

**"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"**

**Aaron Cicourel in Conversation With Andreas Witzel and Günter Mey**

**Abstract:** In this interview, which was conducted mainly by e-mail, we trace the evolution of Aaron V. CICOUREL's thinking and career. In Part 1 we begin with his undergraduate education, then as a doctoral student, as an assistant professor, and his experiences with field research. Part 2 contains his critical reflections on the ecological validity problem which underlie self-contained interviews and surveys that lack ethnographic data. In Part 3 he shows that it is necessary—and this is his specific contribution to the qualitative approach—to reflect on respondents' daily life experiences and understanding of fixed-choice or open-ended questions. Many strategies for valid interviews are also discussed in this part. Part 4 contains CICOUREL's reflections on the broad field of qualitative research and his own current research on daily life decision-making, routine information and communication processing, or activating memory. Different methods for analyzing natural settings are proposed. In the Part 5 general developments in qualitative research—challenges, obstacles and solutions—are pointed out. In Part 6 CICOUREL describes national differences of research cultures in the United States, Europe, and Latin America.

**Key words:** *statistical methods, field research, ethnography, sociolinguistics, ecological validity, qualitative interviews, information processing, communication, national research culture*

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**About the Interview**

This interview is a result of many e-mail exchanges during the summer of 2004. In a first step, Aaron CICOUREL wrote a text following a list of questions given by the editors in their guidelines for the *FQS* Special Issue: Interviews. Then the interviewers asked additional questions that allowed the thread of the story to be further spun and to garner more details in the various sequences of the interview. In September, Aaron and his wife Merryl stayed at Hansekolleg Bremen, Germany. While in Germany, Aaron and Merryl visited Berlin. There were additional encounters with Andreas WITZEL in Bremen and with Günter MEY (with Katja MRUCK) in Berlin. Final revisions and Aaron's authorization of the interview occurred once he returned to the US. [1]

## About Aaron CICOUREL

Aaron Victor CICOUREL is a professor of sociology, specializing in sociolinguistics, medical communication, decision-making, and child socialization. *Contact:* Prof. Aaron Cicourel, Departments of Cognitive Science and Sociology, 9500 Gilman Drive—Dept. 0515, University of California, San Diego, La Jolla, CA 92093-0515, USA, E-Mail: [cicourel@Cogsci.ucsd.edu](mailto:cicourel@Cogsci.ucsd.edu). [2]

His career in overview:

- 1957-1958: Postdoctoral Fellow, U.C.L.A. Medical Center
- 1958-1960: Visiting Assistant Professor, Department of Sociology, Northwestern University
- 1960-1965: Assistant Professor to Associate Professor, Department of Sociology, University of California, Riverside
- 1963-1964: Visiting Professor, Department of Sociology, University of Buenos Aires, Argentina
- 1965-1966: Lecturer in Sociology, Department of Sociology, and Associate Research Sociologist, Center for the Study of Law and Society, University of California, Berkeley
- 1966-1970: Professor, Department of Sociology, University of California, Santa Barbara
- 1970: Professor, School of Medicine and Department of Sociology, University of California, San Diego
- 1970-1971: National Science Foundation Senior Post-Doctoral Fellow, London University
- 1975-76: Guggenheim Fellow, University of Madrid (Complutense)
- Summer 1986: Fulbright Professor, University of San Salvador, Bahia, Brazil
- Fall 1988: Visiting Professor, Department of Sociology, University of California, Berkeley, Professor of Cognitive Science, Pediatrics, and Sociology, University of California, San Diego
- Spring 1992: Fulbright Professor, Autonomous University of Barcelona, Spain
- Fall 1998: Visiting Professor of Sociology, Columbia University, New York
- 1989 to Present: Research Professor of Cognitive Science, University of California, San Diego [3]

CICOUREL is now Emeritus Professor and lives in San Diego, California (USA). Early in his career, he benefited from contacts with Alfred SCHÜTZ, Erving GOFFMAN, and Harold GARFINKEL. [4]

It is not unusual that the work of groundbreaking scientists like Aaron CICOUREL is often contested. For instance, in Germany we find on the one hand Jürgen HABERMAS' opinion who, in a covering text of the German edition of CICOUREL's 1964 central and programmatic book *Method and Measurement in Sociology*, wrote "It is the merit of Cicourel not having deported the problems of data-investigation to the level of research-technique, but having made it an issue of the theory of cognition." In contrast, and in a way typical for the mainstream of German methodology at that time, which did not accept doubts and were isolating themselves against new ideas, like qualitative methods—DÜSBERG and TEGTMEIER (1971, p.23, our translation) noted in a book review of the *Kölnener Zeitschrift für Soziologie und Sozialpsychologie* that this book is "not a productive contribution for the discussion of the problems of measurement." For example, they wrote, arguing against CICOUREL's remarks on standardizing (different interpretations of items) "If a method of measurement should comprehend all influences on the result of measurement, it would be necessary to measure the whole universe each time."—A critique Aaron CICOUREL answered during our interview:

"My concern has been with the way social scientists often ignore biases introduced by the variations in the way different research analysts USE methods. There is no way to avoid

such biases. The best we can do is to try and identify such biases and take them into account when we discuss our results. The book was not an attempt to reveal how we should go about creating measures using different methods. I can only defend my position by reference to the many empirical studies I have conducted. The book *Method and Measurement in Sociology* was deliberately programmatic. The subsequent research I published and continue to publish attests to what I think can or should be done." [5]

