

# **Mapping Goffman's Invisible College**

**Wendy Leeds-Hurwitz**

The mediastudies.press *Goffman in the Open* series

# Mapping Goffman's Invisible College

by Wendy Leeds-Hurwitz

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# Contents

<i>Acknowledgments</i> . . . . .	xi
<i>Abbreviations</i> . . . . .	.xiii
<i>List of Tables and Figures</i> . . . . .	xvii
Chapter One: Introduction . . . . .	1
Chapter Two: Before Penn . . . . .	15
Chapter Three: People at Penn. . . . .	50
Chapter Four: Major Projects at Penn. . . . .	125
Chapter Five: Minor Projects at Penn. . . . .	190
Chapter Six: Penn Adjacent . . . . .	231
Chapter Seven: Beyond Penn . . . . .	323
Chapter Eight: Conclusion . . . . .	333
<i>Appendix: Peripheral Colleagues at Penn</i> . . . . .	366
<i>References</i> . . . . .	396
<i>Index</i> . . . . .	438

## CHAPTER SIX

# Penn Adjacent

There was, of course, a world outside of Penn, and Goffman participated in a variety of multi- and interdisciplinary projects that were neither begun at, nor closely affiliated with, his role at that university. Two of these were organized by scholars at Indiana University, and three by scholars at the University of Texas, Austin; given that all involved other participants from Penn as well, it seems reasonable to label them “Penn adjacent.” All but one of these projects were based in the US. All of them expanded his invisible college, although perhaps thinking of these as a set of overlapping invisible colleges would be more accurate.

### *Indiana University*

The earliest activity was a conference on interaction ethology, which Goffman co-organized with Thomas Sebeok at Indiana University, and which was held in Amsterdam in 1968. The second was the Multiple Analysis Project, known as MAP, organized by Allen Grimshaw, also at Indiana. (The project name was changed while still in the process of being organized: “Partly for acronymic reasons, this title has replaced the earlier ‘Joint Analysis Project.’”)<sup>1</sup> The conference is interesting for several reasons: It is one of the few that Goffman was involved in organizing; it provided the occasion for his invention of the phrase “interaction ethology”; and it demonstrates his own efforts to bring together scholars across disciplines, and thus his emphasis on ideas over disciplinary training and affiliation. MAP is interesting partly because it involved most of the central players among Goffman’s colleagues at Penn, despite being organized elsewhere; partly because of the way it demon-

strated how Goffman at the time was clearly aligned with Harvey Sacks and Emanuel Schegloff in terms of assumptions about how to conduct research on interaction, despite the common knowledge they did not always agree, and frequent statements in print that they took divergent approaches and were not on good terms; and partly because it was a failure—it was (finally, after several decades) completed, yet despite the enormous time and effort (and some money), the resulting publications had little influence, and the original goals were unfulfilled. We can learn at least as much from failures as from successes, especially those as well documented as this one.<sup>2</sup> There is an enormous amount of detail available for this project, yet almost nothing has been written about it, so it seems worth the time to sort out what happened, and especially why it was not successful.

## Interaction Ethology Conference

Hymes, Worth, and Goffman all organized and attended Wenner-Gren sponsored conferences. The Wenner-Gren Foundation for Anthropological Research has supported anthropologists through multiple types of grants as well as conferences and symposia since 1941 (see Douglas 1986; Lindee and Radin 2016 on that organization). Events are interdisciplinary by design: “Wenner-Gren International symposia recognise no boundaries—intellectual, national, or subdisciplinary.”<sup>3</sup> In the 1950s, when funding from a variety of national organizations, such as NIH, NSF, and NEH, became available to anthropologists, the foundation decided to emphasize “‘very good mavericks,’ scholars who fell between the existing categories as fashionably defined” (Douglas 1986, 523). Burg Wartenstein, Wenner-Gren’s eleventh-century castle in Austria, was deliberately established as an ideal location from which to host small, international, interdisciplinary summer conferences (Douglas 1986).

Worth organized a conference with John Adair through Wenner-Gren (*The Navajo as Filmmaker: Some Recent Investigations in Visual Communication*), which was held in 1967 (Wenner-Gren 1967). Hymes organized two events (*The Use of Computers in Anthropology* in 1962<sup>4</sup> and *Revolution vs. Continuity in the Study of Language* in 1964<sup>5</sup>) and participated in at least one other (*Folklore and Social Science* in 1967). Goffman participated in at least two others: (*Ethnic Identity: Cultural Continuity and Change* in 1970, and *Secular Rituals* in 1974), both at Burg Wartenstein, and co-organized a third (see Winkin 2022a on these as well as discussion of Goffman’s participation in the NATO Advanced Study Institute on Nonverbal Behavior Research



Methods in London in 1979, which overlaps with the Interaction Ethology conference in terms of topic and participants); the one he co-organized is the focus here. Interaction Ethology was held in Amsterdam from August 31 to September 4, 1970 (Sherzer 1971; Wenner-Gren 1970). Goffman co-organized it with Thomas Sebeok (who had previously co-organized the conference on computers with Hymes in 1962). Sebeok himself had organized a prior Wenner-Gren conference in 1965, *Animal Communication*, attended by Gregory Bateson, Charles Hockett, and W. John Smith, among many others, setting the stage for this one.<sup>6</sup> (Participants may not always attend to connections and overlaps between conferences, but intellectual historians must notice them.)

Goffman and Sebeok knew one another at least as early as 1962, since Goffman was a participant in an event organized by Sebeok, the Conference on Paralinguistics and Kinesics, held at Indiana University May 17–19, 1962. This was a deliberately interdisciplinary event, with “Five ‘state of the art’ papers . . . written by a specialist in one of the five principal disciplines represented (cultural anthropology, education, linguistics, psychiatry, and psychology)” (Sebeok et al. 1964, 7). As with the Macy conferences, the goal was to encourage debate, and so transcripts of discussion were included in the resulting book. Birdwhistell was also a participant, as were Margaret Mead, Charles Ferguson, Edward Hall, Norman McQuown, George Mahl, Robert Rosenthal, and Harvey Shands, along with dozens of others not mentioned in these pages. Birdwhistell and Mead spoke often during the discussions, and at length; Goffman spoke at several sessions, although a lot less. As at the Macy conference he attended in 1956, Goffman got into a bit of an argument with Mead again, although this time he seems to have placated her quickly through clarifying what he meant (139–40). Most interesting is that, in the session on cultural anthropology, presented by Weston La Barre, during discussion, Goffman says, “I have been asked to present my version of the frames of reference within which the material we have discussed might be placed” (232; it is unclear who asked him, but presumably Sebeok as the organizer) and goes on at great length—three pages—essentially serving as respondent to that session. Oddly, unlike at other conferences, almost no one picks up on what he says, and the conversation moved to other topics. However, Mead herself finally does pick up on something Goffman said in a later discussion, saying:

It would be very nice if we could go away from here with at least a preliminary agreement on the use of some phrase that we could apply to this whole field. Kinesics and paralinguistics, after all, are two.

We have been challenged by Dr. Goffman to say what we are doing and we are, I think, conceivably working in a field which in time will include the study of all patterned communication in all modalities, of which linguistics is the most technically advanced. If we had a word for patterned communications in all modalities, it would be useful. I am not enough of a specialist in this field to know what word to use, but many people here, who have looked as if they were on opposite sides of the fence, have used the word “semiotics.” It seems to me the one word, in some form or other, that has been used by people who are arguing from quite different positions. (275)

Sebeok responds by pointing out that the word *semiotics* is already in use to mean something else entirely, and proposes “the non-linguistic aspect of communication,” which no one likes, as it is not a single word such as she has requested. Finally, Birdwhistell proposes: “There is an old-fashioned word which we have been using and that is ‘communication.’ The word ‘communication’ relates to a series of special derived systems. I see no reason why the word ‘communication’ cannot be elevated to meet this need” (275–76). In fact, that is the term Birdwhistell used ever after for the broad study of social interaction, including both verbal and nonverbal behavior. Both “semiotics” (defined as “patterned communications in all modalities” (5) and “multimodal” (with no definition provided, either on page 7, or during Mead’s presentation, on 286) are used in the preface as cover terms for the topic discussed at the conference, but with a clear preference for semiotics as the more obvious choice (see Mondada 2016 for a good introduction to multimodality). The question of a name for what they were studying recurs across numerous contexts.

To return to the Amsterdam conference. Sherzer was a participant: He apparently “helped Goffman and Sebeok” in some ways as they were organizing it,<sup>7</sup> and published a lengthy description, although there was no proceedings volume (one was never intended: the formal invitation says, “No published report is envisaged; accordingly, no tape or stenographic recordings will be made”<sup>8</sup>). Sherzer reports that “the conference was a natural extension or rather a formal recognition of the unity of the varied interdisciplinary research currently being carried out by the participants” (1971, 19). The names and disciplinary affiliations in table 6.1 come from Sherzer.

*Table 6.1: Interaction Ethology Conference, by Discipline*

<i>Anthropology/Sociology</i> : Gregory Bateson, Erving Goffman, Suzanne Ripley, Harvey Sacks, O. Michael Watson
<i>Linguistics</i> : Kenneth L. Pike, Thomas Sebeok, Joel Sherzer
<i>Nonverbal communication</i> : Paul Ekman, Adam Kendon
<i>Psychiatry</i> : Harvey C. Shands
<i>Psychology</i> : Michael Argyle, John H. Crook, Robert Sommer
<i>Zoology</i> : Stuart A. Altmann, Michael Chance, Irenaüs Eibl-Eibesfeldt, Heini Hediger, Hans Kummer, Glen McBride, Desmond Morris, W. John Smith, J. A. R. A. M. van Hooff

While Hymes was invited by Goffman, and fully intended to participate, he had a conflict with the date.<sup>9</sup> Susan Ervin-Tripp and William Labov were also invited but were also unable to join.<sup>10</sup> Goffman did not know all of the scholars who did attend, but he definitely knew Ervin-Tripp, Sebeok, Sacks, Bateson, Ripley,<sup>11</sup> Sommer,<sup>12</sup> Smith, and Sherzer, and at least some were citing his work long before this event (e.g., Sommer and Osmond 1960). Presumably, he knew Chance, Morris, and Crook as well, for he provided their addresses, along with that of Ripley, to Sebeok for the invitation to participate (Jan 21, 1970, TS).

Goffman recommended the following substitutions in case not everyone invited responded positively: Emanuel Schegloff for Sacks; Ralph Exline or Ekman for Argyle; Joel Schaeffer (who worked with Albert Scheflen) for Adam Kendon;<sup>13</sup> Birdwhistell for Bateson; some of their students in lieu of Chance and Crook, and the same for Smith. He also suggested that they had enough linguists without McQuown (Jan 21, 1970, TS). Sebeok agreed to most of these ideas (though he balked at paying overseas travel for a Smith student) and said this about Birdwhistell: “If Bateson can’t come, I will write to Birdwhistell next—sigh, sigh! (I think that between the two of us we should be able to handle him)” (Feb 4, 1970, TS), so apparently he was not a fan. Overall, Goffman was more worried about the balance of research specializations. “My only concern is that we might end up with too many ethologists who haven’t yet begun to look systematically at humans. One or two we can and should stand; but too many will thin things down a bit



in terms of current work being presented.”<sup>14</sup> This is especially significant because it became one of the concerns he voiced after the conference as well, and it clearly shows his ability to step back and consider general questions such as balance in theoretical approaches. Further along in the correspondence, Goffman wrote: “Our minds are so much alike in these matters that when you have doubt and wonder what I think, and there is no easy time to find out, please assume that I think what you think.” (That letter begins: “Dear Tom: I love you” [Feb 9, 1970, TS].) Goffman was thrilled that they had received the grant to hold the event, and at one point during the organizing discussions said, “I am seeing Lita tomorrow. I will kiss her for you after I kiss her for me” (Feb 26, 1970, TS; Lita Osmundsen was president of Wenner-Gren). The correspondence showed that they had a pretty easy time organizing everything. One of the other concerns Goffman expressed was “to make sure that enough participants are ready without long preamble to present a densely-packed example of actual, ongoing work.” And once the letters of invitation went out, as Goffman felicitously phrased it: “People seem not to be declining” (Feb 16, 1970, TS). In fact, nearly everyone who was invited was able to arrange their schedule to attend—with the notable exceptions of Hymes, Labov, and Ervin-Tripp, as mentioned, three of those Goffman knew best. Goffman acknowledged Sebeok’s organizational skills, saying, “If you run for president, I will become an American citizen and vote for you” (Feb 18, 1970, TS).

Goffman was particularly involved in organizing the event’s structure. “I think evening discussion should emerge as a reaction to and compensation for what occurs formally; and I think this can only happen when those who want to leave or are not wanted have a chance to move off” (Mar 24, 1970, TS). And he drafted the description of what they were trying to accomplish, and the program. His draft says:

The conference is designed to focus on the formal patterns of behavior overtly manifest during face-to-face interaction among humans. The emphasis will be on description as opposed to final explanation, and the approach that of the fine-grain contextual analysis of sociolinguistics and ethology.

It is hoped that the participants will assume that they are in an audience of like-minded peers and will spend no time presenting arguments concerning the significance of the area or the fruitfulness of particular methodologies. The participants will be assumed to have reasonable familiarity with all the published material in the field, and it is to be hoped that we can move directly to a consideration of current finds and hypotheses. (May 6, 1970, TS)

The major topics people were asked to choose between were conversational mechanics, contact phenomena, dominance behavior, spacing and territoriality, facial displays, on differences between animal troops and human gatherings (May 6, 1970, TS). The plan was to meet from 9:30 a.m. to 12:00 p.m., then break for lunch, resuming discussion from 2:00 to 5:00 p.m., with evenings free. (Or, as explained, free for those who wished, while the others would meet to discuss what happened during the day's sessions.) Goffman put the goal of the conference especially clearly in a letter to Sebeok: "Our focus is on observable patterns of objective behavior occurring in natural settings where the context involves two or more people. That excludes the big things like institutions and the internal things like souls. What is involved is the discipline modesty that sends individuals out to look at small behavior" (Apr 29, 1970, TS). Goffman also could be quite explicit about what he did not want people to talk about, once they submitted their proposals. For example, as he wrote to Robert Sommer:

Honestly, the last thing in the world I want to see you do is to elaborate on dramaturgical terms from my retired books. . . . What we are trying to cultivate is what you illustrated beautifully in your small group papers. It is that sort of thing that we built this conference for. My stricture was that you tell us about patterns you haven't published on yet, and tell this to an audience whom you can assume starts with your interests. You don't have to sell us on the work you've done, but just provide additions to the shelf. Of course you can generalize on your findings, that would be lovely, too. But for God's sake, pick the terms that are naturalistically the most convenient for you and not things that I've done. (Apr 30, 1970, TS)

Goffman prepared a revised program and sent it to Sebeok along with an offer to coordinate any further changes since he would be based in the US during the summer. At that point, the major topics were down to three: behavior in gatherings, conversational encounters, and contact phenomena (May 12, 1970, TS).

During the conference, it was Goffman's role to summarize what they had learned, and on the last day he outlined "the subareas of interaction ethology" as follows: traffic systems, states of talk, territoriality, supportive rituals, turn-systems, remedial work done by individuals in interactions, social occasions, and tie signs (Sherzer 1971). Particularly interesting is the way Goffman gracefully combined what he and others, but especially Sacks, had come to understand about the structure of interaction. For example, Winkin (2022a) points out how close this list is to the major topics

in *Relations in Public* (Goffman 1971). It is worth mentioning that Sebeok reports Bateson “took a leading part in our Amsterdam get-together” (1991, 22), which supports Erickson’s suggestion that Goffman had worked with Bateson while at Berkeley. Sherzer’s review of the event concludes: “The success of the conference on interaction ethology was its delineation of a new field pulling together the resources of recent research in a number of disciplines. The potential weaknesses of such a field were also present at the conference—the misunderstandings and oversimplifications endemic to broad interdisciplinary work” (1971, 21). Although no book resulted from the conference, its influence is evident in books that followed from W. John Smith (1977) and Adam Kendon (1990).

Long after the fact, Sebeok wrote about the event, specifically explaining how they arrived at the title.

Goffman and I together once amicably organized and ran an international conference, in total concord on the venue (Amsterdam), the participants, and topics to be discussed (Sherzer 1971). Yet we could not agree on the title. I wanted this title to reflect the identifying trademark, “Semiotics.” After all, it was aspects of that which we were in the Netherlands to discuss. Goffman, who years before had published what I considered his most insightful contribution to semiotics, *Stigma* (1963 . . .), and who later cheerfully offered his “Footing” to be first published in *Semiotica* (1979[b]), kept insisting that he didn’t wish his work pinned down by that term, indeed by any one term. Hence we compromised on the *ad hoc* weasel phrase “Interaction Ethology,” which scarcely anyone, save a participant or two in that conference (e.g., Drew and Wootton 1988, 91), has used before or since (although “interaction order” does appear in the subtitle of this collection of essays on Goffman [ibid]). (1991, 21)

Sebeok at one point used the two titles as synonyms, referring to “the holistic field of interaction ethology (alias semiotics)” (1975, 10).

In fact, Sebeok turned out to be wrong that scarcely anyone would use the phrase “interaction ethology”—for it was noticed and is still in use more than fifty years later (although sporadically, no question). For example, Ciolek, Elzinga, and McHoul say:

We have been inspired by Goffman’s call for an “interaction ethology” in which what he as termed the “ultimate behavioral materials” of interaction—the glances and stances, the gestures and utterances of participants—are examined, not for what they reveal about the participants as individuals, but for what they reveal about how people organize their behavior so that they can become participants in interactional events. (1979, 2)

More recently, Pallante et al. argue that Goffman was “an influential figure within micro-sociology, who suggests that this subfield should be practiced as an ‘interaction ethology.’ His realization was that it was ethologists that had developed the most detailed methodological skillset and procedures for systematically observing behavior in situ, and this should be taken as a methodological model for how micro-sociology should be conducted” (2022, 2). Knapp also mentions the phrase; although he argues against using it (because “human ethology is incompatible with the structuralist approach”), he still feels Goffman’s books “are on a ‘must read’ list for anyone interested in learning about the ways human beings organize their encounters” (2013, 19). Kendon mentions that the phrase interaction ethology is to be found in *Relations in Public* (1971, x), proposing that: “By this he [Goffman] intended recognition of the fact that in the study of interaction from the viewpoint of how occasions of it are organized, there is much in the method of observation and analysis used that resembles the methods of the ethologists” (1979, 21n50). After running through the arguments for the need of a new name to describe studying face-to-face interaction (and citing Goffman 1967 there), Kendon considers multiple potential names various scholars were proposing in the 1970s: his own “the study of the organization of behavior in face-to-face interaction,” Schefflen’s “the communication systems approach,” or Duncan’s “the structural approach,” the ever-popular “nonverbal communication,” of which Kendon said, “Even if it were the case that we could somehow study how people interact with one another *vis-à-vis* apart from the words they use, it is inelegant and hopelessly vague to label a field by saying what it is not” (21n50). He also brings up semiotics, correctly attributing that suggestion to Mead, but he argues quite reasonably that “we cannot use a term which is now well established as the name for a broad field of study as a name for one of this field’s subfields” (21n50). That is what gets him to the phrase interaction ethology. Finally, Streeck and Mehus argue: “Goffman pleaded for a framework of ‘interaction ethology,’ combining Durkheimian categories of norms of conduct with ethological perspectives in the study of ordinary human conduct in face-to-face interaction” (2005, 384). Despite all these comments showing Sebeok to be wrong that no one would ever use the phrase again, he was also essentially correct for, although the term clearly has been used by some scholars, interaction ethology is not currently in widespread use as the phrase describing the approach Goffman and/or others developed.

Once the conference was over, Goffman sent Sebeok a detailed analysis, and proposal for follow-up events. Briefly, he was delighted with how it had gone, but thought they now could do more, especially to bring more sociolinguists to a future conference, since this one had been heavily weighted toward ethologists. Of course, if Hymes, Labov, and Ervin-Tripp had attended as planned, they would have significantly improved the balance. Goffman highlighted the importance of bringing together ethology and linguistics as “the most promising naturalistic disciplines” of the last twenty years and saying that they “have induced a small core of anthropologists, sociologists, and psychologists to engage in naturalistic observations of minute strips of social interaction and to try to employ sophisticated techniques of analysis of a nonstatistical kind” (Oct 7, 1970, TS). He mentioned that the Amsterdam event was the third conference in three years on related topics (presumably, one was the conference on nonverbal communication he worked on with Kleck, explored in chapter 7), “and was incidentally the best.” He argued for holding two more meetings: one emphasizing sociolinguistics, and the other ethology. The former was needed for “edging socio-linguists over into a full-fledged look at social interaction in which speech occurs.” The latter was needed because ethologists “must develop a sense of how extraordinarily difficult it is to make any sound statement of a function in regard to human behavioral practices” (Oct 7, 1970, TS). It was not explicitly stated but implied that he was offering to help organize these events. He proposed a specific structure, at least for the sociolinguistic conference, thus presaging MAP.

### *Follow-up Activities*

Goffman returned to the idea of one or more future conferences a week later in a letter to Sebeok, saying that he would be in San Diego (AAA met there in November 1970, so that is likely what he was referring to),<sup>15</sup> and hoped to see Sebeok there to talk further. He also wanted to share the news that “a group at Texas is attempting to fund a conference on sociolinguistics in the interactional sense,” which would mean that one of the two follow-up events he had earlier proposed would happen (Oct 23, 1970, TS). Goffman was referring to the conference organized by Bauman and Sherzer on the ethnography of speaking, held in Austin, Texas, in 1972 (summarized in the section on Texas, below). The younger generation was taking responsibility for the next obvious step, so neither Goffman nor Sebeok needed to spend time organizing a sociolinguistic follow-up event. In fact, Goffman may well

have discussed the need he saw for such an event with Sherzer, given that Sherzer had been at the Amsterdam conference.

#### CLEVER HANS CONFERENCE

There was eventually a second conference related to Goffman's suggestion for a follow-up among ethologists as well, although it had been under discussion by Sebeok and others previously and so was not a direct result of the Amsterdam conference. In any case, this was one at which Goffman was a participant despite not being represented in the resulting publication, which is something of a story. Briefly, Sebeok organized the Conference on the Clever Hans Phenomenon: Communication with Horses, Whales, Apes, and People, held by the New York Academy of Sciences, May 6–7, 1980 (published as Sebeok and Rosenthal 1981). There was some overlap with Amsterdam: Hediger and Ekman participated, as well as Goffman. In addition, Starkey Duncan Jr., who was peripherally involved with the MAP project, also participated. Sebeok must have written Goffman with the idea for the event in February 1979, for there is an answer in which Goffman says it is “a wonderful idea for a conference,” but unfortunately, he has conflicting plans (Feb 19, 1979, TS). Sebeok clarifies that the event is scheduled for 1980 not 1979, saying, “Do come, if at all possible; the Conference wouldn’t be the same without you!” (Feb 19, 1979, TS).<sup>16</sup> Goffman could participate in 1980, but he said, “I still don’t see what I can write a paper on. Do let me come merely as a carper” (Feb 26, 1979, TS). Sebeok was fine with that, but warned Goffman that the Academy would only cover expenses if he played some formal role: “I suppose this means being a discussant, or ‘carper,’ or something of the sort” and proposed a role as discussant on the final day, although he would have to check in with the Academy about whether that would be approved (Mar 5, 1979, TS). So that was what happened: The Academy accepted Goffman as a discussion leader, so he would not present a paper. He was listed in the program as discussion leader for the morning session, with Sebeok and Rosenthal serving the same role for the afternoon session, on the final day.<sup>17</sup>

But a problem arose when it came to the published results of the event. In August 1980, Goffman wrote to Sebeok:

Just got back from vacation to find some 70 pages of type-script and a request for its edited return in a week. The jibberish I would have to reconstitute into orderly talk would require at least a month to do so, and I have returned to some more duties even more pressing. So I don’t see how I can get anything to you. I



guess you'd best drop me from the proceedings, which I understand was a right. Sorry about all this. The thing comes at just the wrong time. (Aug 7, 1980, TS)

Sebeok was “very sympathetic to your position” and hated to divert him from “more important writing commitments,” but he did not want to go ahead without Goffman, and he explained the logic. It was not only that “the volume will be a much diminished piece of work without your masterful summary statement and interventions.” It was also that: “You made numerous interventions to which many of us severally reacted. Now many of the ‘reactants’ have already returned their ‘cleaned up’ texts, which make specific reference to one statement of yours or another. I just don’t see how I can impose on our colleagues and now ask them to again reshape their texts, on the assumption that you were ‘not there’” (Aug 11, 1980, TS). Sebeok offered a compromise: that Goffman could take a month to sort things out. Unfortunately, no response has been preserved. What we do know is that Goffman was not included in the final volume, so he must have said he simply did not have time to do what was needed. In the published volume, only Duncan (1981) refers to Goffman, mentioning at the same time Kendon, McQuown, and Schegloff. All comments by Goffman (or anyone else) have been removed; the volume reads as if participants had simply submitted their written papers. There is no mention anywhere of Goffman’s participation, and no synthesis provided at the end of the volume, as there was at the event in person.

