary."<sup>8</sup>

As he spoke, Weiant was flattered by the close attention paid to his words, and buoyed by the applause that followed. His nervousness returned though when he saw Mead rise to speak. To his surprise and relief, she not only did not attack him, she supported his entire thesis. "She said she had no objection to ESP research – indeed, she had

cooperated with Gardner Murphy in his statistical research, but found playing with [Zener] cards in the laboratory terribly boring. Then she went on to suggest exactly what I had hoped for: that it would be worthwhile for anthropologists to spend time in an area where sensitives are plentiful to find out what kinds of people are psychically sensitive, and why.”<sup>9</sup>

Weiant left the conference feeling that change would now occur, and he looked forward to research, papers, and seminars on the subject of parapsychology and anthropology. He had only one regret: Swanton had not been there to share it. He had died earlier that year.

But in the months and years that followed, there was no such research. There were no papers. There were no seminars. Beyond the interest of a few anthropologists like Mead, there was nothing to show for his effort but the corridor story an archaeologist’s wife told him about a ghost she had seen in her apartment.<sup>10</sup>

Swanton’s letter of 1952 and Weiant’s paper of 1959 were like large black boulders widely spaced in a river – formidable in themselves, perhaps, but too widely separated to make a barrier. The waters simply parted before them and closed after them, with only the slightest babble of sound. It would take another decade before a challenge to orthodoxy arose that had enough mass to whip the flow of anthropology into a white water. It began with Carlos Castaneda.

In 1963-64 Castaneda, then a graduate student in UCLA’s Anthropology Department pursuing a traditional program was having trouble<sup>11</sup>; it culminated with his dropping out to pursue a contact with a Yaqui shaman he called, Don Juan Matus. In 1967, Castaneda sent to the U.C. University Press in Berkeley a manuscript which he represented as his experiences with Don Juan. *The Teachings of Don Juan*<sup>12</sup> would become the first in a series, that he would add to until his death. Highly controversial from the beginning, *Teachings* was still deemed academically respectable enough for the University Press to publish it and that, in turn, was enough to get Castaneda re-admitted to the department<sup>13</sup>. No one anticipated that the book would become a huge commercial success, and begin a controversy in anthropology that continues to the present.

While still doing his UCLA program, on the strength of his first book, Castaneda was picked up by one of the most prestigious publishers in New York, Simon Schuster where, in 1971, he published a second book, *A Separate Reality*.<sup>14</sup> He was by then – with the exception of Margaret Mead – arguably the only anthropologist in America whose name was known to the general public and his celebrity made other anthropologist’s teeth grind. Fame, wealth, and a conscious desire to remain physically anonymous – no pictures, no interviews -- put him into a different category from his colleagues.

Within a year Castaneda had completed a third manuscript, which he submitted as his dissertation. Although unorthodox in form and content, like the earlier books it was a memoir narrative, it was accepted as partial fulfillment of his doctorate by the UCLA department. In 1972, it was published as *The Journey to Ixtlan*<sup>15</sup> Like its predecessors, it too became an international best seller which further enflamed the passionate feelings Castaneda now excited every time anthropologists got together and discussed their field. The argument was fueled without question by envy and its cousin disdain: "In 1973 Castaneda received a PhD in anthropology for interviewing a mystical old Mexican..." was the way one critic put it.<sup>16</sup> But the conflict went far beyond academic sociology. Castaneda challenged a fundamental consensus in anthropology: how anthropology should study indigenous cultures.

His narratives of his interactions with the Yaqui shaman argued that one could not understand the Shamanic world view, without becoming a shaman. No informant could ever convey this, because so much of it was experiential. More fundamentally yet, all the Castaneda writings proposed the idea that non-technological peoples were not primitive, and were as capable of insight as their technological counterparts; albeit in different areas of human functioning. Deeper yet, his writings espoused a worldview that anthropology had not seriously considered: That an aspect of human consciousness exists independent of time and space susceptible to volitional control; and, there is an interconnection between all life forms from the most primitive to the most complex which must be understood if the universal impulse humans feel towards the spiritual component of their lives is to properly mature. What had been categorized as “magical thinking” suddenly was proposed as a valid perspective.

Complicating the discussion amongst anthropologists was the effect Castaneda's best-sellers had on the larger culture. The books had become a cross-over phenomenon – a very rare event. The impact on the general society was as profound as it was within anthropology. In fact, the two fed off of one another. It is not within the scope of this paper (I have written of this at length elsewhere<sup>17,18</sup>) but SAC can be seen in pure Kuhnian terms as one response to the reassessment that Castaneda forced on anthropology, as well as a continuation of an effort begun by Swanton and Weiant.

## **PART II THE FOUNDING**

Joe Long and I first met by telephone, shortly after the 1973 AAA meetings. I was doing research for *The Secret Vaults of Time*, a book on the use of Remote Viewing in archaeology. He was an associate professor of medical anthropology at Plymouth State College of the University of New Hampshire. We began discussing Swanton and Weiant and, when I heard the passion of his interest, I asked him why he responded so strongly, particularly given the reaction they had received, and the potential negative effect a public association with these subjects could have on an academic career. He answered that his own experience left him no choice. Then he told me the central story of his life as an anthropologist. It had happened in Mandeville, Jamaica, in 1970, while he was doing his doctoral fieldwork. He was studying and comparing orthodox doctors and folk healers when “the coffin” appeared.

