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## INTERVIEW WITH AARON CICOUREL

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## ENTREVISTA A AARON CICOUREL

“Aaron Cicourel: In my recent research papers, I talk about how patients lose their sense of social structure; their self-conception, their sense of agency. Hence, both cognitive, emotional, and cultural skills and knowledge are acquired in childhood and extended during early and middle adult life, before beginning to decline. The aging process differentially affects humans with some becoming gradually more cognitively, emotionally, and culturally compromised. To summarize, you cannot have culture without cognition, and vice versa”.

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After receiving his B.A. in psychology and M.A. in sociology and anthropology from UCLA and his Ph.D. from Cornell University, Aaron V. Cicourel accepted posts as a Post-Doctoral Fellow at the UCLA Medical Center, Visiting Assistant Professor at Northwestern University, Assistant Professor to Associate Professor at the University of California at Riverside, Professor of Sociology at UC Santa Barbara, Visiting Professor at the University of Buenos Aires in Argentina, Lecturer in the Department of Sociology and Associate Research Sociologist in the Center for the Study of Law and Society at UC California at Berkeley, 1965-66, Visiting Professor of Sociology at the University of California, Berkeley, 1987, Visiting Professor at Columbia University 1998, and has been Professor at the University of California at San Diego for 40 years, at the School of Medicine (Department of Pediatrics) as well as the departments of Cognitive Science and Sociology.

Cicourel held a Russell Sage Foundation Post-Doctoral Fellowship at the University of California at Los Angeles Medical Center, 1957-58, was awarded a National Science Foundation Senior Postdoctoral Fellowship at London University in England, 1970-71 to study British Sign Language, a Guggenheim Fellowship at the Universidad Complutense de Madrid, 1975-76, and was a Fulbright Lecturer at the Universidade do Bahia Brazil, 1986, and

Universitat Autònoma de Barcelona. In November 2007, he was awarded Docteur Honoris Causa by Université de Fribourg, Switzerland, and in 2008 he received Doctor Honoris Causa by the Universidad Complutense de Madrid in 2005. He was elected member of the American Academy of Arts and Sciences in 1992. Currently he is an Emeritus Professor at the The Institute for Health and Aging, University of California, San Francisco, doing research on dementia, collective memory and social structure.

Cicourel's publications have been translated into several languages; French, German, Italian, Japanese, Korean, Portuguese, and Spanish.

Having a conversation with Aaron Cicourel is a *total experience*. His capacity for bringing up deep theoretical and methodological issues through everyday examples and lay terms makes lunch a highly enjoyable and academically intense encounter. We met on a Monday afternoon, at the home of Carlos Lozares and Anna Estany. Professor Lozares is a longtime friend of Cicourel; he has invited him to his Sociology department at Universitat Autònoma of Barcelona many times, together with his wife, philosopher Anna Estany, they have been on sabbatical leave at the University of California, San Diego (UCSD). The Lozares home is Cicourel's base each time he returns to Barcelona, which is quite often.

**Dafne Muntanyola:** A couple of years ago I received, as a wonderful gift from you, a box of books from your UCSD office that could not make it up to the Bay Area. Some of the titles were *Language, Truth & Logic* by Ayer, Hobbes' *Leviathan*, *The Language Controversy* by Wenner, *The stratification of behavior* by Schwayder, *Cognition et sciences sociales* by Boudon, Bouvier & Chazet and *Journeys through Ethnography* by Lareau & Shultz. The wide scope of subjects represented puts forward that your theoretical interests rise above disciplinary boundaries. So where would you prefer to see your publications in an ideal library?

**Aaron Cicourel:** I hope a research university would accept papers, books, and correspondence for their library. When you are perceived as encouraging one method or theory that criticizes "the mainstream," you are going to offend people rather than be seen as a challenge that needs to be answered with theory and data that returns the criticism. If you don't challenge competition, you can be "blinded" by being part of a dominant group inside a "box" that seldom looks outside the box for ideas and methods by engaging in competitive dialogue across disciplines. A dominant paradigm is important only if it is challenged from within and without. My perspective of a "challenge" is to seek theoretical ideas and methods from other disciplines.

In the social sciences, publication in journals is where the politics are "heavy." I learned about American social science "politics" very early on when John Kitsuse and I sent a paper to a "mainstream" journal.<sup>1</sup> The response was very negative; the editor and referees never gave us substantive and methodological criticism. They just noted that we had strongly called into question the work of Robert K. Merton.

Early in my career, I found it almost impossible to publish in American sociology journals. Hence, I started writing books and then able to publish extensively in applied linguistic journals and books. Sociologists, therefore, have seldom read a large part of my work.

Kitsuse and I finally were able to publish our paper in an American journal called *Social Problems* in 1963.<sup>2</sup> The reason it got published was because of Howard Becker, an editor of the journal and a rising "star" in qualitative methods and within the area known as "Symbolic Interaction." So, if it wasn't for Becker, our paper may never have been published. Although I could never publish a paper in well-known American sociological journals, I was subsequently

<sup>1</sup> John Kitsuse was an American Professor of Sociology who contributed to the sociology of social problems, criminology and deviance. He is primarily known as one of the founding fathers of the labeling approach to deviant behavior in the 1960s and for his contributions to the perspective of constructivism on social problems from the 1970s on.

<sup>2</sup> Kituse, J. & Cicourel, A. V. (1963) "A Note on the Uses of Official Statistics", *Social Problems*, 11, 2: 131-139.

asked to act as a referee for a few papers submitted to these journals. Some years later, I was able to publish a paper in an American sociological social psychology journal because of the guest editor. I only recently published a paper in the English language British journal "Sociology,"<sup>3</sup> and in a British neuropsychology journal. American sociologists are primarily familiar with my books in English.

**DM:** You're are saying you haven't published in mainstream sociological journals because you refuse being in a box?

**AC:** I am not in only one box, theoretically and methodologically, and have done research in different substantive areas; gerontology, education, juvenile justice, cognition, deaf sign language, methods, sociolinguistics, fertility, law, and medicine. Because of my book *Method and Measurement in Sociology*, I am often viewed as being an "ethnomethodologist," but ethnomethodologists have seldom been interested in my work, much less cite it. Ethnomethodologists (EM) and conversation analysis (CA) scholars dislike my work and do not view it as following ethnomethodological theory and practice. Being perceived by sociologists as an ethnomethodologist means being perceived negatively by sociologists.

**DM:** Do you ever have to label yourself then, for pedagogical purposes for instance?

**AC:** Yes, and it has always been very difficult. For example, theory and method devoted to cognitive and linguistic anthropological study of social process and social structure would be most appropriate. I have always liked the remark stated by the French biologist François Jacob (1998, 767): "Our breakthrough was the result of 'night science': a stumbling, wandering exploration of the natural world that relies on intuition as much as it does on the cold, orderly logic of 'day science.'"<sup>4</sup>

**DM:** You have been for more than 30 years Professor of Cognitive Science, Pediatrics, and Sociology at UCSD. ¿Where did your interest for medical environments come from? ¿How did your work with (and among) medical professionals shape your research?

<sup>3</sup> Cicourel, A. V. (2013). "Origin and Demise of Socio-cultural Presentations of Self from Birth to Death: Caregiver 'Scaffolding' Practices Necessary for Guiding and Sustaining Communal Social Structure Throughout the Life Cycle", *Sociology*, vol. 47, 1: 51-73.