Even so, we know that the discussions apparently were quite impassioned. Goffman mentioned to Sebeok having received a letter from the Academy, “asking if I was concerned about the heatedness of the conference, and I wrote back and told them that they should be ashamed of having such concern, for the history of natural science has been periodically vented and needfully so, by exactly that sort of commotion” (Aug 7, 1980, TS). Susan Fowler (editor of the journal *Lab Animal*) attended but did not present. Her published description of the event makes clear that she was unimpressed with Sebeok and most of the others, saying it had been “an unprofessional, unpleasant attempt of the part of a semiotics professor to discredit the whole area of ape/human communication research” (1980, 356), only serving “as a showplace for its organizers’ prejudices” (359). Perhaps Goffman got off easy by not being included in the published results. In any case, this event was clearly not a good example of positive connections across disciplinary boundaries. Even so, it is noteworthy that at both the Amsterdam conference in 1970, and Clever Hans in New York in 1980, Goffman was the one who

provided the summary and synthesis, as he had earlier for the conference at Berkeley; he was particularly suited to that role, as he was well able to sort out what had been learned and to highlight what work remained.

#### OTHER RELATED CONFERENCES

In addition to these two conferences, there were at least three other events on related topics, far less central to this story, but they probably merit a brief mention. First, Goffman was a panelist on “Ethology and Sociology” at the American Sociological Association meeting in 1972 in New Orleans, sponsored jointly with the Rural Sociological Society. The panel chair was Neil H. Cheek (Georgia State University), and the other two panelists were John Baldwin (University of California, Santa Barbara), and Robin Fox (Rutgers University). No title for Goffman’s paper (or that of any other participant) is listed in the program.<sup>18</sup> Second, Kendon, a participant in the Amsterdam conference, went on to organize the Symposium on Human Ethology for the Australian and New Zealand Association for the Advancement of Science meeting held in Canberra in 1975.<sup>19</sup> It seems reasonable to assume that both of these were influenced by the Amsterdam conference. The third event, Colloquium on Human Ethology, was held in October 1977, sponsored by the Werner-Reimers-Stiftung [Werner Reimers Foundation] in Bad Homburg, Germany (the proceedings volume is von Cranach et al. 1979).<sup>20</sup> The Reimers Foundation, established in 1963, is “dedicated to the fostering of science and research that concerns itself with the studies of mankind, its nature and environment.”<sup>21</sup> Goffman presented “Response Cries,” and it is included in the von Cranach et al. volume (1979c); it was also published separately (1978, and as a chapter in 1981). Other presenters at the 1977 event who had previously participated in the Amsterdam conference were Eibl-Eibesfeldt, Ekman, and Kummer; Jürgen Habermas commented on Goffman’s paper (Habermas 1979). There are other connections between Goffman and ethology as well (see Jerolmack et al. 2024), but those are less obviously linked to the projects, topics, and people mentioned in these pages.

As well, in addition to these conferences, at least one publication was planned as a result of the Amsterdam conference, although it seems not to have been carried through. Sherzer spent time with W. John Smith in Amsterdam (they likely had met earlier at Penn), and they shared enough common interests that a year later they outlined a potential book, involving Goffman and several others, which Sherzer summarized for Hymes.

When in Panama City, I spent quite a bit of time with John Smith, an ethologist in the biology dept. at Penn who I think you have never met. He has been quite excited about what I have to say about Cuna pointed lips (which he originally heard me present in Amsterdam last year) and made use of some of my approach in a series of papers he's been working on tongue gestures (in humans, with insights from some animals). He thought it might be interesting to put together in a book his stuff on the tongue and mine on Cuna gestures and maybe some other things. . . . I talked briefly with Erv in Phila. And he sounds like he would contribute a piece on greetings. John plans to write to Paul Ekman and Adam Kendon (both participants of the Amsterdam conference) to see if they would like to contribute papers focusing on the inter-relation of gesture and speaking, pretty much the theme of the book in a way. John has seen your long letter to me about the pointed lip gesture and we both wonder if you would like to contribute a piece along the lines you developed there, i.e., of the importance of studying communicative activities as such rather than studying gesture, lg, etc., each in their separate boxes. We envisioned such a piece not as an introduction to the book—there would be no introduction (unless it comes out that it is needed) but rather as one of the contributions, a contribution which provides a theoretical perspective greatly needed in this kind of work. John has written to Tom Sebeok who would like such a book for Studies in Semiotics. What do you think about the project? The possibility of your contributing?<sup>22</sup>

A few months later, it became obvious that at least Hymes would be too busy, and Sherzer wrote: "About book that John Smith and I are planning: of course I hope you can talk to Erv and John about it sometime in Phila. But I suppose the AAA election (and I'm betting on you for winning [the presidency] . . . and other commitments have you with your back to the wall."<sup>23</sup> The book clearly moved ahead, for two years later Sherzer wrote Hymes about it again: He wanted to include a revised version of his own just published article in *Language in Society* (1973b), and asked about permissions. At that point, he said, "The book is to be published in the Approaches to Semiotics series and will go to press in about a month (if the others get their papers done)."<sup>24</sup> Perhaps the others (Smith and Kendon) did not complete their chapters, because, unfortunately, it seems the book was never published.

### The Multiple Analysis Project (MAP)

MAP was organized by Allen Grimshaw, a sociologist at Indiana University, but it involved nearly all the major and some of the minor players in Goffman's invisible college at Penn, and the conference at the end occurred on Penn's campus, so it seems worthy of inclusion even though it is a long

and complicated story. Yet even failed projects, sometimes especially failed projects, are worth attending to for what they can teach us.

Initial discussions leading to MAP occurred at the SSRC Committee on Sociolinguistics in 1973, which Hymes was chairing at the time, when Grimshaw, who also served on the committee, brought up the possibility of that group sponsoring a new project. As the minutes record:

Mr. Grimshaw introduced his proposed project on verbal strategies, which would involve the close analysis from several different points of view of a common "text" (e.g., videotape of natural conversation or another form of interaction). Discussion centered around the potential impracticability of asking different investigators, with diverse methods and conceptual interests, to work on the same material, which might not be of great salience to them or suitable for their approaches; the question of the interaction domain from which the material might be selected, and the desirability that this be a socially relevant one, if possible; the danger of creating a kind of contest among the participants, with the mistaken implication that one method would prove superior to the others; and, in general, the technical complexity and great costs of the project. The committee regarded the basic idea of the project as very worthwhile.<sup>25</sup>

Thus, the key elements were present from the very beginning: it was to involve videorecording live interaction and then comparing different forms of analysis of that video. At the same time, group members were already able to point out some of the difficulties that might, and in fact did, occur due to the complex design.

MAP was intended to develop the new topic of sociolinguistics by getting sociologists and linguists to talk to one another through working on a collaborative research project. As Grimshaw later explained to Fred Erickson, at that point a potential participant, MAP "developed out of a concern of the SSRC Committee on Sociolinguistics about the proliferation of methods and theoretical orientations in the analysis of conversational interaction and the ethnography of speaking, which began to develop towards the end of the sixties" (Jan 11, 1979, ADG). He wrote that the goal was to "permit at least a rough test of the relative strengths and weaknesses of some of the many contemporary perspectives, permitting analysts to learn from both the successes and failures of their colleagues and to share what they learned with others" (1981, 360). More formally, he explained:

The Committee on Sociolinguistics (CSL) had three identifying characteristics: (a) interdisciplinary membership and constituencies; (b) a strong comparative interest, and; (c) a unifying intellectual concern with talk in social contexts. In

the early 1970s it seemed to CSL members that the first and second of these characteristics were often contradictory to the third, with the result that sociolinguistic research was simultaneously growing exponentially, and becoming increasingly diffuse and noncumulative. (1989, xiii)

In an effort to resolve this issue, Grimshaw “was charged to seek support for the collection of an extensive data base on a naturally-occurring conversational event and to locate and recruit a group of analysts” (xiv). The result was MAP.

MAP began in 1973, with major publications finally appearing nearly twenty years later (Grimshaw 1989; Grimshaw et al. 1994a), so long after the initiation of the project that they were barely noticed and had little impact on other researchers, then or now. (Even other participants only mention it briefly, if at all, in their later publications [see Erickson 2006]). Over several years, funding for MAP was provided by SSRC, the William T. Grant Foundation, and the National Science Foundation (Grimshaw 1987a).<sup>26</sup> It was “a collaborative project in which thirteen investigators, representing a range of theoretical and methodological perspectives in anthropology, linguistics, psychology, and sociology are individually examining the same data record” (Grimshaw 1982a, 38n2). Initially, Grimshaw was concerned that scholars would miss work in unrelated research strands, but later he realized that even “people studying the *same* sociolinguistic topics are often unaware of relevant work being done by colleagues in different disciplines and even in different ‘traditions’ within disciplines” (Grimshaw 1991, 844; emphasis in original). In the end, the diversity of theoretical and methodological approaches that had been intended as the project’s strength proved to be its undoing, and the project outlasted even the Committee on Sociolinguistics that brought it into being (the committee was disbanded in 1979; Heller 2018 provides details).

MAP’s leader and guiding force was Allen D. Grimshaw, a sociologist at Indiana University, who served on the SSRC’s Committee on Sociolinguistics starting in 1967, much of the time with Hymes, Labov, Charles Ferguson, John Gumperz, and Susan Ervin-Tripp as other members, part of the time as chair or co-chair. Grimshaw also served as associate editor of *Language in Society*, as described in chapter 4. Becoming convinced that the joint study of language and society was essential, he wanted to draw together the various research strands. He credited Hymes with at least part of the logic, since: “[a]ccording to Hymes (1967) there is a sense in which *the study of language is inseparable from the study of society*” (Grimshaw 1969a, 313; emphasis in

original). Grimshaw had met Labov and Gumperz at least as early as 1964 at the SSRC/LSA Summer Institute on Sociolinguistics, held at Indiana University (Grimshaw 1981, 14, 358), and that seminar developed into the Committee on Sociolinguistics;<sup>27</sup> he had met Hymes in the early 1960s. From that point forward he typically acknowledged Gumperz, Labov, and Hymes for their help in publications (e.g., saying that they “have given time beyond the proper demands of collegueship in instructing a neophyte in radically new ‘thoughtways’” [1973a, 49]). Fairly quickly he added Goffman as well, writing at one point, “I fear that sometimes I have absorbed their ideas without realizing where I’ve learned them” (1981, 99; this is a revision of a paper presented in 1972, originally published as Grimshaw 1973b, so the influence dates to at least a decade earlier). At the same time, he was quite open about the fact that “I am a sociologist, not a linguist. My interest in discourse is less in how it is put together structurally than in what its producer-users manage to socially accomplish with it—and how” (1987a, 213).

### *Grimshaw and Goffman*

In addition to Grimshaw’s central role in MAP, he was an important member of Goffman’s invisible college, so it is worth taking a detour to understand a little about who he was, and how they connected, before returning to MAP itself. Grimshaw’s BA was in anthropology and sociology from the University of Missouri, and his PhD was in sociology, from Penn in 1959;<sup>28</sup> that was on the topic of race riots,<sup>29</sup> so he fit in well not only with the group of sociologists and linguists inventing sociolinguistics, but also with the major concerns of the Center for Urban Ethnography once it was established in 1969. He was in regular correspondence with Goffman, Hymes, and Labov on issues related to sociolinguistics from the 1960s, routinely met with them at a wide range of conferences (the American Sociological Association, the Linguistics Society of America, and/or the American Anthropological Association), and exchanged drafts of papers. For example, Goffman at one point wrote Grimshaw: “Won’t be seeing you in Canada, so we might have to wait for Mexico, as I will be in France, although over the next month here in Philly” (Jun 21, 1974, ADG). And at another point, Grimshaw wrote Goffman: “The Temple meetings are March 13–15; I’m committed for the 15th. Per your suggestion, I hope we can all get together—maybe the same crew as last time (Labovs and Hymeses (?) too). I’ll look forward to seeing you” (Feb 27, 1975, ADG). This makes it clear that Goffman, both Grimshaws, both



Labovs, and both Hymeses were in the habit of connecting at conferences, local and national, across multiple disciplinary associations.

In fall 1969, Grimshaw invited Goffman to Indiana University, to give a public lecture, teach a seminar, and have a social evening because “a growing number of people in the university community are becoming aware of the exciting things that are being done in sociolinguistics generally and interaction studies more specifically; they would like to have details from you as someone intimately involved in the most exciting work in the area” (Dec 4, 1969, ADG). Goffman answered by return mail: “I’m afraid I just can’t. It would be terrific to talk about sequencing in conversation to people who know about it, but I have overcommitted myself for the rest of the year and just can’t make it” (Dec 11, 1969, ADG). At the same time, however, he sent back comments on a draft manuscript Grimshaw had included. In response, Grimshaw asked about moving the date to fall 1970 (Dec 17, 1969, ADG). Goffman’s answer has not been preserved, but it was apparently several years before he was able to make time to visit Indiana and give presentations. Of course, the fact that he did manage to make time for that visit at all is significant; as demonstrated in chapters 4 and 5, he had an awful lot of commitments in those years (even so, only one part of the story has been told; he also had significant numbers of conference presentations, nationally and internationally, as well as guest lectures at various universities, on his schedule, and these have not been detailed).

The fact that Goffman and Grimshaw wrote letters of recommendation for one another is one measure of their mutual esteem. In early 1973, for example, Grimshaw wrote: “Thanks very much for your good letter. If I don’t get support I certainly can’t blame you” (Jan 17, 1973, ADG). In spring 1974, Grimshaw was asked to review a proposal by Goffman to the National Endowment for the Humanities, which he did at great length. It was to involve Sacks, both Goodwins, and Jefferson, requiring a 20 percent time commitment from Goffman. In his evaluation, Grimshaw suggested that the proposal was part of a move bringing together students of speech in interaction (such as Charles Fillmore, Michael Halliday, John Gumperz, and Aaron Cicourel), and he said: “It is difficult for me to imagine a more auspicious beginning for the long-range activity than the work here proposed. If successfully carried off this project will have implications of major magnitude for research and theory-building in the range of disciplines within the social sciences and the humanities.”<sup>30</sup> At the same time, however, he concluded:

It is difficult to comment on the research design or plan of work for the project—the investigators have not spelled it out. Neither Goffman nor his student Sacks have been much concerned about the preoccupation with research design which has characterized many of their colleagues in sociology. Goffman's own style has been to keep his eyes and ears open and to find data either where it occurs naturally. . . . In this he has been extremely successful.<sup>31</sup>

Grimshaw went on to mention that the intended technique was to “record and film everyday ceremonials, commercial encounters, or whatever.” He did wish they had done more to link their intentions explicitly to theory, but even so, “I don’t see how given their past work it can fail to be interesting and valuable. . . . I don’t think many people can do the kind of work he [Goffman] does.” He hoped that Goffman would do the writing up, as “Goffman’s work has been widely read and understood by scholars (and lay persons) in a large number of fields,” whereas he had found Sacks’s and Jefferson’s work “obscure and difficult to read.” He said the budget was minimal but assumed they would collect data and then analyze it, so perhaps it would be enough. “In summary, I think this is potentially a project which could be counted as an important coup for the NEH and I urge its support.”<sup>32</sup> Unfortunately, the grant proposal is not in Grimshaw’s files, nor is official notice of whether or not it was funded, but it seems not to have been.

Then in 1976, Grimshaw wrote Goffman: “I have given your name as someone who could be contacted (not as a letter writer); I hope you don’t mind.” This was apparently for an NEH grant of his own, because the letter mentions “Enc: NEH materials” (Jun 25, 1976, ADG). There is a handwritten note on the original letter, returned by Goffman without a new date: “Will do anything I can, with pleasure. A worthy venture.” And in 1980, Grimshaw wrote Goffman that he was applying for a Guggenheim to write up his analysis of MAP, adding, “I have been told that I should list as references ‘top people’ who know my work. May I list your name? . . . My thanks if you feel you can write a reference—I’ll certainly understand if you feel you can’t (I know you think I am wrong-headed in some of my thinking)” (Sep 5, 1980, ADG). Apparently, this time Goffman turned him down, because there is a follow-up letter that begins, “Thanks for your prompt response, I’ve asked someone else to serve as a reference” (Sep 15, 1980, ADG). But they continued friendly correspondence for the next few years, with lots of stories about trips, and they clearly continued to connect at conferences, so this refusal did not threaten their friendship any more than did the problems that developed with MAP.

In addition, Goffman and Grimshaw frequently exchanged drafts. For example, Grimshaw mentioned in a letter to Hymes in 1973 that he had received a draft of a Goffman manuscript: “It’s dense (in the positive sense) and will need a lot of thinking—but I think he is going in a very interesting direction—as he has been right along.”<sup>33</sup> And a few years later, Grimshaw told Hymes, “When we were at Sugarloaf, Erving said something about all of us getting together for dinner sometime during the meetings—I hope that will be possible. . . . I hope it will be possible to see you two, the Labovs, and Goffman (I gather it’s likely I’ll see Sol [Worth] at the Temple meetings).”<sup>34</sup> Sugarloaf was the name of the conference center for the event held in January 1975. So Grimshaw knew not only Hymes, Goffman, and Labov, but also Worth—and knew them well enough to connect repeatedly with them at both large and small conferences, and his papers provide evidence that he was in frequent correspondence with them all.

In addition, Grimshaw was editor of *American Sociologist* from 1976 to 1979 (Grimshaw 1981, xiii) and used Goffman as a reviewer for submissions to that journal.<sup>35</sup> Some of Goffman’s reviews are fairly brief, or only part has been retained in the file, but they seem to be roughly comparable to the ones written for *Language in Society*: good detail at least some of the time, and sporadically amusing. One of the notable exchanges is for a manuscript Grimshaw sent to Goffman for a specific reason: “Since your work is the inspiration of the enclosed, I thought you might be interested” (Mar 6, 1979, ADG).

Grimshaw cites Goffman repeatedly, not just for the chapter “The Neglected Situation” (Goffman 1964a), but also several of Goffman’s books (e.g., in Grimshaw 1966), all at considerable length, clearly indicating that Goffman was to be numbered among the group of important early sociolinguists. A few years later, Grimshaw (1969a) discusses several of Goffman’s ideas in detail, including times when speech behavior defines social structure, stage management, and the manipulation of encounters. In one review essay, Grimshaw specifically mentions that he will not be reviewing “Goffman’s seminal work” (1973c, 575), and in another he several times mentions work by Goffman as being an example of “some of the most important activity going on in this area” (1974a, 10). In several reviews he points out that one author or another does not mention Goffman when, as he puts it, “a reading of Goffman would be helpful” (1974b, 317; see also 1974a). A decade later, neither he nor anyone else would have expected him to include Goffman, and a decade after that, even the mention of Goffman’s name as part of

discussing sociolinguistics had become infrequent. Much later, Grimshaw provides a particularly clear definition of sociolinguistics: “how talk gets used to accomplish social ends in (primarily) face-to-face interaction” (1998, 444). That definition makes obvious why Goffman would have been interested in sociolinguistics as it was developing.

### *Grimshaw at Penn*

In 1970, Grimshaw talked with NEH about the possibility of a two-year retraining grant to learn more linguistics, in order to develop a better version of sociolinguistics; he hoped to spend the time at Penn with Hymes, Labov, and Goffman (his documentation for this does not mention Worth, whose major research area was substantially different, focusing on visual communication).<sup>36</sup> Hymes was quite responsive, writing about which topic out of the three that Grimshaw had mentioned was already of interest to both Goffman and Labov (social interactional universals), which overlapped with Fought’s interests (semantic description), and providing the requested information about courses which Grimshaw might audit. He specifically recommended Goffman’s course, although he did not provide a title or subject. (As a reminder, Goffman only taught one course per year, so if you wanted to take a course with him in a given year, you took whatever he offered.) Hymes also recommended that Grimshaw put his plans on hold for a year, given that Labov would not yet be teaching regular courses in 1970–1971.<sup>37</sup> Thus, in 1972 Grimshaw applied for an SSRC grant to visit Penn for two years to work with all three in order to improve his understanding of linguistics. He sent a draft to them all, explaining that he wanted “to study sociolinguistics in a setting in which the relevance of language to sociological and linguistics issues is accepted and sociolinguistic research is encouraged. Such a setting exists at the University of Pennsylvania with Erving Goffman, Dell Hymes, and William Labov gathered on the same campus and engaged in joint research and training activity.”<sup>38</sup> He proposed working with Hymes on semantics, Labov on rule derivation and formal linguistics, and “with Goffman on rules for social interaction.”<sup>39</sup> All three supported him; even so, he was not awarded the grant and so did not spend the year at Penn. In 1972, once it was clear Grimshaw had not been awarded the retraining grant, Goffman wrote up comments about a paper Grimshaw had sent and said: “I wish even more that we were to have you here for a year” (Feb 9, 1972, ADG). In 1973–1974, Grimshaw had a sabbatical, and again there was discussion of his spending time at Penn. Goffman evidently

tried to arrange for him to teach there, for Grimshaw replied, “I have thought over your kind offer to see about some teaching at Penn next year, and have decided I had better take a rain check. I will be taking courses this summer at the Linguistics Institute which will mean two months away from here. . . . Depending on how things go this summer, I will plan on spending some time in Philadelphia during the fall semester.”<sup>40</sup> He added that Frank Furstenberg in sociology had offered to arrange for library privileges, office space, and possible teaching. Goffman responded: “We’ll take whatever of your time you want to give us,” and “See you at the Institute,” so, apparently, he also intended to participate. His closing was typically cute: “Regards to she who married down to you” (Apr 27, 1973, ADG).

That Summer Institute involved more of those relevant to this story. Grimshaw described it in some detail in a letter to Schegloff much later, in 1996:

I remember with great pleasure the Ann Arbor summer with courses from Harvey [Sacks], Michael Halliday, and Bill Labov and Gillian Sankoff and the workshop/tutorial you and Harvey did on CA matters. I like to tell people about the day when Halliday sat in Harvey’s adjacency pairs class and came up with an extensive alternative reading which really got Harvey’s attention. The entire summer was an intellectual feast.<sup>41</sup>

In this context, it is relevant to note that one of the reviews (by Murray, a sociologist) of a book by Grimshaw lauded him for “publicizing the work of Bernstein, [Penelope] Brown, Goffman, Gumperz, Hymes, and Labov to his inattentive sociology colleagues,” further praising him for his “attempt to carry the message of sociolinguistics to sociologists,” explained as “a missionary endeavor” (1982, 743–44).

Grimshaw not only knew Hymes, Labov, Goffman, and Worth, and corresponded with them all, but he also knew at least some of their younger students and colleagues because Bambi Schieffelin, Steve Feld, and Joel Sherzer are all mentioned as part of a conference panel he was organizing in 1977. Goffman was to be discussant, talking about “The Analysis of Ritual Idiom” (the same phrase is used in Goffman 1971). Other panelists were to be Charles Ferguson, Shirley Brice Heath, William Corsaro, and Karl Reisman. Schieffelin was being proposed as a replacement for Steve Feld, who could not participate, and Sherzer was apparently one of the organizers. In the end, Grimshaw was not on the panel, but Goffman did serve as discussant, joined by Hymes. The chair was Ben Blount (Texas), with other panelists being Bambi Schieffelin (Columbia), Judith Irvine (Brandeis), Joel Sherzer

(Texas), and Gumperz (Berkeley). The title was “Discourse: Speech Acts and Contextualization.”<sup>42</sup>

### *MAP and Berkeley*

Berkeley and Gumperz also both played a role in the origin story of the MAP project. MAP was mostly funded by grants to the SSRC Committee on Sociolinguistics, which included substantial representation from Berkeley (Ervin-Tripp was a member from the start, with Gumperz, Hymes, and Grimshaw, who was mostly at Indiana but also based at Berkeley for a year), as well as Charles Ferguson (a Penn PhD, mostly based at Stanford, who served as the first committee chair when it began in 1963 and for many years after that), later adding Labov, Sankoff, and Sherzer at different times (all affiliated with Penn).

But there is another, quite specific, way in which Gumperz played a significant role in MAP. In a 1973 letter to Hymes, at the very start of the project, Grimshaw explained that the “the major project that I had outlined for the CSL [Committee on Sociolinguistics] could be an important intellectual contribution and that at the same time it did have a substantial research component. . . . I would like to go ahead with the project.” One thing stopping him, however, was that Gumperz felt he had had the same idea, earlier.

It simply is not clear to me precisely if John thought that I was somehow “ripping off” an idea that he had had. I gather that he and the Lakoffs and one or two others in the Bay area had been getting together informally to jointly examine data. I did not have the impression that he had in mind any idea about asking a somewhat diverse group to independently examine common data. Yet at the end of our Sunday session he seemed to imply that he would see the activity I had outlined as competitive to his interests and that he might take an active role in persuading some of his friends and colleagues to not cooperate in a CSL sponsored and supported activity. (Apr 2, 1973, ADG)

The story goes on at great length: Grimshaw and Gumperz met the next day to try to sort things out, and Gumperz apologized for being “obstreperous,” saying that he only wanted “to insure [*sic*] the success of the project.” Grimshaw made it clear that he wanted Gumperz not only to support the project, but also to participate, and was asking Hymes to help ensure things would work out (Apr 2, 1973, ADG). By the end of the letter, Grimshaw decided “maybe I should stop trying to get more training and get down to doing research,” which ties together his unsuccessful efforts to get a post-doctoral training grant to study with Hymes, Goffman, and Labov at Penn, with his efforts to



manage MAP, also tying in Gumperz and colleagues at Berkeley. The letter demonstrates that Grimshaw knew this might be a difficult project from the very beginning, especially given the “Primadonnaesque’ nature of some of the proposed participants” (Apr 2 1973, ADG). As matters turned out, he was right to be concerned.

### *MAP Timeline and Participants*

With this background in mind, it is time to return to MAP. Basically, MAP took a scientific approach to theory building: The plan was to bring all the research traditions then developing together, have everyone analyze the same data, and compare the resulting analyses. It was one of a surprisingly large number of efforts supported by the SSRC Committee on Sociolinguistics. As Heller (2018) explains,

The idea was that bringing these different analytical angles to the same piece of data would allow for the emergence of a truly interdisciplinary understanding of language as social action . . . but the project seems to have foundered over major disagreement about the meaning and value of the experiment. . . . One result, then, may well have been the end of the interdisciplinarity that characterized the early years of the committee, and a retreat into silos.

