Holisms of communication
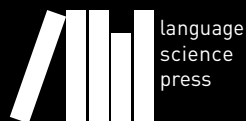
The early history of audio-visual sequence analysis

Edited by

James McElvenny

Andrea Ploder

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The early history of audio-visual sequence analysis

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Acknowledgments

A book like this depends on the collaboration of many. We are grateful for the support of everyone involved, and want to mention the most crucial people here.

First of all, we want to thank all authors for their enthusiasm and patience. They wrote very interesting and insightful papers and responses and were prepared to embark on the adventure of the dialogic book format with us.

The title of this volume, *Holisms of communication*, comes from Erhard Schüttpelz, who suggested it during the very early stages of the project proposal. Despite its enigmatic connotations, "holisms" proved to be a very useful concept for us. It helped to deepen our understanding of the theoretical alliances of audiovisual analysis with Gestalt psychology and the epistemic hope to capture "the whole of the communicative process" that fueled much of the work in early audiovisual communication analysis.

Capturing the whole

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1 Introduction

The advent of motion picture technology in the last decades of the nineteenth century not only brought about a revolution in entertainment, but also in data-producing practices across the human and social sciences. Researchers working in medicine, psychology and anthropology immediately began experimenting with the new technology as a means for making inscriptions of processes that unfold over time (see Erickson 2011). An early example of the study of movement through frame-by-frame analysis is the work of the Viennese cardiologist Ludwig Braun (1861–1936; Braun 1898), but even before him, in the 1880s, there are instances of "chrono-photographic" research, in which the development of a phenomenon is documented through a series of photographs taken one after another at short intervals (see Curtis 2016). This technology was soon used to create ethnographic documentation, as in the work of the French anatomist Félix Regnault (1863–1938), the British anthropologist Alfred C. Haddon (1855–1940), and the German-American anthropologist Franz Boas (1858–1942).[1]

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[1] On the history of ethnographic film, see Brigade (1995 [1975]) and Hochman (2014). In the early years films made for entertainment and for scientific purposes were not always distinct. Film studios of this era often produced simple documentaries of the daily routines and ceremonies of "exotic" peoples of the Pacific, Africa and the Americas for general audiences in the West. Even many feature films constructed around a fictional plot and starring Western actors were shot in "exotic" locations, against the background of semi-staged village life. Some museums and universities even partnered with studios to produce films that simultaneously served the needs of both science and entertainment.

With the introduction of sound-on-film technology in the late 1920s, it became possible to incorporate speech and other synchronized sound into film recordings. Scholars researching human interaction inched closer to the goal of capturing the whole of the communicative situation: voice, gaze, posture, movement, gesture and so on. By the 1950s, early experiments with film started to crystallize into the techniques of audio-visual sequence analysis familiar to us today and, by the middle of the 1970s, these techniques became firmly established as part of the methodological repertoire of the social sciences.

This volume brings together six contributions that explore the pre- and early history of audio-visual sequence analysis, from the late 1920s to the 1960s. The first three chapters address the emergence of initial attempts at sequence analysis in the early sound-film era, among Gestalt theorists at the University of Berlin and researchers attached to the Psychological Institute at the University of Vienna. The majority of these scholars were forced to emigrate to the United States over the course of the 1930s as the National Socialists seized power in the German-speaking countries. In their American exile, they continued their work and brought new impulses into American social scientific research, a transfer of knowledge and techniques that is visible in the leading project on sequence analysis of the mid-twentieth century, the Natural History of an Interview (NHI), which began at the Center for Advanced Studies in the Behavioral Sciences in Palo Alto, California. The NHI project and its aftermath are treated in the final three chapters.

The title of this volume, *Holisms of communication*, points to what is perhaps the most salient innovation encapsulated in audio-visual analysis of communication: the attempt to pin down the communicative whole. Rather than being restricted to just one dimension of a communicative act – such as spoken words, posture or the rudiments of gesture – audio-visual analysis seemed to bring the entire communicative situation into focus. "Holism" may strike some readers as an odd terminological choice to express this new research orientation. Originating in the anti-reductionist philosophy of the somewhat notorious South African statesman and intellectual General Jan Christiaan Smuts (1870–1950; see Smuts 1926), the term was readily adopted in Western esoteric circles and now has an unmistakable air of incense and prayer bowl about it. But this "holism" is not unconnected with the figures and themes explored in this volume: Smuts' thought grew out of the same intellectual environment in the early twentieth century that gave rise to the Gestalt psychology underpinning the approaches explored in the

first part of this book, while an "ecological" conception of interaction and communication plays an important role in the NHI project examined in the second part.[2]

This volume is the first publication to result from a broader project on the history of audio-visual data practices in the social sciences which is embedded in the DFG-supported Collaborative Research Center "Media of Cooperation", based at the University of Siegen. The overarching goal of the research center is to explore the emergence of digitally networked media and the role these play as co-operative tools in our contemporary society. The research undertaken in this center is highly interdisciplinary, encompassing projects in such fields as media studies, sociology, education, and the digital humanities. In uncovering and reconstructing the emergence of audio-visual sequence analysis in the social sciences, our project serves the broader aims of the research center by developing a methodology of historical praxeology as well as critically examining the philosophical commitments, assumptions and practices underlying sequence analysis in the social sciences as it has developed historically and as it is carried out today. At the same time, our project is a contribution to the history of science. With its focus on methodologies and data practices in the social sciences and humanities, and its specific interest in the dynamics of interdisciplinary and transdisciplinary collaboration, the project advances two areas in the history of science which have not received a great deal of attention to date. One scholar who has contributed to both fields, Wendy Leeds-Hurwitz, is among the contributors to this volume.

In planning this volume, our original intention had been to create a complete communicative event in the form of a workshop. We had arranged to meet in Siegen to give talks and discuss these in person. Unfortunately, the coronavirus pandemic of 2020 rendered such a meeting impossible and so we fell back on the mono-dimensional communicative means of epistolary contact, albeit with a modern technological sheen imparted by the use of e-mail. To simulate something of the discussions that would have taken place at the workshop, we solicited written responses to the papers, which are printed here as appendices to each chapter. The authors and respondents who participated in this project come from a broad cross-section of academic fields, and this is visible in the diverse character of the chapters, which have different emphases and exhibit a range of writing styles. This heterogeneity results in a gestalt that reflects the dialogic interaction of scholars across different disciplines.

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[2]In German-speaking psychology, linguistics and other human sciences of the first half of the twentieth century, the terms *Gestalt*, *Ganzheit* (whole) and *Struktur* (structure) were frequently used as near-synonyms, sometimes with varying political connotations (see Harrington 1996; Knobloch 2005: 137–154).

