



RESEARCH PERSPECTIVES *PERSPEKTYWY BADAWCZE*

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A REMINISCENCE OF HARVEY SACKS 1968–1975

Abstract

This is an account of several years in the study of conversation analysis with Harvey Sacks at UC Irvine. In the late sixties and early seventies Sacks provided a series of lectures, research reports, and a methodology detailing how ordinary conversations could be recorded, transcribed, and studied. As the founder of the field Sacks brought us closer to the ideal of sociology as a science. During several years of study with the gentleman I discovered a remarkable intellect and an interesting person that I attempt to describe through a chronology of his progression of thought, relationship to ethnomethodology, and published and unpublished research results. Included are some personal experiences and reflections that I hope provide a better understanding of the man as well as his academic direction.

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There seems to be considerable interest in conversation analysis and I thought that a chronology of events in that milieu involving the work of Harvey Sacks might be helpful in understanding both what Sacks was trying to do and the

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significance of those studies. I will recount several years I spent in study with Sacks as a graduate student at UC Irvine during the early seventies. I thought that it perhaps could contribute to an appreciation of his legacy, research task, and of his person as well. My acquaintance with him began in 1971 when I was a graduate student at UC San Diego and concluded with his untimely death in 1975. The document offered here is simply the fulfillment of a debt I felt I owed Sacks, per our arrangement contracted years ago that I would be allied as an “apprentice”, as a party to his pursuits, and that the responsibilities this entailed needed to be interpreted as a requirement to share those findings if the opportunity to do so became available at some time.

The result of an undergraduate curriculum that had involved some experience with the natural and physical sciences engendered an interest in those methods and a summer internship in the Biology department of the California Institute of Technology enhanced my appreciation. The conclusion of that experience was a profound interest in the methods of natural science, but also a desire to explore other questions that had arisen about how social processes could be decisive for the scientific enterprise, in ways that one did not anticipate.

Thus with a scientist’s bent I lent an ear to the positions and recommendations of professional social science, specifically sociology, and I was intrigued by Sacks’s claim for the possibility of sociology becoming a science. I had learned what science was and the difference between that and what sociology was doing was obvious. There were the departments of “Letters and Science”, ideas and facts, and Dilthey’s distinction between *Geisteswissenschaften* and *Naturwissenschaften* made this demarkation clear.¹ However social scientists seem to move between both in their data and analytic reasonings.

I had been a student at UC San Diego as an undergrad, and remained there as a graduate student in the UCSD sociology department. Several were familiar with Sacks’s work and recommended him to me. I read some of Sacks’s lectures and found them intriguing. It seemed scientifically reasonable what he was doing. He was subjecting “naturalistic” data derived from first an audio recording, and then a careful transcription of that material, to analysis. In other words he was putting the interactional social world “under the microscope” like the physical and natural sciences did with their subject matter, so I was interested. My interest

¹ For more on this see Hamann [2018].

grew and when I later became a graduate student at UC Irvine, Sacks agreed to become chairman of my doctoral committee.

Social theorists and their empiricist graduate assistants were intent upon describing the social structure using rigorous methods. There were a few experimentalists outside of the legacy behaviorists doing cognitive studies with pigeons and mice, those who were engaged in developmental psychology and psycholinguistics, but most were applied mathematicians who provided mathematical models. These efforts most often took the form of propositional logic allied with “game theory” presuming the rational actor. Uniformly, social studies adopted the vernacular categories used by the average social member as well as the investigator in their descriptions of whatever order they deemed present.²

Chomsky and the generative grammarians provided structure for those interested in testing language development and the Piagetian school engendered study of infants and early childhood behavior and cognitive ability. Empiricism in sociology and anthropology using natural language categories was combined with statistics but few probabilistic results were provided. There was much anthropological ethnography but, with exceptions [Birdwhistell 1970, Kendon 1977, Schefflen, Schefflen 1972], little ethology of recorded human interaction.

Using definitions and identifiers, plausibly reasonable studies produced and concluded for the purposes of their collegial fellows, that coherently organized collection of documents that are perusable any day within any upper-tier academic library staffing the research proceedings of social science, or by doing an online search. These studies, with some notable exceptions, stand in contrast to those collections that occupy the shelves and web pages containing research from the physical or natural sciences as articles in journals. The structure and detail provided in probabilistic analyses based on verifiable empirical data that the laboratory sciences displayed were noticeably absent in the social sciences, and for sociology this was particularly true.

Sacks was allied with an interesting group in academic sociology who called themselves ethnomethodologists, led by Harold Garfinkel. Ethnomethodologists pointed out that modern sociology was rather firmly rooted in natural language,

² See Rose [1960].

the vernacular categories of talk that both the lay and professional member utilize in their description of presumably real events. As a result there was a lack of analytic precision compared to those in the natural sciences.

Sacks offered an alternative phenomenological approach that bracketed off social meanings and focused on the structure and operational specificity of conversational activities. After exploring the philosophical underpinnings to their work, I concluded that Husserl and the phenomenologists were merely stating what everyone in the biological and physical sciences already knew and were enacting as the “scientific method” but was not considered problematic – only in the social sciences was there a sense of ambiguity about the claims and possibilities of empirical scientific research. “To the things themselves” is the primary directive of phenomenology and the *sine qua non* for science.