In other words, the result was the exact opposite of the goal of the project.

What follows is a table showing the major participants at three different points in time when the group came together: a meeting in 1975 in Santa Fe to get things started, a conference in 1982 at Penn to discuss what had been learned, and a joint book (finally) published in 1994 (Grimshaw et al. 1994a). These are not the only people or dates that matter to understanding MAP, but they are the most essential points of information necessary to an understanding of the toll taken on this project by changing personnel over time.

This demonstrates that only four out of twenty analysts were present for all three key moments, which is indicative of the problems this project faced. It will be worth examining in some detail who was involved with the project at not just these three moments, but at multiple stages, what the goals were, and what led to the project foundering. In this way it can serve as a cautionary tale. Everyone agreed on at least one thing: as Grimshaw put it to Ferguson, “the idea of common data is, I think, critical to the whole idea” (May 16, 1973, ADG). Yet, as the friendliest reviewer of the resulting book pointed out: “What indeed the corpus would be, and then how it would be recorded, rendered, and distributed, in the company of what other case

*Table 6.2: MAP Personnel Over Time*

<i>Analysts</i>			
	<i>1975</i>	<i>1982</i>	<i>1994</i>
Charles Bird	x	x	
Peter Burke		x	x
Aaron Cicourel	x	x	x
Jenny Cook-Gumperz		x	x
William Corsaro		x	x
Fred Erickson		x	
Charles Fillmore	x	x	x
Erving Goffman	x		
Allen Grimshaw	x	x	x
John Gumperz		x	x
Michael Halliday		x	x
Ruqaiya Hasan		x	x
Dell Hymes		x	x
Elinor Keenan		x	
Adam Kendon	x		
Teresa Labov		x	x
Harvey Sacks	x		
Emanuel Schegloff	x		
Lily Wong Fillmore	x	x	x
Laurence Wylie		x	

Table 6.2: MAP Personnel Over Time (cont'd)

<i>Filmmakers</i>			
	<i>1975</i>	<i>1982</i>	<i>1994</i>
Steve Feld	x	x	x
Carroll Williams	x		
Joan Williams	x		
<i>SSRC Staff</i>			
David Jenness	x		x
<i>NSF Staff</i>			
Alan Bell	x		

relevant information, seems to have been the single most serious cause of friction, and even schism” (Macbeth 1995, 706). That was a major problem. (See Grimshaw et al. [1994b] for a far longer and more detailed explanation of what happened than can be provided here.)

Perhaps the best starting point in this context is the story of how Goffman became involved with the project. Goffman already knew Grimshaw from his days at Berkeley, and so was one of those invited to participate when the project was first conceived.<sup>43</sup> Grimshaw and Goffman were thus friends before MAP started—but more than friends, Grimshaw clearly respected Goffman’s publications, and found them useful: “I have also found the work of Erving Goffman to be a highly valuable source of new ways of looking at patterned social behavior” (1973d, 106). And the reverse was true: Goffman clearly found Grimshaw’s comments useful, as when he wrote: “Thanks very much for the critical comments on my two papers; I have been able to use almost all of them” (Aug 10, 1977, ADG). And so, in return, Goffman sent detailed comments on Grimshaw’s own drafts. A good example is provided by the detailed comments he sent in response to a draft of what was eventually published as Grimshaw (1980).

Re your monograph on verbs of manipulation. Your command of the literature is masterful. Your effort to get from what linguists do into the sociologist's grasp of social life, however, raises questions which I think I could only get to through discussion with you. By the way, your sense of cajol doesn't fit with mine. (Nor do I take a dictionary's definition as necessarily very sensitive, nor the easy out of saying "in my dialect." For surely we should not in this count ourselves as each merely one voice.) I think cajol essentially refers to A's attempt to get B to do something that is easily within B's power but which he declines to do because of being hostile to something or somebody that would be involved in the doing. To cajol is to employ lightly toned, semi-humorous means of showing B that there is a way of defining the situation that would allow him to take the action A is encouraging. Sometimes A himself can be the target of the initial hostility and the cajoling therefore requires a splitting of the self, but ordinarily the target would be a third person. And I don't think deceit has anything to do with it. But, of course, even if we could agree on an explication, a question would be where in sociological space we could go with it. Certainly linguists and now yourself have shown how systematically responsive sentences are to social meaning; but this, it seems to me, is still of primary concern to sociolinguists, not sociology. (Aug 10, 1977, ADG)

This is especially relevant because it fits so well with what Goffman wrote in 1975 in response to Grimshaw's cajoling of him to fit in more activities in his visit to Indiana than he felt appropriate (see below for that part of the story).

As early as April 1973, after MAP was conceived but before it was funded, Grimshaw reminded Goffman of his initial expression of interest, saying: "If we do the project, I certainly hope you will be involved" (Apr 17, 1973, ADG). There is no immediate response in the file, although there is an enthusiastic response a year later. At the same time, Grimshaw wrote to Sacks and Schegloff as well.<sup>44</sup> Schegloff responded that his interest would depend on both the data collected and other commitments (May 3, 1973, ADG). And at the same time Grimshaw wrote to Paul Byers because he had attended sessions that Byers and Alan Lomax had organized at the American Anthropological Association convention in 1970, held in San Diego, on film analysis of culture and communication. He mostly asked for advice in terms of equipment that would be required. "We anticipate that there will be between 8 and 12 teams working on the data," so they needed to determine what would be economical but also functional to write up a grant proposal. His letter contains a particularly clear explanation of the thinking behind the project.

Let me start off by telling you something about a project which may be undertaken by the Committee on Sociolinguistics of the Social Science Research

Council. We have been impressed by the variety of perspectives which have been developing over recent years for the investigation and analysis of extended verbal social interaction. Some sociologists with diverse kinds of perspectives ranging from those of Harvey Sacks and Manny Schegloff and their notions of conversational analysis to people like Erving Goffman with his ideas of frame analysis to Robert F. Bales with his work on interaction analysis have long been engaged in work in this general area. In addition, John Gumperz and Dell Hymes and other ethnographers of communication have been developing various perspectives for such analysis. These two groups of investigators are now being joined by linguists like Charles Fillmore and Robin Lakoff who want to extend semantic interpretation to include more social dimensions and other linguists who are interested in moving beyond the confines of the sentence. Our committee is contemplating a project in which some set of people representative of the several kinds of perspectives mentioned will all take a common piece of data and analyze it using the theoretical frames which they have been developing. (Apr 13, 1973, ADG)

Byers was delighted with the project, saying:

Once in a very great while a letter arrives that signals, to me, an important event or possibility. Your letter of April 13 is such a letter.

Your plan to have people representative of several perspectives analyze the same piece of film-data is, in the view of many of us, the only conceivable way to bring some order to an important but much disordered and confused area of concern. I have sat with several groups hoping to bring off such a project. None has succeeded. If your committee can bring this off, I think it may represent a critical turning point in behavioral research. I believe, incidentally, that such a project will cross many disciplinary lines and do for us what no amount of talk or writing can do. (Apr 18, 1973, ADG)

Byers then responded to all the specific questions. The presentation in San Diego had been by William Condon, and Kendon had used similar machinery for a similar analysis. He provided extensive details about what equipment they used for what purposes, warning Grimshaw about multiple technical issues that might arise. In addition, he explained something that Grimshaw should perhaps have taken more seriously: "In discussions I have had in the past concerning such a project, it has been difficult to find a single data-film idea that would suit everyone" (Apr 18, 1973, ADG). That did, in fact, become a huge issue with MAP. Byers also warned that very few people were skilled at film recording, and recommended Jacques Van Vlack at EPPI (who worked with Birdwhistell). "This aspect of the project is certainly the most critical

since a poor record cannot be repaired whereas analyses can be done over and over." Again, his warning was prescient.

Grimshaw replied a few weeks later that he had found the comments "overwhelming" for while "I knew that I didn't know anything about film and their analysis—I didn't realize just how naive I was. My linguist colleague characterized your letter as suggesting that I was trying to do astronomy with binoculars. The more I think about it the more I think I may have been trying to do astronomy with a child's magnifying glass" (May 4, 1973, ADG). He admitted that his project was not likely to need such sophisticated apparatus since most of the group "are rank amateurs in this kind of activity." He thought they should include "a small meeting prior to the period in which analysis will be done at which we can all learn something about the use of film." He concluded, "The purpose of our activity is to bring together analysis at different levels and see what we can learn about the rule governed nature of social interaction, including verbal interaction" (May 4, 1973, ADG).

In early May, Grimshaw wrote Hymes and David Jenness, staff to the Committee on Sociolinguistics: "To this date I have had confirmation of interest only from Goffman (though I have, as you know, written Sacks-Schegloff, Fillmore, Halliday, and Robin Lakoff [I am assuming that Joel will still want to participate])."<sup>45</sup> So Goffman not only participated but was the first to agree to join in. By late May, Grimshaw wrote to Ferguson mentioning that Fillmore, Goffman, Charles S. Bird (his colleague at Indiana), Sherzer, and he were all likely participants, while Robin Lakoff, Michael Halliday, Joseph Grimes, Sacks, and Schegloff were further possibilities (May 16, 1973, ADG). Despite (or perhaps because of) the early stage of the project, Grimshaw also said, "*Damn it*, I hope we can do this—it would be extraordinarily exciting and could make a really important contribution!"<sup>46</sup> (emphasis in original).

At that stage Hymes thought the project important, but difficult and time-consuming, to the point that most of those who would have the most to contribute would not be able to make the necessary time (including himself).

It is marvelous that you elicited such a constructive response from Byers. It brings home to me the great gap between linguistics as commonly practiced, and the world of those who have attempted to move ahead with direct observation with the aid of visual means. If we could accomplish some interaction and lay a base for further cooperation, as between these worlds, as well as within them, we would accomplish a great deal. The kinesics, paralinguistics, communicative behavior work goes on rather bereft of linguistics or associated with older styles of linguistics. The generative semantics, and indeed ethnomethodology, doesn't seem to require much attention to observation, replicability, hard data. Wouldn't



it be quite a coup to gain ground on this score? A successful meeting on this basis might reveal a group willing to tackle your full project. That is, hang gotten to know each other, to discover personal affinities, and intellectual adjacencies, some might want to push further. (May 8, 1973, ADG)

Hymes's proposal was to first hold a meeting, and assume that people would decide the project was interesting and want to participate, and maybe those who needed more linguistics would acquire that, and those who needed more observation of actual behavior, would decide to add that in. And, in the end, they would have the kind of project Grimshaw wanted. Grimshaw did not answer this directly but simply acknowledged to Hymes that the project was "both complicated and expensive" (May 16, 1973, ADG).

In August 1973, David Jenness of SSRC responded to a progress report that Grimshaw had submitted to the Committee on Sociolinguistics: "I'm happy to hear that people like Goffman and Halliday, and others, are so enthusiastic." Understanding that it would take some travel to consult with relevant experts (Byers and Worth were mentioned by name), "we should allocate up to \$1,000 from committee funds for this project during the coming academic year. Dell Hymes agrees with that, so let us consider it done" (Aug 17, 1973, ADG). Thus, the first stage of sorting out who would be involved and what needed to be filmed, by whom, and where, was settled and funded. The first few steps had been taken.

In September 1973, Byers wrote Grimshaw with details about organizing a meeting with interested parties while Grimshaw would be in New York. Adam Kendon and Albert Scheflen were both scheduled to be involved, and Byers said he would try to also involve Margaret Mead. He also recommended that Grimshaw go to Boston to see Condon and Erickson, as well as to Philadelphia, to connect with Worth, Birdwhistell, and maybe Van Vlack. Byers pointed out that he had only included anthropologists because he did not know many sociologists or linguists (Sep 11, 1973, ADG). There is no answer in the file, but a follow-up letter from Grimshaw thanks Byers for his time at their meeting (other documentation shows that it was in fact held in fall 1973),<sup>47</sup> letting him know that the project was going ahead, and that "I am assuming that you will participate in some way" (Jun 14, 1974, ADG). In fact, Byers did not participate in MAP (nor did Scheflen, nor Mead). However, both Kendon and Erickson ended up part of the group at least for some activities, and since they were both later affiliated with Penn, it may be worth explaining a little more about them and how they became involved. First, Kendon. In a letter of recommendation for Kendon, Grimshaw wrote:

I first met Kendon when the Social Science Research Council Committee on Sociolinguistics was engaged in initial planning for the MAP project; I was introduced to him by Paul Byers who spoke highly of the work that Kendon was doing—at that time at Bronx State Hospital. I spent a day at Bronx with Kendon and others and he demonstrated the work he was then doing. . . . I was positively impressed with both the intrinsic significance of the work he was doing and the comfortable way in which he talked about [it] to me—clearly a neophyte. I subsequently read several of his papers, talked to others about his work, and engaged in some correspondence with him—as I looked for a skilled film analyst to participate in the MAP. As you know, we ultimately settled on Kendon as a participant and have invited him to involve himself in the project. Such involvement, we were aware, would necessitate bringing him from Australia twice—as well as provision of ancillary equipment—we felt that there was no one better qualified here and insisted on inviting him.<sup>48</sup>

A decade later, in a different recommendation letter for Kendon, written to Starkey Duncan, Grimshaw includes the tantalizing comment that he felt that “he [Kendon] has become more sensitive to interactional dimensions (in part because of his long association with and interest in Goffman) over the years” (May 20, 1987, ADG).

As for Erickson, he was the only one originally considered as a potential group member who was untenured when the project began, so initially he was not invited to participate. Grimshaw explained the logic to Laurence Wylie (a late addition to the project): “The feeling was that participants were being asked to make a very major commitment of time and energies for which payoff in terms of publication would be substantially delayed (and subsequent events have certainly proven the correctness of this concern)” (Jan 11, 1979, ADG). But by 1979, it was felt Erickson was in a more secure position, and so he was invited to join. Grimshaw explained MAP to him this way: As sociolinguistics developed in the 1960s, there had been a “proliferation of methods and theoretical orientations” and “little communication among scholars” and “little comparability across investigations and their reported findings.” The original group had been “chosen on the basis of representation of different analytic and theoretical perspectives in anthropology, linguistics, psychology and sociology” (Jan 11, 1979, ADG). Erickson agreed to join the group.

Returning to 1974, and the effort to organize MAP, Grimshaw reported to Jenness that he had spent time in Philadelphia meeting with Goffman, Worth, and Hymes (as well as others unnamed), and that:

Goffman spent two hours telling me why MAP couldn't be done—and ended up by insisting that he be included *in*. . . . Both Sol and Erving seem to believe that it will be *very* difficult to get our participants to actually write papers which permit rigorous comparisons of their interpretations—in part because of the difficulty of writing about what you see in film (Goffman); in part because the participants are really artists and not scientists willing to specify their concepts (Worth); in part because several of the participants are highly defensive and protective and somewhat unlikely to take the risk of exposure to hard criticism (both). (Jun 11, 1974, ADG; emphasis in original)

Unfortunately, they both proved correct in most of these warnings. Grimshaw had met with them separately, something he said he regretted after the fact in a letter to Goffman:

I much enjoyed our session last Wednesday—and that following with Sol. Sol raised still other problems but seems to feel that the risk is well worth taking—he agrees with you, of course, about the difficulties of writing about what is seen on film. He's not optimistic about "scientific" comparisons—but wants to see analyses juxtaposed and an attempt made at synthesis. He's also very critical of the notion of "naturally occurring." I wish the three of us had met together—we would all have enjoyed it. (Jun 12, 1974, ADG)

Grimshaw went on to explain that he also had met with Carroll Williams, who had recommended Feld, who had expressed interest, and said, "I think you're quite right, Erving, about the 'protectiveness' of some of our colleagues about their work. Should the occasion arise, I hope you'll encourage Harvey [Sacks] and Manny [Schegloff] to take part" (Jun 12, 1974, ADG).

Worth followed up on the meeting with Grimshaw by sending a letter strongly recommending Carroll Williams: "He runs a school called The Anthropological Film Center in Santa Fe in which he takes social scientists for a period of four months and teaches them what they have to know about technology in order to do what they want to do." He also recommended Steve Feld, then a doctoral student at Indiana. "He was a student of mine at a summer institute in visual anthropology that Jay Ruby and I held two summers ago at Carroll Williams' place in Santa Fe. He knows more about linguistics than most people in linguistics." In fact, he thought they should both be involved: "I believe that the combination of Steve Feld and Carroll Williams are about the finest combination for the kind of work that you want done, that you can find in the world. And I mean that without reservation" (Jun 17, 1974, ADG). It's important to note that Worth felt particularly strongly that there was a need for people with appropriate training to both create and analyze

films of interaction. Only shortly before, he had written, “*The only group of professionals involved in the making and use of anthropological films who have no training AT ALL in the making, analysis, or use of film are anthropologists.* One can count on the fingers of both hands the anthropologists who are trained to study films, not as a record of some datum of culture, but as a datum of culture in its own right” (1972a, 359; emphasis in original). One of the problems may have been that, despite this warning, Grimshaw did not end up with people trained to both create and analyze filmed interaction. After a formal invitation through which Grimshaw asked potential participants to confirm their involvement, Goffman wrote back, “I commit myself to you” (Jun 21, 1974, ADG). Finally, Grimshaw announced in June 1974 that the Committee on Sociolinguistics was now ready to receive a full proposal, which he would write, and they would submit for grant support.

Goffman was not to be the only Penn faculty member Grimshaw wanted to involve: Hymes, Labov, and Worth also were asked to contribute in various capacities. Hymes was on the board of directors of the SSRC (1969–1972), and chair (or co-chair) of the Committee on Sociolinguistics at various points (1970–1979).<sup>49</sup> In that administrative role, he talked through some of the major issues with Grimshaw in the early days (e.g., May 16, 1973, ADG), and, as already shown, approved initial funding to help Grimshaw organize the project. Although he played a part in the initial planning, as early as 1973 Hymes wrote to Grimshaw that he would have to turn down any more direct involvement: “I would not be able to even consider for a moment such a commitment of time” (May 8, 1973, ADG). Despite this, he remained active in the role of overseer and so was copied on much of the relevant correspondence, eventually becoming involved in the later stages.

Like Hymes, Labov was invited to actively join the project rather than remain in only an administrative role. (He also served on the committee in some years, as in 1972.) Like Hymes, he declined to participate even before things really got underway.<sup>50</sup> And, while Worth was neither on the committee nor formally part of MAP, he became peripherally involved given that he helped Grimshaw locate appropriate filmmakers for the project, as explained. In exchange for his efforts, some years later Grimshaw categorized him to MAP participants as “a friend of our project since its inception” (Feb 1, 1977, ADG). He said the same to Labov the next year, when requesting comments on a draft proposal for additional funding: “I consider you a ‘friend’ of the project” (Dec 18, 1978, ADG).

Grimshaw wrote a detailed letter to Sacks and Schegloff in June 1974, stating that the committee “has instructed me to move ahead in preparing a proposal for the Multiple Analysis Project,” so it was time to confirm who could commit to the project, and what equipment they already had or would need. Carroll Williams, “the top anthropological film producer according to Sol Worth,” was working with him on what they would do for data collection and would likely do the filming and recording. It was necessary to sort out equipment needs first, to know how much to request from the NSF in terms of grant funds. If NSF did not award a grant, he would try elsewhere. His estimate was that they would be funded in fall 1974 and distribute data in spring 1975: “We’ve waited this long—it will be worth waiting a little longer to do things properly.” He made a distinction between “principals” and “associates,” asking to be told of any associates who should be listed, but warning that only principals would be funded for attendance at a meeting to discuss their results (Jun 12, 1974, ADG). Apparently Grimshaw had met with Schegloff and Sacks in January 1974 for an initial discussion of their involvement in MAP, and they had agreed to participate.

At the same time, Grimshaw wrote to Kendon, thanking him for their meeting in fall 1973, and similarly checking in whether he could commit to becoming involved in MAP. In that letter, he says that the committee had met in early June “for long talks with potential participants (e.g., Goffman) and friends of MAP (e.g., Sol Worth).” While he said, “I would still very much like to involve you, in some way, in MAP,” he fully acknowledged that the focus would be on verbal rather than nonverbal behaviors (“We are also constrained by our goal of keeping the size of the final working conference to a manageable size” despite “the full acknowledgment of the importance of kinesic accompaniment, proxemic management, social contexts, etc.”) (Jun 14, 1974, ADG). It is interesting that this makes clear his understanding of *The Natural History of an Interview* project—both its existence and the difficulties it involved, especially that it was never formally published (see Leeds-Hurwitz and Kendon [2021] for analysis of that project). It is a pity that he did not take the lessons learned from that project more to heart.

In July 1974, Halliday wrote Grimshaw responding to one proposed context, academics conversing over dinner, which he considered not terribly interesting. Instead, he preferred that they record something “socially significant” with at least some “goal-directed activity.” But he also expressed delight that they would have the services of “a top anthropological film maker” concluding, “I look forward very much to taking part” (July 10, 1974, ADG).

A few days later, Grimshaw wrote Hymes and Jenness saying, “I now have confirmation on MAP participation from Fillmore, Goffman, Gumperz, Halliday and Schegloff and I think from Cicourel. I think, in brief, that we’re OK on that dimension” (Jul 12, 1974, ADG). As they were preparing the proposal, Jenness told Grimshaw: “Fortunately, we could hardly have a more impressive set of participants; right there is a testimonial to the basic worth of the project from the scientific point of view.”<sup>51</sup> He was correct, which is why it caused such difficulties when these impressive people began leaving the project.

Jenness and Grimshaw were the co-PIs for the NSF grant received by SSRC and used to fund MAP. The proposal was submitted in December 1974. However, the application process was not straightforward, and there were several stumbles. First, they formally amended it in February 1975, requesting the timetable be moved up, with an initial grant of funds for three months so they could begin immediately.

We do this in the understanding that a progress report and further proposal for continued support for the project will be prepared shortly after the end of the initial period. . . . The initial three-month period will include shooting the film footage, its rough processing and editing, and preparing for and the pre-analysis technical seminar, at the Anthropology Film Center in Santa Fe, where the participant-analysts will agree on what analyses are to be undertaken and what procedures will be used.<sup>52</sup>

Grimshaw was worried about not receiving funds until the end of the academic year, which would mean that the selected context (meetings at Indiana) would no longer be available; in addition, Cicourel was about to begin a year abroad, and they did not want to lose his expertise. Jenness and Grimshaw thus submitted a revised budget, including technical costs, project working costs, and personnel costs (small amounts for Grimshaw and Jenness; nothing for the analysts). Their request was honored, and the grant was partially funded in 1975 for that year only (SOC75-10933). Funds were “earmarked” for continuation, although not guaranteed, which turned out to make a huge difference.<sup>53</sup> The proposal title was “Linguistics and Social Analysis of an Extended Interaction: Converging Methodological and Theoretical Perspectives.”<sup>54</sup> In retrospect, this move to divide the grant to permit earlier filming was to prove a strategic error.

In February 1975, Grimshaw wrote Goffman with the informal results:

The news looks good! (How does news *look* anything?) As of yesterday, it appears very likely that NSF will fund MAP. . . . As things now stand, it appears

that we will be funded by three NSF programs (linguistics, sociology, and social psychology) and that the grant will be spread over two fiscal years—but that we will probably get most of what we need for the project . . . we should be able to do the filming in mid-April and have our technical conference (for discussion of the exact data to be used and to familiarize all of you with the equipment we hope to provide) in late June. I'm hopeful that all of you will be able to work on your analysis through much of the next academic year and the summer of 1976 and that we can point toward an analysis-comparing meeting sometime during the late fall of 1976. (Feb 27, 1975, ADG; emphasis in original)

Grimshaw tentatively scheduled a meeting in Santa Fe for three days during June 15–22, and wrote a letter to everyone: “We do need to have the meeting in June because the NSF is asking for a brief report on that meeting before they release the second part of the grant.” The budget included funding for copying and circulating about one hundred pages from each participant or team to the entire group, intended to either explain “your analytic mode” or for “clarifying your theoretical position” (Feb 27, 1975, ADG). Goffman wrote back: “Congratulations and hurrahs on the warm prospect of funding” (Mar 3, 1975, ADG). June would be difficult, as he expected to be in Germany at a meeting with Cicourel, but the first week was possible.<sup>55</sup> However, he hated the idea of circulating materials ahead of time, as he explained:

About circulated, printed materials. I seem always to be bitching, and here is another bitch. The people in this field have followers and writings. I understood the purpose of the experiment was to get these leaders to really look at one another's doings. A face to face presentation, therefore, which falls back on, demands, and presupposes, a hundred pages of text is quite precisely the disease that the conference is meant to cure. If we could each sympathetically read 100 pages of the others' writings, there would be no need for the conference in the first place. So please, Allen, don't neutralize the undertaking. What we want is confrontation, not perusings. (Mar 3, 1975, ADG)

Well, they did get confrontation. In April 1975, Grimshaw was finally able to write to all of what he was by then calling “participant-analysts”: “Official word from the NSF came this week. We are to have the funds for making and processing the film and for the technical seminar in Santa Fe.” They would film what they had initially agreed on: “Graduate Affairs Committee; Graduate Students Association; a dissertation defense,” as well as a secondary context “(e.g., a nursery school board, etc.)” (Apr 28, 1975, ADG).