“It was the height of market day and both shops and street vendors had a lively trade going when the thing appeared. It was a three-wheeled open coffin apparently steering itself into the midst of the crowd. There were three live vultures perched at one end and a dead arm hung limply over the side. As if that weren't enough, a hollow voice issued from the coffin's interior repeatedly inquiring the location of one Jim Brown. Hundreds of people saw it – and heard the voice.”<sup>19</sup>

Long was not among them; he was in a nearby area doing other work. Arriving on the scene shortly after the event, he lost no time in questioning those who had seen, and heard, the incident.

“It was incredible. There were literally hundreds of people in that square and they all saw it, and heard the same words. More than that – and infinitely more important – they had all instantly *reacted* with behavior that showed they saw it. Within minutes the shops were empty, even of storekeepers. Everyone ran out to see the coffin and then just milled around, the way people do when they have seen something that has had a powerful effect on them.”<sup>20</sup>

Every person with whom Long talked told the same story (allowing for the minor difference that came from standing at different perspectives). They even broke off the narrative at the same point. “Apparently, it just drove itself down the street and around the corner. Nobody followed. You can understand why.”<sup>21</sup>

At first Long was inclined to think that since the Jamaicans had had time to talk the thing over, it “was a case in which one or two people have the hallucination and then the emotion of the moment somehow carries the others along.” He changed his mind when he learned that “before there was any talking, they had all spontaneously reacted to the event.” Long became convinced that “they truly believed it had actually happened – self-steering coffin, vultures, hollow voice and all.”<sup>22</sup>

What he could not understand was how it could happen. To Long, who in those days “had read not a line about parapsychology except what appeared in newspapers” and who knew “nothing about the boringly repetitious but scientifically important proofs arrived at in parapsychological laboratories, the answer was simple: “It could *not* have happened. I was perfectly well convinced that it hadn’t. Indeed, I was rather dogmatic about that. There was nothing there. Things like that don’t happen.”<sup>23</sup>

He was, however, honest enough to admit that “I didn’t have a clue as to how to handle the matter. There was nothing in my anthropological training to prepare me for that coffin, and, had I seen it myself, I should doubtlessly have had myself committed.”<sup>24</sup>

Instead, he went back to his research on medicine and folk healers, eventually writing his doctoral dissertation on the subject. Through the years he said not a word about the coffin. “What was there to say?” But he never forgot it. “To this day, I can’t explain it except to say there must have been some kind of unique mass telepathic hallucination. That’s



pretty weak, I realize, but how else to explain that several hundred people are in agreement about an event that cannot occur? As for a prank or purely physical explanation: If the CIA got all their geniuses together and developed the most diabolical mind-control device they could think of – well, it would equal that scene. And even if it could, would they pick Mandeville, Jamaica, to try it out? None of it makes much sense even now.

“Going over my field notes convinced me that I had witnessed a number of examples of psi phenomena, but instead of recognizing them for what they really were, I rejected that explanation because it did not fit into the model of scientific anthropology I had been taught.”<sup>25</sup>

Long had decided to face these phenomena head on. Over the next three years, even as he continued with his regular research, a conviction grew. “Parapsychology and anthropology had much to offer each other.” But because of intense specialization, there was very little crossover even within anthropological specialties let alone with another discipline. As Long saw it, “if someone were seeking cross-discipline work, he would hardly begin with something as questionable – at least from an anthropologist’s viewpoint – as *psi* research.”<sup>26</sup>

But Long put aside his prejudices though and began doing interdisciplinary reading. Out of this intellectual search came an inner resolution of the spirit: “Some effort had to be made to begin this cooperation.”<sup>27</sup> He had recognized that for his colleagues to take the effort seriously, it would have to start from within anthropology. Long decided it might as well start with him.

At first he had thought about publishing articles in the accepted anthropological professional journals, but he discarded this idea. Only a small fraction of articles submitted are ever printed, and in the “letters of the editor” sections of these journals only a limited exchange of thought could be achieved.

Just like Weiant, he had decided to take the measure of the field by starting with a paper, and submitted one for the 1973 American Anthropological Association meeting. His topic was medical anthropology and parapsychology. The paper was responsible and well reasoned, if controversial, and the reaction on the whole was favorable. And there had been change since Weiant’s attempt. Long got requests

for more. It was at about that point that we began our conversations. Over the next several months, we spent a lot of time talking about how to use that call. Doing a full-dress symposium for the 1974 conference seemed the way to go.

Long and I came at the question from slightly different perspectives. I was increasingly interested in parapsychological experimentation, but felt parapsychology lacked an understanding of the anthropological element of the phenomena under study. Long felt, based on his own experience, that anthropologists were not prepared to fully comprehend all the dimensions of the shamanic events that so intrigued them. We found common ground in the shared belief that both disciplines needed to deal in a new way with ineffable psycho-spiritual experiences reported in every culture, in every time. The then prevailing assumption that psychics and shamans, were deluded or neurotic men and women who used “magic” to manipulate belief and gain power, was surely correct some of the time. But we felt that didn’t really go anywhere. The same could be said of priests and rabbis who abused their offices. The real question was what happened if the insights of anthropology were wedded to the parapsychology’s tools to honestly examine the experiences so uniformly reported across culture, time, and space. One major influence that we shared was that we had both read Castaneda and saw the implications of his work.