I wondered if there was an alternative strategy available to the social scientist that could provide more scientific results. What really impressed me about Sacks’s effort was that he had real data, not just versions of things, that preserved the characteristics of the objects being studied. This is a feature of all scientific studies and Sacks took great care to ensure data integrity. The happenstance of a new domain of inquiry became available simply by the invention of the tape recorder. I looked on it as if he had discovered how to use a new telescope that could display objects from ten times as far. It seemed to be a fertile field for future investigations.

One important assumed and taken-for-granted knowledge for adult social members consists of how to utilize the vernacular form of language to do conversation. It is necessary for many jobs and these ordinary ways of moving and talking are justified if only in terms of practicality. We are looking at a natural social organization that has evolved to its contemporary state and as Sacks proved, could be investigated systematically.

The bracketing out of all existing presuppositions about the social world in investigations might seem bizarre for the average social member if they were to practice this technique in everyday life. It is not a denial of reality, it is rather adopting the “theoretic” attitude as described by Husserl’s phenomenology to study processes without beginning with background assumptions, and these include the social members stock-in-trade categories for social activities.

The techniques Sacks utilized to differentiate his work from others operating within social sciences is similar to other scientific fields that advanced in much the same way. Astronomy, biology, and physics all began with lay descriptions

that with the help of technology proceeded to advance and reinvent the field in a productive and scientific way.

Evidence is important to lawyers and Sacks was a lawyer. He had a legal mind, and was attuned to the social dynamic like a businessman concerned with contracts. No less a personage than Stanton Wheeler, former Dean of the Yale law school, recommended him as a legal scholar even before his foray into the field of sociology. But I imagine that his interest in sociology was what made him adept in law.

What Sacks was doing was listening to recorded conversations collected in ordinary settings of everyday life in an unobtrusive manner. The question whether or not this could be counted as a certifiable scientific data collection and analytic procedure was left behind in the wake of results he found he could generate and report. Sacks suggested that the “average” social member was similarly situated to provide an account of such activities, but the fleeting aspect of social encounters did not provide the sufficient inspection possibilities necessary for systematic investigation and understanding. Modern recording equipment and careful transcription techniques provided a solution for this. Those data that he could collect in this way, of recording and transcription, were sufficient for his inquiries.

At first I thought that Sacks was trying to pry out structures from elementary language usage, something many have tried, and was satisfied to explicate some fairly obvious things about conversations. It was interesting if only because academic sociology had never considered examining social interaction using recordings and then transcripts for answers to sociological questions. Further, it was apparent that Sacks believed that an edifying description of the methodic practices of social members could be provided by using ordinary English. It was his practice to deliver his results in lectures. Although it might be suggested that he was “talking strangely”, he adopted the convention of providing non-technical explanations – describing something he felt even the average social member might be able to understand. Given the earlier caveats about using the vernacular and “member’s methods” in sociological descriptions one might think that this would compromise or contradict the possibility of providing a more “scientific” enterprise, but at this early stage of the proceedings a meta-language was not yet available. “Shop talk” at this point could not rely on some presumed background of knowledge that could be used as a foundation, instead he depended on the ability of his audience to simply recognize some truth in what he said.

In order to produce a successful doctoral dissertation a research focus needed to be at least interesting enough to yield sufficient resources to meet institutional standards. My view of such requirements was gleaned from an acquaintance

with similar proceedings in the physical and biological sciences at top-level institutions, and I had been convinced of their scientific adequacy. Discoveries that were awarded doctorates between the forties and sixties, and presumably thereafter, were concerned with the explication of generative structures that were proved to be at least logarithmically interesting and apropos of fact. So I looked for something like this to satisfy my career goals.

I wondered if an elucidation of such methods and studies that Sacks recommended could be empirically researched into a deeper sensibility than sociology had been accustomed. However, his empirical methods were practices he enjoyed without much in the way of explication. If he didn't explain his methods extensively, he did take considerable care in describing the sort of problems that he believed could be solved from investigations of conversation data. He taught what a "solvable problem" might look like in comparison to an interesting question that had no available methodology that could promise an answer or solution.

A word about ethnomethodology would be appropriate here. In academically healthy institutions the methods of natural science proceeded under the aegis that their findings could be independently verified and peer-reviewed by any interested investigator. The constituents of their data and the results that were provided were defined such that their repeatability could be guaranteed. No such results were granted respectability without this caveat. In the field of sociology, despite the considerable rigor and insight that could be displayed, nothing similar had been delivered. The ethnomethodologists tried to explain why this was so.

To a natural or physical scientist, most of what sociology recommended as its subject matter seemed curiously light years from what could be considered a domain of scientific inquiry. The calculus and finely tuned micro investigations of physical and electrical constituents have been the cornerstone of scientific progress for centuries, as bedrock for future studies. However it was the position of the professional social scientist, as well as the average social member, that such studies of social structure could not be made on the same basis. The belief is that there are no similar "social data objects" as there are "chemical data objects" or "biological data objects", though ethnomethodology suggested that there were. But what were they?