Two months later, Grimshaw wrote Jenness a five-page letter detailing progress to that point. Carroll and Joan Williams and Feld had arrived at Indiana, and, after logistical issues involving fleas and miscommunications,



they had filmed the graduate student group, but it proved not to be very useful: “They weren’t good on the criterion of getting much of anything socially accomplished with talk.” So, they tried again, filming another meeting, but the filmmakers weren’t impressed with that session either. (Grimshaw doesn’t mention who that meeting involved, but likely the Graduate Affairs Committee, as planned.) Then they filmed a third session, a dissertation defense, but they had further issues with location: This time the room was too noisy to film, so Marge Zabor<sup>56</sup> located an available sound studio, though there was an issue with getting permission to use it.<sup>57</sup> Eventually they were able to film, and this time, finally, all went well. Grimshaw told MAP participants: “The candidate was relaxed and forceful without being aggressive; the faculty participants all behaved exactly as they had on other occasions.” (Grimshaw himself was a participant, so he was speaking from experience.) Filming lasted for three hours, and the filmmakers were happy with what they got; however, Grimshaw was less happy, worrying “about possible contamination of the analysis by my participation.” However, it might still work:

Bird and I are interested in identifying elements in on-going interaction that are being used; Sacks and Schegloss [*sic*] are interested in adjacency pairs; Kendon is interested in movement accompaniments to verbal interaction; Fillmore is interested in presuppositions or in Searles’ [*sic*] type analysis of speech acts, etc., etc. Only Goffman seems to be very much into attribution—he would do this no matter what the data happened to be. So, I think it might all work out. (Jun 20, 1975, ADG)

Dell and Virginia Hymes stopped in on their way from Pennsylvania to Oregon after filming was completed, and the entire group had a party, joined by Henry Glassie (at that point still teaching at Indiana) as well as Roger Abrahams (at Texas but in Indiana to teach over the summer).

Despite the potential conflicts, Grimshaw did manage to find dates that worked for the majority in July 1975, and so they met in Santa Fe. The initial set of participants was expected to include Bird, Cicourel, Cook-Gumperz, Fillmore, Goffman, Grimshaw, Gumperz, Halliday, Kendon, Sacks, Schegloff, and Wong Fillmore, with Williams and Feld as filmmakers; in the event, all except Gumperz, Cook-Gumperz, and Halliday were able to attend the meeting.<sup>58</sup> Alan Bell, representing NSF, also attended (mentioned in Grimshaw’s letter to him, [Aug 6, 1975, ADG]). However, everything fell apart during the meeting due to strong differences of opinion about what should be accomplished and how. Decades later, in discussing the heated exchanges

among group members in Santa Fe, Grimshaw, Feld, and Jenness point out: “We have often remarked that we wish we had SIR [sound-image recording] of the Santa Fe interaction!” (1994, 17). That would indeed be fascinating. As Grimshaw wrote Hymes at the time:

The Santa Fe MAP meeting was a disappointment of major proportions. David and I will tell you (I believe that David and I have congruent, if not identical, perceptions) that we think that we will ultimately have a successful project—though not without additional pain and not without modifying our current proposal. Charles Bird tells people that I’m an idealist (with strong overtones of naivete)—guess he’s right, I’m simply unprepared for the intellectual arrogance and self-centeredness of some of our colleagues. (Aug 4, 1975, ADG)

Simultaneously he told Jenness, “I’ll have the bruises for some time; I’m also pretty resilient” (Aug 4, 1975, ADG). Jenness responded: “Wow, what things we go through together! Don’t be discouraged” (Aug 5, 1975, ADG). Jenness wrote to Bell (at NSF):

If the Santa Fe *Son et Lumière* [sound and light] spectacular is still re-playing in your mind the way it is in mine, you will be interested to know what happened after you left. . . .

When we gathered Wednesday morning Aaron Cicourel, Erving Goffman, Charles Fillmore, and Adam Kendon had a proposal. They said that it would be a shame to scrap the project; that we all had learned a lot in the meeting; that the co-operative aspect of the project remained an attractive and unusual opportunity. They proposed that the four of them confer during August (when all will be in California), and if possible come up with a proposal for new film footage, of an interactional event to be decided upon and of a technical nature that would meet the requirements of the group. That proposal would be ready by the end of August, to be discussed then with Allen Grimshaw and me at the ASA meetings in San Francisco.

This was a genuine and constructive suggestion—not just a face-saving gesture—to which Allen and the rest of the group acceded. What it will turn out to mean is impossible to foretell. Probably they will now be able to propose the kind of filming that the participants, or almost all the participants, would work with. Whether that kind of filming is something that our film-makers would want to do is another matter, but it shouldn’t be assumed in advance that they wouldn’t agree. Whether Allen Grimshaw and the committee would, receiving the recommendation, feel that it was a good risk to proceed (or whether, instead, for example, they would prefer to make use of the data we now have and re-constitute the group somewhat) also remains to be seen. Whether either course would be justifiable and affordable is something your colleagues would probably have to decide. (Aug 6, 1975, ADG)

Goffman played a central role in the difficulties. Jenness wrote to him at length, while trying to sort out his notes and prepare a report for NSF. He decided to edit the draft in such a way “that it would be clear to Allen Grimshaw and the members of the Committee on Sociolinguistics what the differences are between the Grimshaw-Jenness statement and a statement that would be acceptable to Erving Goffman. In other words, I think there are some real and difficult differences, and at this moment I want to highlight rather than conceal them.”<sup>59</sup>

Grimshaw responded to that draft: “The kinds of changes that Goffman is demanding, both personally and as a representative of his ‘constituency’ cannot be met without changing the project from what it was originally intended to be” (Sep 24, 1975, ADG). It becomes clear that “his constituency” refers to Goffman’s former students, Sacks and Schegloff. On being told about the disagreements, Hymes said he thought the project was a good idea originally (“It was ambitiously admirable to try to pull together all the ‘stars’”) and he was upset at the division among members: “I am puzzled and troubled by so much difficulty in agreeing on what to analyze. I would have thought that it was pretty well understood by the time of the Santa Fe conference what one would have, and that people who were not willing to take a stab at it would not have gone so far” (Aug 8, 1975, ADG). A little later Hymes wrote to Goffman that Jenness was going to be in town “to discuss SSRC Committee problems with me; I have a copy of his letter to you. I hope we can work out the MAP project.”<sup>60</sup> So he recognized immediately that there were major issues to be resolved. Worth expressed his feelings to Grimshaw with vehemence, presumably because those he had recommended as filmmakers were now involved, and he felt responsible for their best interests: “Your ‘geniuses’ are behaving like incompetent doctors who feel that they have to treat nurses as dirt in order to increase their own status” (Aug 5, 1975, ADG).

So, what happened? The status report Jenness mentioned to Goffman was circulated to group members in late September 1975. This was the document needed for NSF to release the second half of their grant. That draft has been preserved; since Goffman and Grimshaw both had the opportunity to send revisions to Jenness, it seems likely to be reasonably accurate. The report says Goffman “characterized the footage as relatively useless for the kinds of analyses they do” (apparently because Feld and Williams had not used a stationary camera, but had chosen what to film from what angle, and selected their own focus of interest); also, it was Goffman

who wanted to say that they had “reported that they would be unable to carry out naturalistic/interactional analyses” given the film that had been made.<sup>61</sup> The planned meeting of Cicourel, Goffman, Fillmore, and Kendon had gone forward (although without Kendon), and they had prepared a new proposal to maintain the original goals but recommended additional filming to expand the data available for analysis because “several participant-analysts felt that the footage collected by the AFC [Anthropology Film Center] team did not meet the analytic requirements for naturalistic interactional study. A principal purpose of the new proposal is to remedy that problem by providing additional film data tailored specifically to the standards of these participant-analysts.”<sup>62</sup> Goffman’s initials are indicated next to these comments as well.<sup>63</sup> There is a footnote added, reminding everyone that: “It will be remembered that the identification of subjects and events for the AFC filming was under discussion and negotiation for more than two years,” presumably representing Grimshaw’s position, as the one who had led that conversation. The subcommittee meeting did not come to a resolution about what new context would be chosen for filming and analysis. They discussed the possibility of inviting Paul Ekman to join the project, perhaps as an additional filmmaker, and Goffman had agreed to invite him. If Ekman were not available, then Kendon would be asked. There was a lengthy list (sixteen items) of what could happen from this point, with Jenness commenting, “Now seems impossible—or very unlikely.” As a result of the new schedule, he wrote, “We will have lost somewhere between six and nine months; it is our hope that the time loss will be compensated for by a stronger design and a more valuable final product.”<sup>64</sup> In sum, they had completed filming, but the film was deemed inadequate by a group including nearly half of the analysts. That was a huge obstacle.

A few weeks later, Grimshaw wrote the filmmakers (Feld, Williams and Williams), saying:

When I talked with Steve after the meeting in San Francisco, I thought something had been worked out—even if that something was not maximally satisfactory in terms of our original design. It now appears that there was no real agreement. Consequently, David and I will probably recommend to the Committee on Sociolinguistics that we start again with a new set of analysts—and the AVC [AFC] data. (Sep 25, 1975, ADG)

That is in fact what happened: The committee concluded that “the intended compromise is unfeasible,”<sup>65</sup> in Jenness’s words, and the problems “intractable,”<sup>66</sup> in Grimshaw’s. Instead, they recommended working with the

data already collected, understanding that this would mean some analysts would need to be replaced on the team. Goffman was clearly to be one of those.

Grimshaw's response to the conflict was to review all the correspondence over the first two years of the project in order to determine whether the issues brought up were evident earlier; he wrote a lengthy letter to Jenness sharing what he discovered. He did admit that he found several issues clearly stated in letters from Schegloff (doubts about the "edited composite" proposed) and Kendon ("explicitly stating his requirement that the film must be in the LOC [locked-off camera] mode"), and a warning about difficulties in a letter from Jenness (that there were "complaints about Feld and Williams pre-screening data"). That he had lost track of these was attributed to the fact that Grimshaw's daughter died about the same time. However, he also re-read the proposal to NSF, which clearly stated what they intended to do, so he pointed out to Jenness that some others also were guilty of not reading carefully.

The fact of the matter is, alas, that I didn't heed the early warning signals, and that our colleagues didn't pay attention to our explicit statement as to how the film was to be made (and what it was to include). We have now reached an impasse and the kinds of changes that Goffman is demanding, both personally and as a representative of his "constituency" cannot be met without changing the project from what it was originally intended to be.

... before we left Austin [where the group had initially met to sort out the project], Goffman was strongly encouraging and insisted that I go ahead with the project in spite of what were already seen as numerous possible difficulties. (In fairness to Erving, he was talking about a much less involved project—simply getting together of something like a G group to mutually examine some tapes or old film—without any intention to publish. (Sep 24, 1975, ADG)

Grimshaw thought it was time to seriously consider who would be leaving the project, and who they might get as replacements. Continuing the letter to Jenness, he wrote:

It now seems highly unlikely that Goffman or Harvey and Manny will stay with the project. If the rumors we have heard are correct, Gumperz will also be unsatisfied with the AFC film; he is also likely to drop out. Kendon has expressed unhappiness with the data and will probably feel constrained to join those who leave. Of the current set, Bird and Grimshaw, Cicourel, Fillmore, possibly Halliday, and certainly Williams and Feld will stay on if the current data are used and new participant-analysts sought to join with us. If Sacks and Schegloff leave it is almost certain that none of their followers or close associates would participate (e.g., Jefferson, Schenkheim [Jim Schenkein], et al.). There is no one

else who does exactly what Goffman does (I will be reading a dissertation by a new person in our psych department who worked at Penn with Goffman, I'm not particularly sanguine). (Sep 24, 1975, ADG)

In summarizing potential replacements, Grimshaw thought perhaps Edward Hall in lieu of Goffman ("Steve has told us that Edward Hall has expressed strong interest in the data. Hall is certainly one of the very most distinguished scholars in the area of proxemics"); Charles Frake, Anne Salmond, or Joel Sherzer as possible replacements if Gumperz and Cook-Gumperz left; Byers if Kendon left; Peter J. Burke if Sacks and Schegloff left. But he admitted that:

In making these changes, or similar ones, we will sustain some major losses. Conversational analysis, as practiced by Sacks and Schegloff and their associates, is an extremely promising and exciting activity and may adumbrate a long-awaited grammar of social interaction. Goffman is clearly a major luminary, if he doesn't have an articulated theory of social interaction, he is certainly moving toward one in *Frame Analysis*. Gumperz, who has no articulated theory, is nonetheless one of the most thoughtful students of social interaction and a superb analyst. Kendon is a strong representative of a field moving from empirical taxonomic work toward some kind of integrated kinesic-ethological perspective. (Sep 24, 1975, ADG)

However, despite his regret at losing several participants, he added, "We would, with the new set of participant-analysts, still have figures of major stature in the field. . . . The final product would be different but by no means trivial" (Sep 24, 1975, ADG).

Jenness responded that he was impressed that Grimshaw was willing to take some blame and felt he should share it. "But I wonder whether the outcome, really, would have been different. As you point out, there may have been no way in which that particular group could have agreed on ground-rules acceptable to us and the committee." He recommended Klaus Scherer as another potential replacement, liking the idea of adding a non-American perspective. "[M]oreover, it would be worthwhile in that Scherer and Erving Goffman had together been working on the Royaumont project, so that in effect we'd be involving a Goffman protégé" (Sep 30, 1975, ADG). An explanation is necessary here. The "Royaumont project" refers to the Fondation Royaumont (Goüin-Lang) pour le progrès des Sciences de l'Homme. To briefly provide context for that organization, "In 1972, the Foundation encouraged Europe's first attempt at cross-disciplinary cooperation between biology and anthropology, establishing the Centre Royaumont pour une Science de l'Homme chaired by Jacques Monod."<sup>67</sup> Goffman was one of a

large group of scholars gathered together by Royaumont to study animal communication and human communication in 1974.<sup>68</sup> “A preliminary list of specialists to be consulted during the planning phase, or who could take part in some capacity or other in the three-year project was established.”<sup>69</sup> Goffman and Birdwhistell were both listed.

There was another attempt to involve Scherer. As the story directly involves Goffman, it is worth a brief sidetrack. Scherer taught at the University of Pennsylvania in 1970,<sup>70</sup> where he sat in on one of Goffman’s courses (Fine 2009). In June 1974, Grimshaw had written to Scherer, then back in Germany:

Professor Erving Goffman, who will be involved in the multiple-analysis project described in the enclosure, has told me that you are planning a similar project in Europe and has suggested that we might profitably exchange information and ideas. I gather from a recent conversation with Goffman that our projects are different enough to warrant independent activity but simultaneously sufficiently similar in goals and in design to assure enhancement of each if we share our problems and resolutions and, on completion of the projects, compare our results. He suggests that on completion we might well want to bring participants from both projects together to mutually review and discuss findings. . . . If Goffman’s suggestion makes sense to you I hope you can provide me with some details on your plans and tentative schedule. (Jun 25, 1974, ADG)

Scherer answered, but apparently felt it was far too soon in the process to compare notes, for Grimshaw responded: “I agree with you that we should try to keep in contact; and also agree with you that since both projects are still in a formative state, there is little more we can do at this point than to keep one another informed. . . . I also hope that it may be possible for us, or at least for some participants in the two projects, to get together at a later date” (Oct 21, 1974, ADG). Given that MAP took over twenty years to complete, meeting to compare results became moot.

Interestingly, Zabor learned of Scherer’s project a few months later, writing Feld about it. Apparently that letter was forwarded to Grimshaw, for it is in his files. She characterized it as “an international project on the joint analysis and discussion of one particular instance of communication in social interaction” and had heard it might include Bateson, Kendon, Goffman, Guy Cellier, David Crystal, and others. Duncan had told her about it, and suggested she write Grimshaw, since “the general plan is for many researchers to study one filmed interaction” and it sounded much like what Grimshaw was organizing (Nov 27, 1974, ADG). Of note is that first Goffman and then Duncan and Zabor tried to help Grimshaw connect with



a comparable international project, although that effort did not in the end lead to either collaboration or coordination.

To return to the 1975 difficulties with MAP. In October, Grimshaw wrote to Goffman to find out whether he was willing to stay with the project given the committee's decision not to implement the changes he had requested.

The work you put in to trying to make the project work, in San Francisco and later on with David [Jenness], was a great contribution and we are all grateful for it. Our hope is that you will feel that your participation is still possible. I believe you could do great things with these data, and that the MAP would be poorer for your absence. If you feel you cannot participate, I will of course understand, even though I will regret it. . . .

Erving, I hope you'll decide that you would like to work with the MAP data. Whatever your decision, I'll continue to enjoy what you do with what you use! (Oct 17, 1975, ADG)

Goffman responded to Jenness rather than directly to Grimshaw (asking him to pass on the response to Grimshaw):

A case, I guess, of the best solution none the less being hard. I do appreciate the very real concern and effort of all parties, and the reasoned and reasonable statement. The course the Committee has decided on assures at least some outcome, which the other did not, and that is a merit. My misgivings about the collected materials aren't [*sic*] of course altered by your decision nor by the fact that I may well have supported your view were I serving alongside of you, and I withdraw. (Oct 24, 1975, ADG)

Despite letters from Grimshaw encouraging them to stay ("I do think your continued participation in the MAP would be valuable for you, just as I know that it would be valuable for the rest of us" [Oct 17, 1975, ADG]), Schegloff and Sacks likewise withdrew within a few days.<sup>71</sup> Jenness and Grimshaw accepted responsibility rather than attributing blame to others, writing to the entire group:

It seems, in retrospect, that mistakes were made by the committee and the organizers of the project, such that the knowledge and the intellectual premises of the several participants were not sufficiently understood and taken into account. Collaborative projects are generally at risk in this respect, and this project has been no exception. . . . Goffman undertook to state not only his own requirements but those of the group of conversational analysts [Schegloff and Sacks] represented in MAP, as he understood them. Goffman's efforts were generous indeed; he realized that while trying to represent other analysts was

a risky task, it would be an endless process were each analyst to try to revise or alter the document from his own individual point of view. (Oct 20, 1975, ADG)

While Jenness was particularly gracious about Goffman's role publicly, he was not always so kind. A month earlier, in a letter to Grimshaw, he had mocked Goffman for claiming to have come up with the idea that led to MAP. Specifically, he said: "In fact, Erving now claims that in some sense he 'gave' the basic MAP idea to both you and Scherer, as if he were a football coach trying out two running backs competitively!" (Sep 30, 1975, ADG). But Goffman did, in fact, outline almost exactly what the MAP project attempted in 1970, in a letter to Sebeok. Whether he also explained that design to Grimshaw and Scherer, of course, is a different matter, but if he claimed he did, presumably it is likely. To understand his comment, it may be helpful to realize that he was evaluating the conference he had just co-organized and managed with Sebeok in Amsterdam. The specific idea was this: "I think it might be useful to give thought to a conference methodology suggestion made in the past. If we could videotape or film a five-minute strip of informal spoken interaction and mail each participant or each local team of participants a copy and then spend two days showing off our several efforts at analysis, much, I think, could be gained" (Oct 7, 1970, TS). Unfortunately, Goffman is not explicit about who he thinks previously had made such a suggestion for what purpose, or when in the past. But it is at least clear that he did, in fact, make such a proposal himself as early as October 1970.

Jenness reported to Paul Chapin at NSF in November 1975, to update him as to progress on the grant, and provide documentation as to the logic of the decision:

Last month, the Committee was finally faced with deciding to proceed with the project as originally intended, thereby losing some highly valued participants, or redesigning the project so as to keep those participants. Because the latter choice would have meant giving up the comparative and collaborative aspects of the project, as originally designed, the Committee chose the first alternative.

Three of the original participants—or two of the nine "teams" of participants—have declined to continue with the project. They are Harvey Sacks and Emanuel Schegloff, and Erving Goffman. We have invited others, appropriately from the interactional and social-science side, rather than from the linguistics side, to take their places. I will be able to let you know shortly who has accepted. (Nov 10, 1975, ADG)

In November 1975, Halliday wrote to Jenness, saying that he would stay with MAP, although he was disappointed in two small issues. The first was that the text to be analyzed was from an academic context, not his favorite. “The second disappointment is over the withdrawal of some of the original participants, with whom I had particularly been hoping to be associated in this project, and whose work on the text I had eagerly looked forward to seeing.” At the same time, he understood how much work Grimshaw had already put in, and so he was not ready to withdraw. “I agreed to work with whatever kind of data was generally accepted, and I am sure that I shall get something from it, the more so perhaps because it will force me to look for different things” (Nov 15, 1975, ADG).

Apparently Chapin wrote to Jenness and Grimshaw questioning the viability of MAP at this stage, arguing that “it would not be fruitful to precede with procedural details for the follow-on proposal” because they did not have commitments for participation from a “broad enough spectrum of expertise in the study of interactive behavior”; and based on their apparent inability to provide “the information each participant felt to be necessary.”<sup>72</sup> In response, Grimshaw laid out the original teams (Bird/Grimshaw, Cicourel, Fillmore, Goffman, Gumperz/Cook-Gumperz, Halliday, Kendon, Sacks/Schegloff, and C. Williams/Feld) and revised teams of analysts (Bird/Grimshaw, Burke, Cicourel, Fillmore/Lily Wong Fillmore, Gumperz/Cook-Gumperz, Hall, Halliday, Kendon, Williams/Feld), specifying that: “We do *not* consider Burke and Hall to be second-level replacements for those who chose not to continue in the project” (emphasis in original). “Most importantly, these highly qualified analysts all have agreed to work on a common segment of the data. We are now ready to go ahead” (Dec 1, 1975, ADG).

However, Chapin and others at NSF were evidently not impressed by this logic. As Grimshaw mentioned in another letter to Chapin:

In a letter to me dated 14 January 1976, responding to an earlier letter of mine, you noted that you and your colleagues had reviewed the AP file and our reasons for urging continued support of the project, and that your “unanimous opinion” was that “results of the preliminary phase of the project, despite some reservations, [did] not justify the investment of further research funds.” My understanding at that time was that a principal basis for the negative assessment was a shared concern that it was unlikely that the several participant-analysts would either agree on what was to be done, or, if they agreed, do it. (Jun 14, 1975, ADG)

In April 1976, as Grimshaw wrote to Halliday, his feeling at that point was: “We won’t be doing the project under the ideal circumstances we had originally

hoped; we can still believe that we are engaged in an important activity and that we can carry it through to successful completion" (Apr 7, 1976, ADG).

In June 1976, Grimshaw wrote to all analysts that, while there had been delays, the audio material had by then (finally) been processed, and both still photographs and a transcription were nearly ready; projectors had been purchased and would be sent to Berkeley for the two teams there (Fillmore/Wong Fillmore; Gumperz/Cook-Gumperz), one to Santa Fe (Hall, Williams/Williams) and another to Bloomington (Bird, Grimshaw, Burke); Varispeech machines would be available shortly, for Fillmore, Halliday, Hall, and the Bloomington crew. The common data set was a twelve-minute selection. "That period should be long enough to give everyone something to sink their analysisic [*sic*] teeth into without being impossibly long for those of you who do very fine-grained work. We decided to use the entire piece because it not only has a variety of kinds of behaviors but also because it includes fragments in which there are, variously, three, four, and all five participants" (Jun 3, 1975, ADG). Grimshaw further requested that everyone begin analysis immediately so there would be initial analyses available by the end of the year; he and Jenness would "draft a proposal for support of the latter phases of the project" at the beginning of 1977 to cover the costs of a conference and preparation of a published volume. "Since one of the reasons we are currently not being supported is reported skepticism that analysts would ever do their several 'shares'—we must be able to show that all of us are sufficiently committed to do the work" (Jun 3, 1976, ADG). At that point, the group he was writing to included: Bird, Burke, Cicourel, Feld, Fillmore and Wong Fillmore, Gumperz and Cook Gumperz, Hall, Halliday, Kendon, Carroll and Joan Williams.

In June 1976, Halliday responded, "I was sorry to learn that NSF were not prepared to go on supporting the project. It has had its ups and downs but no more than any other project of an original nature. Anyway this makes no difference to my own participation; I shall be glad to take part as planned. Ruqaiya and I intend to work on it together" (Jun 16, 1976, ADG). It is a minor note, but an interesting one that none of the spouses (Cook-Gumperz, Hasan, Wong Fillmore, or J. Williams) were ever actually invited to participate. Apparently, it was taken for granted that an invitation to one (the male) spouse included an invitation to the other (the female). In fact, all of them came through when it mattered most, J. Williams in helping with filming, and the other three in writing chapters for the final publication.

In February 1977, Grimshaw wrote everyone again, apologizing for the fact that technical delays meant no one had yet gotten the film segment. He passed on comments from Worth about how they were “part of a long process in which social scientists are going to have to learn how to deal with the mechanics of collecting their data in a much more fundamental fashion . . . unfortunately your experience will give everybody else the necessary knowledge to know precisely what things to be very clear about. . . . being pioneers, we will have undergone an experience from which others can learn” (Feb 1, 1977, ADG). At the same time, he hoped “that each of us will have taken the opportunity to examine some of the data already available (e.g., transcripts, reel-to-reel audio, cassettes)” and that once they had some preliminary analysis, they should send it to him so he and Jenness could prepare the next grant proposal. He included a paragraph summarizing where everyone was physically (several were out of the country at that point, or in transit) and what they were working on (a few working on topics related to MAP, the rest busy with other work).

In April 1978, Grimshaw notified the group that the future of the Committee on Sociolinguistics was in doubt, but that “MAP can continue whatever the fate of the Committee” (Apr 10, 1978, ADG). In fact, the committee ceased existence in 1979 (Heller 2018). He asked whether anyone would object if he added Laurence Wylie to the group, as he was interested in examining the data. “I think that he does very interesting work and that he would be a welcome addition to the analytic team.” (Wylie did in fact join.) Grimshaw also shared that Fillmore, Gumperz, and Cicourel had proposed permitting the sharing of data before the project was completed, something that had been agreed should not happen when it was begun. However, given the time elapsed, he was inclined to support them and was asking if anyone objected: “I do hope, however, that we can keep the results of our several analyses from being totally dissipated before we are able to have our post-analysis conference and before we get a finished product together for publication” (Apr 10, 1978, ADG).