Besides CICOUREL's well-known *Method and Measurement in Sociology*, other publications with a focus on qualitative research include:

- The Educational Decision-Makers (with J. I. KITSUSE; New York: Bobbs-Merrill, 1963)
- The Social Organization of Juvenile Justice (New York: Wiley, 1968; reprinted with a new introduction, 1976, Cambridge Studies in Criminology series, London: Heinemann; reprinted with a new introduction, 1994, Transaction Publishers, New Jersey)
- Language Use and Classroom Performance (edited by Aaron V. CICOUREL, K. JENNINGS, S. JENNINGS, K. LEITER, R. MACKAY, H. MEHAN & D. ROTH, New York: Academic Press, 1974)
- Theory and Method in a Study of Argentine Fertility (New York: Wiley, Interscience, 1974)
- Sociolinguistic Aspects of Gestural Sign Language (published in I. SCHLESINGER & L. NAMIR (Eds.), Current Trends in Studies of the Sign Language of the Deaf (pp.271-313), New York: Academic Press, 1977)
- Interpretation and Summarization: Issues in the Child's Acquisition of Social Structure (published in J. GLICK & A. CLARKE-STEWART (Eds.), Studies in Social and Cognitive Development, New York: Gardner Press, 1977; also translated into German)
- Language and the Structure of Belief in Medical Communication (Studia Linguistica, 35, 1-2, 1981; expanded and revised version in S. C. FISHER & A. D. TODD (Eds.), The Social Organization of Doctor-Patient Communication, Washington, D. C: Center for Applied Linguistics, 1983) [6]

The interviewees would like to personally attest to the merits of this great scientist. Especially for Andreas WITZEL the ideas of Aaron CICOUREL have had very important consequences for his academic career. CICOUREL's focus on the individual's interaction with the environment was the turning point for him as a student of psychology, later working in sociology. The second turning point was CICOUREL's description of the—as he says in the interview, see paragraph 42—"lack of validity in interview and survey questions that often treated questions as self-evident." Andreas studied *Method and Measurement in Sociology* and also *Theory and Method in a Study of Argentine Fertility* (1974) and began searching for methodological alternatives to the mainstream "normative-deductive paradigm," as he outlined in his 1982 book *Methods of Qualitative Social Research. Overview and alternatives*," which was the basis for developing the problem-centered interview (see WITZEL 2000). [7]

## 1. Development of Academic Opinions and Career

### 1.1 Being a student

MEY: What influences, motivations, and perspectives were especially important for your becoming a qualitative researcher? [8]

CICOUREL: As an undergraduate at the University of California, Los Angeles (UCLA), I studied experimental psychology. The focus of most of my classes was on laboratory animals as a way of understanding human learning and motivation. I also took several courses in the anthropology and sociology department and was impressed by such teachers as Donald R. CRESSEY, Melville DALTON, Edwin M. LEMERT, and Ralph H. TURNER, all of whom introduced me to

the symbolic interaction approach and the difficulties and virtues of field research. These four teachers influenced my decision to apply to graduate studies in anthropology and sociology. [9]

I was motivated to do field research because of dissatisfaction with my prior experiences in psychology. I also found that the empirical examples used when learning about statistical concepts and tools in sociology never made reference to observing everyday social interaction in socially organized settings. I continued to take all of the courses on methods and statistics offered in psychology and sociology. The essentially linear-based models that appeared to be inherent in the use of official statistics, census and demographic data, and survey research within the social sciences seemed to obscure the non-linear, dynamic social interaction, verbal and nonverbal behavior that seemed to dominate everyday life settings. [10]

WITZEL: What experiences led to this perspective? Could you describe in detail those experiences that led you to this assessment? [11]

CICOUREL: The one example that I can remember most clearly is when I was analyzing data for my juvenile justice book. I employed two doctoral students at Berkeley to help me code data I was able to obtain from actual police department files on delinquents and also informal files one police department created for those juveniles who the police decided not to file a petition to juvenile court and give the juvenile "another chance." I asked the graduate students to tape record their coding activities while talking out loud about what they were doing. The students were often frustrated because their attempts to use a particular coding framework resulted in decisions that were often arbitrary because no coding category was adequate to capture the variation that emerged. This is a common problem when using data that are not pre-coded as in the use of survey research with fixed-choice questions. The coding strategies are necessary if you want numerical summaries and aggregated data, but I felt these strategies often misrepresented the way official and unofficial police reports were assembled and used by others. [12]

WITZEL: So you continued studying statistical methods? [13]

CICOUREL: In graduate school I dedicated a large part of my time to learning quantitative methods because of being told that success in academic social science required a strong background in such methods. Further, that if I criticized such methods, I would have to show that my concern about their use was not based on an inability to know and use them, but was due to a genuine interest in finding methods that were congruent or in correspondence with the phenomena we call social interaction and the ethnographic conditions associated with routine language use in informal and formal everyday life settings. [14]

As a graduate student of W.S. ROBINSON at UCLA in anthropology and sociology for my Master of Arts degree, therefore, I took all of the classes he offered in statistics. I realized that I needed more training in mathematics in order to understand advanced statistical methods not given in psychology and sociology. I then completed three semesters of calculus as well as a course in advanced algebra. I also audited courses in advanced calculus and differential equations. [15]

Of considerable interest to me at the time was W.S. ROBINSON's (1950) notion of ecological correlations; the difficulty of making inferences about individuals using aggregated census tract data. ROBINSON's work sensitized me to the fact that it was difficult to use existing social science statistical methods that relied on linear models to quantify direct observations of dynamic social interaction in socially organized settings. ROBINSON called attention to what he called the inverse relationship between reliability and validity in the use of isolated interviews and sample surveys. Subsequently, it occurred to me (and others) that interviews should be contextualized ethnographically. [16]