Transcriptions of Sacks's prior lectures were helpful in getting a grip on the materials. Spending three years in his research milieu at UC Irvine fortified these concepts. At the School of Social Sciences were a number of linguists, cognitive psychologists, and social anthropologists who looked at these research findings with some interest. Initially, I believed that coming from an experimental biology background that I would be able to provide some laboratory methods to the pro-

ceedings that could prove or disprove the facts that Sacks was putting together about everyday conversation. I adopted videorecording as my method of data collection. One co-worker objected to my use of the word “specimen” to describe some data sample. But analytic proceedings in other institutions commonly employed a similar vocabulary for their data without hesitation.

On seeing Harvey Sacks for the first time, and later in his seminar, office, and house, and in my home, I discovered an intriguing person. Not only was his intellect extraordinary, but his appearance was a little startling at first. First of all he couldn't have been over four and a half feet tall but perfectly proportioned. His handsome face, stylish garb, and winning smile and voice quickly belied any hesitancy to approach him and try to gain his confidence. Although possibly a bit eccentric, his intelligent manner and startling intellect allowed one to excuse his non-conformity to academic custom. He would pause during lectures and in conversations, and take time to think. Sometimes he would hold the floor in a silence for minutes. Then he would answer sometimes as a lawyer discussing a transcript in an important legal proceeding. It was a bit disconcerting sometimes how he could focus unrelentingly on a conversation detail.

In 1971, after I expressed interest in his work and wondered if a graduate student tenure was available, he invited me to his home in Trabuco Canyon, near the O'Neill Wilderness Park, to discuss the possibility. His house rivaled the best architecture I had ever seen. It was obvious this was a radical person as well as a brave scientist. And scientist was what he was, as well as whatever else he was. I had seen and met others, and knew what they were like. They were rather uniformly fascinated with their subject matter. Nothing could give them more pleasure than to research with increasing depth and reward. But more like a lawyer with a series of wins, Sacks relished discussing the intricacies of his victories. What a gift to be able to ply his trade in an unexplored domain of scientific inquiry. He offered his tutelage in the form of an apprenticeship and I accepted.

I should say a word about the positive role of marijuana in these proceedings. The phenomenological method that Sacks at least initially utilized to discover structures in conversation was catalyzed by the ability to stand a bit apart from the everyday world, wherein these structures were found to exist. This meant, while being slightly high, the seemingly small world of conversation could be scrutinized pleasantly as a phenomenon. Scientific practice always required a distance between that which was analyzed and the researcher. This was not otherwise easily available to conversation analysts in that their research practices and subjects were embedded in the same milieu that they studied.

Sacks's seminars were occupied by analyzing a piece of conversation gleaned from naturally occurring events, where the taping was made as unobtrusively as possible. Then a careful and detailed transcript was wrought that one could easily study, in addition to listening to the audio recording which could play and replay small segments. If a piece of the conversation was interesting enough, or pertained to other structures he was researching he would discuss it. He would sometimes proceed with a monologue on the item, and then take questions. One time he was looking at a transcript segment (from the Upholstery Shop materials) and asked the six or seven graduate students in the seminar to parse a sequence of recorded and transcribed talk. I was familiar with a sense of parse from grammarians, but not how a similar mechanism operated within the practice of conversation analysis. I was puzzled and didn't know what he could mean, but saw the other students penning their versions. I still don't know what Sacks meant by parsing. Other than that I felt I usually understood at least something about almost everything that he said and wrote. What I discovered early on was that he was usually right.

The attractive thing about it all was the vivacity of the research effort. Sacks's exhilaration in finding and talking about these often intricate structures in conversation was contagious. Watching him "working over" some audio recording with a transcript and "pull out" things that you could independently verify in your own data sources, left little doubt that this was a generative fact-finding mission, of the kind one might see in the laboratories of the natural or physical sciences. However, there was really nothing much in his published and unpublished writings that provided a similar experience.

When I began at UC Irvine as a graduate student with Sacks, the Upholstery Shop transcript was being discussed. Richard Frankel had collected shop floor conversations in a New York City upholstery establishment. The transcript was about 200 pages long and I believe it was produced by both Ms. Jefferson and Mr. Frankel. Work among associates tended to hover around a single transcript at any one time. Graduate students would offer their own analysis during the seminar of this or other data they had collected.

Let me say that the proceedings were deemed important and the fight was intense to see who could impress Sacks. Collegial associates, professors, graduate students and other parties would drop into his seminar and seemed to listen to him variously as a character, a charming little man, a strong jawed lawyer from New York, a mind that hovered over the grass like a bird picking out things here and there, finding reward in victories posterity would know he had achieved. He was collecting bushels of facts within a domain heretofore unexamined.

The lectures that were mimeographed and distributed around were not the whole story. I found that in his research proceedings Sacks was quite different than his verbal and published work could or would impart. Like a good trial lawyer, he read a transcript with an inquisitive eye, one that could distill the contractual imperatives implicit in the talk of competent participating adults, wherein even a first slight legal ambiguity must give pause and warrant further investigation.

It wasn't some kind of artful ethnographic description that Sacks demanded from his students. The careful examination of existentially real events could plausibly reveal interesting structures. However Sacks was looking for something more – something more and something more systematic, like the natural sciences regularly offered. In several places in his early investigations we could see what he meant.