<sup>4</sup> Jacob, F.; Monod, J. (1961), «Genetic regulatory mechanisms in the synthesis of proteins.», *J. Mol. Biol.* 3: 318–56, June 1961, PMID:13718526. Jacob, François. *Of Flies, Mice and Men*, translated from the French edition and published by Harvard University Press, 1998

**AC:** I was drafted in the US Army during the Korean War. Because I had a degree in Experimental Psychology, the US Army sent me to a military hospital in Indiana, I worked with Neuropsychiatric patients, most of whom were psychotic. We also had open ward patients, mostly with a diagnosis of neurosis. All patients were very young; the average age was approximately 19. My work was to interview them after they arrived from airplane trips from Korea and California.

**DM:** You were young as well.

**AC:** I was, 22, 21. I worked primarily in a locked ward, so I had to deal with very serious patients, such as those with schizophrenia. I also was required to help give patients electroshock therapy, and I had to discuss patients' illness with their families. I also would give patients psychological tests, because I had a little training for it. I stopped doing testing and did more of what I would call psychological social work. I interviewed each patient, and I had to spend considerable time with each of them. It was very difficult because the people who took care of them often were not certified nurses but primarily male helpers, and not really trained to work with mental patients. They could be physically very abusive with the patients. I mean, they were often physically abusive if they became agitated.

Sometimes, the patients would fight male "caregivers" on the wards. One night I was on overnight emergency duty, and was called while sleeping and told I was needed upstairs at the "closed" ward where the most psychotic patients lived. I went upstairs, and I opened the door and the male nurses were literally throwing patients across the ward, patients were jumping over barricades the male nurses had constructed in the corner of the ward using patients' beds. It was terrible. I tried to get the patients to calm down and go to their beds. So I ordered everyone to stop fighting. I knew all of them. You had to be careful with some of the patients; they would be standing next to you and if you moved your arm suddenly (gesture), they might hit you. I called for help and personnel from other wards arrived.

My experience with psychiatric patients while serving in the U.S. Army also taught me about how bureaucratic authority could be manipulated such that an organization's bureaucratic practices could be altered. The army patients could be easily abused because there was not sufficient oversight during evening and weekend hours. One patient was African-American and belonged to an evangelical sect in South Carolina. His religion required that he pray at different times of the day.

The patient told me that one evening at a previous military training camp, the soldier was praying by his bed. Other soldiers told him to be "quiet." The soldier had

become so involved with his prayers that he apparently became semi-unconscious as if in a trance. The officer gave him a direct order to stop his prayers. I assume the soldier could not hear the officer. The soldier was arrested. He resisted arrest and was physically abused and sent to a high security military prison where he continued his deep prayers and punished each time with solitary confinement. After some time, I do not know how long, he was sent to our hospital for a psychiatric examination. I was the first person assigned to interview the patient. I had seen persons like this soldier when I was an adolescent dating a girl who belonged to a small evangelical sect. The group held regular meetings in a small building that had been a grocery store and converted into an evangelical church. One night I was invited by the girl to come to the church so that after the religious service we could go for a walk. I arrived early and observed a member of the congregation involved in a trance as everyone chanted.

When I spoke to the soldier, I thought of the evangelical church I had known in Los Angeles and inferred that the soldier had been doing something quite similar. I spoke to a psychiatrist I knew about the patient. He said he would have the authority to do something soon. Chief Neuropsychiatric Officer, a Colonel, was going on a leave of absence. A few weeks later, the Colonel was on vacation. I spoke to the Captain, now temporarily in charge of the Neuropsychiatric wards, to see if we could discharge the soldier. I had previously worked in the administrative section of Neuropsychiatry had learned how to discharge patients. I filled out the papers, the Captain signed the papers, and we were able to send the soldier home.

**DM:** Did you have the authority?

**AC:** The captain had the authority and he allowed me to prepare the papers. Some years later, I was observing a friend of mine in psychology, David Premack, walk his baby chimpanzee around the university campus.<sup>5</sup> Premack was one of a small group of psychologists attempting to teach chimpanzees a "language." He used operant conditioning methods derived from behaviorist studies with pigeons. He started to apply operant conditioning with humans at a

<sup>5</sup> David Premack is Emeritus Professor of Psychology at the University of Pennsylvania. He has focused on cognitive differences between the intelligence of animals and humans. Premack's first publication was a new theory of reinforcement (which became known as Premack's principle) and he later introduced the concept of Theory of Mind, with Guy Woodruff. A nonverbal method for testing causal inference designed by Premack made it possible to show that both young children and chimpanzees are capable of causal inference. He also showed that chimpanzees are capable of symbolic behavior. Premack, D., & Premack, A. J. (2003). *Original intelligence: unlocking the mystery of who we are*. New York: McGraw-Hill.

hospital for mentally disturbed patients not far from Santa Barbara. I wanted to learn more about his theoretical and methodological ideas, and how the conditioning could be applied to patients whose cognitive abilities had deteriorated considerably.

I also learned something about health and aging while engaged in dissertation work at Cornell University. Research with the aged was not part of a larger plan. I thought the aged would be a good group from which I could learn about how aging affected a person's conception of self. I was able to meet them at the Senior Citizens Center in Ithaca, New York. I would eat lunch with them every day, so I got to know the people there well, and selected some with high income, low income, and some had been in Cornell, they loved being there because it reminded them of their youth! (*laughs*). I learned a lot by observing them; people often spoke of their years of deterioration. There was one woman who was very special for me. I used to drive her to the Senior Citizen group lunches because she couldn't walk well. One day she said "Aaron, you know, I am getting worse, I want you to hurry and finish your interviews because I think I am going to die soon". This really upset me, but I did see her many times over the few weeks. She died a few months later. Because of Mimi and some others, I decided not to study the aged anymore, it was depressing, just depressing.

**DM:** It was harder than working with the patients from Korea?

**AC:** It was harder, because, for one thing, the patients from Korea were seriously ill; the patients with dementia were aged people who were gradually losing their self-awareness. So what I kept noticing were some things that seemed patterned behaviorally, call it a form of invariance. Some things they did were difficult to describe. Yet quasi-invariant patterns seem to exist, a term that clinicians use often when speaking of the deterioration of memory. In my dissertation study, one person, Tom, was 86 years old. He worked in the Physics department in Cornell and blew all the glass tools (*gesture*). He lived alone, he had one living second cousin in NJ, where he grew up, and he used to work for Thomas Edison. Tom insisted that he and others in Edison's laboratory invented new equipment but never received credit for their inventions. Edison was an accredited engineer and an entrepreneur; he took credit for everything. Tom, I noticed over the year, was beginning to deteriorate. One day the minister from the church he went to called me, he knew I was doing a study and he said: "You know, Tom is not coming to church anymore." This was very important to him, it was the only social contact he had. So I went by and I saw him and I said Tom, what's happening you are not going to church and he said I don't want to go, I'm embarrassed, I can't remember the names,

I can't remember the faces. Tom's experience is typical. The descriptions by Tom were subsequently remembered when I started working with patients with dementia. I hear very similar stories from patients and family members.

**DM:** In your paramount book *Cognitive Sociology* (1974) you opened a theoretical and empirical space for introducing the way social structure creates frames of reference for research on everyday talk and action.

**AV:** *Cognitive Sociology* is a book that few people read. But there are a couple of papers I really like. One is on linguistics and Chomsky.<sup>6</sup> Briefly, this collection of papers was an early and weak attempt to address cognition and language use as essential elements of sociological theory and research. I do not think the book was successful. A few sociologists did begin referring to cognitive issues but few have taken language seriously. The language of open-ended and fixed-questions are embedded in serious aspects of applied linguistics and linguistic anthropology, especially in all field research and survey research. These areas are also strongly constrained by assumed standardized comprehension attributed to respondents' interpretation of the questions posed.