WITZEL: Was the concern with the situated nature of human communication and interaction motivated by your training in symbolic interaction, a subsequent interest in linguistics and cognition, the social philosophy of Alfred SCHÜTZ, and what came to be known as ethno-methodology later, especially the concern with common sense reasoning and language use? [17]

CICOUREL: The simple answer is "yes," but a more complicated answer would have to include other elements. For example, I did not reject all of psychology, but primarily the behaviorism that was then dominant. I learned about elements of social cognition from work by Solomon ASCH, Leon FESTINGER, Theodore NEWCOMB, and Musafir SHERIF. [18]

I also became interested in the neural basis of social cognition and communication because of research on victims of strokes and the behavioral consequences of other forms of brain damage. The interest in cognition was stimulated by an undergraduate paper I wrote in 1949 on a book by D.O. HEBB (1949) called *The Organization of Behavior*. [19]

## 1.2 Early research

WITZEL: Symbolic interaction deals with the process of human communication and interaction. Did you find deficiencies in this approach? [20]

CICOUREL: Although symbolic interaction appealed to me, it seemed to neglect the study of language use during social interaction despite important abstract references to the relevance of gesture and language in the work of G.H. MEAD. None of the professors with whom I studied who were adherents of symbolic interaction examined actual language use. As mentioned earlier, I also found a new source of inspiration in the work of Alfred SCHÜTZ while working with Harold GARFINKEL. For my Master's degree (and later as a post-doctoral fellow at the UCLA School of Medicine) I worked closely with GARFINKEL in the study of quasi-controlled, focused interaction or "demonstrations." GARFINKEL, however, was not interested in intensive ethnographic research. [21]

But let me be clear. In my own research at UCLA for my Master's degree, I did not study socio-linguistic aspects of language use in everyday settings, but pursued my Master's thesis by engaging in participant observation in a small factory in the San Fernando Valley under the direction of Melville DALTON. I found that establishing good relations with employees was difficult. I learned an important lesson about such relations with the workers I observed during their factory tasks for several months. The day I was to interview different factory workers, someone had spread the rumor that I was a "spy" for the owner and it became difficult to interview anyone. [22]

WITZEL: In this context, I remember your study of "Argentine Fertility" (1974), where you attached much importance to establishing contact with the respondents, especially their acceptance of you as a researcher with a delicate research topic. [23]

CICOUREL: The fertility study tried to replicate a previous study done in Jamaica by Judith BLAKE DAVIS. I tried to combine a survey approach using open-ended questions from BLAKE DAVIS' work and learn about the neighborhoods in which each respondent lived by personally visiting each area and taking notes on the general nature of each setting. My assistants and I always became acquainted with respondents in their homes, and I often visited the home after an assistant had completed one or two visits. I managed to become acquainted with four families and visited them on many occasions in order to have a deeper sense of Argentine family settings. [24]

Gaining entrance and cooperation from the random sample of respondents was very difficult because for many respondents the University of Buenos Aires was either viewed with suspicion or simply an unknown entity. Also because of doing research at the municipal children's hospital, I soon found that it was considerably easier to gain access to homes by telling respondents we were working at the children's hospital and using the names of my physician sponsors there. [25]

As far as I know, the research on fertility was the first done in Argentina and especially delicate because of asking questions about contraceptive use, abortion, and frequency of sexual intercourse. [26]

### 1.3 Ongoing research as a doctoral student

WITZEL: Would you share your graduate school research experiences and discuss how they shaped your thinking? [27]

CICOUREL: As a doctoral student at Cornell (ITHACA, New York), I continued to study mathematics and was fortunate to have Jack KIEFER from the mathematics department on my dissertation committee for my minor in mathematical statistics. KIEFER made it clear that the use of a given level of statistical significance should be theory-driven, not by the traditional decision to use a level such as .01 or .05. [28]

WITZEL: So although critical, you seemed to be following the mainstream to succeed in academic social science? [29]

CICOUREL: Yes. I think it is always necessary to be as deeply informed about what is happening in the mainstream in order to clarify what is presumably different about work that addresses field research. I believe (but cannot verify) that quantitative formats were developed in France in the late 19th century. Field research has a similar history, but field research has usually been the less dominant research method. We normally start within the mainstream and rely on it to propose what may be different or similar. Learning about mathematical statistics within mathematics is necessary in order to understand what possible alternatives might exist for creating measurement systems commensurate with the phenomena that are the focus of one's research. Let me underscore the fact that 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 general, I found that symbolic interaction, anthropological field methods, sociolinguistics, and phenomenological ideas did not lend themselves to the kinds of mathematics I had been studying. [30]

WITZEL: Were you further engaged in field research? [31]

CICOUREL: Yes. At Cornell I was also influenced by the work of James GIBSON and Robert B. McLEOD in psychology, and Allan HOLMBERG, Robin M. WILLIAMS, Jr., and William F. WHYTE in anthropology and sociology. HOLMBERG had done extensive field research among non-Western groups in South America, WHYTE had engaged in considerable field research in a Boston neighborhood that led to his book *Street Corner Society*, and subsequently did research in South America. WILLIAMS had done field research as part of a team that studied soldiers during combat for the American military during World War II, and was one of the authors of the *American Soldier* series that influenced the move toward solidifying quantitative social science research after World War II. [32]