Strangely, we did not in the beginning, or later, ever agree on Castaneda’s validity. Long thought it was all fiction in anthropological clothes. I did not agree, at least about the first three books. When I had been a journalist I noticed that when someone was telling the truth there was a certain “feel” to their descriptions. It was part intuition, admittedly, but there were also clues in details that produced verisimilitude. The first three books had the feel of truth. One point upon which we could agree was that whether the story was true or not in an objective sense, its cultural impact, as it touched thousands of people in and out of anthropology, was a force to respect and understand. Culture has never demanded objective truth as a requisite for belief.

We decided that to put together a symposium in 1974 that would address what we both saw as the core challenge of the Castaneda material – that place where anthropology and parapsychology overlap. Tactically, this meant we needed to assemble a group sufficiently prestigious to not only to get on the AAA program, but to give the whole effort a certain

*gravitas*. We also were convinced that this first symposium should be as interdisciplinary as possible. Finally, facing the reality of science politics, we decided to pick scientists whose principal reputation was not dependent on psychic research. <sup>28</sup>

We decided from the beginning that Long would be the public figure. He was the anthropologist. He had a good university affiliation, a wide network of contacts in the field, and the standing to make the application for a symposium to the AAA. I had no academic association. In 1973, I was the Special Assistant to the Chief of Naval Operations, part of a small personal staff under Admiral Elmo Zumwalt, Jr., transforming the Navy into an all-volunteer meritocracy. It was an experience which would prove useful here, because it had shown me how to build bridges between passionately held and conflicting points-of-view. I also had a growing expertise in something Long did not; the rigorous parapsychological protocols to objectively quantify some of the extraordinary human functioning covered by the Castaneda material, and Long's own experiences. I also knew change of the kind Long sought was possible. I had seen that even within the Department of Defense, as structured as it was, if a small group worked with focus and excellence it could achieve transformations, even of large rigid institutions. But my government work made it essential that my name not be associated with what we were attempting. It would have made the already controversial Zumwalt programs vulnerable if the media had made a linkage between one of his special assistants and a symposium on psychic research and anthropology.

By the time Long approached Professor James Officer of the University of Arizona, who was the 1974 program chairman, he had a list any symposium chairman would be comfortable presenting. Heading it were Professor Agehananda Bharati, chairman at Syracuse University, and Professor Norman Emerson, chairman at the University of Toronto, and a founder and past -president of the Canadian Archaeological Association.

Although personally skeptical of psychic research, Officer and the other committee members were impressed by the panel selection, Officer told me, when I interviewed him in 1976. "We couldn't have called ourselves a science and come to any other decision," Officer said.<sup>29</sup> Perhaps the AAA leadership also felt it was time for an open-minded discussion of the subject. One night at the end of a telephone conversation, Long

decided to call it The Rhine-Swanton Symposium on Parapsychology and Anthropology.

Rhine, of course, was Dr. J.B. Rhine, the unchallenged father of what might be called the statistical school of parapsychology, the reigning approach to psychic research in the United States. By getting Rhine's permission to use his name, the symposium publicly received the blessing of the one parapsychologist even the most antagonistic anthropologist would be likely to know, and linked itself to the approach of science.

Long next got permission from Swanton's son to use the 1952 letter and sent it out as part of the pre-symposium information package.

After much preparation Session 703 was scheduled to begin at 8 o'clock in the morning on 2 November 1974, in the Ambassadors' Room of the Maria Isabel Hotel. Ironically, once again, as with Dr. Weiant, the locale was Mexico City.

After more than a year of talking together, Long and I met for the first time in person at the hotel at the beginning of the AAA week. We very excited, and had visions of the session producing a kind of dignified soul searching debate that would bring about profound change. There was that happy sense that comes with trying to do something you believe will produce change for the good. There was also the *frisson* of risk. I was fearful media coverage might hurt the Navy's fledgling meritocracy programs, and force me to resign. Long had similar concerns. He was fearful of the critiques that he knew would follow, as anthropologists sought to reach a "sense of the community"; a process he told me that could take months. He was very realistic about the negative effect it could have on his professional life. He was not yet tenured.

Emerson, whom we had also both just met in person served us as a classic elder. Grizzled, white haired plain spoken, smoking constantly as he talked, he spread the mentor's umbrella over our effort. Considered by many the "Father of Canadian Archaeology", he shared stories of how archaeology was established in Canada, and his own shift of perspective that had brought him to this symposium. Through his wife, he had met a man named George McMullen, whom he described as a "normal working Joe."<sup>30</sup> He said that in some way he could not explain

McMullen could locate archaeological sites, and accurately reconstruct the history of artifacts.

Through the week that followed against the bass line of traditional symposia, a melody of corridor conversations, and impassioned dining room debates had risen and fallen, centered on the Castaneda sessions, and the Rhine-Swanton symposium. The pain the material caused was obvious, even to me who, surely, missed many nuances. Many senior anthropologists felt everything they believed in about scientific anthropology was threatened. Younger men and women fought over whether the approach was valid on its own terms, or embraced it and began to imagine what anthropology in such a new world would be like. Long, as session chairman, found that senior members of the community seemed to recognize him in a way that had never happened before, and Margaret Mead met with me, and agreed to do a formal interview. No one slept much Friday night.

