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Review: The Scientific Reception of Castaneda

Author(s): Stephen O. Murray

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## SURVEY ESSAYS

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### The Scientific Reception of Castaneda

- The Second Ring of Power*, by CARLOS CASTANEDA. New York: Simon & Schuster, 1977. 316 pp. \$9.95 cloth.
- Castaneda's Journey: The Power and the Allegory*, by RICHARD DE MILLE. Santa Barbara: Capra Press, 1976. 205 pp. \$10.00 cloth. \$4.95 paper.
- Seeing Castaneda: Reactions to the "Don Juan" Writings of Carlos Castaneda*, edited by DANIEL C. NOEL. New York: Capricorn Books, 1976. 250 pp. \$3.95 paper.
- Reading Castaneda: A Prologue to the Social Sciences*, by DAVID SILVERMAN. Boston: Routledge & Kegan Paul, 1975. 113 pp. \$10.00 cloth; \$3.95 paper.

STEPHEN O. MURRAY  
*University of Toronto*

A Castaneda enthusiast, writing the lead review essay in a recent issue of the *American Anthropologist*, chided his profession for "ignoring the accomplishments of Carlos Castaneda" (Wilk, 1977:84). The lack of critical examination of Castaneda's work by anthropologists provides an interesting sociology of science problem—whether the work is ethnography or a hoax that has been ignored.

Some of the raw materials for an examination of the scientific reception of the first four Don Juan books are collected by Noel. Negative professional reactions are represented in short reviews by three eminent anthropologists. E. H. Spicer, an authority on Yaqui culture, reviewing *The Teachings of Don Juan* in the *American Anthropologist*, found "wholly gratuitous any connection between the subject matter of the book and the cultural tradition of the Yaquis" (p. 32), despite the great diversity of Yaqui culture Spicer has emphasized throughout his career. Spicer did praise, however, the vivid representation of the relationship between the apprentice ethnographer and the elderly sage. Weston La Barre, an authority on American Indian peyote use, was commissioned by the *New York Times* to review *A Separate Reality* in 1972. The review was too acerbic a castigation of Castaneda's "pseudo-ethnography" and of its audience's quest for simplified mystical titillation for the *Times* to print. They found an anthropologist willing to hype the book instead. The reviews of La Barre, Paul Reisman, and a dismissal by

Edmund Leach are all included in the Noel book.

Noel, a theologian, believes in Castaneda, gives him the benefit of every doubt, and consistently seeks out any glimmering of positive evaluation (even in La Barre's stinging denunciation). Noel's selective perceptions of the pieces he has collected detract from the volume's usefulness. Social scientists' assessments occupy less than an eighth of the book—perhaps because they fail to take Castaneda as seriously as Noel does. The exclusion of the reviews by R. Gordon Wasson from *Economic Botany* (23:197; 26:98–9; 27:151–2; 28:245–6) is inexcusable, especially since they are brief. Wasson, an ethnobotanist, is the preeminent authority on the religious use of hallucinogenic drugs by Mexican Indians, and his work is thought by de Mille to be a major source of Castaneda's ideas. Wasson reviewed the books as they appeared. Skeptical about the identification of plants, the impoverished ethnobotany, the absence of ethnographic context, and the kinds of speech attributed to the character Don Juan, Wasson's uneasiness was allayed by a copy of a dozen pages of fieldnotes in Spanish—notes that could easily have been manufactured, as de Mille suggests. Although he may have been too easily convinced, Wasson's reviews are an important source for anyone interested in the scientific reception of Castaneda's work. Others also missing from Noel's collection include Crapanzo (1973) and Douglas (1973).

Building on Wasson, Spicer, and La

Barre, psychologist Richard de Mille set out to debunk the scientific pretensions of Castaneda. De Mille presents a persuasive case that Castaneda's fieldwork was conducted in the UCLA library. Elements from C. S. Lewis, San Juan de la Cruz, Wasson, and Castaneda's friend, Michael Harner (a specialist in visionary plant use by the Jivaro), and others are traced—sometimes in purple prose. De Mille argues that the later volumes drew heavily on comments about the first volume, especially on Goldschmidt's foreword to *The Teachings* and on Pearce's *Crack in the Cosmic Egg*. Castaneda's willingness to incorporate criticism is further demonstrated by his most recent book, which seems to be a response to criticisms by novelist Joyce Carol Oates (in Noel). She wrote that all that was left was for Carlos to have an intimate confrontation with a female equal. In *Second Ring* he more than complies: he confronts no less than four sorceresses.