**DM:** Already at this point of your career, you show an interest for collective memory, part of your current research on diagnosing dementia. Towards the end of the volume you write:

*The organism's ability to execute many disconnected conceptual processes simultaneously hinges on the difficulty of the tasks involved, the number of events it can attend and follow, the ability to retain and retrieve information and the way experiences are socially organized* (1974, 166).

**AC:** I attempt to address aspects of the quotation you cite in the following paper. (2014) "Collective memory as a fusion of cognitive mechanisms and cultural processes", *Revue de Synthèse*, 135, 6e série: 1-20. What you quote has to do with limited capacity processing of information during all forms of social interaction. What sociologists often fail to recognize is that when we talk about limited capacity processing constrains, our methodology is having an impact on what we can infer about what we claim to know about communal normative behavior.

<sup>6</sup> Noam Chomsky is an American Linguist, Philosopher, Cognitive Scientist, Logician, political commentator and activist. Sometimes described as the "father of modern linguistics", Chomsky is also a major figure in analytic philosophy. He is credited as the creator or co-creator of the Chomsky hierarchy, the universal grammar theory, and the Chomsky-Schützenberger theorem. He has spent most of his career at the Massachusetts Institute of Technology (MIT), where he is currently Professor Emeritus.

Field notes, for example, truncate the amount of information you seek to obtain and comprehend. Even information from videotapes and audiotape recordings are subject to limited capacity processing: by continually observing a video tape and listening to a recording over and over again we can increase but not eliminate information loss. The research is labor-intensive, and social scientists interested in large samples cannot tolerate having to engage in detailed probing of fixed-choice question to help offset the loss of information from survey data. Clinicians, for example, are constrained by the cost of obtaining detailed information, but what saves them is their clinical experience in making inferences from qualitative, analogical clinical intuition from many individual interviews. The clinician's interaction with patients is contingent on moment-to-moment questions and responses and labor-intensive subtle healthcare delivery.

**DM:** What about the Rational Choice Model?

**AC:** My research on medical diagnostic reasoning convinced me that medical clinicians do not use a Rational Choice Model. Today it is called Evidence Based Medicine; the use of correlations between digital-based indicators of illness. Clinicians cannot avoid analogical reasoning when they seek a diagnosis, but they prefer to assume their essential source of information will be from digital evidence. If you do surveys in sociology, even when you do a pretest, you miss exploring ethnographic sources of information by detailed probing of responses. I once designed a questionnaire for a sample of 5000 respondents. I learned that getting information with a survey is useful for generating hypotheses, but surveys require validation by the use of randomly chosen subsamples from the initial survey. The small subsample can be the source of labor-intensive exploration of respondents' comprehension of each question by probing their understanding of what they thought was being asked. In other words, the respondents' ability to explain their responses can clarify their comprehension.

Surveys produce plausible correlations. But those correlations need additional research beyond tabulated outcomes. Although correlations always signify useful hypotheses, the question is how you then pursue them. Nobody wants to invest in qualitative data. I had a few interesting friends in the biology department at UCSD, especially what they call behavioral ecological study of nonhuman animals in the wild. They bring some animals into the laboratory for controlled experiments that further validate their findings of behavior in the wild. Biological behavioral ecologists are scientists who really do ethnography. Jeanne Altmann, a primatologist, published a very nice paper on how to sample behavior in the wild.<sup>7</sup>

<sup>7</sup> Altmann J. (1974). "Observational study of behavior: sampling methods". *Behaviour*. 49(3):227-67. (Cited 8000 times in GoogleScholar).

**DM:** What do you think about trendy neuroscience?

**AC:** Many exciting things have been happening with animals. Neuroscience has heavily influenced many areas of study outside of biology, especially psychology and a small number of linguists and economists. I was fortunate in knowing Martin Sereno at UCSD.<sup>8</sup> Marty is still affiliated with UCSD, but has for many years been a Professor at the University of London.

**DM:** Yes, I met him at UCSD.

**AC:** From Sereno I learned about the difficult problem of looking for neurological correlates of human behavior. I was once asked to referee a linguistic paper that attempted to correlate linguistic behavior with regions of the brain and their enabling particular cognitive skills. I asked Sereno about how I should interpret brain scans that suggested atrophy (I think the term refers to brain cell death or apoptosis). He said this is a typical correlation that can be inferred. He noted there could be several million neurons in a designated area. People I study know that, but their clinical and neuroanatomical knowledge leads them to useful informed speculation. And they are very good at it, and are able to follow additional deterioration over time. My concern is: what kinds of cognitive and cultural knowledge and everyday practice are affected, and when it occurs, under what socio-cultural contexts do they begin to emerge, and how do patients and others cope with such behavior.

**DM:** In conversation in your San Francisco office in 2010, you claimed it was time for sociology to look at the "process of de-socialization" that comes with ageing. I found this approach to medical conditions such as Alzheimer and frontotemporal dementia fascinating. How does this relate to the need of studying memory as a social activity?

**AC:** Back in the 1970s, I did some research on children with enzyme deficiencies I never published. I was able to observe children in a hospital and at home in Southern California. The children I studied in the hospital and as outpatients were under the care of two well-known colleagues in Pediatrics and Neurology at the University of California, San Diego Medical Center. One group was diagnosed with Lesch-Nyhan disease, the second group was diagnosed with Hurler-Hunter disease and San Fillipo disease. Both diseases could be devastating for the patient and the family. Lesch-Nyhan patients would chew their lips and fingers.

<sup>8</sup> Martin Sereno is an American Professor of Psychology, Chair of Cognitive Neuroimaging and Director of the Neuroimaging Centre at University College London and Birkbeck College. His research aims to map visual, auditory, somatosensory and motor areas in the human brain, and he has pioneered new techniques in brain imaging.

The enzyme deficiency affected patients variably. The severe Hurler-Hunter cases I observed were unable to acquire even modest cognitive and cultural skills. San Filipo patients were able to acquire many cognitive and cultural skills and adequate language skills. Neither lived long. I think most died by twenty years of age.

In 1975-76, I was fortunate to have received a Guggenheim award and conducted research on Sephardic Jewish communities in Malaga, Madrid, and Barcelona. I am trying to write a book on the Sephardic research. The fellowship enabled me to also conduct research in Madrid at the Hospital La Paz in the pediatric neurology clinic. The children at the La Paz Clinic enabled me to study one family in which two children had the enzyme Hurler-Hunter deficiency, and the third was normal but was a carrier of the gene. The family originally was from Bilbao. Another family with two children with San Filipo patients were from Toledo. The Madrid family lived in an upper middle-income area, and the family in the Toledo lived in a low-income area very close to the Toledo train station where the husband was employed. The older patient from the Madrid family, 12 years of age, had to be observed continuously; unless watched continuously, she would destroy objects throughout the house, literally ran in front of a car when her mother and I went for a walk one day. The Madrid family was strongly affected; it was impossible to have a home or a social life. Only the wife's mother could take care of the older child when the parents wanted to go out to dinner or be with their friends. Friends were never invited to their home. The Toledo family also had baby with a Down's syndrome, but their social life appeared to be quite active.

I observed similar problems with Lesch-Nyhan and Hurler-Hunter patients in Southern California. The research was depressing and I could not motivate myself to publish my observations and field notes. The above examples and my current research on dementia convinced me that it was important to study the effect that neurobiology could have on a person with compromised cognitive and cultural development, and how cognition and culture would gradually "disappear" with elderly patients with severe dementia.

I inject a personal note. My own research on dementia has been influenced from observing and interacting with my older sister, two and one-half years older than me. She has extreme Alzheimer's disease. I have watched her for over 12 upsetting years. I have learned considerably despite the pain of seeing someone you love losing their sense of self, their cultural ability to identify themselves as a cultural social self.