In June 1978, Grimshaw wrote to Chapin at NSF, again requesting funding. Picking up with the issues indicated as problems in 1976, he said:

It now appears that the several analysts *have* agreed on what is to be done and that they *are* doing it. I write, therefore, to ask whether it is reasonable to inquire whether we might again approach the NSF for funds to complete the project. The principal direct costs involved would be for a four-day conference of participant-analysts and commentators, to critically review the completed analyses, and for production of a final report. (Jun 14, 1978, ADG)

Chapin eventually must have agreed to consider a new proposal, because in December 1978, Grimshaw sent “MAP participant-analysts, CSL members, friends and advisors of the Multiple Analysis Project” a draft proposal of a new NSF grant request. The deadline for submission was February 1979. He enclosed a draft proposal with “an appendix on the (checkered) history of the MAP.” It would also include his CV, everyone’s project summaries, Zabor’s transcript of the common data sample, and a budget. He asked that everyone pay particular attention to their own biographies, and longer descriptions of their parts of the project. He requested suggestions of where to host the conference where they would present and compare their analyses. And he added new information about why the second round of funding for the project had been rejected: “When the NSF declined to provide support for continuing the project in 1976, they did so because it didn’t appear to people there that the project would succeed. Three original analysts had left the project, many of our colleagues had in mind the failure of the ‘natural history of an interview’ project and quite frankly didn’t believe that we would actually do our several analyses.”<sup>73</sup> On the same day, Grimshaw sent his draft proposal for foundation support to Jenness for critique. He had received drafts of several proposals and a few papers from group members, although, despite friendly words in the public announcement, he complained privately about “dilly dallying” by several people and the delays that was causing the group. “I can only hope that I get stuff from delinquents before the final version of the proposal is prepared for submission. . . . I am not very happy about the project, but I suppose that I could write up another version emphasizing the linguistics-humanities implications of MAP for submission to NEH” (Dec 18, 1978, ADG). That letter to Jenness mentioned that there had been a “contretemps” with Hall, and he seemed likely to withdraw from the group as a result. But a letter Grimshaw wrote on the same day to Hall sounds far more positive, mentioning only that it had been several months since Hall had responded; he hoped Hall would stay with the project, but if he would not have time for that, to please return both the documentation and the equipment so they would be available to others.

In March 1979, Grimshaw told Goffman:

I am making progress on my analysis of the film. I am using a modification of the Labov-Fanshel mode of analysis, adding the visual stuff (I have spent over 300 hours on a run through that) and, of course, more people and a different set of questions. I know that you’re not persuaded by some of the things I’ll be doing with the materials—but I hope that sometime we can sit down and talk about what it is that I am doing. I am putting the first analysis on the computer

in a format like Bill's—if you're interested I'll be able to send something for examination in a month or so. (Mar 6, 1979, ADG)

Notice several things here: that Grimshaw, at least, was actively working on MAP at this point; that he used Labov and Fanshel (1977) as a model (and wrote a review of that book in 1979); and that he assumed Goffman would still be happy to talk with him about what he was learning, despite their disagreement about methodology. In fact, he was right, because the answer from Goffman was cordial: “Glad to hear that your MAP project is moving” (Mar 25, 1979, ADG).

In early August 1979, Grimshaw complained to Kendon:

I wish I could give you some sort of report on MAP funding. The proposal has been with NSF for five months now, without any sort of response after the original acknowledgment. There are, I understand, some problems about funding of the Foundation's social science programs, and apparently there will be joint House-Senate conferences before a budget is established. We *will* finish the project sometime, I am going ahead with my own analysis, and enjoying it. I hope that you and others of our colleagues are also finding time to work on the materials. I am not sure whether I told you that Fred Erickson will be doing an analysis—he has the data set which Ned Hall had. I will be seeing Aaron [Cicourel] shortly and hope to find out what he is doing. (Aug 2, 1979, ADG; emphasis in original)

This makes it clear that Hall had left the group, and Erickson had joined in his place, and that Grimshaw was still assuming the group would be able to move ahead and complete the project. A few weeks later, Grimshaw told Kendon: “I am looking forward to reading your contribution, and those of our colleagues, to the MAP—and to discussing them at a meeting. When that will be I don't know, I have still not heard from NSF” (Aug 17, 1979, ADG). The meeting he is referring to here is the one he wanted to hold where everyone would present their analyses; in fact, that meeting did not occur for several more years.

In September 1979, Grimshaw circulated the message to group members that the request for NSF funding had been rejected, but that he had been encouraged to revise the proposal, especially in terms of reducing the budget, and to resubmit. He was willing, even though the budget reduction “will come primarily out of my hide—in terms of a reduction of released time,” and expected to have that ready to resubmit within 4–5 weeks (Sep 20, 1979). But in fact, it took far longer to compile the necessary documentation from everyone. In February 1980, Grimshaw was still working on

the proposal for addition funding while writing his own analysis. He wrote Kendon: "I am working on the MAP materials again. . . . My analysis is still a long way from completion (I am about 1/2 to 2/3 of the way through a first draft analysis) but I am satisfied with what I am doing (though I know others won't be). I am still trying to get the last of the materials I need for revision of the MAP proposal" (Feb 7, 1980, ADG).

In April 1980, Grimshaw wrote Kendon that there had been a group meeting on the west coast at which he had met with Gumperz, Cook-Gumperz, Fillmore, Halliday, and Hasan, and then he had met separately with Wylie.

All were, of course, very busy. All have agreed, however, to send the missing stuff (some is already in hand) and have again expressed interest in working on the project (John and Michael and Ruqaiyah [*sic*] outlined plans [*sic*] specific plans for their work). I am now working on the revision, and trying to find out from NSF if the changes I have projected are those asked for. I'll keep you posted.

May I hear from you soon? It would be nice if we could get this project completed.

I hope you are finding opportunities to work on the MAP materials. I am spending quite a bit of time on the data now, and finding it trying but fascinating. (Apr 22, 1980, ADG)

In May 1980, Grimshaw was still corresponding with Chapin about what they needed to change, and how best to meet the suggestions received to that point. In particular, he was concerned that the budget still came out to about the same, despite cutting reimbursement for his time in half, due to higher expenses for the proposed conference; and the increased length of the proposal, given the request for more information about participants. Even so, he was sure they would be able to make the July 1 deadline (May 19, 1980, ADG). At the same time, he wrote Feld, "I have spent much of my time during the last two weeks working on revision of the NSF proposal (Polly [his wife], I believe, wishes I would just give up the whole business, do my own work, and forget the others. I can't do that—I have too much of an investment—and feel an obligation to those other participants who have continued to be interested and supportive)" (May 16, 1980, ADG). One certainly must admire his stamina and perseverance.

In June 1980, Grimshaw wrote Sankoff that the revised MAP proposal was almost completed, and that "I am going ahead with my own analysis whatever the outcome of the NSF proposal. I think I have some interesting findings; I certainly have ample confirmation of the difficulties (and tentativeness of findings) in working with such materials."<sup>74</sup> At that point,



he outlined the complaints from reviewers to an earlier iteration of the proposal. They said the cost was too high, it was unclear that the proposed participants were sufficiently committed, why a conference was needed at the end (instead of a publication), or even why the project was particularly appropriate. He was trying to answer all the concerns and would be sending a draft around to the entire group of MAP researchers. He had worked especially hard to reduce the budget and sent a draft to Feld for comments: “The fact of the matter is that I am not accountant [*sic*—and that I find all of this very unpleasant” (May 16, 1980, ADG).

The next month Grimshaw told Chapin that the revised proposal was in the hands of the contract administration office at Indiana and would soon be on its way. At the same time, he thought it might be useful to answer the last set of extended questions from Chapin. The major issue remaining was that one reviewer had “complained about the lack of ‘hard-nosed discussants’” at the proposed conference; in response, his new proposal “also includes, I believe, some ‘hard-nosed’ types and, in the persons of Goffman and Schegloff, two original MAP analysts who withdrew because of dissatisfaction with the data.” His willingness to invite Goffman and Schegloff to be discussants of the project from which they had withdrawn is notable. He answered another critique, that of balance between disciplinary approaches, by pointing out that if those he proposed were all able to participate, then “the conference would have quite good disciplinary balance in my view, with five anthropologists, six linguists, three psychologists (including Jenness), seven sociologists, one philosopher and one humanist-social scientist (Wylie)” (Jul 11, 1980, ADG).

In August 1980, Grimshaw told Kendon that he had completed and re-submitted the proposal. “I have no notion of whether we will be funded. . . . But I am confident that we will be able to go ahead and produce some sort of worthwhile volume even if we aren’t funded, at least some of us are going ahead with out [*sic*] analyses, my own reaction is that I find myself more and more intrigued with the data—and more and more disgruntled about some of the detailed work which has to be done” (Aug 4, 1980, ADG). It is perhaps a little odd to be complaining about detail work to Kendon of all people, given that that was his specialty (and the explicit reason Grimshaw wanted him to be part of the project).

In March 1981, Richard Louttit, director of the Division of Behavioral and Neural Sciences at NSF, officially notified Grimshaw that the grant proposal (BNS 80-23112) had been denied. Unfortunately, the same critiques

as had been raised earlier had become issues again. A few quotes from the reviews follow:

- I found myself considerably more persuaded by the caliber of the participants in the MAP than by the attempted theoretical rationale. I suspect that interesting and perhaps important results will follow from allowing these people to discuss their work in relation to a common corpus, particularly if first-rate discussants are obtained. However, I'm not inclined to believe that anything like an assessment of the relative explanatory power of the various approaches will happen, nor that any progress will be made toward an integrated theory of sociolinguistic description.
- First, there seem to be serious problems with the data. Three of the initial investigators left the project because of its inadequacy and even the principal investigator now says (p. 15) that it would be shot differently if it were to be redone. . . . Second, while it is quite true that not [*sic*] single project could bring together everyone working on the analysis of conversation the absence of Goffman and Schegloff (or anyone within the Sacks tradition) as well as some other major researchers (Labov, for example) raises serious questions about whether the project will really be able to do what it is setting out to do.
- . . . there are some important omissions in the review of the relevant previous research. For example, the previous project that the current proposal seems most close to in goals and working procedures is the work of McQuown, Bateson, Birdwhistell, etc. that culminated in *The Natural History of an Interview*. However, no mention is made of this work (and if the first of MAP's objectives is "an [*sic*] comprehensive an analysis of a single speech event as heretofore been accomplished" it should certainly be assessed).
- . . . the fact that Sacks, Schegloff and Goffman chose to leave after viewing the data and meeting the other investigators raises even more fundamental questions about both the feasibility [*sic*] of the project and that data it is based on.
- This is a fundamentally important, and longstanding project. It is in the best sense of the term "interdisciplinary" (which may be influential in the way NSF chooses to fund the project; to me, it would be appropriate to have funds come from both Linguistics and Sociology). The project is all but completed, and with the funding requested, it should reach fruition. The idea of a joint analysis of a single episode of interaction is, of course, good. It will shed light on the underlying presuppositions, methods of analysis, and kinds of findings. By having the investigators from many fields interacting in a single conference, on a common data base, the specifics of similarities and dif-

ferences will be highlighted. . . . This is the best proposal submitted to NSF Linguistics Program I have read. The organization and thoroughness of the proposal serve as a guide for others to follow. This project is complicated; the conference and publication of these materials has staggering organizational problems. If anyone has the organizational ability, talent, and resourcefulness to “pull this one off” it is Grimshaw. He organized the project while on SSRC Committee on Sociolinguistics, has seen it through all its stages of development, and will see it to its conclusions.

- This project has to make it on (a) the will and drive of the P.I. and (b) sheer inertia. . . . We clearly have a star studded cast. The lack of representation of the conversational analysts is a real gap, though, and I tend to agree with the objections they reportedly (App. C, pp. 4–5) raised at the beginning, i.e. the formality of the material to be analyzed and the lack of a fixed camera. (Mar 10, 1981, ADG)

In terms of ratings, the reviewers gave one “excellent,” one “very good,” two “good,” and one “fair.” Obviously, there was no clear agreement that this project should be funded, and it was not.

Grimshaw’s response was to write to Chapin, asking, “Would MAP have been funded if the roof had not caved in?” (Mar 24, 1981, ADG). The reference was to political cuts to the NSF budget. Grimshaw told Cicourel a little time later that “both the Sociology and the Linguistics programs at the Foundation had recommended support; the sharply reduced appropriations for social science programs has meant that even continuing programs have either not been funded or have had funding sharply reduced” (May 1, 1981, ADG). Grimshaw was obviously an optimist; the specific comments in the evaluations would not have convinced most applicants that these were strong recommendations for support. Despite the lack of federal funding, he reported to Chapin that he’d been approached with the possibility of private funding, although it seems that also did not work out. However, he had received a sabbatical for the next academic year and planned to spend it “writing up my own analyses of the MAP materials as a monograph” (Mar 24, 1981, ADG). And he did just that (Grimshaw 1989).

### *MAP at Penn*

The majority of MAP had little to do with Penn, but one part of the story did take place there, drawing in a few additional participants. Discussions began in 1981, between Grimshaw and Feld at a conference, continuing via

letter and telephone, about an “Annenberg possibility’ for winding up the MAP . . . I think that if you and your colleagues are able to work something out we will be able to get a good act together” (Mar 24, 1981, ADG). Feld asked ASC to serve as the setting for what Grimshaw called “a public ‘show and tell.’”<sup>75</sup> That initial request was successful, and Grimshaw happily told Cicourel that:

. . . plans for an MAP conference in conjunction with an Annenberg School of Communications Conference on Visual Communication are likely to be approved. A proposal, which includes both the visual communication conference and a separate MAP program has been tentatively approved by Mr. [Walter] Annenberg, and Steve [Feld] and his colleagues at the Annenberg School are working out logistic and financial details. (May 1, 1981, ADG)

The two events were eventually separated, although there was still support forthcoming for MAP. Grimshaw continued:

It appears likely, then, that after years of delays and disappointments we are going to be in a position to complete our project. . . . Some of us have been using the materials heavily, some in classes and seminars and others in individual analyses. I have, as you know, been heavily engaged in work on the materials. I will still plan on working on my monographic treatment during my sabbatical year in Tucson; I will not want for things to say about the corpus. (May 1, 1981, ADG)

In the end, the Multi-disciplinary Workshop on Analysis of a Naturally Occurring Conversation: The MAP was held November 17–20, 1982, at Penn, with travel, accommodations, and conference facilities all subsidized by Annenberg (Grimshaw 1989). In addition to the major analysts, the group was expanded for this event to include William Corsaro, F. Roger Higgins, Dell Hymes, Teresa Labov, and Elinor Keenan as discussants.<sup>76</sup> Sankoff was also invited to serve as discussant, but turned down the offer as she did not do microsociolinguistic work.<sup>77</sup> And Schegloff was invited to be a discussant; he also declined (Jun 1, 1982, ADG). Grimshaw mentions that Kendon had to withdraw as one of the participant-analysts and might serve as a discussant instead, although that did not happen.<sup>78</sup> That was in April, during the planning stages. By September, Grimshaw was writing to participants asking if they could please complete their papers so the discussants would have them ahead of time; at that point, he only had one paper in hand. According to the draft program included with letter, he was expecting eleven papers (Bird, Burke, Cicourel, Erickson, Fillmore, Wong Fillmore, Feld, Gumperz and Cook-Gumperz, Halliday, Hasan, and Wylie), and these were to be shared

with the five discussants mentioned above, with Feld organizing logistics at ASC, supported by Larry Gross and Jay Ruby.<sup>79</sup> Despite the fact that Goffman died November 19, the workshop was in fact held as planned.<sup>80</sup>

A few months after the event, Grimshaw shared with Schegloff that “the Philadelphia conference was a success (though it ended on a somber note because of Erving’s death). Everybody had papers, though some were at later stages than others; we are hopeful that we’ll have a manuscript completed by late summer” (Feb 7, 1983, ADG). Grimshaw sent Kendon an extensive description since he had been expected yet in the end was unable to attend, so we know what happened.

I thought you might be interested to learn that we did have our Philadelphia meeting, and that things worked out much better than I had expected as late as the time Polly [his wife] and I boarded our flight to Philadelphia. Almost everyone who finally attended the meeting had a paper; some did send them in advance, a few brought copies with them, one or two had papers which were written but not available for distribution. The discussion sessions generally went very well, the nature of the initial discussant’s remarks in each case was, as you would expect, affected by whether or not they had had the paper in advance. Whatever, we now have a schedule (over the next six months) for completion of papers and preparation of final comments, etc., and I am, for the first time in five years or so, fairly confident that the project finally will come to completion. I do wish you had been able to attend and I am very sorry that you were not able to contribute a paper. The fact is that not very many of the contributors have drawn very heavily on the visual record; I suppose that fact is in a way a validation of some of the criticisms of the project. Such problems notwithstanding, I am hopeful that I will have a late draft manuscript by sometime late next summer (or early fall). (Jan 6, 1983, ADG)

Kendon’s response was quite positive: “I’m glad your conference was a success. . . . I am also regretful that I had to drop out of your project which, as you know, I’ve always believed was very worthwhile” (Jan 11, 1983, ADG). Kind words, particularly since Kendon was initially one of those who had strenuously objected to the data collection process.

### *MAP, Birdwhistell, and NHI*

There is a distinct gap in the list of MAP participants overlapping with Penn, and that is Birdwhistell. Szwed is also missing, but there was no reason for him to be involved in a detailed analysis of linguistic data, so his omission seems both obvious and reasonable. Birdwhistell, however, was especially known for this type of work; he was certainly at Penn during the relevant

years, and Grimshaw knew of his work, both from correspondence with Kendon, who included both Birdwhistell's and Goffman's publications in his teaching (and research),<sup>81</sup> and from Birdwhistell's involvement with *The Natural History of an Interview* (McQuown 1971), an obvious precursor project. NHI was both less successful (it was never published) and far more so (clearly having influenced all later research on multimodal communication, as documented in Leeds-Hurwitz and Kendon 2021).<sup>82</sup> A large part of the answer may have been that, as Grimshaw told Kendon directly in 1979, "You are the central person among those involved in the kind of microanalysis of video materials which you do" (Oct 29, 1979, ADG), and so he only wanted Kendon. (Kendon had been a late participant in NHI and so had an uncommonly clear sense of what NHI had been about, how participants analyzed data, and whether it would serve as a model.) In addition, it may well have been that Grimshaw thought of Birdwhistell as only being interested in kinesics (body motion communication) and, although early on he explained that MAP would examine kinesic and proxemic behavior as well as language and paralanguage, in fact, the results strongly emphasized verbal over non-verbal behavior. If so, that is a common error; today, Birdwhistell is only known as the person who invented kinesics, while with his own students he emphasized a far broader analysis of communication behavior. He wanted kinesics to be part of it, but studying just that was not his goal; it was rather one piece of a large puzzle. It is also possible that either Hymes's or Sebeok's opinion of Birdwhistell had convinced Grimshaw not to invite Birdwhistell to participate in MAP. Despite having lobbied hard to get Birdwhistell to Penn, Hymes used the phrase "prima donnas such as Birdwhistell" in a letter to Grimshaw in 1975 (Aug 8, 1975, ADG); and Sebeok's opinion, as previously quoted when discussing the Amsterdam conference with Goffman ("sigh, sigh" [Feb 4, 1970, TS]), may have made its way to Grimshaw as well. Perhaps, like many linguists, he really did not think what he wanted to study should include nonverbal behavior. Whatever the logic, Birdwhistell was either not invited into the group or chose not to participate. Given that there is not a single letter to or from Birdwhistell in the Grimshaw papers, the former seems most likely.

The connections with NHI were, however, explicitly discussed, as well as Grimshaw's comments on its use (or rather, its non-use), in Macbeth's review of Grimshaw et al. (1994a).

But in describing the Natural History of the Interview project (NHI), pursued by Bateson and other fellows at the Center for Advanced Study of the Behavioral

Sciences in the mid-50s, Grimshaw suggests more than a rhetorical interest for it. NHI was the nearest prior approximation of the MAP project, and its failure to come to completion (publication) was cited as grounds for early skepticism about MAP. In Grimshaw's view, "it is difficult to avoid the conclusion that the NHI tropism for completeness proved its downfall. . . The NHI transcript is more complete than any other in existence. It has not been used because no one knows how to ask questions about it" (p. 34 [of Grimshaw et al., 1994a]). (1995, 710)

I agree with Macbeth that NHI was the obvious precursor to MAP; even if Birdwhistell was not to be asked to participate, that should not have stopped Grimshaw and other MAP analysts from using NHI as a relevant guide. But they did not.

### *MAP-Related Publications*

At the start of MAP, Grimshaw invited Hymes and Labov to participate (though both declined), as well as Goffman (who accepted, was part of the group for several years, but later resigned), and used Worth as a resource. He also invited Sankoff to participate in the conference at Penn where the major analyses were (finally) presented; she also declined. By the end, MAP involved several additional Penn faculty members in some capacity who had not initially been part of the group—specifically, Kendon,<sup>83</sup> Feld, Erickson, and T. Labov—but neither Goffman nor any other Penn faculty prepared major analyses for MAP. As with the project, Grimshaw took the lead on publications, not only publishing several articles of his own on MAP (1982a, 1987a, 1987b) but also editing a special issue for *Sociological Methods & Research* published in 1982, under the title "Sound-Image Records in Social Interaction Research." Grimshaw invited MAP analysts to participate in that issue. For example, he asked Kendon to write about "kinesic and proxemic features and the coding and analysis thereof," despite the fact that "I know that the proxemic part of that isn't really your 'thing'" (Oct 12, 1978, ADG). The tentative publication date was set as May 1980, so Grimshaw requested a manuscript be submitted in June 1979. He enclosed a postcard which Kendon signed, checking the box for "YES, I am interested in submitting the paper you describe."<sup>84</sup> In the end, Kendon had no time to write up an article, but Corsaro (1982) and Erickson (1982) eventually contributed, as did several scholars not otherwise part of MAP: Mark S. Cary (1982), Starkey Duncan Jr. (1982), and Adrian T. Bennett (1982). Grimshaw himself contributed an astonishing three pieces to the special issue (1982b, 1982c, 1982d). In the introduction he argued that "anthropologists, psychologists, and psychia-

trists have historically been more receptive to such materials than have been sociologists” (1982b, 116). “Such materials” meant videotapes being used for research. Clearly, he hoped that MAP would change that.

Grimshaw (1982c) names Labov and Fanshel (1977) as a prior, relevant resource (following up on the earlier comment to Goffman that he was using a modification of their method for his own analysis [Mar 6, 1979, ADG]). He outlines the logic of the MAP project which had led to this special issue. In the body of the text, he cites Goffman, Labov, Birdwhistell, Kendon, Feld, Williams, Gumperz, Corsaro, Erickson, Ferguson, Sacks, and Schegloff (thus many of those affiliated with Penn or MAP or both), as well as NHI as an obvious precursor project, like Labov and Fanshel, as well as much of the relevant literature on multimodal communication available at that point. He mentions a few details about the filming for MAP but focuses mostly at a more general theoretical level. He acknowledges help from Condon, Erickson, Feld, Kendon, and Worth (as well as others who were not connected to MAP at any point), for “identification and (at least) partial resolution of the numerous and complex problems of the MAP” (142). Presumably in acknowledgement of the issue that led Goffman to leave MAP, he has a footnote stating that “such requirements vary, of course, and when (as in the MAP) several investigators are analyzing the same record, that record is likely to be less than optimal for any individual researcher” (144n23).

In addition to the journal special issue, Grimshaw began a book during his 1981–1982 sabbatical (eventually published as Grimshaw 1989), was negotiating with publishers for a contract for both the sole authored and the edited books by 1983,<sup>85</sup> and was far enough along to send a draft of his own book for review by Jenness in 1987 (Feb 25, 1987, ADG). Also, Grimshaw had an earlier volume of selected essays (1981) which included one detailed analysis of the MAP data and discussion of the project’s goals and methods (“Instrumentality Selection in Naturally Occurring Conversation: A Research Agenda”). As with the major volumes mentioned below, this collection did not attract particularly good reviews. As Murray points out, “How much about the maintenance (let alone ‘construction’) of vital social structures can be discerned from intensive analysis of the deliberations of one PhD committee planning what to tell the successful candidate remains to be seen” (1982, 744). In addition, Grimshaw presented on MAP at various conferences, such as at the International Conference on Social Psychology and Language held at the University of Bristol in July 1979 (1981, 350), and



the Visual Research Conference in 1990 (documented in Grimshaw 1990a; Scherer 2013).

Grimshaw both edited and contributed to a second volume (Grimshaw et al. 1994a), which included a lengthy introduction by Grimshaw, Feld, and Jenness; chapters by Burke, Cicourel, Cook-Gumperz and Gumperz, Fillmore, Grimshaw, Halliday, Hasan, and Wong Fillmore; commentaries by Corsaro, Hymes, and T. Labov; and a substantial epilogue by Grimshaw summarizing what they had learned. Grimshaw and Goffman remained cordial, for as late as 1981 Grimshaw wrote to Goffman, “I can’t remember whether I sent you a copy of the first real analysis paper from my work on the MAP materials. . . . I’d much appreciate reactions” (Jul 27, 1981, ADG). Aside from the fact that the 1994 collection is dedicated to Goffman, the very title, *What’s Going On Here?*, is an homage to Goffman, who wrote in 1974: “I assume that when individuals attend to any current situation, they face the question: what is going on here?” (8).