Having had an opportunity to peruse the rather extensive set of studies concerned with “conversation” that has accumulated since the years of my acquaintance with Sacks and his demise, it became apparent that something of the focus of that work, the orientation, and definition of the subject matter had been lost. The clarity of his task became reckoned toward the end of his life, and the studies that preceded often must be considered in light of the progress that had been made. I hope to impart some of that.

The process whereby a scientific enterprise establishes its credentials is not apparent from the first. But we might as well start with the initial research proceedings where the clarity of the enterprise and its purpose was not so in evidence. So let's begin at the beginning, or thereabouts. Take the transcript of the *Proceedings of the Purdue symposium on ethnomethodology* [Hill, Crittenden 1968], in which Garfinkel, Sacks and others answer questions from sociologists, it is a pivotal discussion on what ethnomethodology and conversation analysis – at that time indivisible – is all about. How would a physical or natural scientist react to this? They might go, Oh my God, they still haven't decided on methods. They are studying member's methods but they themselves don't have a method or a consensus on a method, or what is relevant about the subject matter in the first place?

We need to recap what we discussed earlier. In the natural and physical scientific establishment a required rigor is presumed to be present in studies that recommend one or another probabilistic fact. This rigor is most emphatically displayed in the presentation of data that is independently collectable and verifiable. The claim

has been that, although such studies in the established sciences have no trouble locating their data as for example, specifically “chemical objects” or “biological objects”, the practice in the social sciences has no such data objects with equal specificity that may be used to provide scientific results with equal standing. This was the problem that Sacks and the ethnomethodologists addressed at the Purdue symposium. They claimed that there were “social objects” and through a fledgling research practice these “social objects” could be identified and analyzed.

To illustrate what they could mean, Sacks talked about his analysis of the utterance “Rita is Una’s mother” where he described a member’s identification problem, and how that problem could be solved by social members [Hill, Crittenden 1968: 30ff.]. He found that how social members solved problems was evidenced by the results of their application of a set of techniques oriented to the practical contingencies present within the domain of conversation. He found these sorts of solutions to problems operative in massively present instances of everyday conversation. The reality of social structures would be described as sets of processes that social members commonly used in the practical social world to solve problems they faced in the ordinary conversations of everyday life.

In the *Purdue symposium* the idea of what a “member’s solution” to a “member’s problem” could look like is displayed. A possible solution to the identifier problem was to select a category from a collection that parties may subsequently use to identify a person in that conversation. For example the collection of identifiers in the “family” collection. He suggested if there is a solution for competent social members in conversation for one-person identification by using a two-person identifier, there might be analytical interest in trying to determine if similar one-person identities could be provided by the use of three-person identifiers or n^{th} -person identifiers, and thereby place the person in a group with which further inferences could be made.

A similar issue provided the focus of Sacks’s doctoral thesis, which examined the problem of identification as a serious one for social members. This members’ practice in conversation was described in terms of a “consistency rule”, where a first enlistment of a category from a collection inferred that subsequent categories from the same collection be used to describe that party or other parties for the purposes of the conversation. He suggests that it is not the psychological identity that matters for the social process, but the social identity.

The claim was that the initial “identifying device” implicated a constraint on subsequent utterances, or even “topics”, by restricting their ability to provide identifications from any other collection except the one from which the initial

identifier was selected. Rather than employ some kind of statistical study to assess the basis of the consistency rule, Sacks used specific instances of data for the argument.

I had read Wittgenstein's description of something similar in *Remarks on colour* where he describes various analytical approaches that could be used in describing color. He found that use of an identifier determined a collection of identifying categories that could then be consistently utilized to describe colors within that situated "language game". The point was that this determination constrained participants to adopt similar constituents of that collection for the purposes of identification. A physicist in talking to another physicist might identify a particular color as a wavelength of light that differed from another because of its absorption in a certain kind of reflective media, where the variables were tightly controlled and sophisticated instruments were used to measure. And on the other hand Wittgenstein considered how another person with the same "data", for example, the difference between two shades of green, might explain it in terms of how much blue was mixed with the yellow. The initial category of description was selected from a collection which contained other such identifiers, and this unavoidably constrained the type of discussion that could take place within that interactive environment.

Sacks logically proceeded to carry his dissertation hypothesis one step further by considering the conversational organization of "topics". If we assume that identifiers provided in a conversation will be economically and consistently selected from the same collection from which an initial identifier is provided, conversationalists might be further constrained by a "topical coherence" requirement to which subsequent speakers must attend.

It might seem at times that there was a lot of talk about little. But soon enough it was clear that the landscape was belligerently abundant in details about which we had little clue, only Harvey Sacks did. An interesting fact about this guy was that he was operating pretty much alone. Others working in related areas would call the endeavor "conversation analysis", and that was alright with him.

Tangential investigations that may or may not invite polemical commentary were always avoided by Sacks. He would be polite, but when bullied would always try to turn attention to the data, where he felt comfortable. I think he felt his subject matter an orphan, which on occasion would be granted a handout by linguistics or anthropologists, and less often by sociologists. He reckoned the sensibilities of social scientists to his work was controlled by the same practical orientation with which any social member brought to an examination of the

social world. Sacks was discussing something about which anyone might have an opinion, and his lack of seemingly sophisticated methods did not recommend that his version could be granted some special accord. His entreaty to these forms of attack was always to return to the data, which for him were recorded and transcribed conversations, and if you could argue an alternative account on that basis he would listen.