My dissertation was a participant observation study of retired persons in Ithaca, New York, which I combined with visits to an important center of research on aging conducted by Cornell Medical College at an estate in Dutchess County, New York. In my dissertation, I tried to apply the sometimes graphic and also abstract theoretical observations in early published (1955-57) work by Erving GOFFMAN and unpublished, abstract theoretical concepts suggested by Harold GARFINKEL. [33]

WITZEL: Were these considerations the beginning of new methodological concepts? [34]

CICOUREL: If there was anything new in the air, it was the idea of asking how variations in rates of official statistics (work done with John I. KITSUSE) were linked to the organizational activities of the agencies that produced the statistics and thus the rates. We asked the following question: "If the rates of various student types are conceived to be products of the socially organized activities of the personnel, then the question is 'How do these activities result in making a student a statistic in a given category'." (CICOUREL & KITSUSE 1963, pp.9-10). [35]

I pursued this line of research more intensely in my study of juvenile justice (CICOUREL 1968; reprinted 1976 and 1994). For example, our concern with day-to-day activities within a given

organization began by directly observing and recording (whenever possible) the day-to-day activities within informal and formal organizations and noting how such activities contributed to the way those observed created their own accounts about what the researcher had previously observed and recorded. More recent developments include the use of video recordings (when possible) and a focus on speech, gestures, and body movement within social interaction. [36]

#### **1.4 Assistant professorship**

WITZEL: Now could you tell us about your work after completing your dissertation? [37]

CICOUREL: As an assistant professor at Northwestern University, I continued to audit courses in advanced mathematics but grew more disillusioned. It became obvious that there was no obvious correspondence between the kinds of behavior that I found of interest and the quantitative methods I had learned. But I was able to interact and teach with Donald T. CAMPBELL and extensively discuss with him a diverse set of research methods. From CAMPBELL I learned about the work of Egon BRUNSWIK, then in the psychology department at Berkeley where CAMPBELL had received his PhD. BRUNSWIK's work taught me about problems of experimental design and ecological validity in controlled research, namely, the extent to which results in the laboratory represented conditions in a broader ecological setting. Psychology, BRUNSWIK noted (1957) had become "a science of the organism" rather than the more appropriate study of the organism "in contact and interaction with the environment" (p.6). BRUNSWIK also stated that the traditional psychological view of cognition is part of what he had called the "second phase" that focused on the "intraorganismic" part of the cognitive process, namely "the 'utilization' of cues" (p.10) or what happens inside the head. He preferred, instead, to talk about a third phase or "act" (as in a play) that provides the crux of the play, namely, "the utilization of the sensory input relative to the distal object [that] may be appropriate or not" (pp.10-11). [38]

Approximately fifty years ago, two psychologists at the University of Kansas did what could be called qualitative research. The two of them, Roger G. BARKER and Herbert F. WRIGHT (1951) wrote a book called *One Boy's Day: A Specimen Record of Behavior*. It involved following a young boy from the moment he awoke in the morning to the time he went to bed in the evening. But this work did not have much impact on other psychologists in this country. [39]

W.S. ROBINSON's work on ecological correlations seemed to be related to BRUNSWIK's observations about cognition and ecological validity. [40]

## **2. Missing Ecological Validity and its Consequences for Interview Methodology**

WITZEL: What consequences did you draw from the ecological validity problem? Could you please explain it in more detail? [41]

CICOUREL: Inspired by ROBINSON's and BRUNSWIK's work, I began to speculate about how ethnographic field work could help us understand the lack of validity in interview and survey questions that often treated questions as self-evident despite the attempt to explore their wording and content with respondents during a "pretest" or the use of open-ended questions with additional follow-up questions. [42]

The ecological validity problem can be stated as follows: To what extent is the content of questions asked commensurate with the socially distributed knowledge possessed by the respondents? Do the questions asked address topics, beliefs, attitudes and opinions the respondents routinely discuss in everyday life during social interaction with others? Further, to what extent can we assume that given the absence of ethnographic information about different communities, we can ignore the extent to which the wording and content of the questions are comprehended similarly by the entire sample? Are the questions, therefore, different from or are they in correspondence or congruent with observing the way respondents express themselves in their daily life encounters with others? [43]

WITZEL: Would you discuss the implications of this for interview methods? [44]

CICOUREL: The serious problem associated with self-contained interviews and surveys without ethnography is that we sample bodies but not their everyday behavioral environments. We lack systematic observation of respondents' daily life activities and the condition under which attitudes, opinions, beliefs, and folk knowledge emerge and are displayed. [45]

Conducting interviews with closed- or fixed-choice questions or sending respondents questionnaires to fill out fixed-choice survey questions has often been an end in and of itself. There has been little or no interest in conducting systematic observation of the ecological settings in which respondents lived or played or worked, including their discourse practices. It is customary to interview a small sample of respondents before constructing a sample survey. The general idea was to explore questions in some detail before settling on a particular set of them for the larger survey. The small subset of a sample (the "pretest") was intended to satisfy validity issues, but respondents, even if interviewed at home, often answered questions with guarded enthusiasm. The quality of surveys, therefore, could be partially improved by using tape recorders. Random recordings would provide a sense of how the language used in the pretest oriented respondents to the study's goals and the extent to which they resulted in modifications of the final questions employed. Tape recording a random subset of the final questionnaire sample could enable the research analyst to compare the way interviewers and respondents carried out the task. [46]

WITZEL: Could we say that quantitative oriented researchers who construct questionnaires use qualitative methods in their pretest in a less rigorous way? [47]