Saturday morning, when we got to the Ambassador's Room over an hour before we were to begin, people were already coming in, and there was a kind of electric energy. The room was designed to hold perhaps 200 but by the time the session began there were almost 400 people present. They filled every chair and lined every wall two deep. The morning began calmly enough with Jule Eisenbud, a psychiatrist whose famous research on Ted Serios' thought-photography most found fascinating. Professor Robert Van De Castle, past president of the Parapsychology Association (an affiliate of the American Academy for the Advancement of Science) and a member of the sleep and dream laboratory at the University of Virginia Medical School followed, talking about his work with indigenous peoples. The cultural anthropologists particularly could identify with his work testing the psychic abilities of Central American Indians.

Then came Agehananda Bharati. A former Hindu monk he was, despite that heritage, now fervently convinced that the mystical/psychic traditions of the East or any other culture could not yield hidden or "privileged information" because "such information does not exist."<sup>31</sup> He claimed that "psychokinesis is fraudulent – all of it." He played his skeptic's role to the hilt. Coming to the podium for his presentation, he ostentatiously took from his briefcase an enormous magnifying glass and waved it about saying, "Excuse this contraption... I must use it

because I lost most of my vision looking for valid psychical phenomena in India.”<sup>32</sup>

He based his position on the peculiarly anthropological argument of *emic* versus *etic*. Of these two terms. First developed by linguistic anthropology, of which Bharati was a theorist, *etic* originally meant the range of sounds the human larynx can produce, and *emic* this range compared with the way a specific culture chooses and arranges those sounds from the total range available. In rough coinage, however, the words, in 1974, had come to have a different meaning. In the vernacular, *etic* now stood for objective “absolute” truth, while *emic* meant subjective observation.

Next came Dr. Evan Harris Walker, a quantum physicist at a classified government laboratory, who provided a theoretical mathematical validation of psi phenomena. We knew that not many in the audience would understand his highly mathematical presentation, but its presence on the schedule made it clear that serious work was going on in even the hard sciences towards creating a model for these phenomena. After Walker made his presentation, Long proposed that the discussion might proceed to what psi *meant* and what it could be used for.

But that was not going to happen; the audience wanted a confrontation and, at a deeper level, I think, they wanted to begin the process of building a new consensus, after the Castaneda material and the Rhine-Swanton papers had challenged orthodoxy. They wanted the most respected people on each side of the issue to fight it out and, by 11:30 that morning this had come to mean specifically Bharati taking on Emerson. After Emerson’s paper, in which he talked about his work with McMullen, and McMullen’s correct location and reconstruction of Iroquois sites and his correct reconstruction of the history of an argellite carving, it happened. When discussion was opened to the floor, Bharati rose and screaming and waving his hands in the air, said to Emerson “You’re either lying or cheating... I simply don’t believe you... it can’t happen... I don’t care what kind of evidence you’ve got.”<sup>33</sup>

Bharati had now dismissed the work of both Eisenbud and Emerson, but it was the way in which he had done it that caught attention most. The contradiction between Bharati’s emotional outburst and his paper, which called for objective (*etic*) standards to be applied to any research involving parapsychology and anthropology, was not lost on the

audience. One woman anthropologist in a stage whisper that could be heard by all nearby said to her companion, “How much more *emic* can you get than ‘I simply won’t believe it?’”<sup>34</sup>

What surprised me then, although it wouldn’t today, was how much his disparagement lacked substance. Emerson’s reports of the Iroquois work, emphasized location information that had been tested by the spade, and found to be accurate. Archaeology, could provide clear, indisputable, testable information, in a way that the Castaneda work never could that something worth study was going on. Shamanism may be subject to several interpretations, but whether an artifact is where a psychic says it is, is identified as described, and is positioned as Remote Viewed is not. The viewer is either right or wrong. Bharati – and everyone else – instinctively recognized this. Every question from the floor was addressed to Emerson. Only his paper was discussed.

Eisenbud, after giving his own presentation had been quietly watching the exchange from the back of the room. Asked by Long to give a critique of the session, he walked to the lectern and said tartly, “I take special umbrage, of course, at Dr. Bharati’s statement that all psychokinetic phenomena are fraudulent...a flat broadside, a blanket statement. Now this is not *etic*, and this is not *emic*.” Then drawing on the terminology of the physician he was, and looking around the crowded hall, Eisenbud said with a faint smile, “It shares some of the *emic* characteristics [but] it is sheer *emetic*... We find that some thinkers and some investigators rationalize what they are doing in terms of hard-headed super criticism, or ‘scientific critique’ when what they are doing is puking. When they can’t stand certain data, they puke... and it comes out as a paper that gets into the philosophy of science.”<sup>35</sup>

Later that day, I met with Professor Mead for the book interview to which she had agreed. In the excitement of the session, none of us had noticed whether she was present and, to my further embarrassment later, I forgot to ask. But she clearly knew what had gone on, and that was all she wanted to talk about. Many in anthropology apparently do not know that for many years Mead used her prestige, reputation and sheer physical presence to influence science to honestly evaluate the altered states and psychic realms of consciousness so much a part of so many different cultures. It was she who years ago had defended the work of Professor Gardner Murphy when he was attacked by his fellow psychologists for his interest in psychic consciousness; it was she who

encouraged Weiant in 1959; and it was Mead's powerful plea that, after two previous rejections, led to the affiliation of the Parapsychology Association with the American Academy for the Advancement of Science, in which she had held high office. She was, then, the Curator *Emerita* of Ethnology at the American Museum of Natural History.