The following two sections give a more detailed overview of the contributions presented in this volume and their surrounding historical and intellectual context. Section 2 sketches the subjects of the first three contributions, the Gestalt psychologists from Berlin and Vienna who were forced into emigration in the 1930s, while section 3 outlines the NHI project, initiated in 1950s California, which is treated in the final two contributions.

2 From Berlin and Vienna to the USA

A key milieu in which techniques of audio-visual sequence analysis first began to coalesce is the research group around the social psychologist and Gestalt theorist Kurt Lewin (1890–1947), who worked at the Psychological Institute of the University of Berlin until his emigration to the United States in 1933. The signature approach that he developed in the first part of his career was the theory of psychological "topology", which adapted diagrammatic representations and formulas from mathematical topology to the representation of the "life space" of subjects and the putative psychological laws active in their minds (see Lewin 1936). With the successive emigration of Lewin and his closest colleagues from Europe, the "topology group" that he founded came to span across two continents, bringing together such figures as the European Gestalt psychologists Fritz Heider (1896–1988) and Kurt Koffka (1886–1941), as well as the prominent American anthropologists Margaret Mead (1901–1978), Ruth Benedict (1887–1948), Edward Tolman (1886–1959), and William Stern (1871–1938), among others. The group continued after Lewin's death in 1947 up into the 1960s (see Lück 2001: 17–19).

From 1923 onwards Lewin produced films as part of his research in Berlin into child behavior (Kreppner 2010: 249–253), with the goal of investigating affective-psychological processes, whose "characteristic properties do not appear in individual, momentary states, but only in the whole of the process" (*characteristische Eigentümlichkeiten nicht im einzelnen, momentanen Zustand, sondern erst im Ganzen des* Geschehensablaufs *zutage treten*; Lewin 1926: 414). On a theoretical level, he expanded the static figure/ground axioms of Gestalt psychology to deal with *dynamische Gestalten* and *Zeitgestalten*; that is, gestalts that unfold and change shape over time (on these constructs, see, e.g., Koffka 1928).

Lewin's theoretical notions would seem in turn to have contributed to the development of the concepts of "sequence" and "indexicality" in the later ethnomethodology of the American sociologist Harold Garfinkel (1917–2011) and his followers (on Garfinkel's ethnomethodology, see Garfinkel 1967; 2002). Lewin's dynamic rendering of the notion of "field" from Gestalt psychology created an

indexical perspective on signs and highlighted the importance of sequences as an environment of action which serves as a "ground" against which signs are positioned. There were also similarities between Lewin's and Garfinkel's experimental practice, which promoted further cross-pollination between their respective circles of students and collaborators. Both Lewin and Garfinkel focused on everyday situations and sought to conduct experiments using a minimum of equipment, and both liked to create experimental situations where a conflict or disturbance is introduced among subjects, whose reaction is then observed (see Garfinkel 1967: 58; Lück 2001: 28).

In terms of the use of film, one of the most significant members of Lewin's circle was Fritz Heider. Schooled in phenomenology in Graz by the philosopher Alexius Meinong (1853–1920), Heider focused in his experiments on the naïve psychology of participants and their "common sense knowledge". A central insight coursing through his writings is that there is no fundamental break between common sense explanations and scientific theories (an observation shared with other phenomenologists and pragmatists). It was his ambition to develop an explicit account of how this common sense knowledge functions in day-to-day life. A famous example of this is his "attribution theory", a pillar of early social psychology which deals with how people attribute motives to others in explaining their behavior (see Heider 1958). In developing attribution theory, Heider conducted a series of influential experiments in which he presented subjects with short films of animated geometric shapes and asked them to describe the "behavior" of these shapes.

Lewin and Heider are the focus of the first two chapters in this volume. In chapter 1, Helmut Lück explores their intertwined biographies and friendship. He pays particular attention to the interplay between their film-based research and their contributions to psychological theory. In chapter 2, Clemens Knobloch then looks in particular at how Lewin and Heider adapted their ideas and methods to the prevailing empiricist-scientist spirit of psychology in the United States after their emigration, and how their ideas and methods were received and adapted in American social psychology.

A second center of film-based psychological research in this period was the Psychological Institute at the University of Vienna, led by the husband and wife team of Karl (1879–1963) and Charlotte Bühler (1893–1974). Karl Bühler is known chiefly for his "organon" model of language and research into deixis (Bühler 1934), and is considered a founding figure of psycholinguistics (see Levelt 2012; Hoskovec 2018; Friedrich 2018), while Charlotte Bühler made pioneering contributions to developmental psychology (Bühler 1922; Woodward 2012). From the 1930s onwards, film was used extensively at the Vienna institute (Kreppner 2010:

234, 242). The initial impetus for the use of film would seem to have come from Charlotte Bühler. She had encountered film-based research techniques during a study trip to the Yale Clinic of Child Development in the United States from 1924 to 1925, where the developmental psychologist Arnold Gesell filmed infant behavior and reflected on the potential of film for psychology in general (Gesell 1928; 1934). Researchers at the Vienna institute prized film because it provided them with a means to play back sequences of actions and events repeatedly and to slow these down for closer examination. There are indications that Karl Bühler revised his doctrine of deixis in light of the film studies conducted at the institute (see Czwik 2018).

A leading figure for the use of film at the Bühlers' institute was the developmental psychologist Käthe Wolf (1907–1967). In addition to co-supervising several dissertations that employed film-based methods or analyzed the medium of film itself, Wolf began work on a book manuscript on the application of film in psychological research, which has unfortunately been lost (see Czwik 2018). Wolf also organized the phenomenology study circle *Husserlstudien* (Czwik 2018: 43) at the institute; it is therefore no coincidence that phenomenological concepts and terminology play a key role in the analysis of film within the Bühler group. In chapter 3 of this volume, Maria Czwik looks at a key dissertation supervised by Wolf, the *Bildhaftigkeit des Films* ("Graphic quality of film") by Hans Herma (1911–1966), which Czwik treats as representative of the film-oriented research undertaken in Vienna. This dissertation focused on the specific ability of film to transport viewers from their immediate embodied environment and place them in a new perceptual world constructed by the film. Czwik examines how perceptual psychology was applied in Herma's study and the implications of his findings for the further development of the field.