CICOUREL: One of the problems with pretests is how the researcher selects this sub-sample. Another problem is the extent to which the research analyst reports the kinds of problems he or she encountered with the initial questions used and why was it deemed necessary to change the questions? Ideally, the pretest should be a randomly selected part of the target or final sample, but after the pretest the sample would be discarded while identifying the kinds of problems that occurred with the initial questions. An even more ideal strategy would be to take another random sub-sample from the final or target sample and interview them again but this time changing the original fixed-choice questions into open-ended questions as a way of approximating aspects of validity. A general problem and advantage of survey research is that theory is often stated in broad terms, but if the sample is large enough, there will almost always be a way to claim significant findings. [48]

Because of my concern with validity issues, I began to assume that small case studies required very strong theory in order to produce meaningful generalizations. By "strong theory" I mean specifying the kinds of social interaction patterns we should expect to observe, and the expected kinds of speech events likely to occur if there is systematic sampling of the research setting at different times of the day and week or year. Systematic observation means the ethnographic study of repeated, stable and changing organizational behavior, including seasonal adjustments in work or living conditions or changes in one's life course. [49]

WITZEL: Could longitudinal methods be helpful for analyzing differences in stability and change in organizational behavior? [50]

CICOUREL: Yes. The research pursued by Bremen University (Special Research Center 186 "Status Passages and Social Risks in the Live Course", funded by the DFG 1988-2001) on the occupational careers of men and women is a good example of the value of longitudinal research. Perhaps I should now ask you to respond by telling the reader how this research program combined an unusual group of survey research specialists and qualitative researchers. The studies achieved a significant measure of validity because of their longitudinal design and revealed unique differences between survey research and the use of more qualitative interview techniques (SCHAEPER & WITZEL 2001; ERZBERGER & KELLE 2002). [51]

### 3. Specific Contribution to the Qualitative Approach: Reflecting the Respondents' Daily Life Experiences as a Necessity in Understanding Respondents' Answers

MEY: What do you think has been your most significant contribution to the field of qualitative research? [52]

CICOUREL: The closest thing to a "contribution" has been my attempt to clarify the drawbacks of using fixed-choice and open-ended questions that were not reflected in the respondents' daily life experiences, including their understanding of each question and the kinds of knowledge domains each question presupposed. For example, what Donald A. NORMAN (1973; see also CICOUREL 1973) called the "paraphrase problem": Is the format and content of a question commensurate with the way the information is organized in memory that would enable the respondent to answer? [53]

WITZEL: Is this problem associated with that of the indexicality of communication? [54]

CICOUREL: Indexical expressions imply either ambiguity or unstated elements of meaning. Their local comprehension, therefore, involves the interaction of prior or present comprehension of information. For example, phonology, syntax, semantics, pragmatics, prosody, mundane knowledge, and the perception of the social setting within which such expressions occur can all influence their meaning. Interview and survey questions invariably involve indexical expressions. The paraphrase problem, therefore, could include aspects of an expression's indexical properties. The notion of indexical expressions as used by ethnomethodologists assumes explicit reference to the respondent's local understanding of an expression, but to my knowledge does not refer explicitly to the organization of their memory of past experiences that might be relevant for understanding how a respondent might formulate or choose a fixed-choice or open-ended answer to a question. [55]

WITZEL: You made reference in this context to the "misleading 'hardness' of data coded from material from fixed-choice and open-ended questions" (CICOUREL 1974b, p.99). What solutions do you propose for this problem? [56]

CICOUREL: One way to resolve the "misleading hardness" problem is for the research analyst to acquire knowledge about the respondents' daily life activities and their respective environments, preferably by direct observation of particular social ecologies. Knowing about the respondents' past activities and the kinds of groups and voluntary organizations to which they belong can help in formulating questions and anticipating answers. In various research projects, I and others have tried to informally ask open-ended and specific task-oriented questions while accompanying or observing respondents at work or at some other activity. [57]

When my research involved formal social organizations, therefore, I first tried to resolve the validity problem by observing the kinds of routine activities that were part of employees' daily task environment. For example, how many people worked therein, and what were their official and unofficial task assignments or duties, what kinds of daily routines did they pursue, and how did they assemble reports or records or statistics generated officially and unofficially? In one case, my organizational research (CICOUREL 1968) required observing police, probation officers, and judges while they engaged in routine social interaction with juveniles and adults, and how such activities were transformed into unofficial records and/or official statistics. [58]

Another type of research (CICOUREL 1974a; 1982; 1992) sought to link the way physicians interacted with patients to what they said in informal or formal written accounts. For example, we looked at the kinds of hand-written "progress notes" that were created, and the dictated official medical histories physicians constructed that summarized their experiences with patients. The focus was on the extent to which it was possible to link the observed and recorded medical history and physical examination to what became an official medical/legal document. [59]

WITZEL: Could narratives also serve as a solution by including the respondents' daily life experiences, their language use and problems of memory? [60]

CICOUREL: Narratives are a rich source of information about respondents' daily life experiences, language use, and especially memory problems. A general problem is how to compare a respondent's accounts with those of others who were involved in the same experiences? One possibility would be to ask them about local, regional, and national events that occurred and were felt to be relevant for their lives, and then try to see if local, regional, and national newspapers have stories about such events and if the respondents' account were commensurate. Also, the accounts of friends and relatives could be elicited about the accounts given by respondents. The issue of language use is complicated. If we are talking about language use with the researcher, then it would be very useful to have a comparison with more spontaneous interaction with others. [61]