First standing with her famous cleft walking stick, from which he purse hung and, later, sitting, She told me she had had an interest in this aspect of human functioning because of personal experience with a family friend. One of her early papers, she said, was a study of the friend and her anomalous abilities. Mead was very concerned about getting the subject properly studied. It was obvious she cared a great deal both about the careers of researchers willing to undertake this work, and for the feelings of the gifted individuals able to manifest these abilities.

“The trouble with this whole field...they either want to prove that it is true, or that it isn't true...They already have their conclusions... they don't want to find out exactly what is there... It is this kind of thing that I regard as totally unscientific. You have to realize that in culture after culture the gifted sensitive always doubts himself. You know I advocated, and I still am advocating...that the sensitives are a special type of people... and they occur with about the same frequency in every culture whether they are picked up or not... The seeming disparity between cultures is accounted for be whether the culture does pick them up or not. That's why you seem to get a lot of sensitives in places like the Kentucky mountains, or the Scottish mountains...because the culture expects them to be there; recognizes them when they do occur; and teaches them how not to be destroyed.”<sup>36</sup>

After the interview I met with Emerson and Long and, I believe, Professor Roger Wescott from Rutgers, who was very interested in both the symposium and the idea of starting an organization. As we sat around on the beds in Emerson's room, drinking room service coffee and cokes, all three encouraged me to become publicly involved as soon as I could, pointing out that writing a book about archaeology and Remote Viewing, would give me a public presence in any case. I said I would not publish until I had left government, and shared my conversation with Mead. Emerson had known her professionally for many years and thought her counsel wise, and we all took her words as very good advice.



Throughout the week the conversations amongst Long, Emerson, and myself had expanded, and we three, along with occasional contributions from Walker, Van de Castle, and Wescott increasingly turned to forming an organization within anthropology. Mead's comments gave us the final impetus we needed to take a next step. It was in this last meeting, buoyed by the success of the symposium, and challenged by Mead, that our commitment to create what has become the SAC finally jelled. Unlike the single communications of Swanton, Weiant, and Long, himself, the symposium had been the work of more than one person. For the first time in a formal session, anthropology had heard not only its own, but researchers from other fields as well speak out on these issues; and this discussion had moved things to a new plane. From my experience with the Navy, though, I knew we did not yet have enough people to create the critical mass necessary for a formal organization. Emerson and Long agreed. But there were enough boulders in the stream, in the Castaneda sessions and the Rhine-Swanton symposium, to create a white water of dialogue within anthropology over what these new vistas meant to the field. The key was to maintain momentum.

A second AAA symposium was held in 1975; significantly, someone else chaired it, another was held in Houston in 1977 with the University of Virginia's David Barker as chairman. Both kept the dialogue going, and each produced more AAA members interested in getting involved with a organization dedicated to exploring these aspects of individual experience and general culture. Perhaps the most significant thing to come out of the symposium though was the involvement of Shirley Lee who, energized by what she had heard, decided to become involved and who offered to begin a journal, which she would fund and edit: *Phoenix: New Directions for Man*.<sup>37</sup> It was done rather oddly, however. Instead of moving forward the incorporation and non-profit filing of our nascent organization she decided to create a separate foundation, Phoenix Associates. I don't believe, and Long's memory seems to agree that it ever became a functional entity.<sup>38</sup> Lee's ownership of the journal, and our failure to resolve issues of its relationship to the growing if still informal organization, which seemed apposite and unremarkable at the time – it was her money and she was doing the work we wanted done and, in any case, we had no corporate structure -- would turn out to be a mistake in the years ahead. When it began, however, those of us she invited to serve on the editorial board were just pleased and reassured to discover that more papers than we could publish were being submitted.

That same year, 1977, also saw the involvement of a number of key people in the history of the SAC: Phillip Staniford, an Associate Professor in the Department of Anthropology at San Diego State University became a colleague, as did Professor Wayne Unterreiner, Chair of the Anthropology Department at California State University - Fullerton; Michael Winkleman, a graduate student in anthropology at University of California – Irvine; and, Dan Hawkmoon Alford a linguistic specialist at California Institute for Integral Studies who presented the first paper on linguistics.

My own situation also changed. *The Secret Vaults of Time*, my book on the use of Remote Viewing in archaeology was completed and about to be published, and I had accepted a fellowship at the Philosophical Research Society in Los Angeles, where I had begun what would become a 20 year program of parapsychological experimentation. It started with a project supported by of the Institute for Marine and Coastal Studies of the University of Southern California to use Remote Viewing and a submarine to explore whether parapsychological phenomena could be explained by electro-magnetic radiation.

The 1977 meetings marked a critical point in our history in another way: With the addition of these new people we finally had the critical mass needed to get an organization off the ground, and we began seriously discussing creating a formal academic association. As a first step we decided to attempt a conference of our own. However, the logistics of starting from nothing proved formidable, and the lack of a formal organization made the financial arrangements almost impossible. Staniford though was friendly with officers in the Southwestern Anthropology Association (SWAA) and, they expressed a willingness to have us associate with their conference.