Another important film researcher in the Bühler circle was René Spitz (1887–1974), who collaborated with Wolf on several projects. Central aspects of his psychology of early childhood are based on film documentation of emotionally neglected children in institutions. Starting in the 1930s, he documented the development of infants in orphanages as well as the interaction between mother and child on film. He used the footage for what he called "film analysis" and also produced educational films on infant psychology from the same material (e.g., Spitz 1945; see also Kreppner 2010: 245-246; Geissmann & Geissmann 1998: 213). Spitz developed a methodology of shooting films at a high frame rate that could then be replayed in slow motion for efficient observation (Geissmann & Geissmann 1998: 214).

With the *Anschluss* of Austria onto Germany in 1938, the Bühlers suffered harassment at the hands of the new National Socialist regime, and eventually

decided to leave Austria. After a semi-itinerant two years in London and Oslo, they finally emigrated to the United States in 1940, where they spent five years in Minneapolis before settling in Los Angeles in 1945. Karl Bühler never really gained a foothold in America: in Los Angeles he became a psychologist in private practice and taught occasionally at the University of Southern California. Charlotte Bühler, on the other hand, had much greater success in her American exile: she became Chief Psychologist at the Los Angeles County General Hospital and Professor of Psychiatry at the University of Southern California. Although the Bühlers and the research that had been conducted at their institute in Vienna were not unknown in America, they did not achieve the same level of integration into American psychology as the Berlin Gestalt theorists.[3]

But another member of the Bühler circle at the Psychological Institute, Paul F. Lazarsfeld (1901–1976), was able to adapt his research to the new American environment. At Columbia University, he developed a style of empirical social research that shaped the social sciences in the United States as well as in the German-speaking countries after World War II (see Fleck 2007). As the director of the Bureau of Applied Social Research (BASR), Lazarsfeld was a pioneer of radio research and – more generally – studied the relationship of mass media, communication, and public opinion. He conducted research on audience reactions to feature films, but it is unclear whether he used film technology for the production of data himself. Lazarsfeld left the Bühler group in 1933, during the very early stages of their interest in film. From 1944 to 1946, Wolf worked alongside Lazarsfeld at the BASR.

3 From Palo Alto to New York and Pennsylvania

The single most innovative and influential project of the mid-twentieth century for audio-visual sequence analysis is without doubt the Natural History of an Interview (NHI), which was initiated in 1955 by the émigré psychiatrist and psychoanalyst Frieda Fromm Reichmann (1889–1957) at the Center for Advanced Studies in the Behavioral Sciences (CASBS, Palo Alto) and continued until 1968 at the various home institutions of its members. The project was directed toward achieving a better understanding of communicative dynamics in psychiatric interviews and produced the first systematic analyses of both verbal and non-verbal aspects of social interaction. The methods pioneered in the project

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[3]On the life and work of Karl and Charlotte Bühler, see the entries on these two in Maas' continually updated "biographical catalog" of German-speaking language researchers persecuted and driven into exile during the National Socialist period: https://zflprojekte.de (Maas 2010–).

fed into a number of widely used approaches in the social sciences. One of these later became known as "context analysis", an approach under which social interaction is treated as a "semiotic ecology" created by its participants, in which the receptive activity of the listener is considered as important to the interaction as the productive role of the speaker. The analysis of such semiotic ecologies is only possible with a continuous audio-visual recording of the participants in an interaction (see Erickson 2011: 181). Among the tools variously invented or developed further within the NHI project are transcription systems for verbal and non-verbal communication as well as the theoretical and methodological concept of micro-analysis of interaction (McQuown 1957), which combines elements of American pragmatism, Gestalt psychology, psychoanalysis and the tagmemics of the linguist Kenneth L. Pike (1912–2000; see Pike 1967 [1954]).

The NHI was a collaborative effort undertaken by researchers from across the social sciences. The team assembled by Fromm-Reichmann included such figures as the anthropologist Ray Birdwhistell (1918–1994), the linguists Norman McQuown (1914–2005) and Charles Hockett (1916–2000), and the psychiatrist Henry Brosin (1904–1999). Initially, the members of the project spent several weeks analyzing sound recordings of psychiatric interviews (see McQuown 1957; Lempert 2019), but when the anthropologist Gregory Bateson (1904–1980) joined the team, he brought with him sound-film recordings that served as the subsequent focus of the project's analytic efforts. Bateson had already used film extensively in his research: he and Margaret Mead (1901–1978) employed film and sequential photographs as part of their ethnographic documentation in Bali in the 1930s (Bateson & Mead 1942). In these efforts, Bateson and Mead were following the example of their anthropological elders, Franz Boas, who was Mead's doctoral supervisor, and Alfred C. Haddon, who had taught Bateson at the University of Cambridge. Before joining the NHI project, Bateson had already used film to record and analyze psychiatric interviews in a collaboration with the psychiatrist Jurgen Ruesch (1909–1995; see Ruesch & Bateson 1951; Ruesch & Kees 1956; Engelke 2014; 2018).

The film Bateson brought along to the NHI project was of a counseling session he had conducted with a local Palo Alto mother, known in the project under the alias "Doris", and her pre-school-aged son. This material became the focus of the NHI over the next five years. In chapter 4, Henning Engelke critically examines this film material, looking in particular at the circumstances under which the recording was made and the respective roles of Bateson, his interviewee Doris, her son, and the cameraman. Engelke discusses this film material in terms of his ongoing work on the impact of audio-visual sequence analysis on the development of documentary filmmaking for academic purposes (see Engelke 2007; 2018).

Even though the final report of the NHI, completed in 1968, was never published,[4] the theoretical concepts and methodological practices developed within the project – in particular techniques of segmentation and transcription systems – were taken up in a number of subsequent approaches to audio-visual sequence analysis. From 1958 onwards, a group around Albert E. Scheflen (1920–1980) at Temple University in Philadelphia developed, with the participation of Birdwhistell, a method of context analysis in psychotherapy, which they called the "natural history method in psychotherapy" (see Scheflen 1963; 1966). The work of Scheflen and his team flowed into the further development of interaction studies by Adam Kendon (Kendon 1990). NHI also had a visible influence on the development of "paralinguistics" – the study of prosody, intonation and other modulations of the voice – developed by the linguist George L. Trager (1906–1992; Trager 1958) as well as "proxemics" – the study of the use of space in human communication – developed by the anthropologist Edward T. Hall (1914–2009; Hall 1963; 1966). The analytic perspective and key concepts of NHI also re-appear in the pathbreaking research on verbal and non-verbal behavior in interaction later undertaken by the sociologist Erving Goffman (1922–1982; Goffman 1963), who had studied with Birdwhistell at the University of Toronto. Further important lines of reception run through the sociolinguistic work of Dell H. Hymes (1927–2009) and John J. Gumperz (1922–2013; Hymes 1962; Gumperz & Hymes 1972), as well as from 1967 in the "micro-ethnography" of Frederick Erickson (2011: 281) in education studies. Today, we find traces of NHI-related work in different traditions of video analysis (e.g. Heath et al. 2010, Knoblauch et al. 2012, Mondada 2013).