One general methodological goal of my different research projects was to alert readers to how we might go beyond participant observation by seeking recordings and documents or statistical summaries that were intended to provide a personal or "official," convincing "story" (TILLY 1998) about someone's experiences and assessments of their own activities. In other words, how do different respondents and research analysts seek to summarize (and hence compress and/or suppress many details about) their observations or interaction with others? [62]

WITZEL: In your study about Argentine fertility you used an interview method that avoids the traditional question-answer format. This common format can create—as you say—misleading, "appropriate" outcomes with arbitrary and convenient cutoff points for both interviewer and respondent. Could you please outline your alternative ideas of interviewing? [63]

CICOUREL: In order to avoid the traditional question and answer format where respondents are contacted, an interview is scheduled and then completed, we require labor-intensive field methods for gaining access to and observing respondents' everyday activities. It is common for field researchers to know respondents through participant observation before conducting informal or scheduled interviews. If this is not possible, I try to meet with respondents informally by saying we first want to give them a sense of what the study is about, and indicating that we hope to visit them soon in order to ask a few questions at a time that is convenient for them. After what is hopefully a "cordial" exchange during the first or subsequent meeting, the respondents should be asked if it would be possible to tape-record the scheduled interview in order to insure accuracy. [64]

The general strategy is undoubtedly familiar to most readers. Field research plans, as most readers know, seldom work out as expected and the researcher should be prepared have her or his questions prepared in advance and have a tape-recorder with them in the event the respondent suddenly states that he or she can do the interview immediately. If respondents balk at the idea of an interview that is tape-recorded, the researcher can state that he or she will simply pose a few questions informally. A standard strategy after recording or writing down answers during an interview is to turn off the recorder or put down one's pen or pencil, lean back, and ask the respondent to express her or his views about the question asked. When specific questions or topics emerge, the researcher can use this as an opportunity to probe the content of the question. I sometimes tried to squeeze in an additional meeting by only completing half of the interview and asking if I could return at another time to complete the interview schedule. I also asked my assistants in Argentina to try this strategy. [65]

In my Argentine fertility study, we would ask respondents after the interview if there were any questions that seemed to be awkward or inappropriate or misleading. I asked my assistants to go over each interview schedule and tell me which questions they thought were awkward, inappropriate or misleading, or if they would also tell me which respondents seemed not to understand a question, or appeared to be unhappy with a question, or seemed to giving misleading information, I paid the assistants more money for this additional information than they were paid for their original interviews. [66]

WITZEL: Would you please elaborate a little on these strategies? [67]

CICOUREL: A difficult issue that can never be anticipated is the fact that the interpersonal relations that emerge from contacting respondents (initially by telephone and/or letter), and meeting

them face-to-face always makes many aspects of field research problematic. Satisfying the spoken (and unspoken) doubts of respondents cannot be avoided and presumably detracts from the validity of the information. Achieving trust and friendly interpersonal relations is always a goal, but we can never be certain. We would like to assume, perhaps naively, that if we have the respondents' trust, they may not mislead the researcher as much and even reveal information viewed as confidential. Researchers often find it difficult to acknowledge that they were unable to achieve optimal interpersonal communication with some respondents. These problems are present in all research with human and non-human animal subjects. [68]

Once the interview begins, many interviewers ask general questions and encourage the respondent to provide details. The questions should include follow-up probes such as "could you tell me more about that" or "what was that all about." In addition, respondents often need to be reminded that they are the ones with the information and that the researcher is the novice in the interview setting. [69]

#### 4. Current Research

MEY: What are the foci of your current research and thinking? [70]

CICOUREL: It is well-known that problem solving in the everyday world can be simulated in laboratory settings, and that such simulations depend for their ecological validity on the way human animals behave outside of the laboratory in daily life settings (BRUNSWIK 1957). In most of my recent work, I try to clarify daily life decision making under uncertainty and limited capacity processing, or what can be called the *hidden elements* of daily life decision-making. [71]

MEY: Can you provide an example and describe the everyday contexts in which you do this research? [72]

CICOUREL: An example would be when there are multiple demands on a person's processing resources, or what has been called the problem of cognitive overload. Another way of thinking about cognitive overload is calling it the stress we experience when we are so busy that we get frustrated and are likely to make mistakes. [73]

I have focused on decisions in medical settings. The overload physicians experience when trying to take notes while listening to the patient, looking at their facial expressions, the texture and color of their skin, and their voice intonation and ability to express themselves, are good examples of what I am talking about. [74]

I have examined two types of overload:

1. What can be viewed as normal overload when health care personnel attend to multiple sources of information yet must compress, ignore, and miss information while trying to summarize mentally and/or in written notes (or later in a medical history) their experiences.
2. Not being able to cope with excessive stimuli, emotional distress and the danger of experiencing information processing errors. This type of overload or stress has been studied in human and non-human primates and reveals similar negative consequences. [75]

I have studied routine physician-patient and non-professional health care staff and patient communication for twenty-five years in order to understand the kinds of constraints that are "normal" and sometimes a burden when making organizational decisions. As a faculty member in both sociology and pediatrics, I gave classes to medical students and residents who were specializing in pediatrics. I supervised and video-taped post-doctoral residents at our university hospital as they interviewed mothers and their children and gave the children a physical examination. The resident and I would then review the video-tape. I had to evaluate the resident's performance for the Department of Pediatrics. I became very sensitive to the way the resident's attention, emotional, and memory resources strained their ability to engage in efficient, attentive interpersonal relations with mothers and their children. I also saw similar conditions when I did

research in the Divisions of Infectious Diseases, Rheumatology, Neurology, Anesthesiology, and Family Medicine. [76]