In 1978, Staniford and Shirley Lee organized an all-day symposium at the SWAA meetings scheduled in San Diego that year. Long and I were invited to make presentations. We also decided to submit two symposia for the 1978 AAA meetings, which would be in Los Angeles, with Long as the Chair. Several of us, although not Long, decided to present papers, mine was on Project Deep Quest,<sup>39</sup> the submarine experiment, Alford presented a paper on language and psi.<sup>40</sup> We all felt that this two-meeting plan would be a good model for the unincorporated organization we had begun calling The Association for Transpersonal Anthropology (ATA).

That 1978 SWAA session independent of the AAA was, in many ways, our public beginning as ATA which, after several reincarnations, has become SAC. For the first time we were more than just a symposium on the AAA program. The meeting was also important because as a result of the day-long session two new players became involved, who would play a commanding role over the next several years. Priscilla Lee (no relation to Shirley), was a Stanford trained Ph.D. in anthropology. She, and her long time friend Kathleen Rawlings, had a passionate interest in what we were calling consciousness anthropology. Most important of all, these two women had the time and the commitment to provide the organizational skills, clerical support, and money to move the organization towards incorporation in a way that those of us who were their male colleagues never had.

What none of us fully appreciated at the time was that this first SWAA conference also marked the beginning of the tensions which would almost destroy the organization a few years in the future.

Everyone agreed that the AAA symposia were a good idea, and the SWAA joint meeting also seemed the right move. But did that mean affiliation was appropriate? That was the question. Those opposed argued that holding AAA symposia was the apposite and adequate connection. There was no need for affiliation; there would be no benefit derived from closer involvement, and there might be significant negative considerations, in terms of limiting what we could cover, and the effect the more experiential material might have on the careers of those associated with it. Discussion of this issue though always friendly, became an intense and constant debate.

As in 1974, the AAA 1978 meetings were again in turmoil over Castaneda. It was apparent to everyone as soon as they registered. What was notable was the difference between the reception of our symposia and those concerning Castaneda. The sessions around his work – he was notably absent, of course -- were, if anything, even more tumultuous and conflicted than those four years before. Reputation destroying conflicts broke out over what he represented, and whether what he was doing was anthropology. In contrast, although our symposia evoked some controversy, they did so at a civil level; there was no repeat of the 1974 confrontation between Bharati and Emerson. Even critics thought the sessions a success.

But if we were not identified with the Castaneda battles in the minds of the AAA, these same issues were increasingly a source of tension within our group. Those of us who wanted formal affiliation, were open to experiential events, but we also wanted papers at our meetings that showed intellectual rigor. The contingent led by Philip Staniford, many of whom were undergoing personal experiences of transformation, disdained that, and sought to have ATA focus principally on ritual and personal quest. They saw ATA as an adjunctive organization, where an aspect of themselves not welcome in their academic life could find a home. This schism, which had been with us since 1974, was growing ever more vehement but, once again, we talked our way through it.

1978 was the crucial year in many other ways, as well. Winkleman completed a particularly noteworthy dissertation on the religious practises of South America cultures, and the effect drumming and chanting had on the nervous system.<sup>41</sup> *Secret Vaults* came out, enjoyed success, and the film I had made of the submarine experiment was broadcast on national television. Once again though, it was a new woman who created the most important difference to the organization. Geri-Ann Galanti, then a graduate student working on her anthropology doctoral dissertation: a study of psychic readers and their clients in Los Angeles.<sup>42</sup> She came to both AAA symposia, and went up to Long during a break volunteering to start a news letter for those interested “in the intersection of parapsychology and anthropology.”<sup>43</sup> She circulated a list and, “I don't remember how many people signed up -- I'm guessing somewhere between 50 and 100.”<sup>44</sup> The first issue of *Newsletter for the Anthropological Study of Paranormal and Anomalous Phenomena (NASPAP)* came out in May 1979. It published research reports and short takes on research interests.

Beyond presenting papers at the annual meetings, neither Long nor I was very involved during 1979 and 1980. In February, shortly after receiving tenure, and preparing to join Winkleman at University of California – Irvine on a visiting appointment, suffered his injury and was completely incapacitated for six months, during which time he underwent five operations, some quite major.<sup>45</sup> I was out of the country for much of 1979 year and the early spring of 1980, in Alexandria Egypt, as Research Director of the Mobius Group's Remote Viewing archaeological project.

But, if we were absent, others were very busy. Rawlings and Priscilla Lee, Shirley Lee, Staniford, Galanti, and others pushed forward the task of incorporation. On 25 May 1980 the informal Association for Transpersonal Anthropology became a California corporation. Long was elected president, I think I was vice president and, I believe, the board was Rawlings, both Lees, Staniford, Galanti, Long, and myself. I had just recently returned from Egypt.

On the day of incorporation, Galanti graciously gave her newsletter to the new organization, and its name changed from *NASPAP* to the *Newsletter of the Association for Transpersonal Anthropology (NATA)*. The June 1980 issue describes the first ATA annual meeting, which was held under the auspices of the Southwestern Anthropology Association that year: “The SWAA annual meeting held in San Diego April 9-12, included an all-day symposium entitled, “Impersonal, Personal & Transpersonal: Paradigm Shifting, Anthropology Coming of Age.

“The intent of the symposium, coordinated by Phillip Staniford (SDSU) and Shirley Lee (Editor, *Phoenix: New Directions in the Study of Man*), was to explore the implications of the forefronts of the physical sciences for studies of consciousness, and to study aspects of shamanism, healing traditions, and the interface between anthropology and the study of paranormal phenomena.