In chapter 5, Wendy Leeds-Hurwitz and Adam Kendon offer a comprehensive account of the NHI project and its impact. Leeds-Hurwitz and Kendon first describe the NHI and its historical and intellectual background in detail, outlining the group's meetings and the contributions of each member to the overall project. They then assess the innovations made within the project and the reception of these in subsequent scholarship in interaction studies.

The volume closes with a document from the archives: the transcript of a speech given by Ray Birdwhistell in 1980, quite probably one of the last public statements made by Birdwhistell in his lifetime. The speech revolves around one of the most important questions regarding the "holistic promise" of film analysis in the social sciences: What does an audio-visual recording of human interaction actually show? Seth Watter stumbled upon the transcript of this speech in

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[4]McQuown (1971) is the unpublished manuscript of the NHI project's final report. It is part of the McQuown papers held in the Special Collections Research Center of the University of Chicago Library and has circulated to other libraries in microfilm copies. The McQuown papers are a treasure trove of archival documents relating to the NHI project.

an unlikely corner of the Rare Book and Manuscript Library at Columbia University. He has edited and annotated this document and supplied it with a critical introduction, providing us with a very suitable conclusion to the volume.

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Part I

From Berlin and Vienna to the USA

Kapitel 1

Kurt Lewin und Fritz Heider: Ihre Freundschaft, ihre Filme und ihre Theorien

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The two psychologists Kurt Lewin (1890–1947) and Fritz Heider (1896–1988) came from very different backgrounds, pursued different research interests and took very different approaches to their research. However, both began their careers in the milieu of Gestalt psychology, which determined the direction of their own theories: for Lewin, his field theory, and for Heider, his attribution theory, including balance theory. Lewin and Heider knew each other and were friends for around 25 years, until Lewin's early death. This chapter examines the development of their theories in connection with their biographies, their friendly collaboration and the mutual influence they exercised on one other. Particular attention is paid to Lewin and Heider's scientific films and their research methods. We show that Heider was greatly influenced by Lewin, but that he ultimately found Lewin's field theory to be unsuitable for representing and understanding social situations. Heider attempted to overcome the problems of Lewin's field theory and as a result developed theoretical ideas more appropriate to the needs of social psychology.

1 Einleitung

Zwei Männer warten in Berlin im Winter 1926/27 auf die Straßenbahn. Der eine ist 36, der andere ist 30 Jahre alt. Beide sind promovierte Psychologen; sie unterhalten sich über mögliche theoretische Grundlagen ihres Fachs. Es hat etwas geschneit. Da zeichnet der ältere mit seinem Regenschirm einen kleinen Kreis in den Schnee. Das soll eine Person sein. Darum herum zeichnet er ein Oval. Das

soll der Lebensraum sein, in dem sich diese Person befindet. Dann zeichnet er ein kleines Pluszeichen in das Oval. Dies soll das Ziel sein, das die Person anstrebt. Doch kommt noch ein Strich hinzu, der den Bereich mit dem Pluszeichen und den Bereich mit der Person abtrennt. Dieser Strich steht für eine Barriere.

Hier zeichnet natürlich Kurt Lewin (1890–1947) den Grundgedanken seiner topologischen Theorie in den Schnee; der jüngere Mann ist der Österreicher Fritz Heider (1896–1988), nun seit November zum zweiten Mal für einige Zeit in Berlin, um die Berliner Schule der Gestaltpsychologie näher kennenzulernen (Heider 1984: 75).

Lewin und Heider waren ca. 25 Jahre lang befreundet, von Heiders erstem Aufenthalt in Berlin 1922, bis zum frühen Tod von Kurt Lewin im Februar 1947. Fritz Heider hat seinen Freund 40 Jahre überlebt und in seinen späteren Arbeiten immer wieder auf ihn und seine Arbeiten Bezug genommen. Lewin und Heider haben auf die Sozialpsychologie und die Psychologie insgesamt so anregend gewirkt wie kaum zwei andere Psychologen ihrer Zeit.

Hier sollen die theoretischen Ansätze der beiden und besonders die gegenseitigen Einflüsse dargestellt werden. Es bestand zwischen den beiden Männern kein Lehrer-Schüler-Verhältnis, sie lehrten nie geneinsam an einer Hochschule, es gibt keine Publikationen in Co-Autorenschaft von Lewin und Heider, und schließlich hätten Lewin und Heider kaum unterschiedlicher sein können – in ihrem Temperament, ihrer Art zu forschen, zu lehren und zu publizieren: Lebhaft und rastlos der eine, besonnen und grüblerisch der andere. Aber ihre gegenseitigen Einflussnahmen waren vermutlich weit größer als bisher beachtet.

2 Ihre Wege und ihre Freundschaft

Kurt Lewin (1890–1947) kam aus bescheidenen deutsch-jüdischen Verhältnissen, er war 1914 Kriegsfreiwilliger, studierte Philosophie, Psychologie und Medizin, wurde geprägt durch den Berliner Institutsdirektor Carl Stumpf und in seinem Wissenschaftsverständnis besonders durch die Neukantianer Ernst Cassirer und Alois Riehl (Schönpflug & Heidelberger 2007). Lewin stand während seiner Berliner Zeit sozialistischen Kreisen nahe. Seine Psychologie wurzelte in der Gestaltpsychologie, obwohl Max Wertheimer, Wolfgang Köhler und Kurt Koffka nicht zu seinen Lehrern, sondern bald zu seinen Kollegen zählten. Lewin war an theoretischen Fragen des menschlichen Wollens interessiert, er war ein Experimentalpsychologe, der zu verschiedenen Themen geforscht hat und auch die Methodologie des psychologischen Experiments durch bislang nicht verwendete Techniken, wie z.B. die absichtliche Täuschung der Versuchspersonen, erweitert hat.