In examination of topical coherence as a warrantable mechanism for further talk it became clear there were additional “sequentially implicative” items present that allocated turns on the basis of understandings that had nothing at all to do with “topics” or “topical coherence”. A more general approach was adopted that tried to explain what sequentially implicative relevance any particular item might have, and how that item achieved its relevance in its operations within the local turn allocation “machinery”.

In the matter of a conversationalist wanting to tell a story Sacks found that the resources and mechanisms that conversationalists used for providing any “extended description in conversation” were significantly constrained. Any multi-turn opportunity involves the requirement that the speaker must control a present and future allocation of turns with which to provide their desired content.

An orientation to the contingency of turns and members’ practices to obtain them are displayed in the use of sequentially implicative devices whereby future turns are enlisted. It was found that members relied upon culturally specific mechanisms to generate a “warrantable” turn or turns for subsequent utterances. This practice was investigated initially by simply collecting instances where multiple turns were allocated on the basis of a story prefaces like the one above.

Although the dynamics of turn allocation did occupy his interest, Sacks was intent upon providing his students and colleagues descriptions of conversation that served to illuminate the depth and scope of his inquiry. His practical circumstance entailed that he was personally and principally responsible for the fate of this academic enterprise. He good-naturedly accepted the burden of the business, and would off-handedly joke about his role.

Sacks’s personality matched the novelty of his fledgling enterprise. It would take a while to get to know him before one could be convinced of how seriously he regarded the endeavor. He wore his hair long, and usually smoked, and would put his feet up on occasion as he expostulated upon a piece of conversation data, and as mentioned took a long time sometimes before he replied to a question, or provided a comment. His background was telling.

Sacks received a B.A. in sociology at Columbia College, an LL.B. at Yale Law School, and a doctoral degree in sociology at the University of California, Berkeley. He tried not to be intimidating and sometimes succeeded, but what one learns to know about lawyers is that they mean business. So the kind of care and attention to detail that is characteristic of both scientists and lawyers was supplied in his arguments that one learned to be careful about because they were rather deeply rooted in empirical fact.

One wondered if his legal training and style had led him to believe that conversation was a living example of law – the law a practicing attorney would know – practical law, law that existed in the sinews and crannies of the business world, the law of “your word was your bond”, the arena where agreements and contracts were hammered out within a competitive arena wherein the allocation of commodities and the dynamic of the marketplace displaced any doubts about the realities of the interactional world.

Sacks took care in acceding to the scientific requirement of assuring the integrity of conversation data. The work of transcribing recorded conversation was a considerable task. Without the help of Gail Jefferson the work probably could not have been done. He would take time to meticulously examine recording and transcript, to ensure its quality. Also he had class work to consider. I’m sure he initially spent much time considering how he should fill the two or three hours a week of lecture time he was required to provide undergraduate students. This wasn’t an established science. He could presume no background or sophistication with the subject or methods of data collection.

The assignments in Sacks’s classes consisted of asking his students to record and transcribe some piece of a real conversation. He would then maybe spend time showing them that various conversation structures could be detected. His classroom strategy was to provide an overall appreciation of conversation, displaying his analysis of various bits and pieces. It was as if he had accepted Wittgenstein’s portrayal of the spoken language as an ocean of text and speech, as an “interactional city”, with main thoroughfares, avenues, and back alleys.

These undergraduate lectures were transcribed and distributed to students and other interested parties. I encountered these transcribed lectures in a box left unattended on the 7th floor balcony of the UC Irvine social sciences building. Later a collection of these lectures was organized by Dr. Jefferson into a two-volume set called *Lectures on conversation* [Sacks 1992]. Much of what became known about Sacks was provided by these. However one must realize that his reliance on ethnographic particulars in his lectures was markedly different than

the more focused practices of his research. In the one case he was satisfying the practical requirements of his role in the university, in the other he was involved in an untethered pursuit of discovery in a nascent science.

Although he had accumulated a considerable body of recorded and transcribed conversation data, with concomitant research findings as exemplified in his lectures, he wasn't particularly adept or inclined to formulate results in the form of publishable research papers. How much or little he could be considered reputable in the field of social science or specifically sociology did not concern him much. He was too busy with his subject matter to care.

The nature of his studies were such that he could often share little with his collegial associates. The views of professional social scientists were typically not in agreement, for reasons it would be futile to discuss. All his graduate students seemed to need to realize that he was genuinely pleased with his research results and these were displayed with an enthusiasm that was infectious.

Sacks hoped that his graduate students with a scientific bent could perhaps find reward in their own discoveries on the basis of his methodologies. It became apparent to me shortly after joining his entourage at UCI that the graduate student's task would be to select one of the many areas of interest Sacks had explored lightly and upon which he would like further detail and analysis to be provided. So I rather soon selected one that could be presumably considered a "subject" and began to try to formulate a more detailed reasoned analysis on the basis of transcribed data. I discovered the availability and possible utility of video recordings and so adopted that as my data source when possible. I provided transcripts of video recorded conversational events and some analysis when required in Sacks's graduate seminar. "Turn allocation techniques" became the focus of my studies during the several years of affiliation with him.