In a draft of a research paper I recently completed on Primary Progressive Aphasia, I talk about how patients lose their "sense of social structure." The sociological lesson is that cognition and culture gradually disappear with progressive dementia. "Losing one's sense of social

structure" refers to a gradual loss of real-life cognitive and cultural performance. I believe sociology should integrate its level of analysis with other social sciences, particularly with cognitive science and developmental psychology, and strive to identify invariant elements of cognitive and cultural changes over the life cycle.

**DM:** ¿Would you say that this invariance, understanding this loss of togetherness, is only attainable with an integrated model of social analysis?

**AC:** During my weekly meetings at Berkeley with my long-time friend and colleague at UCSD, Roy (D'Andrade), we often speak about the invariant aspects of culture and cognition.<sup>9</sup> For example, that culture cannot exist without cognition, and cognition cannot exist without culture. Tomasello's book (1999) on the cultural origins of cognition is an important book that has been very useful; he clearly recognizes the necessary connection between culture and cognition, despite not addressing the cultural part of cognition in daily life problem solving as D'Andrade did (1981; 1995; 2001).<sup>10</sup>

**DM:** Durkheim's prologue to *Les formes élémentaires de vie religieuse* (The Elementary Forms of Religious Life) illustrates what you are saying pretty well: *La connaissance est formée de deux sortes d'éléments irréductibles l'un à l'autre et comme deux couches distinctes et superposées* [Knowledge is made of two types of elements not reducible to one another, like two distinct and superimposed layers.] (1998:21). It does seem that if you talk about cognition among sociologists of culture or knowledge you are accused of reductionism.

**AC:** Yes, it is an unfortunate mistake. Each level has its own organization (Cicourel, 2006); and each earlier level

<sup>9</sup> Roy G. D'Andrade is Emeritus Professor of Anthropology at UCSD and one of the founders of the theory of cognitive anthropology. A unifying theme in much of his work is the problem of identifying and describing cultural models (also known as folk models, or the often implicit, culturally shared ways that people assume the world works); in recent years he is particularly concerned with conceptualizing cultural models through schema theory. In 2002 he was awarded the NAS Award for Scientific Reviewing from the National Academy of Sciences.

<sup>10</sup> Michael Tomasello is an American developmental psychologist, Co-Director of the Max Planck Institute for Evolutionary Anthropology, Germany. Tomasello gathered evidence to prove that strictly humans share two dimensions: reading intentions and interacting with others socially. Joint attention is another cognitive capacity that he explores. He found evidence to prove that infants can begin to engage in joint attentional interactions because they understand that others around them are also agents of joint attention. It means that infants have the capacity to identify with adults and distinguish the underlying goal of those around the infants through their actions. Books include *Origins of Human Communication* (MIT Press, 2008), and *Why We Cooperate* (MIT Press, 2009).

of biological evolution enabled subsequent, more complex levels to emerge in which cultural evolution was essential. The more evolved levels, however, cannot be “reduced” to earlier levels despite their interdependence. People with mental deterioration such as Alzheimer’s Disease and Primary Progressive Aphasia gradually lose the knowledge of objects and events and their self conception. Patients with aphasia can tell you about an activity and describe cultural and cognitively relevant elements, an object’s utility, that is what you do with it, but not remember the name. A particular man I am studying could not remember the name of a fruit, a pear, but he recognized its appearance and knew he liked eating it.

**DM:** So he had experiential knowledge about it.

**AC:** He was capable of consciously describing cognitive, emotional, and cultural features of objects, events and experiences. Modern medical practice is conducted under bureaucratic constraints that are monetary and organizational. They must abide by normative time constraints that limit how long they interview the patient. For example, a patient may consult a neurologist and is asked what “brings” her or him to the clinic. The patient may respond by saying he cannot remember particular words when speaking with her or his family or with friends, neighbors or colleagues at work. The neurologist can readily recognize elements of a diagnosis of Primary Progressive Aphasia (PPA) because of the patient’s spontaneous problems with several words. He asks for examples, but he does not always have the time to patiently probe him “When did you last have this problem? Where were you? Who were you with?” Such details may not be elicited, yet the clinician can readily perceive that she or he is receiving elements often associated with early onset of dementia.

Thus physicians are obliged to truncate interviews by using fewer questions than they would like to ask, and often may ask leading questions. One advantage in diagnosing appropriately is often due to seeing patients at a time when the disease is at a state of development that their training has made possible to recognize that one or more possible illnesses may appear evident. Physicians pursue their skilled intuitive inferences in order to first eliminate alternative possible contributing pathophysiological possibilities, but often find they are unable to pursue questions requiring more extensive moment-to-moment probing to acquire more extensive analogical evidence about the patient’s subjective experiences motivating her or him to consult a physician.

The study of medicine is of special interest because it consists of achieving a diagnosis by first using analogical reasoning to understand the patient’s perspective of their symptoms and then seeks complex sources of basic and

applied science to go beyond correlation outcomes. That is, seeking evidence within pathophysiology and biochemical research. An important sociological insight is the fact that clinical medicine is embedded in informal and formal daily life bureaucratic practices. Physicians have difficulty recognizing how the very way in which they conduct their work is in itself dependent on cognitive and cultural processes that frame the way healthcare delivery is accomplished.

Sociologists studying medical issues recognize but seldom focus on the study of diagnostic reasoning and treatment as bureaucratic practices constrained by monetary issues, local interpersonal relations and political (Congress and The White House) healthcare policies. Many medical sociologists focus on the role of differential economic, social and ethnic access to health care; cultural views of health care which can impede appropriate healthcare delivery. This focus, however, does not address how local and state politics and federal congressional, judicial, and executive office seldom engage in systematic studies of the role of the relentless internal activities of special interest groups. For instance, by doing ethnographic studies on how their huge monetary budgets diminish the quality and coverage of healthcare delivery. Social scientists are obviously aware of these special interests, but there are few long-term studies of how political lobbying groups, corporate money, congressional politics, and a president’s signing off statements about how a new law becomes interpreted is implemented state by state, county by county, and city by city by healthcare professionals, and within particular bureaucratically constrained health care facilities.

**DM:** What you are saying about the clinic reminds me of Michel Foucault and his volume *The Birth of the Clinic*.

**AC:** Pierre Bourdieu first told me that Foucault was a very accomplished historian. I have only read a little of his work, I had expressed my concerns to Bourdieu about how Foucault’s views have influenced many social scientists whose research interests and findings often seemed to be weakly related to the kind of data used by Foucault’s historical research, the kinds of conclusions reached. Pierre Bourdieu read my book with John Kitsuse on the educational system and he was really surprised, he had never thought about going into a classroom with a tape recorder and record what the teachers were saying, much less parents when interviewed in their homes while their children were tested by my research assistants.<sup>11</sup> I did not think Foucault’s theoretical generalizations from historical materials were congruent with the way contemporary

<sup>11</sup> Cicourel, A. & Kitsuse, J. (1963) *The Educational Decision-Making*. University of California Press.

ethnographic studies of social science areas and health-care cite his work. The key issue for me is the extent to which we can compare ethnographic research materials available today to the kinds of data Foucault had at his disposal.

**DM:** Would you call yourself an ethnomethodologist of the medical world?

**AC:** I don't think of myself as an ethnomethodologist. Garfinkel called the kind of research I do "remedial." That is, an attempt to alter contemporary sociology's orientations to the use of survey data and field research. Ethnomethodology (EM) and conversation analysis (CA) focus on different data. Conversation analysis is very important at a very micro level of analysis, but their data, even when using materials from everyday settings, do not address more abstract substantive levels of analysis of interest to sociologists. Some applied linguists and linguistic anthropologists have employed CA methods to segments of connected speech events. EM and CA tend to focus on independent theoretical and methodological commitments. These commitments make it very difficult for contemporary sociologists to use their research strategies.