Paradigmatisch für Lewins Vorgehen in seiner Berliner Zeit ist die Untersuchung des Ärgers als psychologischem Problem durch seine Schülerin Tamara Dembo (Dembo 1931). Den Versuchspersonen wurden unlösbare Aufgaben gestellt.[1] Manche Personen versuchten über sehr lange Zeit, oft in mehreren Sitzungen, Lösungen zu finden, teils in heftigen Auseinandersetzungen mit der Versuchsleiterin (Dembo 2002).[2]

Dies gilt auch für ein unveröffentlichtes und ca. 70 Jahre lang unbekannt gebliebenes Experiment von Dembo, in dem den Versuchspersonen überhaupt keine Anweisungen erteilt wurden (sog. Warte-Experiment: Dembo 2002; van der Veer & Lück 2002: 71ff.). Dembo hat diesen Versuch mit ca. 25 Versuchspersonen durchgeführt, von denen manche über eine Stunde gestanden und vergeblich auf Anweisungen gewartet haben. Das psychologische Experiment bekam bei Lewin durch die unverzichtbare Rolle von Versuchsleiter bzw. Versuchsleiterin schon Anfang der zwanziger Jahre eine sozialpsychologische Dimension, die es vorher nie hatte, weder bei Wundt, noch in der Würzburger Schule, noch in der Gestaltpsychologie (Danziger 1990b,a). Mit seinen Doktorandinnen und Doktoranden entwickelte Lewin wichtige theoretische Konzepte wie die *psychische Sättigung*; mit dem Konzept des *Anspruchsniveaus* schuf Lewin den Beginn der Leistungsmotivationsforschung (Hoppe 1930).

Durch Ernst Cassirer erhielt Lewin einen starken Einfluss. Dessen Unterscheidung von *Substanzbegriff* und *Funktionsbegriff* wurde bei Lewin zur Forderung, die Psychologie müsse von einer *Aristotelischen Denkweise* zur *Galileischen Denkweise* voranschreiten. Dementsprechend lehnte Lewin die (statische) Unterscheidung von Typen, Entwicklungsstufen und Durchschnittswerten ab. Überhaupt arbeitete Lewin wenig mit statistischen Werten.

Nach seiner Zwangsemigration 1933 gelang Lewin in die USA eine zweite Karriere. Begeistert von der amerikanischen Demokratie führte er seine berühmten Untersuchungen über die Wirkungen verschiedener Führungsstile auf die Gruppenatmosphäre durch (Lewin u. a. 1939); er entwickelte Methoden der angewandten Gruppendynamik.

Als Zionist sprach Lewin zu Gruppen jüdischer Organisationen über Erziehungsfragen, über Vorurteile und andere Themen. Obwohl in den USA Antisemitismus weit verbreitet war, sah man bei Lewin nicht die „Defekte", wie man sie angeblich sonst bei Angehörigen „der jüdischen Rasse" fand. So stand es in einem Gutachten von Edwin G. Boring über Lewin (Winston 1998: 34). Ein Angebot der

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[1] Eine Versuchsperson war der russische Psychologe Alexander Lurija, der sich in dieser Zeit in Berlin aufhielt.

[2] Die Auseinandersetzungen gingen bis hin zu Tätlichkeiten.

Hebräischen Universität in Palästina lehnte Lewin nach intensiven Bemühungen, dort zu forschen und zu lehren, schließlich ab.

Zu den Arbeiten seiner letzten Lebensjahre gehörten Untersuchungen und Empfehlungen für amerikanische Ministerien und Behörden bis hin zum militärischen Geheimdienst (Binder 2019). Nach Kriegsende engagierte sich Lewin besonders für den Abbau von Vorurteilen gegenüber Minoritäten. Die Entwicklung der Methoden von *Selbsterfahrungsgruppen* und die *Handlungsforschung* (*action research*) waren seine letzten, zukunftsweisenden Entwicklungen (Marrow 1969; 2002).

Fritz Heider dagegen kam aus einer etablierten österreichischen Familie, die in Graz lebte. Für die damalige Zeit waren die Eltern sehr liberal. Der Vater war Architekt, ging in seiner Freizeit vielen Interessen nach, die Mutter war Amateurschauspielerin. Eine schwere Augenverletzung, die sich Fritz zu Schulzeiten beim Spiel mit einer Spielzeugpistole zuzog, führte zum Verlust des linken Auges und prägte seinen späteren Lebensweg. Er wollte Maler oder Schriftsteller werden und hatte weit gespannte Interessen. Sein Vater riet, kein musisches Fach, sondern Architektur zu studieren. Bald wechselte Fritz Heider vom Architektur- zum Jurastudium, das ihm aber auch nicht zusagte. Schließlich betrieb er eine Art Studium Generale in Innsbruck, Wien und Graz und wendete sich immer mehr der Philosophie und Psychologie zu. Er schloss sein Studium mit einer Promotion bei dem Grazer Philosophen und Psychologen Alexius Meinong ab. Meinong (1853–1920) stand mit Christian von Ehrenfels (1859–1932) im Mittelpunkt der sogenannten Grazer Schule der Gestaltpsychologie. Es folgten Fritz Heiders „Wanderjahre", wozu ein Besuch in Berlin, eine kürzere Zeit der Lehrtätigkeit in einem Internat und ein längerer Aufenthalt in Italien gehörten, es gab aber auch Phasen des Selbstzweifels und der Unsicherheit.

Schließlich fand die von ihm selbst im Rückblick durchaus positiv bewertete „verlängerte Adoleszenz" (1984: 72) ein Ende. Ihm war inzwischen ganz klar geworden, dass die Psychologie ihn ein Leben lang fesseln würde. Zu dieser Entscheidung „trug nicht zuletzt Lewins fortgesetztes Interesse bei" (1984: 73). Lewin hatte für Heider Jobs in Berlin vermittelt und sich u.a. für die Veröffentlichung der gekürzten Dissertation von Heider eingesetzt (Heider 1926). So packte Heider seine Sachen und fuhr im November 1926 nach Berlin, besuchte dort Lehrveranstaltungen von Lewin und dessen Kollegen und lernte Lewins neue Untersuchungen kennen. Von mehreren Angeboten nahm Heider das von William Stern in Hamburg an. So konnte er dort ab Frühjahr 1927 als Assistent unterrichten. In Hamburg erlebte Heider das „goldene Zeitalter in der Geschichte des Instituts" (Heider 1984: 79). Er hatte Kontakt mit Ernst Cassirer, der ja auch Lewins Lehrer gewesen war. Jakob Johann Uexküll, der den Begriff der Umwelt in die

Psychologie eingeführt hatte, gehörte zwar nicht zur Hamburger Psychologie, nahm aber dort häufig an Veranstaltungen teil. Für Heider sollte Uexkülls Umweltlehre später wichtig werden. Die Entlassung William Sterns aus rassistischen Gründen und die Zerschlagung des Hamburger Instituts durch die Nationalsozialisten erlebte Heider in Hamburg nicht mehr, denn er verließ das Institut bereits im August 1930. Stern hatte ihn gefragt, ob er für einige Zeit bei dem Gestaltpsychologen Kurt Koffka arbeiten wolle, der kurz vorher in die USA emigriert war. Ohne lange zu überlegen sagte Heider zu, ging in die USA und blieb dann bis zu seinem Tod 1988 dort.