Other graduate students at the time included Alene Terasaki who shared an interest in similar mechanisms and provided results in the form of an analysis of "announcements" as story prefaces. Judy Davidson offered interesting data and commentary upon the relatively uncharted domain of "closings". Anita Pomerantz was concerned with compliments. Jo Ann Goldberg was working mostly on telephone conversations. At this point in time there had been two graduate doctorate degrees awarded by Sacks, one to Jim Schenkein and the other to Gail Jefferson, both of whom had found faculty positions elsewhere. I focused on the inference

from Sacks's analysis of "second stories"³ that turn allocation can proceed on the basis of mechanisms like the "story preface" and proceeded to study them.

Garfinkel and ethnomethodology's complaint was that the act of retrieval of conversation "facts" missed something essential in understanding the activities that CA (conversation analysis) examines. He and others suggest the "meat and potatoes" of social interaction is lost by focusing too narrowly on how the conversation is accomplished. It seems to them that the important thing is the social meaning of the interaction and how that matters. Further, some regard transcripts as an unjustified edit of the interactional circumstance that misses important features of the context and so limits understanding. CA notation is considered a "sterile" view of such proceedings, and is too "clinical" in its descriptions.

Sacks might humbly reply the assumption is simply conversation has many "jobs" and how people might accomplish one is a studiable item. All are dependent on a series of learned techniques that enable them to obtain a turn or series of turns, allocate them, and be ready to supply content in a warrantably appropriate manner. Discovery of these and a detailed description of how they operate was within his purview. This might not explain well or entirely other orders of "social reality" that may be present and, *prima facie*, deemed more important to consider. These "real world" structures commonly available to the average social observer as well as professional analyst Sacks's methods seem to miss entirely.

Does the transcriptionist's role expropriate the chances for a cogent analysis in a manner we need to explain? Sacks would say only slightly, as on occasion something external to the conversation not captured by a transcript may influence the dynamic process that he was trying to explicate. He regarded the present state of the "data", in its transcribed form, from actual audio and video recordings as sufficient for his task-at-hand however limited that might be conceived. So in answer to the question "Is there enough gas in the car?" the answer was yes for his purposes but not necessarily for others; and to Sacks, that was in fact the end of the story.

The problem of continuing in this research direction is plausibly more practical nowadays than in previous years. However the difficulty of suspending one's attachment to "readily available things to say" based on the "semantic implicativeness" of words and utterances may make this task daunting if not impossible for the average person and CA practitioners as well. Sacks accepted the premise and tenets of the scientific method within the domain of empirical

³ Sacks 1992 I: 764 (Part VII Spring 1968, April 24); and Sacks 1992 II: 3 (Part I, Fall 1968, Lecture 1).

research without limiting his discussion to those items that could be discussed under this context. His eclecticism was famous despite the fact that when cornered he would describe himself a “positivist” and sometimes even as a “technician”. He steadfastly remained allied to his data, in that he believed it was here that answers were to be found. The ineffability of conversation in its multitudinous forms can be documented by more poetic descriptions that perhaps let us understand it in a more insightful way; however, he believed that his investigations might shed significant light on a relatively uncharted realm of realities albeit on a more primitive level with perhaps a less apt ability to describe.

The contingency of turn allocation can be seen in much of the data. The constraints involved not only that a turn become available and that devices be available to satisfy the requirements incumbent a turn, but also that listeners are present and are listening. Current speaker is always faced with constraints on subsequent turn allocations, and it has been documented that this is particularly acute in circumstances wherein there are multiple parties competing for a turn, or multiple turns are required for a story to be told.

Sacks outlined the properties of “stories” as they are delivered in conversation. He dealt with story beginnings, constituents of stories, and story endings. In his discussion of “story starts” he states:

Let us consider another solution to the problem of starting talk under restricted rights. I’ll begin by introducing a term, “ticket”. . . . the item used to begin talk is an item which, rights not otherwise existing, serves to warrant one having begun to talk. For example, one turns to the other and says, “Your pants are on fire”. . . . It is not just any opening, but an opening which tells why it is that one has breached the correct silence, which warrants one having spoken. Tickets, then, are items specially usable as first items in talk by one who has restricted rights to talk to another. And the most prototypical class of tickets are “announcements of trouble relevant to the other” [Sacks 1992 I: 265].

Having located the use of a “story preface” item within a category of mechanisms that have the job of obtaining multiple subsequent turns of talk, he went further and asked the question of how anyone could succeed in attempting to “obtain the floor” – even for a single utterance. Such items were characterized as “turn allocative mechanisms” and the task became trying to determine their structure and how that was related to their effectiveness, or lack of it. This can be contrasted with the allocation of turns proceeding on the basis of formal systems that create agendas and speaking turns for meetings, for example.

During his seminar class, Sacks once offered a prize for the person who could offer an explanation for why compliments were rejected. Many years later I came up with a candidate answer that had simply to do with that the time interval between the utterance that accepts a proffered compliment and the sequential next taking the form of a request could be very small. And in general it is interesting how very brief increments of time can be decisive for conversation activities. There can be an instant of turn allocation wherein parties mutually recognize the fact and proceed on their way. Other arguments might have been that rejecting compliments simply signals modesty and is polite.