One final related publication: Grimshaw’s edited collection, *Conflict Talk* (1990b) was an indirect result of the MAP conference held in 1982. As he reports:

Five years ago this summer several of us whose work appears in this volume had a “show and tell” mini-conference during the course of which we viewed a large number of sound-image records particularly rich in conflict talk. This experience further convinced me of the potential value of more explicit attention to conflict talk. I therefore invited a number of colleagues who seemed to me to have interests or data which would make them valuable contributors to a book on conflict talk to write chapters on the topic. This volume is the result. (1990c, ix)

It seems likely the five years mentioned here should be counted from the conference to the time he was writing the introduction, not when the volume appeared in print, and so probably refers to the MAP at Penn conference in 1982. The overlapping participants between that event and his 1990 book are Teresa Labov and William Corsaro.

### *MAP’s Reception*

The reviews of all of Grimshaw books resulting from MAP (Firth 1996; Halkowski 1991; Macbeth 1995; Murray 1982; Schwartzman 1992; Scollon 1995; Stubbs 1991; Zimmerman 1995) were primarily negative, and neither of the major volumes most directly connected (Grimshaw 1989; Grimshaw et al. 1994a) seems to have made much impact on other researchers, as they

have hardly been cited. Frequently, reviewers compare MAP to Labov and Fanshel (1977), clearly preferring that book. Scollon's 1995 review, appearing as it did in *Language in Society*, and given that Grimshaw served that journal as associate editor, is the longest and most complimentary, but even it concludes that "a project of such complexity and duration, involving such well-known and busy scholars, is bound to result in a final product of some difficulty" (430). Scollon highlights the gaps remaining from the departure of Goffman, Schegloff, Sacks, Bird, Kendon, and Erickson from the project, and suggests that "the reader is left with tantalizing questions about how those others might have looked upon this body of work" (431). Tantalizing indeed. A different reviewer points out that "Grimshaw asks and answers the question that may occur to many readers of this text: Has it been worth the effort? Not surprisingly, his answer is yes, but he is well aware of the kinds of criticism leveled at this approach (for example, it is work that does not produce reliable findings and cannot be replicated, and it is concerned with trivial, inconsequential and obvious aspects of daily life)" (Schwartzman 1992, 389). Grimshaw sent the first few reviews he read around to all the authors, saying he was "deeply disappointed" not to have reviews published in any major linguistics journal, and "I am also disappointed, of course, that the three reviews we do have essentially disattend the substantive content of our several contributions." He concluded: "Unless, as I think very unlikely, another review appears, the MAP would seem to be completed. I am deeply grateful to each of you for your efforts in our joint project. Things never worked out as I (at least) had hoped, we had problems from the first day. But I think we had to try it and 'I did my damndest!'"<sup>86</sup> In a handwritten note on the copy of this letter to Hymes, he said, "I'm disappointed but not totally devastated," although certainly all the negative reviews had to be a hugely depressing conclusion to twenty years of effort.

MAP lasted from the first planning in 1973 to publication of an edited collection in 1994, and was a major undertaking for Grimshaw, requiring participation by nearly thirty others, including Goffman. The goal was to examine the various approaches used within sociolinguistics, test them against the same data, and in this way learn which worked best. While Grimshaw (1994) outlined some encouraging results (interdisciplinary development, contributing to the theoretical foundations of sociolinguistics, demonstrating how sociolinguistics could contribute to sociology, and pedagogic potential), at the same time, Grimshaw, Feld, and Jenness summarized the problems surprisingly bluntly in their introduction to the 1994 volume:

Even as the Committee endorsed the project, however, there were adumbrations of several of the difficulties which would continue to plague the project over its almost twenty year course. Committee members and interested colleagues argued over such matters as selection of an event for analysis, modes of data collection, definitions of responsibilities of the participating analysts, funding priorities and, most particularly, identification of an optimal “set” of collaborating participant-analysts. Some of the disagreements were based on intellectual biases, some on failures in communication and some, possibly, on clashes of vested interested. Some were resolved, some persisted, and some were exacerbated – and new difficulties emerged throughout the project’s course. (10)

As a result, despite the time and effort of so many, for so long, as of 2006 Labov could conclude: “Forty years after the Social Science Research Council set up a Committee on Sociolinguistics, the amount of interaction between linguists and sociologists remains minimal. Despite important work on discourse and conversational analysis, very few sociologists have acquired the basic tools of linguistic analysis, and very few linguists have contributed to the thinking of sociologists” (99). Despite that, as Scollon’s 1995 review of the final group report of the MAP findings (Grimshaw et al. 1994a) states, “many of the most important researchers in sociolinguistics” analyzed the data for that book (428). And so, we are left asking: What happened? Why did MAP fail when so many seemingly similar projects succeeded? Scollon points to several possibilities:

(a) it is very difficult to produce a data record which researchers of divergent persuasions will consider to be a suitable body of data; (b) with the exception of Halliday, who analyzed every clause within the agreed 12-minute common segment, this group of analysts selectively attend to only portions of the data, and indeed to very different aspects of the “same” record; and (c) the work of this group of scholars is largely complementary, not competitive. (430)

Further analysis will wait for the conclusion. First, it makes sense to finish discussing Goffman’s connections with both Indiana and Texas.

### *Goffman’s Visit to Indiana*

Impressively, Goffman and Grimshaw remained friends, getting past their disagreements about MAP. It’s worth another detour to examine what it was that Goffman did during his visit to Grimshaw at Indiana University in November 1975. First, it’s important to know that Grimshaw had organized the SSRC Committee on Sociolinguistics Conference on Continuing Language Socialization, held November 14–16, 1975, at Indiana, inviting

Goffman as a participant, as well as Gillian Sankoff, Courtney Cazden, and others representing anthropology, linguistics, psycholinguistics, sociology, and English (Grimshaw 1981, 123). Since Goffman already had agreed to visit for that purpose before they conflicted on data collection for MAP, Grimshaw had arranged for him to stay longer to also visit a seminar he was then teaching on Goffman's contributions to sociology. That visit did in fact occur, despite being scheduled for only a short time after Goffman withdrew from MAP. In one of his letters about the logistics of that visit, Goffman wrote he would never think "that such business could strike deep enough to wither affection" (Oct 20, 1975, ADG). The man certainly had a way with words.

The visit to Indiana has nothing to do with MAP, and everything to do with the friendship between these two scholars. In May, Grimshaw wrote Goffman that he had wanted him to give a lecture to his own course but then had discovered that Sebeok wanted Goffman to give a guest lecture to a course Sebeok would be offering on semiotics as well, and that it would make sense if they combined efforts. Sebeok's course was Signaling Behavior in Man and Animals I, which had received funding as a Pilot Program in Semiotics in the Humanities, determining how to use semiotic theory to unify the humanities, and to some extent, also the social, behavioral, and natural sciences. The public announcement of that series states: "Lecturers will include at least ten visiting semioticians of international renown."<sup>87</sup> As the note circulated to relevant faculty includes the comment "I believe Goffman will be one of the visitors," it seems that he was counted as a semiotician, by no less an expert on the topic than Sebeok (see also comments to that effect in Sebeok 1991). Part of what is interesting is that both Sebeok and Grimshaw had met with Goffman on prior occasions to sort out the idea of a visit, Grimshaw while on a visit to Philadelphia, and Sebeok during the conference he co-organized with Goffman and held in Amsterdam. As Grimshaw wrote Goffman: "Tom's goal as outlined to you when the two of you met in Amsterdam is to have you participate in a 'pilot year' class and other activities associated with him [*sic*] projected program on semiotics," and, since that project was funded by a grant, it had multiple additional requirements beyond just a guest lecture in the course: Goffman would need to be available to people in sociology, give a public lecture, and consult on library holdings. Their joint efforts on the 1970 conference makes it clear why Sebeok thought it appropriate to invite Goffman to present lectures

within his program, not just to Grimshaw's students, and why he was insulted when that did not work out.

For his own part, Grimshaw wanted Goffman to give a guest lecture in his course Sociology of Erving Goffman. In addition, to qualify for reasonable compensation, Goffman would need to meet with students in the deviant behavior program as well. The combined proposal was therefore for a dense three days, including at least seven major activities: two class lectures, one public lecture, meeting with a library representative, meeting with Sebeok's committee, likely an evening event with "university bigwigs and maybe a couple of outsiders," as well as "some kind of partyish event where you could meet faculty and students." Goffman would stay with Grimshaw, as the Labovs usually did when they visited Indiana. Apparently, they had already discussed what Grimshaw should include in his course, and that there should be no attempt to cover all Goffman's publications, so Grimshaw proposed talking about maybe five books, including *Presentation of Self*, *Frame Analysis*, *Asylums*, and whatever else Goffman preferred. Finally, the letter makes clear that Goffman was doing a lot of traveling at this point because Grimshaw hopes it arrives before Goffman leaves for Germany, and says he looks forward to their connecting in both Santa Fe (for MAP) and San Francisco (for the American Sociological Association) (May 12, 1975, ADG).

Goffman's response was cute:

About my visit. You have set it up in a pattern that prevailed some years ago, where faculty persons spent three days flogging themselves at another university, a change in air providing a contrast to the flogging of themselves at home base. The money, untaxed, was used for a down payment on a summer cottage.

Let's say we skip the University's presentations, library consultations, and ceremonial dinner. If you continue with your foolhardy plan to teach a course on my books, I will come and visit you near the end of the term and spend a session without script answering questions from the students. The same day, if possible, I would like to present something semiotic to Tom's students. For this I would expect tourist air fare and whatever modest honorarium such a double colloquium visit would ordinarily entail. Would this be possible? (May 19, 1975, ADG)

He spent far more time discussing which of his books, and what ideas of his, the students should be exposed to in Grimshaw's course (see Smith 2022c for further analysis of that discussion). However, Sebeok was not impressed by Goffman's response when Grimshaw forwarded it to him

and told Grimshaw so: “I am sorry to say that the terms of our grant are quite clear cut in requiring certain specific obligations from all our visiting scholars. . . . Unless Erving is willing to comply with this . . . I just don’t see how the expenditure of the honorarium could be authorized” (Jun 3, 1975, ADG). Grimshaw passed that response on to Goffman, saying he didn’t think it would be onerous to comply but, if Goffman preferred, they could return to just a lecture to his own course, and a public lecture, for less money (Jun 9, 1975, ADG). That is in fact what happened, much to Sebeok’s dismay: “I am greatly disappointed that the arrangements with Erving Goffman haven’t worked out in the way we had hoped and planned; the students in semiotics are also immensely disappointed.” At the same time, he offered that he and his wife would still like to entertain Goffman at some point during the visit (and copying Goffman on the offer [Aug 29, 1975, ADG]). When Grimshaw followed up that fall, he provided logistical details, and also said “Let me know if there’s anyone else you’d like to see” (Oct 7, 1975, ADG). Goffman’s response was to say that, “partly in the interests of world harmony I wrote to Tom and volunteered to show his seminar some pictures about pictures; haven’t heard from him yet, but if he agrees I propose to take that 2 hours out of whatever else is happening. OK?” (Oct 20, 1975, ADG). The “pictures about pictures” likely means he intended to show students some of the images included in his next book, *Gender Advertisements* (1979a). Sebeok (1977) has documented the visit Goffman did in fact make to his seminar, so the semiotics students were not disappointed in their hopes to hear from the great man himself.

Goffman discussed numerous technical details about the guest lecture for Grimshaw’s students ahead of time, such as that they could ask anything they wanted of him and then provided a lengthy response about reviews of his books, which Grimshaw had requested he send.

By and large I see a biased selection of reviews; some of the favorable ones are sent me by their writers, and often I only see the other ones when publishers send them in. The real issue of course is that favorable ones can easily be as witless as unfavorable ones—perhaps easier. A good review I think should be a piece of literary analysis, touching on themes that no one theretofore had noticed. Only the British seem to be able to do this with social science, perhaps because their journalists have a deeper education than ours, in the main. (Oct 20, 1975, ADG)

In the same folder, there is an undated list of forty-seven questions from Grimshaw’s students for Goffman to review and hopefully answer during his visit. These include everything from “How do individuals learn the behaviors

you describe in your books?” to “Have you devised any way of testing any of your theories?” and “How much of civil inattention is determined by an individual’s socialization and sex?”<sup>88</sup> There is also a list of thirty-eight reviews of Goffman’s books, and three general essays on his ideas, all of which were made available for students to read ahead of the class meeting.<sup>89</sup> And, finally, a copy of the course evaluation, mostly about Grimshaw’s teaching, a little on the course content (“May be more interesting if the sociologist that was studied had a more diverse field of research. Everyday behavior by Goffman got kind of old after a few weeks”), and a place for students to add comments on Goffman’s visit (“Goffman’s visit was helpful. At first I found Goffman very hard to follow, but the longer I listened, the more interested and involved I became in what he was saying. It really began to pull together what I had read.”).<sup>90</sup> As Grimshaw wrote to Sankoff in 1983, “He had a bad cold, but he sat and talked with the students for several hours—they thought he was great. I have fond memories of his visit” (Oct 21, 1983, ADG).

Overall, it is simply astonishing that such a short time after Goffman left MAP, taking Sacks and Schegloff with him and causing complications for Grimshaw and everyone else involved with that project from that point forward, he and Grimshaw were able to put history behind them and first organize all the details, and then actually manage a visit lasting several days, especially given that Goffman stayed in Grimshaw’s home. Perhaps people were simply more polite and considerate in the 1970s, but it seems unlikely that such behavior could be expected today of colleagues who had quarreled professionally to the point of leaving a joint project. It demonstrates an impressive ability to separate the personal from the professional, on the part of both Goffman and Grimshaw. They were personal friends who professionally disagreed.

### *Grimshaw and Goffman after MAP*

As further evidence that Goffman and Grimshaw remained on friendly terms long after their MAP disagreement, in 1981 Grimshaw reported to Hymes that Goffman had invited him “to organize two ‘featured’ sessions on sociolinguistics for his ASA meetings in San Francisco. Featured sessions differ from ‘regular’ ones in that they include only invited papers, that those papers can be somewhat longer, that they in some sense are ‘showcased’ . . . I am delighted that Erving is taking this opportunity to legitimize studies of language in social use.”<sup>91</sup> In response, Grimshaw invited Susan Ervin-Tripp, John Gumperz, Susan Philips, Stanley Lieberman, Bud [Hugh] Mehan, and

Deborah Tannen to be panelists. Then he went on to explain why he had not invited Hymes earlier:

Erv has taken the position that we really couldn't ask you [Hymes] to participate unless we could "offer you" a plenary sessions [*sic*—something that's not possible within the organizational framework of the ASA meetings. He now tells me that he has approached you informally and that you have said that you would be happy to participate as a discussant. I would like to invite your participation in whichever of several possible capacities seem to you to be most appealing.<sup>92</sup>

These two sessions were in fact held at ASA in 1982. Grimshaw was listed as organizer and presider for both. The first, "Language as a Social Problem," included Tannen and Cynthia Wallat (National Institute of Education), along with Philips, Gumperz, and Mehan as presenters, and Corsaro and a new name, Albert K. Cohen (University of Connecticut), as discussants. The second, "Levels of Analysis in Sociolinguistics," included Ervin-Tripp, Lieberman, and Sherzer as presenters, with Sankoff and Guy E. Swanson (Berkeley) as discussants.<sup>93</sup> Of course, this is the ASA convention that Goffman could not attend at what turned out to be the end of his life. This matters because it demonstrates Goffman's ability to put friendship ahead of disagreement about research methods, and his continuing commitment to the goals that he and Grimshaw had shared until he left the MAP project in 1975.

If evidence of Grimshaw's final feelings about Goffman after MAP is needed, it is easy to document that he chaired the "Hughes-Goffman Memorial Session" at ASA in fall 1983. That they had remained friends despite the substantive disagreement about how to proceed with MAP is manifestly evident in his comments, in addition to the actions of Goffman just mentioned. Specifically, he wanted "to share some experiences—a few—that may help give all of you glimpses of the way he went about the business of being a close friend. Erving did not have an agenda for his friendships. In my case, I think he may have taken me on charitably, a sociology hardship case."<sup>94</sup> Other participants in that panel were Hymes, Howard Becker, and John Lofland.<sup>95</sup> The published ASA schedule shows that the organizer was Robert Habenstein, with Becker, Grimshaw, Arlene Daniels, as well as Charles Edgley and Hans O. Mauksch as participants. Hymes has written up the story of the event: Becker and someone else talking about Hughes (presumably either Mauksch or Edgley) presented their papers, then Lofland began, but collapsed partway through, effectively ending the session.<sup>96</sup> Grimshaw wrote up his memories, describing Goffman's importance, for the readers of *Language in Society* (1983); Hymes did much the same, focusing on Goff-



man's reviews of submissions to that journal, but published his analysis in *Theory and Society* (1984). Lofland wrote up his comments as well, for a third journal, *Urban Life* (1984). In his oral comments, Hymes pointed out the connections between the two men being honored, saying of Hughes that he served on the SSRC Committee on Sociolinguistics, and so, "I knew him as a father, or better perhaps, an uncle of sociolinguistics."<sup>97</sup> Also in fall 1983, Grimshaw taught his course on Goffman again,<sup>98</sup> and continued teaching it at least as late as 1987, when he invited Kendon to present a guest lecture. The response: "I would certainly like to talk to your Goffman class. It is my hope that I shall be able to work up a piece on 'closings'—which would have a suitably Goffmanian flavor to it."<sup>99</sup>

### *University of Texas, Austin*

Indiana University was not the only campus that can be characterized as "adjacent" to Penn, in the sense of having overlapping people and projects. The other major example is the University of Texas, Austin, and so several stories relating to that university will be explored as well. The three most relevant scholars there were all Penn alumni: Sherzer, Bauman, and Abrahams. Joel Sherzer was a student and then colleague of Goffman and Hymes, attended a class by Labov before Labov even arrived at Penn, and was part of Goffman's conference in Amsterdam. Richard Bauman had a more peripheral connection, given that he was primarily based in American civilization rather than one of the departments where Hymes or Goffman was most active, but he did earn a master's in anthropology before Hymes left, so was also his student, and has been significantly affiliated with the ethnography of communication research tradition ever since. Also, he served as the outside reviewer for Goffman's book *Forms of Talk* (1981). Roger Abrahams started and ended at Penn, knew Szwed particularly well, and was based at Texas with Sherzer and Bauman for some years between. So, it makes perfect sense that they were the next generation of scholars expanding on what they had learned at Penn and sharing it with a larger audience. They had a head start given the connections made at Penn, especially in terms of the role Hymes played in the SSRC Committee on Sociolinguistics, which funded several activities beyond MAP either held in Texas or organized by Texas faculty members.

## *Working Papers in Sociolinguistics*

As Sherzer once explained, “Working papers are a good way to spread stuff around” in order to ensure that potentially interested others could easily learn what people were working on.<sup>100</sup> He initially proposed that Abrahams edit a series of *Working Papers*, but Abrahams declined; and so then Sherzer suggested he, Hymes, someone else from Texas, and someone else from Penn become co-editors.<sup>101</sup> For the first several issues, starting in 1970, the title remained *Penn-Texas Working Papers in Sociolinguistics*.<sup>102</sup> However, with no one at Penn doing editorial work, the title was soon shortened to *Working Papers in Sociolinguistics*. Sherzer and Bauman served as co-editors, and there were many Penn connections among the contributors. The series was officially published by the Southwest Educational Development Laboratory (SEDL), based in Austin, Texas, where they both held affiliations.<sup>103</sup> (“SEDL was established as a regional educational laboratory in 1966, and its work focused primarily on states in the Southwest. . . . SEDL’s mission was to strengthen the connections among research, policy, and practice in order to improve outcomes for all learners.”<sup>104</sup>) At different times and in different amounts, support was provided by SEDL,<sup>105</sup> by the SSRC Committee on Sociolinguistics (indirectly from NSF),<sup>106</sup> and by NIE.<sup>107</sup>

Sherzer went to the SSRC Committee on Sociolinguistics in 1973 with the concern that he and Bauman were spending so much mailing out copies that they were running out of funds and would have to cease publication. The committee agreed to include the series in the grant proposal they were then preparing for NSF on one condition: “that the series accommodate not only pre-publication versions of papers eventually to be published but also materials that would not be published, for a variety of reasons, but that deserved wide dissemination.”<sup>108</sup> Sherzer agreed; the series was included in the grant proposal, funding was granted, and badly needed support was delivered.<sup>109</sup> The committee was happy to contribute because they highly valued turning what members had learned through their research into publications of some sort (“Mr Jenness stressed the importance of having a conference report, or some other means of disseminating what had been learned”<sup>110</sup>). The committee did support research but always asked to have the results presented through a conference and then publication once a project was completed. This could sometimes take the form of just a short notice in the SSRC’s own newsletter, *Items* (e.g., Ervin-Tripp 1969; Ferguson 1963, 1964, 1965; Grimshaw 1969b; Hymes 1972b), although they really preferred books (e.g., Hymes 1971; Shuy 1973a).

In 1980, Goffman objected to publishing a submission to *Language in Society* because the author had “already Texas-paperyed the heart of it,”<sup>111</sup> a wonderful description if you understand it. By this he meant that all the important points had been previously published in the *Working Papers in Sociolinguistics* and so should not appear again in *LiS*. This comment makes it clear that Goffman was not only familiar with, but regularly reading the *Working Papers*, and knew Hymes (as the editor of *LiS*, and the one to whom he submitted his review) could be counted on to be doing the same. It is especially interesting that Goffman apparently viewed it as inappropriate to accept something from the *Working Papers* for *LiS*. As someone who was encouraged by Hymes to publish in the *Working Papers* at that time (Leeds-Hurwitz 1980), my understanding was that it would be a good way to let people know what I had to share, but not the final resting place for a publication. Part of the issue may have been that, also in 1980, Sherzer reported, “success has been causing us to become less and less ‘Working Paper’ and more and more ‘journal’; we have tried to fight this trend. Keeping them free of charge has been a problem, since over 700 people now get them and we’re on a fixed grant from NIE.”<sup>112</sup> Most publications would love to have such problems. The series continued until 1982—at least the last issue I’ve seen is Douad (1982)—so it was an enormously successful series, demonstrating that Sherzer and Bauman had learned not just content but strategy from their professors at Penn.

## Ethnography of Speaking Conference

In 1970, Hymes, Sherzer, and Abrahams discussed the possibilities of a conference to be held at the University of Texas, Austin, because “it is literally the place where Penn and Berkeley (the two major U.S. centers of sociolinguistics) meet” (that is, students who had earned their degrees either at Penn or Berkeley then took positions at Texas), and because “a movement towards interdisciplinary work seems underway.” Sherzer went on to emphasize the role of folklore: “In a sense folklore would be the center or focus of such a conference because it is the place where linguistic and anthropological concerns meet when one takes an ethnographic and interactional point of view,”<sup>113</sup> a view very much in line with how folklore operated at Penn. This conference ended up being organized by Bauman and Sherzer in Austin, April 20–23, 1972 (Bauman and Sherzer 1975, 103), and was co-sponsored by the SSRC Committee and the University of Texas, Austin.<sup>114</sup>

*Table 6.3: Ethnography of Speaking Conference,  
April 21–23, 1972, Austin*

<i>Primary organizers:</i> <sup>115</sup> Richard Bauman, Joel Sherzer, Dell Hymes
<i>Committee on Sociolinguistics representatives:</i> Charles Ferguson, Allen Grimshaw, Dell Hymes, Joel Sherzer
<i>Penn participants:</i> Dan Ben-Amos, Erving Goffman, John Szwed
<i>Penn alumni participants:</i> Roger Abrahams, Richard Bauman, Regna Darnell, Michael Foster, Judith Irvine, Elinor Keenan, Susan Philips, Anne Salmond, Peter Seitel, Sheila Seitel, Joel Sherzer
<i>Texas participants:</i> Roger Abrahams, Richard Bauman, Benjamin Blount, David DeCamp, Nicholas Hopkins, Barbara Kirshenblatt-Gimblett, <sup>116</sup> Edgar Polomé, David Roth, Mary Sanches, Harry Selby, Joel Sherzer, Brian Stross, Rudolph Troike
<i>Not-yet-at-Penn participants:</i> Roger Abrahams, Barbara Kirshenblatt-Gimblett, Gillian Sankoff
<i>Berkeley participant:</i> Dale Fitzgerald
<i>Berkeley alumni participants:</i> Benjamin Blount, Michael Foster, Harvey Sacks
<i>Beyond Penn, Texas, Berkeley participants:</i> Keith Basso, Victoria R. Bricker, James J. Fox, Gary Gossen, Karl Reisman
<i>Results: Explorations in the Ethnography of Speaking</i> (Bauman and Sherzer 1974)

The funding for the event came from SSRC's Committee on Sociolinguistics, through their NSF grant. Bauman and Sherzer did the work of organizing, with a lot of advice from Hymes, to the point that, when the draft invitation letter was sent to him, signed by Bauman and identifying Sherzer as co-organizer, Hymes objected: "As to the letter: what happened to Dell Hymes? I thought he was involved in planning the conference?" And later, as a postscript: "In San Diego in November Joel was worried about the conference being squeezed or taken over by Goffman and Sebeok. Little did I expect to get squeezed out, when I reassured him then."<sup>117</sup> Presumably, this was understood to indicate actual annoyance rather than teasing because Bauman sent an immediate apology for the "crossed signals" and an explanation that the intent "was to do you honor, not insult you."<sup>118</sup> This event was viewed by all concerned as a follow-up to the Amsterdam conference organized by

Goffman and Sebeok (as in Goffman's letter to Sebeok [Oct 23, 1970, TS]), which explains why Hymes suggested Goffman and Sebeok might try to take over the event in the quote above.