Was Heider vertrat, stand im Gegensatz zur Psychoanalyse und zur Charakterologie, die in Deutschland dominierte. Vor allem stand Heiders Auffassung im Gegensatz zum Behaviorismus, der in der amerikanischen Psychologie lange den Mainstream bildete. Inhaltlich war Heider an interpersonellen Beziehungen interessiert, einem zentralen Thema der Sozialpsychologie.

Heider arbeitete gern im Stillen für sich. Wenn er eine Lösung für ein Problem gefunden hatte, ging er zufrieden spazieren. Er führte so gut wie kein Experiment durch, veröffentlichte wenig und hatte fast keine Schüler. Sein Auftreten als akademischer Lehrer war offenbar ohne besonderes Charisma (Schönpflug 2008). Heider erreichte erst größere Bekanntheit, nachdem er unter großen Schwierigkeiten seine einzige psychologische Monographie, *The psychology of interpersonal relations* (1958), veröffentlichte (Vervielfältigungen des Manuskriptes zirkulierten schon längere Zeit vorher unter Kollegen). Zum Zeitpunkt der Veröffentlichung war Heider bereits 62 Jahre alt. Die deutsche Übersetzung erschien erst 15 Jahre später als „Psychologie der interpersonalen Beziehungen". Dieses Buch war ein „Fremdkörper" in der amerikanischen Psychologie, weil Heider vom *common sense* ausging und interpersonelle Beziehungen in Märchen, Fabeln und freien Beschreibungen beschrieb, wobei er selbstkritisch die Betrachtungsweisen, die Wortwahl und Wortbedeutungen (z.B. „können") analysierte. Autoren, auf die er sich bezog, waren Solomon E. Asch, Gustav Ichheiser, Egon Brunswik, George Caspar Homans und andere. Das Buch benötigte mehr als ein Jahrzehnt, bis es häufiger zitiert wurde. Dann aber blieb die Zitationshäufigkeit über die Jahrzehnte hoch (Reisenzein & Rudolph 2008: 128f.). Das Buch enthält die Entwicklung und Darstellung der Attributionstheorie einschließlich der nach Heider dazugehörigen Balancetheorie (s.u.). Beide Theorien dominierten einige Jahre später die internationale Sozialpsychologie, wie sich auch quantitativ an vielen hundert Veröffentlichungen aus den Jahren 1971–1980 zeigen lässt, denn in dieser Zeit hatte die Attributionstheorie die populäre Dissonanztheorie des Lewin-Schülers Leon Festinger abgelöst (Fisch & Daniel 1983). So trug Heider erheblich zu sog. *Kognitiven Wende* der Psychologie bei, die eine Abwendung vom

Behaviorismus darstellte und besonders für die amerikanische Psychologie von großer Bedeutung war, weil dort der Behaviorismus stärker vorherrschte als in anderen Ländern.[3]

Als Heider in einem Interview für eine erste Aufsatzsammlung zur Attributionsforschung (Harvey u. a. 1976) gefragt wurde, was er von den Ideen von Watson und Skinner halte, da sagte er: „Well, I think Skinner has said some very interesting things" (Heider 1976: 10). Mehr nicht. Eine solche Äußerung mussten die behavioristisch sozialisierten Psychologen damals als Naivität ansehen oder für eine Stilblüte halten. Heider verwies aber gleich darauf, dass er zum Thema Belohnung und Strafe Kurt Lewin den Vorzug geben würde, der das Thema kognitionspsychologisch behandelt hätte.

In Heiders wissenschaftlichen Arbeiten und seinen Lebenserinnerungen (1984) ist immer wieder von Lewin die Rede. Ihm verdankte Heider wohl die wichtigsten Anregungen überhaupt. Fritz Heider und seine Frau Grace (geb. Moore) übersetzten Lewins *Grundzüge der topologischen Psychologie* ins Englische, sie nahmen beide regelmäßig an den jährlich stattfindenden Treffen der *Topology Group* teil, die Lewin schon unmittelbar nach seiner Emigration 1933 organisierte.[4] In jedem Fall war Heider über die Entwicklungen der Arbeiten Lewins sehr gut informiert. Dies zeigt sich z.B. an einem Aufsatz von Heider (1959) über Lewins Methodologie, in dem er der amerikanischen Leserschaft anschaulich zeigt, dass in Lewins „Kriegslandschaft"-Aufsatz (1917) sehr viele Konzepte der Feldtheorie vorweggenommen sind, so dass dieser Aufsatz programmatische Züge für Lewins Lebenswerk trägt.[5]

Schon als junger Mann hatte Heider eine Einstellung zu seiner eigenen Arbeit gefunden: „Ich darf nicht um äußere Belohnungen arbeiten, es darf mich nicht kümmern, ob Anerkennung heute kommt oder erst morgen" (1984: 49). Eine solche Äußerung hätte man von Lewin sicher nicht hören können. Lewin war nicht übertrieben ehrsüchtig, machte aber seine Forschungsergebnisse zeitnah bekannt. Zudem war er mitteilsam, kontaktfreudig, dazu allerdings etwas unorganisiert. Auch nach seiner Emigration hatte er in kurzer Zeit Kolleginnen und Kollegen als Freunde gefunden. Bekannt ist, dass Lewin gerne seine Ideen ge-

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[3]Zur Rezeptionsgeschichte siehe die Beiträge im Themenheft der Zeitschrift *Social Psychology*, 2008, Band 39 (3) zum 50jährigen Jubiläum des Buches.

[4]Die Idee zu diesen Treffen hätte Fritz Heider gehabt, schrieb Lewin an Donald K. Adams (Lück 1989: 258). Eher ist anzunehmen, dass Lewin selbst diese Idee hatte, denn Heider zitiert einen entsprechenden Brief (1984: 113).

[5]Dass Erfahrungen aus dem Kriegs*feld* den Anstoß zur Feldtheorie gaben, ist von mehreren Autoren gesehen und diskutiert worden (u.a. Günzel 2008, Wieser 2014, Binder 2019: insbes. Kap. II).

meinsam mit Gruppen von Studierenden entwickelte. Ein wichtiges Medium für ihn war der Film.