I strongly suggest you read a chapter I wrote about 10 years ago entitled "The ambivalent relationship between ethnomethodology, conversational analysis, and mainstream sociology in North America." The chapter was originally presented by French sociologists, several of whom were closely associated with Bourdieu, at the University of Liège, Belgium, February 2006 and published in *Rapports ambivalents entre sciences sociales européennes et américaines*.<sup>2006</sup> Arcipelado Edizioni, 243-275, 2006.

**DM:** Boxes define expected practices then.

**AC:** Exactly, it is a cultural definition of an area of science that scientists recognize but do not debate. I remember that I was on the committee in 1962 that was trying to reconcile whether or not biochemistry should be chemistry or biology. They let it stay in biology, but today in UCSD it is chemistry.

**DM:** Your books on *Method and measurement in sociology* (1964) and *Advances in social theory and methodology: Toward an integration of micro- and macro-sociologies* (1981) are useful readings for understanding the use of methods in sociology, and specifically, for moving past the false qualitative and quantitative dichotomy. In an interview from 2004, you stated your position by claiming: "I am NOT Opposed to Quantification or Formalization or Modeling, But Do Not Want to Pursue Quantitative Methods That Are Not Commensurate With the Research

Phenomena Addressed" (in Conversation with Andreas Witzel and Günter Mey).

**AC:** The theoretical perspective I suggest is that every theory must have a methodological side, otherwise its utility is limited for guiding empirical research. Durkheim and Weber each carefully employed whatever data was available to them, but there is a fairly strong interest by sociologists, especially in Europe, and the US, to focus on theory or favor publications that stress theoretical issues while diminishing the equally necessary focus on methodology and theory driven substantive research and replication.

I took several Philosophy classes at UCLA. I learned about analytic philosophy, I audited a class with Reichenbach, and a few Visiting Professors from Oxford and Cambridge.<sup>12</sup>

From my undergraduate studies in experimental psychology, I realized the necessity of understanding the role of theoretical issues and applied applications of mathematics for creating models and predictions. While studying for my master's degree in sociology and anthropology at UCLA, I began taking advance methods classes in psychology, lower level mathematics such as trigonometry, a sequence of three classes in calculus, a course in advanced algebra, and auditing classes in differential equations and advanced calculus. I had also taken all of the statistics classes in psychology and sociology.

When I went to Cornell for my doctorate, I minored in mathematical statistics, My sociology adviser was Robin Williams and he was very supportive in my concern with finding a "metric" that would help me understand if daily life reasoning and decisions could be viewed formally without sacrificing the essential analogical nature of connected speech events, gestures, prosody, and facial expressions.<sup>13</sup> One of my doctoral committee members

<sup>12</sup> Hans Reichenbach developed logical positivism in the XXth century, a scientifically inspired philosophy and an uncompromisingly empiricist epistemology. Criticism and justification of scientific methodology formed the core of almost all his philosophical efforts, which he promoted in a crescendo of books, in the journal *Erkenntnis*, which he founded and edited with Rudolf Carnap, and within a group of philosophers, mathematicians and scientists he led in Berlin. The fruits of some of his insights are only belatedly having their full impact. For example, several of the recent accounts of causality employ ideas that can be traced to Reichenbach's *The Direction of Time*.

<sup>13</sup> Robin M. Williams was a Sociology Professor at Cornell University and devoted much of his career and writing to studies of intergroup tensions, race relations, war and peace, ethnic conflict, and altruism and cooperation. His best-known works include *The American Soldier* (1949); *Schools in Transition* (1954); *What College Students Think* (1960); *Strangers Next Door* (1964), *American Society: A Sociological Interpretation*, and *The Wars Within: Peoples and States in Conflict* (2003).



was Jack Kiefer in the mathematics department.<sup>14</sup> Kiefer would not attend my doctoral preliminary examination after I showed him how sociologists applied statistics to social research. He told me about his objections to social statistics and I have always remembered his teachings.

At Cornell, I also became intrigued with studying both formal and applied linguistics. The work of Franz Boas and his student Edward Sapir had already been familiar because of a course I taken at UCLA from an outstanding linguistic anthropologist named Harry Hoijer.<sup>15</sup>

**DM:** You have been a Professor in the Department of Cognitive Science since 1988, and written numerous papers on the interaction between language, action and cognitive mechanisms. Mainstream sociologists fear taking into account the role of cognitive mechanisms as integral to culturally framed social interaction such as the representational re-description model by Annette Karmiloff-Smith (1992) or Michael Tomasello's (2003) analysis of human development. Recognizing the interaction of cognition and culture would deepen the role of social interaction in research on topics that matter to sociologists. In other words, empirical research at the cognitive and linguistic level of social interaction is for me the foundation of the notion of social structure. It would be a very radical shift in the study of social structure and would modify the way classic macro issues are studied in sociology, such as social stratification, domination or change.

I believe that your own research enables the integration of cognitive and social levels of research. For instance you say "*The integration of different levels of analysis (···) during research on social interaction requires noting the central role played by connected speech acts and conversation in daily life problem solving tasks or mundane activities*" (2006: 28). Would you agree with my analysis, and could you comment on the need for conceptual integration in sociology?

**AC:** I am convinced of the importance of Karmiloff-Smith's work because of her developmental work on the

continuous impact of memory on human evolution as essential for understanding the socialization of the young, and essential for the study of adult social interaction and such abstract notions as attitudes, beliefs, decisions, norms, and values.<sup>16</sup> Levels of analysis should be recognized as a necessary move towards interdisciplinary research instead of conceptualizing them only within a sociological "box."

**DM:** Howard Becker claims there was no theoretical or empirical homogeneity within the members of the several generations of the Chicago School. Do you agree with his claim that the Chicago "School" was a School of Activity and not a School of Thought? Does it make sense to talk about a Chicago School at all?

**AC:** I agree with Becker's approach to symbolic interaction. Becker has consistently been one of the most important students of the symbolic interaction group.<sup>17</sup> He was perhaps the first to seek a systematic approach to how field research should be pursued by the group. He also published several important field studies using his view of symbolic interaction. He and Strauss were supportive of early work associated with ethnomethodology, including a paper by Garfinkel.<sup>18</sup> Becker made it possible for John Kitsuse and myself to publish a paper that other sociology journals would not publish.

<sup>16</sup> Annette Karmiloff-Smith is a professorial research fellow at the Developmental Neurocognition Lab at Birkbeck, University of London. She argues against approaches that take a modality-specific approach to developmental disorders - approaches that state, for example, that autism arises because of a failure of the theory of mind module. She has argued that these approaches assume a "mosaic-like" approach to cognitive development - according to which different systems within the brain develop separately from each other, based purely on information coded in the genes. The real picture of development is much more complicated and comes about as a result of back-propagating interactions between gene, brain, behavior, and the environment; modules appear relatively late in development.

<sup>17</sup> Howard Saul Becker is an American sociologist that is considered part of the second Chicago School of Sociology which also includes Erving Goffman, and Anselm Strauss. Becker's 1963 book *Outsiders* provided the foundations for labeling theory. He made major contributions to the sociology of deviance, sociology of art, and sociology of music. Becker also wrote extensively on sociological writing styles and methodologies. He is often called a symbolic interactionist or social constructionist, however he does not align himself with either method.

<sup>18</sup> Anselm Strauss was an American sociologist internationally known as a medical sociologist (especially for his pioneering attention to chronic illness and dying) and as the developer (with Barney Glaser) of grounded theory, a method of qualitative analysis widely used in sociology, nursing, education, social work, and organizational studies. He also wrote extensively on Chicago sociology/symbolic interactionism, sociology of work, social worlds/arenas theory, social psychology and urban imagery.