De Mille's argument that the books must have been written in English because of the English clichés and idioms and the lack of Spanish and Indian locutions is less convincing. Spanish is indubitably Castaneda's native tongue, so if he could write such English, he presumably could translate fieldnotes into it. The real point is that these fieldnotes, which are a frequent topic and practically a central character in all five books, should be available to other scholars, if they exist.

The most devastating part of De Mille's analysis for Castaneda aficionados is his chronological ordering of the events said to have occurred during 1961–2. Some were reported in *The Teachings* and other in *Journey to Ixtlan*. The shift in emphasis on hallucinogens was widely noted, but until de Mille put the sequence of events in order according to the dates in the two books, no one seems to have noticed how little sense the total sequence made. This ordering *should* have been done by Castaneda's Ph.D. committee, for, contrary to the popular impression, neither of the first two best-sellers earned Castaneda a graduate degree in anthropology. His 1973 Ph.D. dissertation entitled *Sorcery: A Description of the World*, deposited in the UCLA library and otherwise unavailable under that title, is *Journey to Ixtlan* plus an abstract in academic language.

The major responsibility for assessing the authenticity of what purported to be fieldwork rested on the faculty granting Cas-

taneda's Ph.D. By 1973, accusations of hoax had been published. Frustration at the lack of ethnographic context, at the failure to specify either Spanish or Indian words for key concepts, and at the lack of cultural documentation had been expressed by anthropologists. Presumably, members of the UCLA anthropology faculty were aware of the widespread doubts about the adequacy and existence of Castaneda's ethnography, even if they entertained no such doubts themselves. Presumably, there was a defense of the dissertation at which documentation could have been demanded. We do not know what occurred at that defense, but Castaneda's committee members have neither disavowed their famous student nor have they leapt to his defense, as they might have done if Castaneda had convinced them.

De Mille asks, "Why has no anthropologist complained in public about what happened at UCLA?" (p. 83). The answer is fairly simple. No one outside of UCLA knows enough about what occurred to be *certain* anything untoward happened. There is a presumption that UCLA is competent to decide on whom to confer degrees, and the professionals who evaluated Castaneda's doctoral work are presumed to have sought appropriate documentation from the degree candidate before awarding the degree.

The more fundamental question of the lack of critical scrutiny of Castaneda's oeuvre is more complex, but also reveals more about the structure of academic science. The organized skepticism that is supposed to operate in science is often largely invisible to outsiders. With the exception of La Barre and of Keith Basso (1973:246), anthropologists who believed Castaneda's work was a hoax did not publish their judgements. "Scientific communities rarely undertake exposés of those they regard as incompetent; informal communication usually ensures that their work is treated as suspect or, in some cases, written off" (Barnes, 1972:287). Regarding Castaneda's work as incompetent, many anthropologists did not bother to announce their judgment, thus leaving it to a psychologist wholly unfamiliar with the universe of discourse in ethnoscience to pull together the case that the "ethnography" was a hoax.

Rejection of Castaneda's work occurring within networks of informal communication in anthropology was invisible to scholars in other disciplines and to the general

reading public. Simultaneously, there were visible legitimations: UCLA conferred a Ph.D., the University of California Press published the first volume, a reputable anthropologist, Walter Goldschmidt (who was the cultural anthropologist on the University of California editorial board at the time and chairman of the UCLA anthropology department), wrote a foreword to it, and also included a section of it in his own textbook. Some anthropologists have publicly praised Castaneda's work (e.g., Reisman), and there have been recent exhortations to take it as exemplary (e.g., Silverman, Noel, Wilk). Ignoring what is regarded as non-science may be standard operating procedure in all scientific disciplines, but when the suspect work is taken seriously by some scientists and is widely diffused, there is a need for public discussion. At this writing, it appears that one is belatedly beginning.

Of course, there are no institutional remedies available for scientific malpractice. Castaneda can not be "defrocked." He does not hold an academic position, nor does he present his work in professional forums. But his work can be examined as the ethnography his admirers claim it is. De Mille presented sufficient evidence of fraud for Marcello Truzzi (1977) to call on UCLA to consider revoking Castaneda's doctorate and on the American Anthropological Association to investigate the case. (A good precedent for serious scrutiny of an influential piece of social science work is the American Statistical Association's analysis of the Kinsey reports.) While it is convenient to ignore charlatans, incompetents, and scientific tricksters, anthropology is ill-served by those anthropologists who reject Castaneda's work only in private.