“Some of the presenters at that symposium were Fred Wolf, Michael Winkelman, Danny Alford, and James Ebert. The evening program included two films (on *Project Deep Quest* and *The Alexandria Project*) by Stephan Schwartz. Joe Long summarized the program.”<sup>46</sup>

The following year’s meeting, 18-21 March 1981, was held in Santa Barbara. Now that we were a formal entity, ATA shared sponsorship with SWAA. Everything went well in the beginning but, towards the end, deteriorated. The fundamental schism between those interested in only an independent society, focused principally on experiential events, and those interested in affiliation, who wanted to have intellectually sound research papers be at least an equal focus of the conference came spewing into the open. The tension in the two positions had begun to affect friendships, marking a cooling, for instance, between myself and Long and Staniford.

As an extension of this struggle, the journal was also becoming an issue. Shirley Lee controlled it completely, and was in the non-affiliation camp. Increasingly those of us on the editorial board who, in her mind, fell in the other camp, found the relationship strained. It became increasingly obvious that a journal the board and membership did not control was an unsatisfactory arrangement. It is a measure of Galanti's commitment to ATA as an organization that this issue never arose with her newsletter, even though she had owned it exactly as Lee owned *Phoenix*.

At the meetings in March in Santa Barbara, the schism reached a crisis, and a majority of the members voted in favor of affiliation, and that the journal should be properly peer reviewed, not just have a sham editorial board. Lee agreed to this.

Throughout the Spring and summer the situation became nastier and increasingly personal, particularly with regards to the journal and Shirley Lee. She and Long had several very unpleasant phone conversations began to send out derogatory letters concerning Long to several hundred people making charges about him and, apparently, even sent his correspondence to a clinical psychologist trying to drum up a case against him.<sup>47</sup> She saw me as the principal manipulator on behalf of affiliation and rigorous peer review (stemming from my own conversations with her), and began saying that I was controlling Long; a charge she apparently made to his face in one phone call he described as "vitriolic".<sup>48</sup>

On 1 June 1981 Long wrote Shirley Lee after a particularly bitter telephone conversation on the 25 May.<sup>49</sup> The gist of the letter was that ATA was not her private corporation, and she had no right to usurp his, Long's duties as president (she had apparently had some contact with a Dr. Diaz, then president of SWAA. He also rebuked her for not following through on the journal peer review commitment she had made, and ordered her, as ATA president, to enact a proper review policy. Long told her that the failure to implement this policy would result in "complete disaffiliation between Phoenix and ATA."<sup>50</sup>

At this point, as Long recounts it, Lee contacted "Dr. Marking President of Plymouth State College."<sup>51</sup>

A few weeks later, Long felt obliged to send out a letter to the membership spelling out everything that had gone on, and announcing

that “I [have] made the unilateral decision to separate ATA from Phoenix.”<sup>52</sup>

The problem was that Phoenix and ATA were too intertwined, and Shirley Lee would not voluntarily sever the connection. For Long this meant that “ATA was doomed,” and “...I have made the tentative decision, supported by Pris, Geri-Ann, and others, to dissolve ATA and start a new organization, but without Phoenix.”<sup>53</sup> Realizing though that he could not really do this without the consent of the full board and members, he proposed several options, the most realistic of which was to split ATA and the newsletter *NATA* off from Phoenix Associates and *Phoenix*.

Later, in August, a group of us met at Asilomar State Conference Center, where we had a panel at the meetings of the Association for Transpersonal Psychology, and later at the home of Priscilla Lee in Portola Valley. Long could not attend. This time the bipolar tension took over the meeting. Hours and hours of discussion took place at the APA meetings, and afterwards, as we tried to find a compromise most could live with.

From the August 1981 *NATA*: “As some of you may be aware, The Association for Transpersonal Anthropology has been experiencing some internal difficulties. The major conflict has been over the direction that ATA should take. Some felt it was important to maintain academic standards and try to obtain affiliation with the AAA. Others were more interested in exploring experiential and personal approaches, and less concerned with the “legitimization” of the transpersonal.

“At a recent ATA meeting... the attending members agreed that it was possible and important to maintain a balance between the two directions. Therefore, it was voted that candidates for offices of ATA should be committed to this stance.... At present, ATA is being guided by the members of *Phoenix's* editorial board and the Editor of *NATA*. Joseph K. Long, President, and Philip Staniford, Vice-President, have both resigned.”<sup>54</sup>

For most of 1982, the struggle for the soul of the organization continued. In the way typical of small organizations, much of this was done through indirection. One of the stranger manifestations was yet another name change, this time to the Association for Transpersonal Anthropology

International (ATAI). The change was promoted by those who wanted to give our association a greater sense of being an independent unaffiliated association. Those of us who favored the mix of experiential, and intellectual, and affiliation thought the change a waste of effort, but did not oppose it. Besides, with some new non-American members we were international; thus did we become ATAI.

The resignation of Long and Staniford, in contrast, was a much more significant event. Many thought it would lead to the open schism we were trying to avoid. Once again, however, a critical consensus emerged and a series of compromises was worked out; one of which was to elect Staniford – from the experiential camp -- as President for 1983.

The meetings that year were held in San Diego, once again with SWAA. They were marred by genuine tragedy. During the conference, Staniford died unexpectedly. Long, whose own health and domestic problems were overwhelming him retreated to a passive role. As a result, for reasons of personal loss, not professional consideration, the issue of the schism receded into the background.