I have been studying the extent to which notes and official reports reflect verbal and non-verbal re-descriptions that summarize routine and complex daily life experiences. I assume that all bureaucratic settings (e.g., office, meeting-room, and laboratory) are information systems whose ability to maintain and store records of past activities are always compromised. We cannot avoid this necessary fact of everyday life and organizational activities, but we should learn about such activities in order not to reify official and unofficial accounts of past and current events. [77]

MEY: Your research does focus on a broad array of topics. What other areas have you been exploring? [78]

CICOUREL: In order to understand routine information processing constraints in organizational settings, the research analyst must learn or know technical aspects of the task environments he or she studies, for example, its discourse properties, including a working knowledge of the local and larger organizational environments in order to create meta-level representations or cultural mental models of one's actions and of those subjects or respondents observed. [79]

An obvious issue here is the fact that the reporting activities of the research analyst, and those he or she studies, necessarily truncate how data are presented because of the selective attention and memory limitations of the researcher and those observed. [80]

MEY: From this research, what issues need to be addressed? [81]

CICOUREL: Let me summarize:

- What shared stated or implicit information is needed for successful communication? We need to distinguish between information in people's heads, information that is collaboratively produced and socially distributed over different encounters, and in artifacts such as records, objects, and the physical setting (KIRSH 2001).
- What constitutes an activity space?
- What coordinating mechanisms must individuals and small groups or personnel (who collaborate across physical and social boundaries) possess in order to synchronize activity, distribute tasks and activities, and manage current resources?
- Can enough of an activity's context be represented by participants so that they can recover their understanding of the prior state of affairs after interruptions?
- What perceived and tacitly recognized information processing and knowledge resources are essential for activating memory cues to allow participants to recover past and locally situated and distributed thinking? [82]

In regards to memory and local work conditions:

- Local work conditions activate two forms of long-term memory organization, *explicit or declarative* memory for facts, *implicit or non-declarative* memory for skills (SQUIRE & KANDEL 1999, pp.15-17).
- Memory and other mechanisms that make it possible to exploit information systems are the "life-blood" of social interaction and discourse in the workplace and during research.
- The different kinds of memory activated guide the participants when they seek to pose questions, comprehend patient (respondent) responses, explain to patients before, during, and after procedures, including different kinds of written reports. [83]

MEY: Do you see special requirements for the design of such studies and do you think specific methods are needed? [84]

CICOUREL: Observing variations and commonalities within and across natural settings require:

- systematic observations on different days and times,
- recordings (whenever possible) of actual work activities,
- focused, open-ended elicitation of information from subjects while they work,
- observe practical environments to pinpoint routine and unusual (multiple) demands on work personnel. For example: the kinds of annotations that can enhance one's ability to recover local aspects of the environment, including the significance of artifacts needed to meet the demands of the task at hand.
- Research analysts should engage in sufficient field research to identify organizational constraints,
- observe actual task performance across the organization,
- infer possible power relationships and their affect on communication and decision making,
- assess what they and subjects are perceiving and processing and the demands attributed to participants. [85]

A general goal has been to recognize that we may not always have adequate data nor the time to engage in ethnographic, ecologically valid observation using different temporal and spatial samples of a given setting, but we should at least conceptualize the organizational and attention/memory/emotional constraints even when our databases do not directly allow us to address such issues. [86]

## 5. General Developments in Qualitative Research

MEY: What do you consider to be the most important areas being explored by qualitative researchers today? [87]

CICOUREL: A general issue for students of social interaction has been how we can move beyond variable amounts of participant observation to include transcripts of interviews, transcriptions of routine social interaction, and picture clips from videotapes or film. Some students do not believe it is necessary to engage in participation and/or systematic observation before and during the time that oral and/or visual recordings are produced. The general idea is that transcriptions inherently embody social interaction because they can be shown to display uniformities in the way speech acts are structured. Others, however, would first choose to focus on speech events with or without first engaging in ethnographic field work. The focus of attention, therefore, is on the kinds of practices that seem to be structurally pervasive in the sense that it is possible to identify types of speech whose absence would require some kind of repair or would create dissonance and misunderstanding. [88]

Another strategy has been to create digitalized representations of verbal acts that can be aggregated and subjected to statistical analysis. [89]

WITZEL: To do the kind of research you do what obstacles did you have to overcome in your discipline? And what obstacles lie ahead? [90]

CICOUREL: There are two major obstacles to engaging in field research. First, there is seldom agreement among research analysts as to what constitutes appropriate data. Second, no clear strategy has been developed to create a consensus among a large group of field research advocates about how they should pursue their studies by developing uniform strategies and consensus about methodology. [91]

One consequence of this lack of consensus is reflected in the differential adherence and support for different levels of analysis. Different levels of analysis can be viewed as re-descriptive systems for characterizing different aspects of social science research. [92]

WITZEL: Would you please outline these different levels? [93]

CICOUREL: Levels of analysis within academic sociology usually refer to a structural perspective that addresses aggregated data obtained from official statistics from such agencies as the United Nations (e.g. World Health Organization), various governmental centers, industrial and commercial entities, non-governmental groups, historical archives, or from sample surveys and similar methods. Another level would be observing and/or recording social interaction directly in actual everyday settings or organizations, or using open-ended questions to ask about personal experiences and previously observed daily life activities. A third level of analysis would include cognitive activities such as memory or learning, other perceptual problem solving tasks also measured by reaction time and the content of responses, or problem solving measured by evoke-related potentials (using electrodes on the outside of the head in a sound chamber), or functional magnetic resonance imaging. [94]