Let me say a word about how conversation is difficult to study because we may have some degree of involvement with the characters, semantics, or obvious motivations that might be of interest to us and thus distract from the research direction offered here. For the most part one must be content with ignoring what members might regard as the “meaning” of an utterance. It is rather the present researcher’s task to explicate how the item is used to satisfy the requirements of conversation. Although the materials may often display a worldly character that invite both investigator and participant a share in its understanding, it is seldom useful for the purposes of the methodology recommended here. These sets of common understandings that may attach to utterances must be suspended in an Husserlian sense.

A systematic investigation will be found to be able to proceed with little trouble within this framework as the turn implicativeness of many devices is quickly apparent within this purview without resorting to content considerations. Viewing conversation as an economic system where turns are commodities that are allocated and distributed through a variety of finely wrought social understandings and mechanisms was one of Sacks’s principal achievements.

A paper “Recycled turn beginnings” conceived by Sacks and written by Schegloff suggested a preference to remain within a research domain that focused on turn-allocation [Schegloff 1987]. It is an apt and cogent description of a certain mechanism that is utilized to obtain a turn-of-talk that repeats the device that initially attempts to obtain the turn:

- R: Well the uhm in fact they must have grown a culture, you know, they must’ve- I mean how long- he’s been in the hospital for a few days, right? Takes a bout a week to grow a culture
- K: I don’ think they grow a I don’ think they grow a culture to do a biopsy.

[Schegloff 1987: 75]

Because the turn beginning is overlapped, one hypothesis might be that the repeat is oriented to the hear-ability of the turn-beginning and the repeat is provided because the party may believe that the current speaker may not have heard it. Another points to the economy of the use of a repeat as a warranted constituent of a turn. Sacks often spoke of the “economy” of conversation in many of its parts, in the sense that a “minimization of necessary resources” is often preferred:

A: Yeah my mother asked me. I says I dunno. I haven't heard from
her. I didn't know what days you had {classes or anything
B: {Yeah an I didn' know

[Schegloff 1987: 80]

Situations in which there is competition for next turn can be differentiated from turn allocation when there is no competition for the next turn. Many conversations proceed without trouble or competitive overlap. However the examination of errors and repair was given research precedence over an alternative focus on the mechanisms of turn allocation.

In 1971 while a graduate student in sociology at UCSD I was interested in seeing Harvey Sacks in person and hear him speak. He had been invited by Harold Garfinkel to address a graduate seminar at UCLA. I drove up from San Diego and, through some misunderstanding of the venue, headed towards Harold's house in Palos Verdes. Arriving there I was informed that the talk had been relocated to a classroom on campus. I arrived late and a bit out of breath. Sacks was in the midst of discussing a “squelch joke”. He was in stylish garb and spoke with authoritative tone and sometimes paced.

He was discussing a joke that he recorded in a group therapy session and had transcribed. He regarded jokes as a special case of stories and proceeded to outline in great detail the constituent parts and how their performance was constrained by contingencies in the telling. He meticulously led us through the transcript like a skilled lawyer examining a deposition. It starts with a joke preface that is first accepted and then heckled and then it is finally completed after multiple parties' interjections. The punch-line was greeted with few laughs. What he noticed in it was an economy of form.⁴

Sacks discussed the simplicity a minimization provided for the parties identities, the methodic presentation of the two scenes, and the ability to be told with relative ease despite the interference. He also maintained that this facilitated re-tellability and served to preserve it as a socially-realized cultural artifact that

⁴ For a complete transcript see the published version [Sacks 1974].

could be handed down to subsequent generations of twelve-year-old girls as news relating to the ambiguity of sex. He noted that the joke's punch-line was delivered as a complete grammatical sentence, in contrast to many utterances where such grammatical rules are only sometimes evidenced in spoken language. Such items were seen as explicitly not sequentially implicative and often served as a conversation close:

Dorrinne: Uh-you know, it's just like bringin the- blood up.
 Theresa: Yeah well, THINGS UH ALWAYS WORK OUT FOR THE / / BEST
 Dorrinne: Oh certainly. Alright / / Tess.
 Theresa: Uh huh,
 Theresa: Okay,
 Dorrinne: G'bye.
 Theresa: Goodnight,

[Sacks, Schegloff 1973: 307]

Sacks comments:

One can, for example, propose that the sentence as an analytic object remains the unit for study. Insofar as actual speech doesn't produce only those sentences, then forget about natural speech, or forget about natural speech for now. Or, one might see whether there are means of accounting for the production of these various sentence-parts which constitute complete utterances. That may involve having to give up the notion of a "sentence". Or it may involve turning the sentence into one important analytic object, but not necessarily the only one. Or one might propose that the sentence is a special case of some other unit. One procedure we might use in order to assess a direction that should be taken from among possible directions, is this: We could ask what it is that provides for the occurrence of integral cases of sentences as composing utterances. That might give us certain sorts of rules [Sacks 1992 I: 648].

Of course, the written word is literally staffed by such items, and therefore we can presume a fundamental difference between reading and conversing. So we found these grammatical sentence items all over conversation and tried to explain them. The Chomskyian "deep structure" analysts viewed a grammatical sentence as a product of static learned rules, but considering them in a performance context was not within their purview. So our doom was mete. The principle finding of the study indicated that complete sentences had a relative lack of "turn implicativeness" for a next turn. This was significantly evident in announcements, second-pair parts, and story punch-lines.