<sup>14</sup> Jack Kiefer was a Professor at the the Department of Mathematical Statistics at Cornell University. He was a Fellow of the Institute of Mathematical Statistics and the American Statistical Association, President of the Institute of Mathematical Statistics, Guggenheim fellow, and in 1973 he was named the first Horace White Professor of Mathematics at Cornell.

<sup>15</sup> Harry Hoijer was student of Edward Sapir's at the University of Chicago (PhD, 1931), He began his career in linguistics with intensive fieldwork on the Coahuiltecan language, Tonkawa, though shortly thereafter he turned to an intensive study of Athapaskan, including several Apache languages, Navajo, Sarsi, and Galice. Hoijer moved to the new Department of Anthropology at UCLA in 1940. He coined the term Sapir-Whorf Hypothesis.

I am not sure how to address the notion of “The Chicago School.” I guess the key element is their dedication to field research initially in localized urban settings, studying everything from watchmakers to street gangs. I no longer have enough knowledge of the different publications by this group to give you an adequate response except to say there is some kind of general belief that perhaps there is diversity, but I have too little information to know if the “group” has undertaken any kind of historical assessment for an approach that has existed for some 100 years. The tradition exists today in several locations, especially in the Department of Social and Behavioral Sciences at the University of California, San Francisco. Anselm Strauss founded the department. Strauss was a very perceptive observer and could describe socially organized activities with clarity, but his use of interview material did not engage in word-for-word analyses of speech events and gestures in social interaction. My bias is to include local conditions preceding and following the interview fragments cited as data, and how participants were assessing each other’s activities and perspectives.

**DM:** Looking back at the beginning of your career, what were your relationships with the different generations of the Chicago School, and specifically with Erving Goffman?

**AC:** I have always had difficulty understanding how symbolic interaction, or sociological social psychology can or cannot be integrated with works by Durkheim, Weber, and Simmel. I think Harold Garfinkel’s dissertation, not his work afterwards, is the best example of seeking a way to integrate classical theory and the study of daily life social interaction using a serious substantive concern with everyday decisions.

Herbert Blumer<sup>19</sup> was someone who provided sociology with a serious critique of the illusiveness of trying to document details and patterns of social interaction, and the difficulties one encounters trying to capture daily living. His students would carefully read George Herbert Mead’s *Mind, Self & Society* (1934), but only a

few referred to other books of his, such as *Movements of Thought* (1936), *The Philosophy of the Act* (1938), *The Philosophy of the Present* (1932).<sup>20</sup> A study that captures Blumer’s ideas empirically was research by his former student Melville Dalton. While doing field work for his doctoral dissertation, Dalton supported himself economically by studying his own daily life work as an industrial chemical consultant. Dalton published a book called *Men Who Manage*. The book is a nice example of how Blumer might have pursued his theoretical ideas empirically.<sup>21</sup>

I sensed a bit of tension with Howard Becker after an incident that happened years ago when I was revising my manuscript on *Method and measurement*. Becker had written a paper on qualitative methods published in the *American Journal of Sociology*. I had quoted that paper and sent it to Becker for his permission to quote a passage in his paper. He replied immediately and said he would not grant me permission to quote the passage because the research details were not quite what he had said in his paper. Apparently, there was some kind of error in his analysis of data he presented. Another incident occurred a few years later when my Juvenile Justice book was published and he reviewed the book for *Science*. In his review, he noted at the end of a favorable review that the book lacked an important issue; the cause of juvenile delinquency. My view was that the study was not about the “causes” of delinquency, a substantive issue I felt was too embedded in local and national political beliefs and practices, but instead how local police biases and routine bureaucratic practices among law enforcement agencies and the judicial system show conditions existed that were systematically biased against low-income Caucasian, Latino, and African American adolescents, and how these practices distorted official statistical tabulations.

I met Goffman on New Year’s Eve 1957 at Harold Garfinkel’s house. A few weeks later, I gave a methods class to graduate students in nursing at the Claremont Hotel in Berkeley. I told Goffman about lectures and he said he was on his way to Berkeley and could meet there for dinner one night. We had several meetings.

<sup>19</sup> Herbert Blumer codified of the fundamental theoretical and methodological tenets of the sociological perspective that he called symbolic interactionism. Individual and collective actions of any scale or complexity reflect the meanings that people assign to things, as these meanings emerge in and are transformed within the context of human group life. Blumer incorporated these assumptions into his vision of social life as an ongoing stream of situations handled by people through self-indication and definition. He synthesized the pragmatist philosophy of Mead with Cooley’s notion of sympathetic introspection, particularly as it informs contemporary ethnography, to develop a sociologically focused approach to the study of human lived experience. Because his rendition of symbolic interactionism invariably portrays people as possessing agency, as reflective interactive participants in community life, he routinely called into question analyses of social life that rely on more stereotypical factors-oriented approaches.

<sup>20</sup> George Herbert Mead is a major figure in the history of American philosophy, one of the founders of Pragmatism along with Peirce, James, Tufts, and Dewey. He published numerous papers during his lifetime and, following his death, several of his students produced four books in his name from Mead’s unpublished (and even unfinished) notes and manuscripts, from students’ notes, and from stenographic records of some of his courses at the University of Chicago. Mead’s theory of the emergence of mind and self out of the social process of significant communication has become the foundation of the symbolic interactionist school of sociology and social psychology.

<sup>21</sup> Dalton, Melville. 1959. “Men Who Manage.” in *The Sociology of Economic Life*, edited by Mark Granovetter and Richard Swedberg. Boulder, CO: Westview Press.

At the time, I was a post-doctoral fellow at the UCLA medical center and spent most of my time trying to write a book with Garfinkel on theoretical and methodological issues. We completed several chapters, but when I then went to Northwestern University for two years as a Visiting Assistant Professor, we corresponded and encountered several disagreements. Garfinkel visited us at Northwestern and stayed in our small apartment. We were unable to mend our differences, including the major one about the study of traditional theories, on methodological and substantive research. Garfinkel believed my criticism of traditional theories, methods and substantive areas was simply “remedial” and he was opposed to such research, preferring instead to ignore such concerns (see my chapter on “The ambivalent relationship between ethnomethodology, conversational analysis and mainstream sociology in North America” (2006).

In the fall of 1954, when Garfinkel arrived at UCLA as an Assistant Professor, I asked him if I could audit an undergraduate class on “norms,” but he refused to let me audit the class. He used a term that annoyed me. He said I was a “free loader,” someone who could learn about his ideas but did not have to take the required papers. We had a loud, angry discussion outside the office of the Chair of the Sociology Department, Leonard Broom. He came out of his office and told us, in a negative voice and facial expression, to lower our voices.<sup>22</sup> This incident annoyed Harold very much; he was afraid the Chair would hold this against him. As a consequence we didn’t speak to each other for several months. Although I never took a class from Garfinkel, I benefited enormously from many conversations while completing my Master’s degree at UCLA. Egon Bittner became a graduate student in the Spring of 1955 at UCLA. Egon and I jointly help Garfinkel with his new “demonstrations.”<sup>23</sup>

In the spring semester of 1955 after having finished my MA degree classes before going to Cornell University for my doctorate, I learned from my statistics

<sup>22</sup> Leonard Broom was the second sociologist appointed to UCLA’s newly established department of sociology and anthropology. In 1955, he co-authored (with Philip Selznick, UC Berkeley) one of the first and most successful textbooks in sociology. As a Professor Emeritus at The Australian National University he became an active and influential voice in the development of sociology in Australia.