Labov, Gumperz, Ferguson, and Grimshaw were all invited as representatives of the Committee on Sociolinguistics, but there was no funding available for them as observers rather than participants, and in the end neither Labov nor Gumperz participated. As at other conferences, Goffman served as a respondent, in this case to the panel "Verbal Genres in Social Interaction." As such, he does not appear in the resulting volume. A month after the event was over, Goffman and Szwed were apparently still having difficulty getting reimbursed for expenses, and wrote to Sherzer, who asked Hymes to please sort it out.<sup>119</sup>

This conference resulted in a book, *Explorations in the Ethnography of Speaking*, edited by Bauman and Sherzer (1974) and published by Cambridge University Press through an introduction by Hymes to Elizabeth Case, the editor there who oversaw publication of *Language in Society*.<sup>120</sup> In addition to chapters by Bauman and Sherzer, the book includes multiple scholars previously mentioned in these pages (Hymes, Grimshaw, Sankoff, Philips, Abrahams, Sherzer, Kirshenblatt-Gimblett, Darnell, and Sacks<sup>121</sup>), as well as others who were by then part of the larger network related to the ethnography of speaking (especially Gumperz's students from Berkeley), though not relevant to other projects presented in these pages. Comparing the two years between conference and publication to the fourteen-year gap between the summary conference for MAP in 1982 and the final publication in 1994 highlights just how slow all the parts of MAP were, even when the data collection and analysis are not considered.

## Comparative Ethnographic Analysis of Patterns of Speech in the United States Conference

Sherzer was the primary organizer for another conference, despite that one being held in Philadelphia. In 1972, after the successful conclusion of the ethnography of speaking conference, he wrote David Jenness at SSRC about what he initially described as "a working conference on discourse." Designed to "focus on the study of discourse in social life," he wanted to hold it in Philadelphia to ensure participation by Labov, Goffman, and Hymes.<sup>122</sup> The major organizing occurred during a meeting on May 18, 1974, with Sherzer, Hymes, Szwed, Peggy Sanday, and Roger Shuy (in linguistics at Georgetown University, also a member of the SSRC Committee). The date was set for

January 17–19, 1975, and Sugarloaf Conference Center at Temple University was chosen as the location. A title had been agreed upon—Comparative Ethnographic Analysis of Patterns of Speech in the United States—and a tentative list of participants developed.<sup>123</sup>

*Table 6.4: Comparative Ethnographic Analysis Conference,  
January 17–19, 1975, Temple University*

<i>Primary organizers:</i> Joel Sherzer, Dell Hymes
<i>Committee on Sociolinguistics representatives:</i> Allen Grimshaw, Dell Hymes, David Jenness, William Labov, Joel Sherzer, Roger Shuy
<i>Penn participants:</i> Renée Fox, Erving Goffman, Dell Hymes, William Labov, Peggy Sanday, John Szwed
<i>Penn alumni participants:</i> Susan Philips, Joel Sherzer
<i>Beyond Penn participants:</i> Aaron Cicourel (sociology, University of California, San Diego), Lucia Elias-Olivares (anthropology, University of Texas, Austin), Peg Griffin (linguistics, Georgetown University), Eduardo Hernandez (linguistics, Stanford), Thomas Kochman (communication, University of Illinois), Ray McDermott (education, Rockefeller University)
<i>Rapporteur:</i> Virginia Hymes
<i>Observers:</i> Lee Ann Draud, Susan Thomas, Nessa Wolfson
<i>Results:</i> Link to Language and Interaction Institute, but no publication

Shuy offered to have the LSA's Committee on Linguistics and the National Interest co-sponsor the event, while admitting they had no budget and could not contribute financially.<sup>124</sup> However, SSRC rejected that, as they had a policy not to co-sponsor activities.<sup>125</sup> After the committee had approved the working conference, Hymes followed up in a letter to Jenness, suggesting they needed to consider what would follow: "I wonder if it should not itself be a step toward a larger activity, perhaps longer conference? From which a useful publication might come? . . . My strongest feeling is the need to get together people who do care about the problem, and want to do something to develop research."<sup>126</sup> Given that he was both based in Philadelphia and on the Committee on Sociolinguistics, Hymes took over organizing much of the event, with Jenness handling logistics. As a working conference, there

was no expectation of formal presentations (thus no publication resulted); instead, participants were asked to circulate some of their own prior publications and think about questions of “common problems and interests” and “beginning to block out more clearly and effectively the dimensions of the subject.” Hymes at one point summarized the subject as being “What does [*sic*] all our interests in sociolinguistics have in common.”<sup>127</sup> Notice that this is exactly the opposite of the MAP project, which could be said to have asked analysts to each do something quite different and then come together to evaluate whose approach worked the best. As it was far more successful, there is obviously something to be said for taking this sort of positive approach, encouraging collaboration rather than competition. The way in which the topics were organized is particularly interesting:

We believe that all of us share in some respect a sense of there being a range of barely broached problems with regard to the place of language in social life in this country. The source of that sense may be attention to phenomena [supportive and remedial exchanges (Goffman), ritual insults and regional narrative styles (Labov), doctor-patient conversations (Cicourel, Shuy), teacher-student and student-student interactions (McDermott, Philips), etc.]. In the most general terms, the subject could be said to be a serious sociology of language, or ethnography of speaking, of the country.<sup>128</sup>

This letter was sent to those who presented. While not included in this list, Grimshaw was also invited, and also attended.<sup>129</sup> Labov wrote that he was unable to attend,<sup>130</sup> which resulted in a cranky letter from Hymes, pointing out that they had previously discussed the event and the date and Labov had committed to attending.<sup>131</sup> In the end, he did participate, as well as Virginia Hymes (in the role of rapporteur), and Lee Ann Draud, Susan Thomas, and Nessa Wolfson (other Penn affiliates, listed as observers).<sup>132</sup> After it was over, Fox sent Hymes a list of “persons doing valuable work” for the group to know about, which included Elijah Anderson, then being recruited for sociology (and who did in fact take the offer).<sup>133</sup> There was a tentative link between this conference and the Language and Interaction Institute; Hymes wrote Sherzer at one point: “In any case, hope to use January to stimulate more, lay plans for something further. Our Provost is interested in helping to start some sort of institute here; we’ll see what happens from an initial meeting.”<sup>134</sup>

After the conference was over, Dell Hymes sent Jenness a lengthy report of what had happened, prepared by Virginia Hymes in her role as rapporteur, consisting of thirty-four pages detailing who said what.<sup>135</sup> It includes a num-

ber of points made by Goffman, which are highlighted below. In response to Shuy describing interaction between patient and doctor,

Goffman raises the question of the need to generalize. What he sees is a demand function acting on the interviewee in the interview of this sort which is between a specialist and a lay user of his services. He gives the example of an ordinary customer coming into a hardware store. As a lay person he uses terms for things which a carpenter would not, and the merchant can act as if he does not recognize the term the lay person uses, or he can accept it and then use a question to move to a more technical term, thereby putting the lay person down or holding the distance of roles between specialist and lay person. [This general outline can be seen to apply to a wide range of situations in our society where a “specialist” holds the non-specialist at arm’s length by his use of technical language.] (3)

Others join in the conversation: Fox, Shuy, Philips, Hymes. Then Cicourel was asked to talk about his work in medical settings. Goffman questioned him when he finished: “Goffman asks if there is any reason to believe that the kinds of processes physicians go through in making summaries differ from for e.g. what the person taking notes at the conference would do in giving a summary of what happened this morning” (7). Cute! Cicourel said he thought they would be the same, but the notetaker (V. Hymes) disagreed and said so. Further comments from Cicourel, Fox, Sanday, Hymes, Labov, and Grimshaw followed, then a break for lunch. In the afternoon McDermott was asked to talk about educational contexts, with Shuy, Hymes, Philips, and Labov all asking questions. Then Goffman again:

Goffman suggests that the weakness of McDermott’s paper is that all it can do is give an example of the kind of thing you might find if you do this kind of work. In the paper it is not being said that the status the kids come in with is merely validated, reinforced there. A place consists of all these “keepings” (in place); the same kids may be kept in place in the same ways in all kinds of sectors of the society. The paper is saying that in the classroom the places are being re-created in each interaction. But is it really doing more than put meat on the bones of what class discrimination is? The old version that there is a social structure may still have some validity. To support the more radical claim the paper must go further. If in fact what happens in fine detail to people of one class is repeated in a wide variety of situations then you have rich documentation for the existence of a class. It would be very dramatic that through complex means the same old lines are drawn. (14; emphasis in original)

Labov and Cicourel chime in, and then “Goffman suggests that on the basis of what happened to the Chinese children in San Francisco classrooms (they achieve) you could argue that the classroom has its own dynamic” (15). Then



come remarks from Sanday, McDermott, Hymes, and Fox. Then McDermott shows the group a film. Philips, Griffin, and Hernandez comment. Then it was Labov's turn to present on narratives, with Kochman and Grimshaw responding, and then "Goffman raised the [ethnographic] question of who you can tell a story in front of if he has heard it before. Husbands do it in front of wives, but who else can you do it with?" (18). Sherzer, Labov, Grimshaw, Sanday, Hymes, Fox, and Kochman all chimed in. "Goffman also raised the possibility that working class people are faced with the fact that they are in a world where their words count for nothing; they create in their stories a world in which their words *do* count" (20; emphasis in original). Next to speak are Griffin, Sanday, and Fox, followed by a brief comment from Goffman—"that he didn't see 'structure' in the stories Labov had told" (20). Labov and Sanday discussed this for a bit; then Grimshaw, Fox, Sherzer, and a break for dinner.

In the evening session, Sherzer started the discussion of Chicano socio-linguistics. Hernandez took over, then Oliveras, with Goffman then "ask[ing] how you would distinguish between a situation in which a person had to switch [linguistic codes] and one in which he had the option of switching" (23). Sherzer offered to play an audioteape as an example, but Labov and Grimshaw had further comments, and then Goffman "suggest[ed] that a distinction can be made between licensed instances of code-switching and code-switching situations" (23). Sherzer played the tape; Grimshaw, Labov, and Hernandez talked about it: "Sherzer remarked to Goffman that he (Goffman) makes constant switches in style and that though we can't predict any of them we can *interpret* each of them" (24; emphasis in original). Labov argued that point, then Philips asked to hear Olivares talk about her work with code-switching in children. Labov, Griffin, and Hernandez commented; Philips returned the floor to Oliveras. Hernandez, Philips, Shuy, Cicourel, Labov, and Grimshaw all commented or asked questions.

The next morning the group reconvened with Hymes asking that they try to identify people doing relevant work in this area and specifically asking Grimshaw to talk about a conference he was organizing on language acquisition. Kochman, Philips, Shuy, Fox, and V. Hymes asked questions or made comments, then "Goffman warned against calling lexical accretion socialization" and "Kochman mentioned the problem of how children learn to handle power relationships." Goffman "warned of the need to work in all these areas in ways that are generalizable" (27). Hymes asked Szwed to talk about Black language use, and to compare that to what had been discussed

about Chicanos. Szwed gave a summary, with Sanday, Griffin, Hymes, Philips, and Cicourel responding. At that point, Hymes asked the group to consider “the relationship of the training of linguists and social scientists to the ability of the field to contribute to problems of a linguistic nature in society” (29). In response, Griffin asked how the Warm Spring Sahaptin used V. Hymes and Philips as linguistic experts. Kochman had “a modest proposal” that the group should focus on the purpose of ethnography (30).

In response to Kochman, Goffman asked whether it was in fact the case that there was a consensus among linguists concerned with social policy that there is an answer to problems of bilingualism. Hymes answered that this is not the case; that what we are talking about is that decisions should not be made in ignorance of the *facts* of bilingualism, as they are now being made. Goffman stated that he didn’t see how a local community could make these decisions. Local communities don’t know what is best for them and we don’t know what we’re doing. (30–1; emphasis in original)

McDermott, Cicourel, and Philips all discussed, then Goffman argued “that we don’t know who the community is—we don’t have access to the community, only to representatives of it” (31). As program planner for AAA’s next convention, Sanday offered to accept papers on these themes. McDermott, Grimshaw, and Hymes made further comments, and Goffman “returning to the argument about what the community wants, asked if bilingualism as an issue hadn’t actually arisen out of the social sciences and not out of the needs of the communities” (32). Hernandez argued with this, Fox provided some general types of studies, and Szwed supported Goffman. Then Goffman “agreed that the functions of literacy should be studied. There is a stigma on its absence because if totally illiterate you can be caught out [as you can’t so easily for not being able to read critically]. We mustn’t confuse the *need* for literacy with the stigma attached to not having it” (33; emphasis in original). Hymes, Cicourel, Hernandez, Griffin, Grimshaw, Sherzer, and Fox all contributed to the discussion, then Shuy pointed out that they had done more to describe needed work than who was doing that work. And then the conference adjourned.

After the event, Hymes wrote to Jenness (who had participated, representing SSRC), that he was particularly pleased about the way “interaction among participants was good throughout, reinforcing pre-existing links and creating new ones,” using further conversations with Sanday, Fox, and Hernandez as examples. In addition, he felt “the conference confirmed our pessimistic appraisal [when they had difficulty in thinking of more people

to invite]. There is very little being done.” Rather than hold a larger event as originally anticipated, he had discovered that they needed to do more to “change the character of the research that is going on, and the training (and goal orientation) that shape it. Back where we started on the committee ten years ago in a sense: how to get linguistic and social science (I stress here, ethnographic) modes of work together??”<sup>136</sup>

What do the comments reported by Goffman demonstrate about his contributions? He frequently took a step back, trying to move up a level of analysis or generalization; he either made longer comments than others or perhaps was just more memorable. He participated less in the quick back and forth reported for some others. Overall, he was clearly involved, and helping others think through their ideas. Unfortunately, this event did not lead to any publication, although it would be brought up for the next few years in Hymes’s letters, mostly in connection with the fact that they had discovered very few people beyond group members who were doing the sort of work they all considered necessary to adequately study the US, so further training was obviously required.

## *Conclusion*

This chapter has covered two major activities (and a visit) at Indiana University linking Goffman to Sebeok and Grimshaw, as well as two conferences and a working papers series organized through the University of Texas, Austin, all with Sherzer as one of the primary organizers; in all cases, others at Penn also played significant roles. The conference Goffman co-organized with Sebeok was a success, with several events occurring afterwards in which Goffman participated; the research project Grimshaw organized was not. It is worth considering in some detail what happened with MAP, since it was the one notable failure despite considerable effort by a large group over several decades.

### *How to Fail at Interdisciplinarity*

Not all projects succeed, whether begun by people within one discipline or across several. No one starts a new project expecting to fail, and failure is not typically a pleasant experience, but failure, especially if time is spent learning why a specific project failed, is heuristic: It will teach us more than just looking at successful projects. While many of the small projects were never implemented, which can be characterized as another sort of failure, MAP

is the largest project to be started and then to actively fail, so that serves as an appropriate case study. As with most other activities introduced in these pages, MAP was not only multi- but interdisciplinary: The entire goal was to help move sociolinguistics along in its development, creating something new that neither linguistics nor sociology could adequately investigate alone. So, the question must be asked: Why did MAP fail when so many of these other (especially the Penn-based) interdisciplinary projects succeeded? Presumably not because it was based at Indiana instead of Penn; that seems unlikely and would be contradicted by the success of the Interaction Ethology conference, also managed by someone based at Indiana (in this case, Sebeok).

Grimshaw clearly saw several dangers typical of interdisciplinary work and tried hard to avoid them. He clearly laid them out for others. The first was “that I will be seen as arrogant in a claim of competence I do not have” (since he was a sociologist, and sociolinguistics required equal knowledge of linguistics), which he resolved by admitting what he did not know. The second was “fundamentally misconstruing work in disciplines from which I borrow,” which he resolved by consulting experts (especially Hymes and Labov, but also Goffman). The third was “that my treatments of linguistics and sociology, respectively will be too elementary for those familiar with a topic or concept discussed—and too obscure and incomplete for nonfamiliar,” which he resolved by walking a fine line (1989, xv). Knowing these dangers, he did his best to resolve them. So, we must look elsewhere for the answer. One obvious difficulty is that MAP was a collaborative research project, when so many of the other efforts examined in these pages were more about sharing information at conferences or describing results in various publications, rather than conducting joint research, which can be far more difficult to manage successfully than it appears. We can return to the comment by Jenness as early as 1975 to MAP participants that “collaborative projects are generally at risk in this respect, and this project has been no exception” (Oct 20, 1975, ADG), but we need more specifics. Multiple potential answers exist, each of which will be briefly considered.

The first possibility is a failure of leadership. MAP resulted in the publication of two—much delayed—volumes (Grimshaw 1989; Grimshaw et al. 1994a), but failed in terms of making a difference to anyone beyond the immediate authors. Perhaps the question to ask is why so many of the other projects in these pages succeeded when this one did not. Murray (1994), who has expanded upon Mullins (1973) and studied just this question across far more topics and decades, attributes most of success or failure to intellectual

and/or organizational leadership. As he has demonstrated in detail, both intellectual and organizational leadership are required for any group, but they are especially significant in an interdisciplinary effort. Grimshaw was likely not the strongest leader in either capacity, yet he ended up attempting both. His skill lay in synthesizing large amounts of material and summarizing what he had learned (e.g., Grimshaw 1973a, where he explains sociolinguistics to that point, or any of his review essays, such as 1973c, 1974a), rather than in organizing a diverse group of individuals into a cohesive team ready to work on common materials. There is evidence for this directly from him: Comments in his letters convey his nervousness about the project, such as when he told Goffman, “Feld and Williams are in town now and will be filming tonight and during the week. I am a nervous wreck, I’ve never been involved in a project whose success or failure is absolutely out of my hands—there’s nothing I can do (except, as Williams remarked last night, pray)” (Jun 9, 1975, ADG). Perhaps the most surprising fact is not that MAP ultimately failed, but that it kept going for twenty-one years. Even if he was not the strongest leader, Grimshaw certainly showed dedication and stamina.

A second possibility was changing membership, matched to few meetings where all participants came together to discuss their progress and compare results. MAP involved a particularly large and unwieldy group, which moreover changed membership constantly, as documented in table 6.1, none of which made this an especially easy group to manage. Participants did not live near one another, but were spread across the country, and many left the country for individual research investigations at one or more points. One result was that the entire group almost never met in person, which certainly did not help matters. In fact, they met only three times: once to have an early conversation in Texas, before things got started, to sort out some basics; once in New Mexico, to view the film and agree upon the process; and once at Penn to discuss what they had learned from individual (or team) analyses. But there were no sessions having the goal of sorting out exactly what interaction should be filmed (the largest problem they faced) or for sharing analyses in process (the Penn meeting in 1982 being rather about sharing results of separately completed analyses), and either of those might have helped matters at least a little. These lacks may well have been the cause of the changing membership, for if people do not feel comfortable with a project, they are less likely to prioritize it—especially over their own individual commitments. And everyone in this group was an active scholar, having many other projects demanding attention.

A third possible issue was the question of choosing a goal for a project and then sticking with it until the end. MAP's goals changed several times, rarely a recipe for success. At one point, the goal of the project was to meet in person to compare the results of initial analyses of the same data using different approaches to determine which ones worked best, a goal which was never properly met. Later, the goal was to hold a conference to present findings to a broad audience, which more or less did happen, but with a smaller audience than initially anticipated (not part of a national conference, for example), with only some of the original participants presenting only limited results, and with no effort devoted to comparing what results they had obtained in order to discover which worked "best"—the issue which initially formed the entire objective. (Clearly they needed someone in the role of "carper" that Goffman typically enjoyed, although that was never his intended role in this group.) At a different stage, the goal was to write up what they had learned for a broad audience, which they did, but those results were published a distant fourteen years after the conference at which they were presented, and again, comparison of different approaches completely disappeared, never making it into the final product. It is possible that three different goals might all be accomplished in a single project, but moving the goal posts makes success less likely.

A fourth possible problem was that funding was always in flux. Remember Fishman's comment quoted earlier on the importance of having sufficient available funding. It turned out he was absolutely correct. The Center for Urban Ethnography, to name only one of the major project examples, had significant funding, and so was able to achieve its goals (especially training a new cohort in a new research topic), and so we count it as successful. MAP never had adequate funding. There was some support (especially for Grimshaw's efforts to get things started, with a small but quick grant from the Committee on Sociolinguistics), and they did receive the first portion of a major grant from NSF, but when they requested an early disbursement to film before the semester ended, they unwittingly set themselves up to forfeit the remainder of that support. While the committee gave them additional funds based on the Grant Foundation support, what they received was never enough, and never predictable; they had money to cover the costs of filming, distributing equipment for analysis, and some travel, but that is all. What made matters worse was the tantalizing fact that more money was always potentially forthcoming, which is part of the explanation for the lengthy delays in completing the analyses; since analysts were never paid for

their time, this project was always the last to be completed, while individual projects on other topics took priority (especially those that were funded)—a choice which must be acknowledged as perfectly reasonable. This was true even for Grimshaw, who published several other books while waiting for MAP to reach conclusion. The vast majority of the project was completed in hopes of obtaining major grants that never arrived. It is no wonder that goals and participants were constantly in flux.

A fifth possible problem had to do with self-imposed constraints, including an early agreement not to publish until the project was completed. While that made sense when everyone thought it would be a matter of a few years, it made no sense at all when the final book was not to be published until twenty-one years after initial commitments. That is at least part of the explanation for the fact that, as Scollon points out, “Many readers may be surprised to see that some of these analysts ever had contact with others in this project, since their citations rarely make mention of the MAP or of the other participants” (1995, 432).

A sixth possibility has to do with disagreements, and this was probably the most important issue. Goffman withdrew from the project for a reason, and perhaps the failure of MAP to have much impact can be attributed to him being absolutely correct in arguing that the original data set chosen for analysis was not well thought out. One of the few people to have an opportunity to analyze video with Goffman was Charles Goodwin, who tells us: “And he was also wonderful at working in video: we would show video, or he would bring video and it was incredible the way he would look at that” (Goodwin and Salomon 2019, 9). This suggests that Goffman, in fact, was being reasonable to reject the specific video footage made available to the group because he knew something about what would be required for competent analysis. Of course, it was not just Goffman who withdrew before the final analyses were published: Sacks, Schegloff, Kendon, and Erickson did as well; that is, all those who would have wanted to analyze a different type of data in a different way from what was chosen, filmed, and made available for examination. Kendon is particularly important here, as the person involved with the group for a substantial length of time who also had extensive experience analyzing filmed interaction (and focusing on nonverbal behavior); when Grimshaw went back to the comments participants had made early on, he realized that Kendon had argued for a type of recorded visual interaction quite different from what ended up being filmed. The result of these departures from the project meant that no one took on

the role of expert in microanalysis of interaction (the role that Birdwhistell should reasonably have been expected to have filled but did not, for reasons never made explicit—perhaps just because he was neither a linguist nor a sociologist and those were the initial groups tapped for the project, perhaps for his reputation by that time of being difficult, or perhaps because he had not succeeded in developing kinesics into a long-term project). Remember that some of the reviews of the second grant proposal to NSF basically argued that since Goffman had left the project, it was obviously unworthy of support.

A seventh possibility is the question of whether members, and especially the leader, have the relevant knowledge to succeed in their efforts. Grimshaw had never filmed interaction nor tried to analyze filmed interaction before this project. That was why he asked Worth who to use as filmmaker and followed his advice faithfully. But there is a substantial difference between what a filmmaker knows how to do technically, and what a more practiced analyst might have known to ask him to do. That gap in knowledge is likely part of what led to the failure of this project. It means that not only did Grimshaw not know how to manage a large, unwieldy group of researchers having little in common in terms of assumptions or methods, but that he did not himself know enough about what they were attempting to set up a potentially successful context. Both the data and the methods to be used were new to him, as well as many of the group participants, which may have been at least one new element too many.

An eighth possibility has to do with the perception of competition between group members (as opposed to cooperation). Hymes may have been right when he told Grimshaw that “it was ambitiously admirable to try to pull together all the ‘stars’” (Aug 8, 1975, ADG). Presumably, a project having one star with followers has an easier path to success, and perhaps no one could have managed this project well. But in this case, the various academic stars had quite different assumptions about what needed to happen and how to proceed, and so there was disagreement. That Grimshaw was not the star who shone brightest in the group may have also been a contributing factor. He was always modest, perhaps to a fault, about what he knew compared to the others, presumably part of why he was not the strongest leader, either intellectual or organizational.

Taken together, these possibilities explain why this particular project failed and also provide a list of what needs to happen for other projects to be successful. The mere application of time and effort on the part of the leader and at least some group members, as clearly was evident in MAP, are



insufficient for success. Success in research is typically a question of having good ideas and carefully working out their implications rather than choosing a path and sticking to it, even when setbacks occur. The entire goal of the project, remember, was to test several methods of sociolinguistic analysis and compare the results. For this, specialists in different methods needed to come together, analyze the same data in significantly different ways, and then come to an agreement about which method was the most successful. But none of that happened: Given the changes in group membership, the surprisingly few meetings over an absurdly long time, the lack of agreement as to appropriate data or any meeting where different methods of analysis could be compared, the mediocre leadership and lack of relevant knowledge on the part of the group leader, the minimal funding and changing goals, of course this project failed.

In terms of what MAP demonstrated about Goffman, we can say that at least part of the blame for MAP's failure lies with him: He rejected the data and then dropped out of the project altogether. While he was not alone in this, his reputation was greater than that of most others in the project, so his departure was seen as a signal to the grant reviewers of its unworthiness for essential funding. Perhaps he should have simply refused to participate at all, as Hymes, Labov, and Worth all did from the start, claiming they were too busy; he also was busy in those years. It was likely his friendship with Grimshaw that initially convinced him to join. What seems most surprising about the entire story is the fact that his departure from MAP did not damage (let alone destroy) their friendship, as both his explicit comments in letters and their multiple activities after his withdrawal demonstrate.