Sacks discussed pauses and silences in conversation, differentiating between the two. He never accounts for the preference for "no gap" and why silences are dispreferred, other than that they can sometimes lead to a conversation close. Sacks's maxim "No gap, no overlap" may serve to explain an orientation to

preserving a “continuing state of incipient talk”, but he does not provide an explanation for it. When there is a suspension of the turn allocation machinery for participants, when parties become aware that a turn for a subsequent next utterance has not been supplied, they enlist a variety of mechanisms oriented to the possibility that the conversation may close if they do not supply one.

So that if there’s a seeming completion and no one has been selected, then the silence that ensues is such a silence as anyone’s talk can occupy, and the beginning of someone’s talk involves that they now seize the floor. Once they’ve seized the floor they have exclusive rights to produce an utterance. One could expect, then, that persons would attempt to seize the floor and have ways of attempting to seize the floor which would stand in some independence of having already prepared an utterance. That is to say, they could do it before they had formed up the sentence they were going to say. Which would then lead one to an expectation of a partial separation between what they seize the floor with – that being an issue of getting it in first – and what they’re going to say. Once they’ve seized the floor there can be a silence. That silence is now not a silence in the conversation such that anybody can start talking, but a silence in the course of their utterance, i.e., a pause [Sacks 1992 II: 497].

He found that speech particles often preface an utterance beginning:

(Rah:II:17. TS by G. Jefferson.)

IDA: Uh I went last Wednesdih yih know •hh
 Oh ↑by the wa:y=
 JENNY: =Oh didche ↑keep fi:t,

[Glenn 2003: 106]

And repeats often signal a conversation close:

JOHNSON: ...and uh, uh we’re gonna see if we can’t uh tie in our
 plans a little better.
 BALDWIN: Okay / / fine.
 JOHNSON: ALRIGHT?
 BALDWIN: RIGHT.
 JOHNSON: Okay boy,
 BALDWIN: Okay
 JOHNSON: Bye / / bye
 BALDWIN: G’night.

[Schegloff, Sacks 1973: 307]

Repeats in conversation closings often take the form of a reiteration of arrangements:

GP2–28

- 1 R: What's your address please.
 2 (0.6)
 3 P: Eighty four Tern Way.
 4 R: Okay then,
 5 (0.5)
 6 P: [So it's th-]
 7 R: [Thank you,]
 8 (0.5)
 9 P: That's the sixteenth?
 10 R: =The sixteenth, [at ten pa]st eleven
 11 P: [Okay then.]
 12 (0.3)
 13 P: Ten past eleven, thank you.
 14 R: Thank you,
 15 (0.2)
 16 P: T[hank you,]
 17 R: [B y e .]
 18 P: =Bye

[Stokoe, Sikveland, Symonds 2016: 4]

At some point in 1974 for personal reasons I took a leave of absence from UC Irvine, and a few months later I got word that Harvey had died. I returned and tried to resume my education but bereft of a committee chairman, and given the lack of published results and formulated research studies, this proved to be impossible. I went in different directions and when my association with UCI and Sacks's research ended I became a software engineer and presently work for a mobile app development company. But I retain an interest in these proceedings, and hopefully I have been able to impart some of the knowledge that I gained in association with the late Harvey Sacks, and have given a glimpse of where that research effort stood at the end of his life, and where it might lead.

So does the systematic collection of conversation data and a delineation of what members' problems and solutions look like within that framework constitute a legitimate scientific enterprise? Researchers must make this judgement on their own, but there is no doubt for me "that's one step for man, one giant step for mankind". Finally as a tribute to Harvey Sacks, for the man and the scholar,

let me say that I believe his most fitting legacy could be described as how he personified Hegel's notion of the idea, the confluence of concept and reality, the role of the scientist.

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Glenn Rockland

WSPOMNIENIE O HARVEYU SACKSIE Z LAT 1968–1975

Streszczenie

Ten tekst jest relacją na temat kilku lat studiowania analizy konwersacyjnej u Harveya Sacks na Uniwersytecie Kalifornijskim w Irvine. W późnych latach sześćdziesiątych i wczesnych latach siedemdziesiątych wykłady Harveya Sacksa, jego raporty badawcze oraz opracowana przez niego metodologia szczegółowo przedstawiały, jak można nagrywać, zapisywać i analizować zwyczajne rozmowy. Jako założyciel dziedziny badawczej Sacks przybliżył nas do ideału socjologii jako nauki. W ciągu kilku lat studiów u dżentelmena jakim był Sacks odnalazłem w nim wybitny intelekt oraz interesującą osobę, którą próbuję tu opisać poprzez chronologię rozwoju jego myśli, stosunku do etnometodologii oraz publikowanych i niepublikowanych prac. Włączam kilka osobistych wrażeń i refleksji, które są – mam nadzieję – pomocne w lepszym zrozumieniu tego człowieka i jego akademickiego ukierunkowania.

Słowa kluczowe: Harvey Sacks, analiza konwersacyjna, etnometodologia, socjologia