<sup>23</sup> Egon Bittner studies led him to the University of California at Los Angeles where he did his PhD with Donald Cressey. He joined the Brandeis Sociology faculty in the 60s. He was best known for studies of the relationships between police and society. These studies, which elegantly bracketed conventional stereotypes of the police, proceeded from, but were not limited by ethnomethodological premises and led Egon and many of his students to cruise about in squad cars and hang out in police stations to gather data. His research sought the behavioral bases of the uses and abuses of this application of force.

Professor William Robinson that there was to be an informal seminar at his home with Garfinkel, Professor Richard Morris, and two graduate students to hear Harold’s presentation prepared for a departmental seminar.<sup>24</sup> Garfinkel was, according to Robinson, very nervous about his presentation, fearing it would too radical for the Chair, especially, and several faculty members. After the presentation I made several comments implying that Garfinkel had interpreted a paper by Edward Rose to make his own views appear more favorably. He immediately challenged my interpretation. He then said I may have made a good “point.”

I was pleased to have participated in Garfinkel’s research; it was different from anything I had learned about surveys, interviewing and ethnography. The “demonstrations” were similar to semi-experimental social psychological studies. I believe Garfinkel’s “demonstrations” may have been adapted from his classes in experimental social psychology in the department of Social Relations at Harvard. After the informal session, he asked me to come to his office hours for a meeting. At the meeting, a few days later, he asked if I would be an unpaid assistant in a project with him on some new demonstrations he was about to begin. I was curious about what he was doing. He told me to read work by Alfred Schütz.

Before leaving for Cornell he gave me Schütz’ home address in New York City and told to write Schütz and request a meeting. I wrote Schütz and he responded, suggesting a time and date. My wife Merryl and I went down to New York. She went to see a cousin and I went to Schütz’ apartment on the Upper East Side on 5<sup>th</sup> Avenue. I had 2 hours with him, and I was both fascinated and intimidated by the way he described the kind of background a person should have to understand phenomenology. He stated it would be very helpful if I knew Greek and Latin, and that French and German were also helpful. I told him I was fluent in Spanish, weak in French and starting to learn a little German.

I was surprised by his asking me how “was Mr. Garfinkel?” He noted that he had not heard from Garfinkel for a long time. He described several papers he was working on and gave me a few reprints and two unpublished papers. I asked about his teaching at the New School for

<sup>24</sup> Richard Morris was a Professor of Sociology at the University of California at Los Angeles. He was the author of *The Two-Way Mirror: National Status in Foreign Students’ Adjustment* (1960) as well as *The White Reaction Study* (1967 an important work on urban race relations. Morris earned his Ph.D. at Ohio State University in 1952, where his doctoral dissertation employed the paradigm method in order to develop a general model of social stratification. During his tenure at UCLA, he also served as Acting Dean of the School of Social Welfare, and later as Chairman of the Department of Sociology. He was also an associate editor of *American Sociological Review*.

Social Research and what he would recommend I study vis-à-vis research methods. He indicated that he had given considerable thought to how to interview subjects and that his work at a bank for many years had been very useful for learning how to pursue probing questions to learn more about a person's understanding of a problem and the reasoning processes they seemed to follow. The discussion helped me understand the importance of creating a conversational context when seeking details about a topic and presenting clients with counterfactual ways of thinking about what they had reported earlier in the interview.

For the next few years, I puzzled about how Schutz' way of interviewing was useful for understanding the notion of a world known in common and taken for granted. Garfinkel's dissertation was an ingenious way to operationalize abstract sociological concepts by focusing on a research method that made problematic the problem-solving task that assumed instructions and practices required for the research task were self-evident, that is, a kind of trust needed to negotiate daily living even when local appearances and demands seemed counterintuitive or strange for the task at hand. In other words, suspend one's taken for granted, self-evident aspects of the task at hand, and then present subjects with outcomes that appeared to contradict their expected reasoning. Subjects were deceived until the end of the demonstration. Deception that was essential for the success of the demonstration. In the U.S., such deception would probably be prohibited today.

**DM:** You suggest that social interaction is a key dimension of social order. Do you think your findings on communication and bureaucratic rule following can be applied to any professional environment?

**AC:** Yes. The most recent example can be found in my paper on John Rawls.<sup>25</sup> It was published in the British journal *Classical Sociology*. I had a class with Rawls when I was in Cornell, before he went to Harvard, and it was at Cornell that I read his just published paper on "Two concepts of Rules," *Philosophical Review*, 1955. I found Rawl's work to be very useful for my subsequent research on the study of juvenile justice (1982), and later work on evidence on data from the Santa Barbara Public Defender Office. In my earlier research on juvenile justice, I was concerned with differences between middle

income, low income and minority adolescents.<sup>26</sup> I studied two cities in Southern California. I also referred to Rawls' work in the juvenile justice book. The ethnographic research took four years doing, and I was fortunate to be appointed in both counties as a Deputy Probation Officer without pay. The two appointments allowed me to obtain permission from the two police departments so I could accompany them during daily and evening work activities. The contextual information obtainable with studies in actual settings over a long period of time allows the research analyst to observe and participate in daily life work and informal settings that legal scholars do not address consistently.

**DM:** Drawing on my own research on ethnography of artistic work, I ask: ¿What do you think is interesting to look at?

**AC:** I would start by asking how ballet emerged, in what kinds of settings, sponsored by what socioeconomic group(s), in what kinds of politically organized communities, and how it became institutionalized as a notable culturally evolving economic activity. From the historical standpoint of finances, this was not an issue, with kings and manufacturers musicians got attention but they never got really wealthy. Chopin, Beethoven, they were like privileged servants, but they lived better, dressed better, and could become a distinctive type of "celebrity". How and under what circumstances could different kinds of dancing and music emerge in different cultural settings, and what kinds of new infrastructures were required for activities to become institutionalized such that particular persons could be provided entrance and others a livelihood that enabled them to become what today we call "professionals?"

At the same time, who were doing the folk songs? What sorts of conditions appeared to facilitate classical ballet, and what kinds of folk dancing could have preceded its emergence and development in selective parts of the world? One of the most important cases is American jazz. I grew up in a neighborhood with blacks, Latinos, and Asians. I used to go to nightclubs because I lost hair early and could take young women to a place that opened from 2 to 5 a.m. I can remember the songs, (*he sings*) I remember because it was so unusual. This was something that was really offbeat.

**DM:** You sing very well!

**AC:** I am a very amateur singer, but I would always sing to my children and grandchildren at bedtime. My father had a good singing voice, but was not a professional,

<sup>25</sup> John Rawls was an American philosopher, and taught at Harvard and Oxford. *Theory of Justice* (1971) is regarded as one of the primary texts in political philosophy. Rawls employs a number of thought experiments to determine what constitutes a fair agreement in which "everyone is impartially situated as equals," in order to determine principles of social justice.

<sup>26</sup> Cicourel, A. V. (1968) *The Social Organization of Juvenile Justice*. John Wiley & Sons.

yet when I was young and accompanied him on trips to San Francisco when he worked as a driver for a florist, we would stop in Fresno at an Armenian restaurant and he would sing in Turkish. I think they paid him something and we always received a free dinner. He did the same thing in San Francisco in the 1930s and sang in Greek in a Greek restaurant. My father used to sing to us when we were babies and young children. He sang in fifteen century Spanish/Portuguese ("Ladino"), French, Greek, Turkish and English. I only remember a few of these songs in their entirety, but I do remember a few of these fragments of songs in the above languages. What I know about the arts comes from my wife, a knowledgeable and a long-time professional artist. She is also quite knowledgeable about classical music and opera.