In various publications, I have addressed what is called the "mainstream" and how my work both overlaps and differs from it. The research covers such areas as education (both primary and secondary schools), the study of juvenile justice in two cities and their organizational settings, a work on Argentine fertility where I tried to repeat an important mainstream study using open-ended questions and some observation, and a large number of journal articles and book chapters on medical communication, diagnostic reasoning, the sign language of the deaf, language socialization in Mexico City and Buenos Aires, and general works on discourse analysis and sociolinguistics. [95]

## 6. Experiences in Different National Research Cultures

MEY: Throughout your career you have traveled frequently to Europe and Latin America. You have been in contact with many researchers all over the world. The following questions seek to elicit information about such experiences: What have been the most important encounters for you? [96]

CICOUREL: I will focus on two issues: Relations with colleagues in the host country, and some issues that emerge in conducting research in a foreign country. [97]

When discussing research methods with host colleagues, cultural differences about what is field research and how it should be conducted are always present because of differences in what is perceived as appropriate observation, what questions can be asked, and how each group describes the way they obtain access to different respondents or groups. I have always found that colleagues in other countries make use of theoretical perspectives differently despite agreeing on research methods. Survey research appears to be more a standardized method as does the use of statistical applications. The impact of national and local political conditions within disciplines can vary considerably and can influence the way people obtain new positions and the way they move to other positions. In the past, there have been more national political influences on the way research funding is obtained. In general, national political differences in everyday life always seem to be more intrusive than has been my experience in the United States and Great Britain. [98]

Considerable time is required to learn about the views of colleagues when in other countries and a foreign visitor must always be careful to proceed cautiously in order to learn about differences among the people, especially when academic and general political issues are discussed. Visits to foreign countries almost always lead to exchanges between countries and this can be a delicate issue when you must be the host in your own country. You learn a great deal about your host colleagues and their way of life from the way you are lodged and how you shop for food, and your experiences when invited to your new colleagues' homes. Foreign research personnel initially must rely on native perceptions of national and local formal and informal or unstated norms or regularities. Discussions with your host colleagues about what topics are desirable or what appears to be appropriate can influence the way questions are composed and the kinds of specific words that will be used. Discussions about such issues and

when and how should a researcher observe settings, the kinds of permission needed and from whom, require (often long) conversations in which negotiation is required in order to understand questions and answers within the local research context. [99]

A key issue here is how well the foreign research person knows the language of the host country, and her or his ability to speak with and understand persons from different levels of income and education. When we read results from someone's research, it is very difficult to assess how well they were able to speak to and interview native speakers in other countries. One indirect source of information is the way the research person describes how he or she was able to obtain access to different settings and people. Further, what kinds of observation and participation were possible with native speakers that could be perceived as "informal" or "casual." [100]

MEY: What are some of the main differences between national/disciplinary research cultures you have come into contact with during your travels? [101]

CICOUREL: I think I addressed most of these issues in the last question, but can say a few words about my attempts to help colleagues in other countries with their research projects. First, it is important to recognize that for many years after World War II, Europeans looked to the United States for guidance about research projects. I think those days have been over for several years and Europeans and Latin Americans (the areas I know best) have developed their own norms (but they still overlap with U.S. norms) for engaging in systematic research. To understand differences, it is necessary to observe actual research groups, and they are not always accessible to foreigners. The principle difference I would cite is the way departments and research institutes develop their own hierarchies, and as I noted above, the way they establish working relationships among themselves. This would not seem to be any different from what is expected in any country where Western and Eastern European theories and methods had been adopted long before World War II. Although there exists occupational mobility within and across universities and research centers in Europe, for example, the key point is the degree of mobility between universities and within departments and research institutes. Movement in the United States, for example, between departments, research centers and institutes, and universities is quite common within a region and across the entire country. There is much less mobility in Europe and Latin America. The academic marketplace is quite large in the United States and there is much more diversity in terms of the kinds of universities and colleges that exist and the research capabilities of their faculties. [102]

MEY: What are some of the similarities? [103]

CICOUREL: The most impressive similarity is the use of a similar vocabulary when discussing the design of research and research findings. It is quite easy for persons from different cultures to identify common research interests despite diversity in the way they may be organized as research teams or groups. [104]

MEY: A final question, Aaron. If our computers ate the notes of this conversation, and we had to rebuild this conversation from scratch, what is the one thing you would like us to remember?<sup>1</sup> [105]

CICOUREL: A central issue in all social science research is the relationship between the reliability and validity of the methods and data used. The lack of validity in most social science research means that research analysts seek the quickest and easiest route to obtaining publishable results. If the training of social scientists strongly emphasized the need always to explore the relationship between reliability and validity, our findings could be linked more readily to supporting and/or changing our theories. [106]

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<sup>1</sup> This final question was inspired by Bob WILLIAMS (2004), who asked in an interview with Bob DICK, "What would you do if the pc ate the notes of this conversation?"—a wonderful idea and somewhat similar to a question Günter MEY (1999) asked in his study on narrative identity. At the end of interviews with adolescents, they were asked, "Please imagine a movie is being made about you; how would it show that it represents to your life?" This is a question that is often still used in current studies on identity at the Developmental Unit at the TU Berlin. In the context of the interview with Aaron CICOUREL, the question developed by Bob WILLIAMS fits better.

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