Some will wonder if it matters whether Castaneda invented Don Juan or faithfully recorded his transferences onto an Indian who existed (with whatever cultural background). After all, there are insights into the ethnographic experience of confronting a strange worldview and into the difficulties and preconceptions of those who set out to order other cultures for our science, whether or not Don Juan is a fictional character. Working with informants and dealing with the process of understanding a different worldview need to be taken as problematic and examined, whether Castaneda imagined Don Juan while sitting in the UCLA library or not. An informant of de Mille remarked that Castaneda "certainly

got farther into his native informant's head than most of us do. Of course, if Don Juan was already in his head to begin with, he didn't have far to go to share the native's reality, did he?" (p. 73). To accept the other possible (allegorical) values of Castaneda's work, it is necessary first to establish how far he did go. So long as the work purports to be ethnography, the question of fraud arises.

Fraud in science is always serious, because, "of the work he utilizes, no scientist personally checks more than a small fraction, even of that he is fully competent to evaluate" (Barnes, 1972:279). Scientists must routinely take most findings on trust, and anthropologists must do so more than most. Experiments on laboratory animals or samples of college students have the advantage of being easily replicable. Laboratory animals and college students are readily available, but one cannot order a Yaqui informant. Ethnographic fraud is particularly serious because replicability is more or less impossible. Trust is thus even more fundamental than in other sciences. It seems to me that passing over alleged fraud is a disservice to anthropology, even if the needs of the general public and of other social scientists for critical judgment are ignored. Castaneda has not cooperated with those wishing to assess his work, but that is not sufficient reason to evade the responsibility to evaluate work widely recognized as ethnography and heralded by some as exemplary.

Meanwhile, Castaneda has produced a fifth book. Whether driven by a compulsion to write, seduced by the rewards of doing so, or made impatient by the delay in being exposed, Castaneda keeps producing books. One of the curious features of the reception of Castaneda's work is that *Tales of Power*—the least plausible to skeptical rationalists—met with the greatest professional approval in print (Wasson and Wilk), so it is possible credulity will stretch further and assimilate *The Second Ring*. I can only report that my own credulity does not and that the book is by far the worst written of the five Don Juan books.

The "impeccable warriors," Don Juan and Don Genaro, left this world at the end of *Tales of Power*. They are not revived à la Sherlock Holmes for the new book. Rather, a cabal of sorcerer's apprentices unmentioned heretofore are waiting for Carlos's leadership. Their memories are stuffed with messages and instructions from Don

Juan (he has become a cosmic ventriloquist). We are not told whether the new characters are Indian or mestizo, but along with Juan and Genaro, they are said to form a line of sorcerers extending back to the Toltecs. Since the Toltecs had already dispersed at the time of the Spanish conquest, the question of ethnic identity has a new answer, more confusing than the Yaqui one.

By the end of this volume, Carlos has accepted his role as the new Nagual, so sequels describing the adventures of his band of inept sorcerers are quite likely. Any remaining true believers will be puzzled that Castaneda, having committed himself to the life of the warrior, managed to return yet again and make another book. Most readers will have a difficult time taking the book seriously.

Silverman regards Castaneda's work as "a fitting occasion to re-view the basis of the sociological enterprise" (p. x). Occasions have not been lacking in recent years: the bases of the sociological enterprise have been subjected to considerable scrutiny—much more than has the work of the avatar Silverman proposes. Silverman takes the precaution of bracketing the facticity of the alleged field experience, so his version of an ethnomethodological program falls outside the purview of this review.

But in focusing exclusively on the first volume, Silverman should recall, to those who are historically-minded, the kind of ethnomethodology then current. That was

the era of "breaching experiments" and maximal nihilism about the possibility of doing social science. Perhaps the whole cycle of Don Juan books is a giant breaching experiment, showing how trust allows invention to be taken as ethnography. This might explain deliberate internal contradictions and mounting implausibility, but for the purposes of such an experiment, the designers (Castaneda or Castaneda and Garfinkel) should have published the report of the experiment long ago. Then we could discuss the defects of the design instead of exposing the hoax.

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JANET ABU-LUGHOD  
Northwestern University

When the history of recent sociological thought is written, the 1970s will be identified as the decade in which Marx was finally taken seriously—not the vulgar Marx of the Manifesto (which lent itself all too easily to simple-minded theories of economic de-

terminism, remarkably compatible, except for denouement, with the American faith in materialism and technology) but the analytic Marx of Capital and the social historian Marx of the Eighteenth Brumaire and the Grundrisse. One aftermath of the 1968