## *Endnotes*

<sup>1</sup> Minutes, Committee on Sociolinguistics, June 2–3, 1974, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Committee on Sociolinguistics, January–June 1974.

<sup>2</sup> Jutant and Vergopoulos (2024) provide an example of treating failure seriously for the lessons we can learn.

<sup>3</sup> <https://www.routledge.com/Wenner-Gren-International-Symposium-Series/book-series/BLANTWGISS#>.

<sup>4</sup> <https://wennergren.org/symposium-seminar/the-use-of-computers-in-anthropology/>.

<sup>5</sup> <https://wennergren.org/symposium-seminar/revolution-vs-continuity-in-the-study-of-language/>.

<sup>6</sup> For a full list of participants in that event, see <https://wennergren.org/symposium-seminar/animal-communication/>. The resulting publications were Sebeok 1968; Sebeok and Ramsay 1969.

<sup>7</sup> Hymes to Jenness, Jan 25, 1972, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Committee on Sociolinguistics, January–June 1972.

<sup>8</sup> Conference on Interaction Ethology: Third Circular, Apr 13, 1970, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Sebeok, Thomas, 1955–1982.

<sup>9</sup> “I understand that you and Erving Goffman have discussed your participation” (Sebeok to Hymes, Apr 13, 1970, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Sebeok, Thomas, 1955–1982). “I am very sorry to say that Erv and I, while talking about the desirability of my taking part, didn’t check the calendar” (Hymes to Sebeok, Apr 16, 1970, TS).

<sup>10</sup> For Ervin-Tripp, see Goffman to Sebeok, Mar 6, 1970, TS; for Labov, see the list of invitees sent from Sebeok’s office to Goffman (Feb 3, 1970, TS).

<sup>11</sup> Ripley has written: “At Berkeley my interests in anthropology have largely been formed in association with Ethel Albert, Theodore McCown, Dell Hymes, Robert Murphy, Clifford Geertz, Lloyd Fallers, May Diaz, George Poster, Gerald Berreman, William Shipley, and Erving Goffman” (1965, 1).

<sup>12</sup> Sommer to Sebeok, Apr 17, 1970, TS.

<sup>13</sup> Adam Kendon knew many of the Penn faculty described in these pages, especially Goffman, Birdwhistell, and Grimshaw. While he never taught full time at Penn, he was an adjunct there from 1988 to 1990, through the Institute for Research in Cognitive Science (Rogow 2013), mentioned in chapter 5.

<sup>14</sup> Goffman to Sebeok, n.d., ca. Feb 1970, TS.

<sup>15</sup> <https://annualmeeting.americananthro.org/general-info/future-past/>.

<sup>16</sup> The two dates on the letters are in fact the same, so either there was a phone call, or someone made a typo.

<sup>17</sup> Ellen Marks, Conference Director at the New York Academy of Sciences, to Goffman, Feb 27, 1979, TS.

<sup>18</sup> [https://www.asanet.org/wp-content/uploads/1972\\_annual\\_meeting\\_program.pdf](https://www.asanet.org/wp-content/uploads/1972_annual_meeting_program.pdf).

<sup>19</sup> “Adam Kendon: Curriculum Vitae,” 1988, ADG.

<sup>20</sup> Hymes participated in another symposium at the Reimers Foundation, this one in 1971, so he may have been the connection for Goffman. Alternatively, Kendon knew at least von Cranach, as von Cranach contributed a chapter to a book he edited (Kendon 1973), so he may have been the connection. Or they may have met at an earlier conference.

<sup>21</sup> <https://www.reimers-stiftung.de>.

<sup>22</sup> Sherzer to Hymes, Jun 24, 1971, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Sherzer, Joel, 1968–1987.

<sup>23</sup> Sherzer to Hymes, Sep 1, 1971, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Sherzer, Joel, 1968–1987.

<sup>24</sup> Sherzer to Hymes, Jun 11, 1973, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Sherzer, Joel, 1968–1987.

<sup>25</sup> “Minutes, Committee on Sociolinguistics, March 24–25, 1973,” DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Committee on Sociolinguistics, January–April 1973.

<sup>26</sup> The funding from the Grant Foundation has an interesting back story. NSF declined the third proposal from SSRC to fund the Committee on Sociolinguistics. (They had funded

the first two, supporting most of the committee's activities to that point—lots of small research projects and conferences across the US.) So Hymes repurposed that proposal and sent it to Philip Sapir, Director of the William T. Grant Foundation (Hymes and Eleanor Sheldon of SSRC to Philip Sapir of Grant Foundation, Jan 21, 1973, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Committee on Sociolinguistics, May–December 1973). Sapir wrote back to say they had been awarded \$95,000 to spend over the next two years, and so they were back on their feet again, remarkably quickly (Philip Sapir to Sheldon, copied to Hymes and Grimshaw, Mar 12, 1974, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Committee on Sociolinguistics, January–June 1974). It probably helped that Hymes knew Philip Sapir, as he was David Sapir's brother and Edward Sapir's son.

<sup>27</sup> Gumperz explains: "In 1964 at a summer session at the LI [linguistic institute] at Indiana, where the [SSRC] sociolinguistics committee was first formed" (Murray 2013).

<sup>28</sup> Grimshaw vita, n.d., DHH, Subcollection 2, Series I: Correspondence, Grimshaw, 1983–2004.

<sup>29</sup> <https://repository.upenn.edu/dissertations/AAI5904624/>.

<sup>30</sup> Grimshaw to William Emerson, Director of the Division of Research Grants, National Endowment for the Humanities, Apr 15, 1974, ADG.

<sup>31</sup> Grimshaw to William Emerson, Apr 15, 1974, ADG.

<sup>32</sup> Grimshaw to William Emerson, Apr 15, 1974, ADG. Awkwardly, Grimshaw concludes this letter with a post-script asking for current requirements for NEH applications because, "Who knows, the Social Science Research Council Committee on Sociolinguistics may at some future date come to NEH for support for its joint-analysis project." As a reminder, that was the original name of MAP.

<sup>33</sup> Grimshaw to Hymes, Mar 5, 1973, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Series V: Language in Society, 1968–1992, Subseries A: Early Correspondence, Grimshaw, Allen, 1970–1979.

<sup>34</sup> Grimshaw to Hymes, Feb 26, 1975, DHH, Subcollection 1, Series V: Language in Society, 1968–1992, Subseries A: Early Correspondence, Grimshaw, Allen, 1970–1979.

<sup>35</sup> E.g., Goffman to Grimshaw, Jan 19, 1977, ADG; Goffman to Grimshaw, Jun 29, 1977, ADG.

<sup>36</sup> Grimshaw to Hymes, Apr 29, 1970, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Grimshaw, Allen, 1966–1986, folder 1, 1966–1977.

<sup>37</sup> Hymes to Grimshaw, May 3, 1970, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Grimshaw, Allen, 1966–1986, folder 1, 1966–1977.

<sup>38</sup> Grimshaw to Hymes, Dec 22, 1972, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Grimshaw, Allen, 1966–1986, folder 1, 1966–1977.

<sup>39</sup> Grimshaw to Hymes, Dec 22, 1972, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Grimshaw, Allen, 1966–1986, folder 1, 1966–1977.

<sup>40</sup> Grimshaw to Goffman, Apr 17, 1973, ADG. He took a course on Textual Cohesion, taught by Halliday at the Summer Linguistic Institute of LSA in Ann Arbor in summer 1973 (Grimshaw 1987a).

<sup>41</sup> Jul 10, 1996, ADG. He also mentioned the workshop in Ann Arbor that Schegloff offered with Sacks in an earlier letter (Jan 13, 1976, ADG).

<sup>42</sup> <https://openanthroresearch.org/index.php/oarr/preprint/view/40/74>.

<sup>43</sup> Grimshaw taught at Berkeley 1968–69 as a visiting faculty member in sociology and South Asian studies, “teaching courses . . . (with John J. Gumperz) in sociolinguistics, as well as sociology” (Grimshaw 1981, xi); furthermore, he was in India at the same time as Gumperz, and apparently nearby since they connected while there (Grimshaw to Hymes, Mar 5, 1984, DHH, Subcollection 1, Series I: Correspondence, 1951–1987, Grimshaw, Allen, 1966–1986, folder 3).

<sup>44</sup> Grimshaw to Sacks and Schegloff, Apr 17, 1973, ADG. Oddly, he wrote them a single letter, as if they were two parts of a whole, rather than colleagues sharing assumptions. He knew at least Schegloff by 1967, as there is an early letter in the file.

<sup>45</sup> Grimshaw to Hymes and Jenness, May 5, 1973, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Committee on Sociolinguistics, May–December 1973.

<sup>46</sup> Grimshaw to Hymes and Jenness, May 5, 1973, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Committee on Sociolinguistics, May–December 1973.

<sup>47</sup> He specifically thanks Kendon for a meeting in fall 1973 (Grimshaw to Kendon, Jun 15, 1974, ADG).

<sup>48</sup> Grimshaw to Madeline Mathiot, May 6, 1975, ADG.

<sup>49</sup> <https://items.ssrc.org/from-our-archives/the-scope-of-sociolinguistics/>.

<sup>50</sup> Grimshaw to Jenness, Sep 24, 1975, ADG.

<sup>51</sup> Jenness to Grimshaw, Sep 17, 1974, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Committee on Sociolinguistics, July–December 1974.

<sup>52</sup> Grimshaw and Jenness to Alan Bell, Nation Science Foundation, Feb 26, 1975, ADG.

<sup>53</sup> Jenness to Paul Chapin at NSF, Nov 10, 1975, ADG.

<sup>54</sup> Grimshaw to Chapin, Jun 14, 1978, ADG.

<sup>55</sup> The event in Germany was a seminar organized by Thomas Luckmann and Richard Grathoff at the University of Konstanz; see Winkin (2022a) for details.

<sup>56</sup> Zabor was first a Birdwhistell student at Penn who later earned her PhD at Indiana (Zabor 1978). She prepared the major transcript for the MAP project.

<sup>57</sup> Grimshaw to MAP participants, Feb 1, 1977, ADG.

<sup>58</sup> Grimshaw to Jenness, copied to Hymes, Feld, C. Williams, Sep 24, 1975, ADG.

<sup>59</sup> Jenness to Goffman, Sep 12, 1975, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Committee on Sociolinguistics, May 1975.

<sup>60</sup> Hymes to Goffman, Sep 28, 1975, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Goodenough, Ward H., 1970–1986. (This letter appears to have been misfiled.)

<sup>61</sup> “The Multiple Analysis Project: A status report (15 September 1975),” Sep 15, 1975, ADG.

<sup>62</sup> “The Multiple Analysis Project: A status report (15 September 1975),” Sep 15, 1975, ADG.

<sup>63</sup> The initials showing who said what were inserted by Jenness (Jenness to Goffman, Sep 12, 1975, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Committee on Sociolinguistics, May 1975).

<sup>64</sup> “The Multiple Analysis Project: A status report (15 September 1975),” Sep 15, 1975, ADG.

<sup>65</sup> Jenness to MAP participants, Oct 20, 1975, ADG.

<sup>66</sup> Grimshaw to Halliday, Oct 17, 1975, ADG.

<sup>67</sup> <https://www.royaumont.com/en/the-foundation/the-cultural-project/>.

<sup>68</sup> <https://www.normalesup.org/~adanchin/causeries/royaumont.html#chomskypiaget>.

<sup>69</sup> <https://www.normalesup.org/~adanchin/causeries/royaumont.html#chomskypiaget>.

<sup>70</sup> <https://www.vox-institute.ch/wp-content/uploads/2020/02/Sherer.pdf>.

<sup>71</sup> Schegloff to Grimshaw, Oct 30, 1975, ADG; Sacks to Grimshaw, Oct 28, 1975, ADG.

<sup>72</sup> The letter from Chapin is not in the Grimshaw papers, but Grimshaw quotes from it in his response (Grimshaw to Chapin, copied to Jenness, Dec 1, 1975, ADG).

<sup>73</sup> Grimshaw to MAP participants (Bird, Burke, Cicourel, Feld, C. & L. Fillmore, Gumperz, Cook-Gumperz, Hall, Halliday, Kendon, and Wylie), and CSL (Ferguson, Heath, Hymes, Hugh Mehan, and Sherzer), and Friends (Rolf Kjolseth, Labov, Sankoff, Shuy, and George Bohrnstedt), Dec 18, 1978, ADG.

<sup>74</sup> Grimshaw to Sankoff, Jun 2, 1980, ADG. Grimshaw and Sankoff knew each other at least by 1971 and connected at International Sociological Association meetings while she was still based in Montreal (Sankoff to Grimshaw, Dec 11, 1971, ADG). Also, their terms on the Committee on Sociolinguistics overlapped.

<sup>75</sup> Noted in Grimshaw to Kendon, Jun 1, 1981, ADG.

<sup>76</sup> Grimshaw to Corsaro, Higgins, Hymes, Labov, Keenan, Apr 14, 1982, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Grimshaw, Allen, 1966–1986, folder 1, 1981–1983.

<sup>77</sup> Sankoff to Grimshaw, n.d. [ca. April 1982, based on content], ADG.

<sup>78</sup> Grimshaw to Corsaro, Higgins, Hymes, Labov, Keenan, Apr 14, 1982, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Grimshaw, Allen, 1966–1986, folder 1, 1981–1983.

<sup>79</sup> Grimshaw to all MAP participants, Sep 18, 1982, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Grimshaw, Allen, 1966–1986, folder 3, 1981–1983.

<sup>80</sup> Grimshaw wrote to Goffman (and Sankoff) during his last few weeks, trying to connect while he was going to be in town (Nov 6, 1982, ADG), but clearly that did not work out. Apparently, he did connect with Sankoff just a few days after Goffman died (Grimshaw to Sankoff, Dec 13, 1982, ADG). Between running the conference at Penn and Goffman's death, Grimshaw was so exhausted that he skipped the AAA convention where Hymes gave his presidential address, and he wrote Hymes to apologize: “I was just drained after the Philadelphia experience” (Dec 14, 1982, ADG). Nonetheless, he did manage to write up his thoughts on Goffman for *LiS* and sent that along for review; this was eventually published (Grimshaw 1983). He sent that draft to several others for review, including Glassie (Dec 14, 1982, ADG).

<sup>81</sup> As described in some detail in “Adam Kendon: Curriculum Vitae,” 1988, ADG. In fact, much to my surprise, in the attachment to the CV, titled “Synopsis of published work and current and future research,” Kendon says: “My work in face-to-face interaction has been largely inspired by Erving Goffman,” going on to explain further: “My method has been to use film and video recordings of naturally occurring interactions, analyzing these following methods that were originally developed by such workers as Gregory Bateson (1971), Ray Birdwhistell (1970) and Albert Schefflen (1973). The approach I have followed,

inspired as it has been by both Goffman and the work of those just mentioned, among others, has been termed the ‘natural history’ approach, because it is non-experimental and entails structural description of specimens of interactions, which are gathered by making audio-visual recordings of naturally occurring interaction episodes.” Kendon (1988) provides much more detail about the connections he saw between his own work and Goffman’s.

<sup>82</sup> In fact, Grimshaw, Feld, and Jenness stated in print that “If influencing whole traditions of research is considered as a criterion, however, the NHI must be recognized as having succeeded far beyond the modest expectations of its initiators” (1994, 33).

<sup>83</sup> Kendon taught at Penn from 1988 to 1990 (Müller 2007). Because this was after Goffman died, he is not included in chapter 3 with other Penn faculty who connected with Goffman.

<sup>84</sup> The postcard is attached to Grimshaw’s original letter (Grimshaw to Kendon, Oct 12, 1978, ADG).

<sup>85</sup> At that point he reported having had interest expressed by four potential publishers: Ablex, Cambridge, Penn, and Texas. Both books were eventually published with Ablex (Grimshaw to MAP analysts, Feb 7, 1983, DHH, Subcollection 2, Series I: Correspondence, Grimshaw, Allen D., 1983–2004).

<sup>86</sup> Grimshaw to MAP analysts, Jun 10, 1996, DHH, Subcollection 2, Series I: Correspondence. Grimshaw, Allen D., 1983–2004.

<sup>87</sup> “Announcement from Acting Graduate Advisor to Faculty and Graduate Students,” May 6, 1975, ADG.

<sup>88</sup> “S441 Topics in Social Theory. Special session: Some questions for Goffman,” n.d. [fall 1975], ADG.

<sup>89</sup> “S441 The Sociology of Erving Goffman. Supplementary bibliography: The critical response,” n.d. [fall 1975], ADG.

<sup>90</sup> “Course and Instructor Evaluation, S441,” Dec 1, 1975, ADG.

<sup>91</sup> Grimshaw to Hymes, copied to Goffman, Aug 10, 1981, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Grimshaw, Allen.

<sup>92</sup> Grimshaw to Hymes, copied to Goffman, Aug 10, 1981, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Grimshaw, Allen.

<sup>93</sup> [https://www.asanet.org/wp-content/uploads/1982\\_annual\\_meeting\\_program.pdf](https://www.asanet.org/wp-content/uploads/1982_annual_meeting_program.pdf).

<sup>94</sup> Grimshaw, draft of comments for Erving Goffman/Everett Hughes Memorial Session, American Sociological Association, September 2, 1983, n.d. [fall 1983], ADG.

<sup>95</sup> Grimshaw to Becker, Hymes, Lofland, Jul 25, 1983, ADG. The letter further explains that Arlene Daniels was the original chair but was unable to attend, and asked Grimshaw to take on the role.

<sup>96</sup> Hymes to Pier Paolo Giglioli, copied to Grimshaw, Sep 4, 1983, ADG.

<sup>97</sup> Untitled manuscript [clearly identifiable as Hymes’s 1983 paper to the ASA panel on Hughes and Goffman], Hymes to Grimshaw, n.d. [1983], ADG. The vast majority of the comments are about Goffman and are almost verbatim what was published in Hymes (1984).

<sup>98</sup> The description says: “Erving Goffman (1922–1982) was a major figure in the intellectual life of the mid-twentieth century, with an influence which reached well beyond the boundaries of his own discipline of sociology. A thoroughly original thinker, Goffman invented and expounded new ways of discovering the elegant orderliness of what he called

'everyday life'—self presentation and impression management, team performances, talk. The purpose of this course is to introduce sociology majors to the critical foci and central theoretical issues in Goffman's work. A substantial portion of his published work will be reviewed, and attempts made to attend to major critical commentary on his work and to assess his likely lasting influences on social thought" ("S441: Topics in Social Theory: The Sociology of Erving Goffman," fall 1983, ADG).

<sup>99</sup> Kendon to Grimshaw, Dec 15, 1986, ADG. The guest lecture is confirmed in a later letter by Grimshaw to Kendon, naming date and time. With his letter, Kendon included the program of Erving Goffman: An Interdisciplinary Appreciation, held at York, England, July 8–11, 1986 (published as Drew and Wootton 1988), and for which Kendon delivered a plenary (Feb 20, 1987, ADG).

<sup>100</sup> Sherzer to Hymes, Mar 26, 1972, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Committee on Sociolinguistics, January–June 1972.

<sup>101</sup> Sherzer to Hymes, Sep 11, 1970, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Sherzer, Joel, folder 1, 1968–1972.

<sup>102</sup> Sherzer to Hymes, Jul 20, 1970, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Sherzer, Joel, folder 1, 1968–1972.

<sup>103</sup> Bauman and Sherzer to Jenness, copied to James Perry, Executive Director of Southwest Educational Development Laboratory, Aug 9, 1974, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Committee on Sociolinguistics, July–December 1974.

<sup>104</sup> <https://sedl.org/about/>.

<sup>105</sup> James Perry to Jenness, Aug 7, 1974, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Committee on Sociolinguistics, July–December 1974.

<sup>106</sup> <https://eric.ed.gov/?id=ED126692>.

<sup>107</sup> Sherzer to Hymes, Aug 31, 1980, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Sherzer, Joel, folder 3, 1980–1987.

<sup>108</sup> Minutes, Committee on Sociolinguistics, March 24–25, 1973, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Committee on Sociolinguistics, January–April 1973.

<sup>109</sup> Jenness to CSL members, Mar 26, 1974, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Committee on Sociolinguistics, January–June 1974.

<sup>110</sup> Minutes, Committee on Sociolinguistics, May 15–16, 1975, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987: Subseries D, Social Science Research Council, Committee on Sociolinguistics, May 1975.

<sup>111</sup> Goffman to Hymes, Jun 10, 1989, DHH, Subcollection 2, Series I: Correspondence, Goffman, Erving, 1968–1982.

<sup>112</sup> Sherzer to Hymes, Aug 31, 1980, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Sherzer, Joel, folder 3, 1980–1987.

<sup>113</sup> Sherzer to Abrahams, copied to Hymes, May 11, 1970, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Sherzer, Joel.

<sup>114</sup> Hymes to Jenness, Oct 11, 1974, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Conference on Comparative Ethnographic Analysis of Patterns of Speech in the United States, 1972–

1975.

<sup>115</sup> Conference on the Ethnography of Speaking agenda and participants, n.d. [1972], DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Conference on the Ethnography of Speaking, 1972.

<sup>116</sup> Like Roger Abrahams, at the time, Kirshenblatt-Gimblett was teaching at Texas; she later moved to Penn.

<sup>117</sup> Hymes to Bauman, copied to Sherzer (and apparently Abrahams, given that he also responded), Feb 25, 1971, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries E: Other Committees, University of Pennsylvania, University of Texas at Austin, Conference on the Ethnography of Speaking, 1970–1972.

<sup>118</sup> Bauman to Hymes, copied to Sherzer, Mar 5, 1971, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries E: Other Committees, University of Pennsylvania, University of Texas at Austin, Conference on the Ethnography of Speaking, 1970–1972.

<sup>119</sup> Sherzer to Hymes, May 4, 1972, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Conference on the Ethnography of Speaking, 1972.

<sup>120</sup> Bauman to Case, Oct 8, 1971 and Jan 17, 1972, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Committee on Sociolinguistics, 1971.

<sup>121</sup> Sacks was not only Goffman's student, but, as Hymes mentions, "Goffman had me serve on Harvey Sacks's dissertation committee" (Hymes 2003, 338), which makes his invitation to this event quite obvious.

<sup>122</sup> Sherzer to Jenness, copied to Hymes, Dec 14, 1972, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Conference on Comparative Ethnographic Analysis of Patterns of Speech in the United States, 1972–1975.

<sup>123</sup> "Memorandum on small conference on 'Comparative Ethnographic Analysis of Patterns of Speech in the United States,'" prepared by Hymes, May 1974, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Conference on Comparative Ethnographic Analysis of Patterns of Speech in the United States, 1972–1975.

<sup>124</sup> Shuy to Hymes, copied to John Hammer and Jenness, May 28, 1974, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Conference on Comparative Ethnographic Analysis of Patterns of Speech in the United States, 1972–1975.

<sup>125</sup> Jenness to Hymes, copied to Shuy and Grimshaw, Oct 8, 1974, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Conference on Comparative Ethnographic Analysis of Patterns of Speech in the United States, 1972–1975.

<sup>126</sup> Hymes to Jenness, Jun 4, 1974, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Conference on Comparative Ethnographic Analysis of Patterns of Speech in the United States, 1972–1975.

<sup>127</sup> Hymes to Sherzer, Sep 13, 1974, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Sherzer, Joel.

<sup>128</sup> Hymes to Jenness, draft of letter to participants, Oct 23, 1974, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Conference on Comparative Ethnographic Analysis of Patterns of Speech in the



United States, 1972–1975.

<sup>129</sup> “When we were at Sugarloaf, Erving said something about all of us getting together for dinner sometime during the meetings—I hope that will be possible. . . . I hope it will be possible to see you two, the Labovs, and Goffman (I gather it’s likely I’ll see Sol [Worth] at the Temple meetings)” (Grimshaw to Hymes, Feb 16, 1975, DHH, Subcollection 1, Series V: Language in Society, 1968–1992, Subseries A: Early Correspondence, Grimshaw, Allen, 1970–1979).

<sup>130</sup> Labov to Jenness, Nov 20, 1974, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Conference on Comparative Ethnographic Analysis of Patterns of Speech in the United States, 1972–1975.

<sup>131</sup> Hymes to Labov, copied to Jenness, Nov 29, 1974, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Conference on Comparative Ethnographic Analysis of Patterns of Speech in the United States, 1972–1975.

<sup>132</sup> Hymes to Jenness, Report of the conference to SSRC’s Committee on Sociolinguistics (presumably prepared by V. Hymes), Jan 24, 1975, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Committee on Sociolinguistics, January–April 1975.

<sup>133</sup> Fox to Hymes, Jan 22, 1975, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Conference on Comparative Ethnographic Analysis of Patterns of Speech in the United States, 1972–1975.

<sup>134</sup> Hymes to Sherzer, Sep 13, 1974, DHH, Subcollection 1, Series I: Correspondence 1951–1987, Sherzer, Joel.

<sup>135</sup> Untitled report of the Conference on Comparative Ethnographic Analysis of Patterns of Speech in the United States, prepared by Virginia Hymes, n.d. [February–April 1975], DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Committee on Sociolinguistics, January–April 1975.

<sup>136</sup> Hymes to Jenness, Jan 24, 1975, DHH, Subcollection 1, Series II: Conferences and Committees, 1955–1987, Subseries D: Social Science Research Council, Committee on Sociolinguistics, January–April 1975.

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