3 Ihre Filme

Es ist verständlich, dass vor allem Gestaltpsychologen mit Filmen gearbeitet haben, um optische Täuschungen, Gestaltgesetze usw. im stehenden und bewegten Bild festzuhalten. Die Ikone der frühen Filme ist Wolfgang Köhlers Serie kurzer Aufnahmen von den Intelligenzprüfungen an Schimpansen, gedreht 1914 auf Teneriffa. Dies waren die ersten Filmaufnahmen, die überhaupt auf Teneriffa gemacht wurden. Das Interesse von Köhler war es, Beweismaterial für die erstaunlichen Leistungen einsichtsvollen Verhaltens der Anthropoiden zu gewinnen. Hierfür hatte er gute Gründe: Zuvor waren nämlich ähnliche Beobachtungen anderer Autoren an Primaten bezweifelt worden.

Kurt Lewin kannte diese Filme; er sah sie spätestens nachdem Köhler 1921 Institutsdirektor in Berlin wurde. Offenbar dienten die Filme als Orientierung für seine eigenen Aufnahmen, mit denen er etwa 1923 begann. Er filmte vor allem Kinder in Konfliktsituationen. Mal war es nicht erreichbares Spielzeug hinter einer Abzäunung, mal Obst, das eine erwachsene Person absichtlich hochhielt, Furcht beim Überqueren eines kleinen Bachs über einen Steg oder die Verlegenheit beim Begrüßen einer erwachsenen Person. Im Gegensatz zu den Aufnahmen von Köhler zeigten die Kinder das Verhalten aktuell und vielleicht sogar das erste Mal. Berühmt geworden sind zwei dieser Filme: Ein Kind – es ist Wolfgang Köhlers Tochter Karin – steigt mit einem Ball eine steinerne Gartentreppe aufwärts und der Film „Hanna und der Stein", in dem ein Mädchen versucht, sich im Garten auf einen Stein zu setzen. Dies misslingt Hanna mehrfach, weil sie sich herumdrehen, also vom Ziel abwenden muss, um sich setzen zu können. Die Problematik des Umwegs, ohne den das Ziel hier nicht erreichbar ist, ist auch ein Thema von Köhlers Versuchen gewesen. Er hatte die psychologische Lage für Tiere anschaulich beschrieben: Dicht hinter einem Zaun liegendes Futter wird von dem Tier nur erreicht, wenn es den Umweg aus dem hinten offenen Käfig nutzt. Während Köhler mit dem Konzept der *Einsicht* argumentierte, nutzte Lewin das der Feldkraft.

Die frühen Filme von Lewin stehen in sichtbarem Zusammenhang zu der topologischen Psychologie, die Lewin in den zwanziger Jahren vor allem im Zusammenhang mit Entwicklung und Erziehung entwarf (Lewin 1931b). Drei Zwecke erfüllten die Filme Lewins gleichzeitig: Sie dienten seinen Forschungsinteressen, er nutzte sie als Anschauungsmaterial für Vorträge und Vorlesungen (Lewin 1930)

und sie dienten privaten Interessen, denn sie zeigten Personen aus seiner Familie und Bekanntschaft. Zu Lewins Vorgehen gehörte auch, dass in den Filmen unerwartete Verhaltensweisen auftauchten, die nachträglich topologisch erklärt wurden. Zum Beispiel ist in einem Film zu sehen, wie ein Mädchen nach heftiger Enttäuschung wegläuft, was Lewin entsprechend seiner Feldtheorie als anschauliches Beispiel für das sog. „Aus-dem-Felde-Gehen" diente.

Trotz des Alters von annähernd 100 Jahren sind die Filme immer noch aufschlussreich. Das gilt auch für den wiedergefundenen Film „Das Kind und die Welt" (Lewin 1931a), der die Entwicklung des Kindes von der Geburt bis etwa zum 12. Lebensjahr zeigt. Besonderen Reiz hat der Film durch Perspektiven aus der Sicht des Kleinkindes, durch Originalton und durch unbemerkt gefilmte Szenen mit spielenden Kindern in Berliner Hinterhöfen (Lück 2006a; Elteren & Lück 1990).

Am Anfang der Entwicklung einer eigenen Theorie von Fritz Heider steht eine kleinere experimentelle Studie, die er zusammen mit seiner Studentin Marianne Simmel (1923–2010) am Smith College durchführte. Im Experiment wurden die Versuchspersonen aufgefordert, einen Film von zweieinhalb Minuten Dauer zu interpretieren, in dem drei geometrische Figuren (ein großes Dreieck, ein kleines Dreieck und eine Scheibe oder Kreis) gezeigt wurden, die sich mit unterschiedlicher Geschwindigkeit umherbewegen. Die einzige weitere Figur im Feld bildete ein Rechteck, von dem ein Teilstück, ähnlich einer Tür, sich öffnete und schloss. Heider: „Ich erinnere mich noch gut, mit welchem Vergnügen ich ihn zum ersten Mal vorführte. Und es war sehr eindrucksvoll, wie beinahe jeder, der ihn sah, ihn in Begriffen menschlicher Handlungen und menschlicher Gefühle wahrnahm" (1984: 137).

Auf diese Wahrnehmung von Absichten im Verhalten der abgebildeten „Personen" kam es Heider an. Die Veröffentlichung von Heider und Simmel ist eine der beiden wissenschaftlichen Publikationen von Heider aus dem Jahr 1944, in der der Begriff *attribution* erstmalig in seiner psychologischen Bedeutung auftaucht. Wenige Jahre später entwarf Heider auf der Grundlage naiv-psychologischer Erklärungen seine Attributionstheorie. (Er selbst wies allerdings mehrfach darauf hin, dass er nicht der Begründer der Attributionstheorie sei.) Er fand, dass praktisch alle Versuchspersonen – auch bei unterschiedlichen Instruktionen – die Bewegungen der Symbole im Zeichentrickfilm als zielgerichtete Handlungen von Personen, weit seltener von Tieren, beschrieben. Dies bestätigte sich über Jahrzehnte in vielen Replikationsstudien in vielen Ländern. Jedoch häufen sich seit den neunziger Jahren Studien, in denen zum gleichen Film die Versuchspersonen Beschreibungen gaben, in denen die Symbole als solche, und inzwischen nicht mehr als Personen, benannt wurden (Curci-Marino u. a. 2004; Lück 2006b). Die

Gründe für dieses veränderte Verhalten sind bislang nicht geklärt, sie liegen möglicherweise in veränderten Sehgewohnheiten durch die häufige Betrachtung von Zeichentrickfilmen und Nutzung von Computerspielen.