Several notable newcomers arrived that year, Matthew Bronson who, like Alford his good friend, was a linguist, Dennis Dutton, Patricia Hunt-Perry, and Jeffrey MacDonald.

We held our first solo conference, organized by Priscilla Lee and Rawlings during 27-29 April 1984 at Vallombrosa Center in Menlo Park. To my surprise and, I think the surprise of many others, the issues around the schism came back with more power than ever, and from another quarter. Shirley Lee and Ron Campbell ended their friendship over a dispute concerning control of the journal with such hard feelings many members felt they had to take sides. This time the split could not be papered over and one group of members left to start a new organization. Lee, who still owned *Phoenix* joined this group and took the journal with her depriving the organization of its peer-reviewed journal. The experiential group, having a majority of the board, also took the corporation. To their surprise, I think, Long, who had changed his views recently did not go with them.

That summer, a group of us met at Galanti's house in Venice, and formed a new organization, the Association for the Anthropological Study of Consciousness (AASC). It was a group that had, by now, been together



for some time, and we tended to fall into long established patterns. The founding Board of Directors were many of the same people from the earlier organization, with some new additions: Dennis Dutton, Galanti, Keith Harary, Patricia Hunt-Perry, Priscilla Lee, Long, MacDonald, Rawlings, myself, and Margaret Wilson. Long was asked to be President. Lee once again became Treasurer, and Galanti continued in her role as Secretary. We immediately applied to the AAA to present a symposium at the 1984 meetings. By now the organization had some presence in the context of the larger organization, and our proposal was quickly accepted. A number of us presented papers, and the session was well-attended, although far less dramatic than years past.

Over the next year a kind of minor academic Avignon Papacy existed; two organizations each professing interest in the same things. But the ground was moving. To the surprise of some, particularly those who went with Shirley Lee, that organization and the journal began to wither, and the seemingly weaker AASC (no journal, no corporation) began to thrive. I think this happened because the membership of AASC finally were of one mind, and had one goal – to create a fully professional organization balanced between the experiential and the intellectual, one that would, at some point, affiliate with the AAA. If the timing of the last was a little too vague for some of us, the idea was nonetheless clearly implanted and accepted. The ATA essentially defined itself negatively by what the members did not want, and found that meaningful experiences alone were not enough to sustain an organization. Also without the intention to view things through the formal prism of anthropology, it was unclear what the ATA was about.

AASC's strength could be seen in the fact that its first conference was held in April 1985, with no involvement with SWAA, and drew the largest turnout ever. Once again the venue was Vallombrosa. By the end of the conference we were convinced that AASC would survive and prosper. In 1985 we held another AAA symposium this one at the meetings in Washington, D.C.. The proceedings were published as: *A Summary of Data and Theories from Parapsychology Relevant to Psychological Anthropology*.<sup>55</sup> We also established a new quarterly newsletter, the *AASC Newsletter*, with Jeffrey MacDonald as editor. Galanti had become the Society's almost permanent Secretary, and her university teaching schedule did not leave her time to also do the newsletter. Priscilla Lee continued as she had for some years now, as the Treasurer.

Vallombrosa entered into a relationship with the Institute for Transpersonal Psychology, and independent graduate program, and this made it impossible for us to return there for the following year. So, in 1986, we used the Presbyterian Conference Center in Los Angeles, 4-6 April, beginning a relationship that would go on until the conference center was sold for development in 1990. At the annual meeting we agreed to incorporate, and this was finalized on 10 June 1986. During the board meeting, for the first time, I agreed to hold an office in addition to serving on the board. I had never done this before because running The Mobius Society, as both its Chairman and Research Director, left me little time to take on another non-profit activity. But now, I thought, having found a balance between the experiential and the intellectual, the affiliation issue could be resolved once and for all – by affiliating. I agreed to serve as president, providing the others would back me in getting that task accomplished. Everyone agreed they would. Winkelman was elected for the next term as President. Galanti was Secretary, again, Lee once more was Treasurer. The Board was myself, Winkelman, Rawlings, and Unterreiner

After the conference I went to Washington and met with the AAA, discussing what was involved, and getting the paperwork to begin the process. Priscilla Lee, MacDonald, and Galanti helped me to get the paperwork in order and I submitted the first phase to A. Lehman of the AAA on 24 July 1986.<sup>56</sup> At the time, we had 194 full members paying dues of \$20.00 per year, and 21 student members paying \$15.00. Preparations were completed for the second phase of paperwork that would be required.

While finishing the application I was also planning a new expedition and shortly after the meetings at the Conference Center were held, 4-6 April 1987, I passed the presidency to Winkelman and left on a year long marine archaeological expedition year in the Bahamas.

When I returned I was dismayed to discover that the application had not moved at all. I called AAA and discovered that not having heard from us, they had dropped the matter. Joe Long was elected president for 1988. Our friendship had revived, and we discussed how to get affiliation back on track. I wrote him in June, sending him copies of everything that had been prepared two years before.<sup>57</sup> Now firmly convinced that affiliation was a good idea, Long resubmitted the application and, then, passed the task on to Galanti when she was elected

president for 1989. In November 1990, at the AAA annual meetings, after one more name change to our present Society for the Anthropology of Consciousness, Galanti, who was then president, and who pushed it through, represented us in the official ceremony welcoming SAC into the AAA.

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