**DM:** Tell me about how you became chair of Harvey Sacks' dissertation committee at Berkeley.

**AC:** In 1963-65, I was a Visiting Professor at the University of Buenos Aires, Argentina. I was invited by Professor Gino Germani who was visiting the Berkeley campus to work with Kingsley Davis in 1962.<sup>27</sup> Before leaving California for Buenos Aires, the Academic Vice Chancellor at UCR asked me to become the first chair of the sociology department upon my return. My decision was contingent on receiving tenure from a university faculty committee. The committee consisted primarily of professors from UCLA. The committee approved tenure but stated that they wished the two books (*Method and Measurement* and *The Educational Decision Makers* (with John Kitsuse) had been reviewed in the journals. The Vice President for Academic Affairs ruled that tenure should be denied until the books were reviewed. Upon my return to UCR, I insisted that the Vice Chancellor for Academic Affairs send my file to the Berkeley campus to be reviewed by their faculty. This was done and my tenure was approved. As the new Chair, I tried to recruit two senior professors. One was invited but during his visit the Vice Chancellor would not match the professor's salary at Northwestern University. I was quite angry and after several discussions with the Chancellor, convinced him to reverse the decision and he did. The professor then called and told me he felt the UCR administration was not "profesional" and declined our offer. This

<sup>27</sup> Gino Germani emigrated from Italy to Argentina during Italian fascism, and taught sociology at the University of Buenos Aires. He created new lines of social research on modernization, secularization and political life of the modern society. In 1966 he escaped from Argentina's coup d'état and became Professor of Latin American Studies at Harvard University.

Kingsley Davis was an American sociologist and demographer who coined the terms population explosion and zero population growth. His specific studies of American society led him to work on a general science of world society, based on empirical analysis of each society in its habitat.

series of events angered me considerably and I decided to leave UCR. My wife was very unhappy about this outcome. Fortunately, the Santa Barbara campus was recruiting and I was told that an appointment there was likely, but I had to find a temporary position elsewhere at a different campus. Meanwhile, I received a tenured offer from New York University but we decided not to leave California. I was told by someone at Santa Barbara that I was being considered favorably for a professorship. In order to wait for their decision, I needed a temporary position in California. I asked Leo Lowenthal and Philip Selznick if they could help me spend a year in Berkeley (1964-1965).<sup>28</sup> At the time, I still belonged to the Law and Society Center at the Berkeley law school directed by Selznick. Lowenthal was then chair of the Sociology department. They arranged for me to be a Lecturer with security of employment in sociology and a Visiting Research Sociologist in the Law School. I taught an upper division class on introductory sociology, and a graduate class in sociology, and did research on my Juvenile Justice book at the Law School.

When Garfinkel learned I would be at Berkeley for one year, Sacks was acting Assistant Professor at UCLA, Garfinkel asked me to help Harvey Sacks obtain his doctoral degree.<sup>29</sup> Harvey had to complete his dissertation at Berkeley in order to remain at UCLA. I had met Harvey soon after he arrived in Berkeley and we were in close contact. This started a process whereby I would become chair

<sup>28</sup> Leo Löwenthal joined the Institute for Social Research in 1926 and became its leading expert on the sociology of literature and mass culture as well as the editor of the journal *Zeitschrift für Sozialforschung*. The Institute's members swiftly fled Germany when Hitler came to power. They settled in New York, where Columbia University gave them shelter. Löwenthal, like former members Marcuse and Fromm, chose to remain in the United States. After seven years as Research Director of the Voice of America, and another year at the Stanford Center for the Advanced Study of the Behavioral Sciences, he joined the Berkeley Speech Department in 1956 and shortly thereafter the Department of Sociology. The private seminar Löwenthal conducted with graduate students interested in the sociology of literature was launched during the student strike of 1970 and continued to meet through the last months of 1992. Some of Löwenthal's publications were *Literature and the Image of Man* (1957) and *Literature, Popular Culture, and Society* (1961).

Phillip Selznick received his PhD in Sociology in 1947 from Columbia University. He was on the faculty of the University of California, Berkeley, between 1952 and 1984, initially with the Department of Sociology and later with the School of Law as well. Selznick was a major proponent of the neo-classical organizational theory movement starting in the 1930s. One of his most influential papers, entitled "Foundations of the Theory of Organization" (1948) laid out his major contributions to organization theory.

<sup>29</sup> Harvey Sacks was an American sociologist that pioneered extremely detailed studies of the way people use language in everyday life. Despite his early death in a car crash and the fact that he did not publish widely, he founded the discipline of conversation analysis. His work has had significant influence on fields such as linguistics, discourse analysis, and discursive psychology.

of Sacks' dissertation committee. No one on the faculty at Berkeley, including Goffman, wanted to be chair of Harvey's committee. Because I officially had a form of tenure at Berkeley, I was allowed to serve as the chair of Harvey's committee. I was able to convince Herbert Blumer and Gerald Berreman (anthropology) to serve on Harvey's committee.<sup>30</sup>

**DM:** How did you receive an appointment at UCSD?

**AC:** After almost four years at Santa Barbara, two of my colleagues at UCSB and I were very unhappy with a decision by the sociology department not to award tenure to Walter Buckley. Tom Scheff, Tom Shibutani and I had several meetings with the Academic Vice Chancellor and Chancellor and finally convinced to support Buckley's appointment. I was very disappointed with the Vice Chancellor, someone who been part of the faculty before UCSB was established.

I also joined my colleague in psychology, David Premack, about the Vice Chancellor's decision not to allow Premack and myself to become directors of an existing but moribund center for research in human development. The new chair of biology was also unhappy because the Vice Chancellor was not supporting his recruitment of a new biochemistry colleague. In addition, Premack and I encountered considerable opposition establishing a new linguistics department. I began to think the Vice Chancellor would make it difficult for the campus to become an important research university. I then seriously decided to apply for a new position at UCSD. UCSD had established strong

departments of linguistics, philosophy and psychology (among many others, especially in the physical and biological sciences, and I recognized a number of distinguished faculty had been hired. I was soon offered a position at UCSD and accepted. I remained until I retired as Professor Emeritus.

**DM:** You recently moved from UCSD to San Francisco's Institute for Health and Aging in San Francisco, and to Berkeley's Institute for the study of Societal Issues. Berkeley is one of the most culturally diverse and, I dare to say, progressive environment in the US. The social atmosphere in the Bay Area contrasts sharply with La Jolla, where you lived for more than 40 years. La Jolla discriminating housing practices (with real state reserved for Caucasians lasted from the 20's until the early 60's. My first impression when I first stepped into the UCSD was that of a beautifully green and easy-going campus where African- American and Latino students, faculty and staff, were beginning to be invisible.

**AC:** The move to Berkeley has had difficulties because my work has never been supported after most of the faculty in the departments of anthropology and sociology, and this contrasted with the previous faculty members I knew for a number of years. I have, however, been very pleased with my appointment at UCSF in the Institute for Health and Aging, and the wonderful opportunity I have had to do research on dementia in the Department of Neurology in the Memory and Aging Center. I have published two papers, both in the UK, on my research on dementia, and have a very long new paper I am currently revising for publication.

<sup>30</sup> Gerald D. Berreman received his Ph.D. in cultural anthropology from Cornell University in 1959 and was a UC Berkeley Emeritus Professor of Anthropology widely recognized for championing socially responsible anthropology and for his work on social inequality in India. He joined the UC Berkeley Department of Anthropology in 1959 and retired in 2001 after a distinguished career that featured a 41-year study of caste, gender, class and environment in and around the Indian village of Sirkanda and the urban area of Dehra Dun. Berreman was known for his campaign to establish an ethics code that said anthropologists' primary responsibility should be to the people they study. He also was an early proponent of transparency in social science research.

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