Ein Vergleich der Filme von Lewin und Heider ist nur zum Teil möglich. Für Heider bildete sein Film das Stimulusmaterial, um damit Wirkungen verschiedener Instruktionsbedingungen zu erkunden. Lewins Filmtätigkeit war zunächst dokumentarisch, ähnlich dem von Köhler, aber noch kreativer. Dies ist erkennbar an eher zufälligen Aufnahmen, die sowohl in Berlin als auch in den USA entstanden und von ihm konstruktiv für neue Betrachtungsweisen des gefilmten Verhaltens genutzt wurden.

4 Kurt Lewins sozialtechnologische Arbeiten

Lewins erste Frau, Maria Landsberg, war Studienrätin, seine zweite Frau, Gertrud Weiss Lewin, Erzieherin. Lewin hatte vier Kinder, von denen der Junge aus erster Ehe behindert war. So wundert es nicht, dass Lewin der Reformpädagogik nahestand, auf erziehungswissenschaftlichen Kongressen referierte und dazu seine Filme über Kinder in Konfliktsituationen zeigte.

Gemeinsam mit Ronald Lippitt und Ralph K. White untersuchte er in Iowa die Wirkung verschiedener Erziehungs- bzw. Führungsstile auf die Gruppenatmosphäre. Aufgrund eigener Beobachtungen verglich er Erziehungspraktiken in den USA mit denen in Deutschland und interpretierte diese feldtheoretisch; in mehreren Vorträgen behandelte er Probleme der Erziehung jüdischer Kinder usw. Lewins Feldtheorie erwies sich als anschaulich und „praktisch": Demokratie war nicht nur die bessere Regierungsform, Demokratie ließ sich erlernen. Ein solcher Befund wurde in den USA gern aufgenommen. Dagegen war die deutsche Nachkriegspsychologie skeptisch bezüglich experimenteller Führungsstiluntersuchungen. Noch 1964 schrieb der Ganzheitspsychologe Leipziger Richtung Albert Wellek:

> Es ist Lewins in gewissem Sinne echt amerikanischer Geniestreich, daß er den Amerikanern eine experimentell – zumal im Kinderexperiment – begründete Nachweisung der Überlegenheit der *Demokratie* lieferte. Er entwickelt und kontrastiert pädagogische (gleich politische) „Führungsstile", und zwar eben den demokratischen in Abhebung vor und über dem „autoritären" einerseits, dem anarchischen andererseits – in einer goldenen oder dialektischen Mitte zwischen den beiden letzteren. Die „Dynamik" des Handlungsfeldes ist eine Eigendynamik, in der das Individuum mehr passiv

als aktiv, mehr getragen als tragend und deshalb eher inhaltsarm erscheint – wiederum ein der amerikanischen Art von Kollektivismus entgegenkommendes Konzept. (Wellek 1964: 251)

Welleks seltsame Darstellung mit ihrer unterschwelligen Kritik lässt ahnen, dass die experimentelle Sozialpsychologie selbst in den 1960er Jahren in der Bundesrepublik noch einige Zeit bis zur Anerkennung benötigte. Dass Wellek in Lewin praktisch einen zum Behavioristen gewandelten Gestalttheoretiker sah, dem das Verständnis für die Psychologie abhandengekommen war, ist eine weitere Fehleinschätzung: „In der Vektorenanalyse der Lewinschen Topologischen Psychologie lebt schließlich sogar das ichlose Passivitätsdenken des englischen Empirismus fort oder wieder auf […]" (Wellek 1964: 251).

So gesehen ist der Befund von Dirk Paul Bogner (2017), dass Lewin in der Weimarer Zeit von den Erziehungswissenschaften kaum wahrgenommen wurde und dass er in den heutigen Erziehungswissenschaften (noch) keine Rolle spielt, nicht abwegig. Daran, dass Lewin nicht Pädagoge, sondern in erster Linie Psychologe war, lag dies nicht, es lag eher an den Erziehungswissenschaften.

Ein Autor, der die Feldtheorie explizit auf die Gesellschaft insgesamt angewendet hat, war der Lewinschüler Junius Flagg Brown (1902–1970). Brown nutzte die Feldtheorie nicht nur für die Darstellung des Lebensraums des Individuums, sondern für Gruppen und Gesellschaften (Brown 1936). So stellte er z.B. die drei Schichten einer Gesellschaft mit ihren verschiedenen Freiheiten topologisch dar. Aus mehreren Gründen fand seine Theorie aber nur geringe Verbreitung.

Einige Autoren haben in den letzten Jahren unabhängig voneinander auf die große Ähnlichkeit der Lehre zur Demokratieerziehung von John Dewey (1859–1952) und der Forderung Lewins zur demokratischen Erziehung auf der Grundlage der Feldtheorie hingewiesen (Krainz 2015; Bogner 2017: 428ff. Binder 2019: 186ff.). Die Ähnlichkeit der Ansätze von Dewey und Lewin ist bemerkenswert. Sie wurde schon 1948 von Gordon Allport erkannt, der schrieb,

[…] dass die Demokratie eine soziale Struktur darstellt, die weit schwerer zu erreichen und zu erhalten ist als die Autokratie. Beide sehen einen engen Zusammenhang von Demokratie und Sozialwissenschaften. […] Dewey, so können wir sagen, ist der hervorragende philosophische Exponent der Demokratie, Lewin ist ihr hervorragender psychologischer Exponent. (Allport 1948: xi)

Die Studien von Lewin zu den Führungsstilen legen die Frage nach seinem Demokratieverständnis nahe. Binder (2019: 195ff.) zeigt, dass Lewin kein theoretisch-philosophisches oder historisches Verständnis an den Anfang stellte, sondern

dass er *praxeologisch* arbeitete: Die Lewin-Gruppe in Iowa ging von einem allgemeinen Verständnis von Demokratie aus und von dem, was sich in demokratisch geführten Gruppen zeigte. Eine inhaltliche Diskussion dessen, was Demokratie ausmachte, unterblieb weitgehend. Dies gilt auch für die Unternehmensberatung, die Lewin über viele Jahre ausübte.[6]

5 Feedback

In seiner Aufsatzsammlung über *Menschenregierungskünste* hat Ulrich Bröckling (2017: 197) aus einem Gespräch von Steward Brand (1976) mit Margaret Mead und Gregory Bateson zitiert, in dem sich Mead und Bateson an einige Einzelheiten der ersten Macy-Konferenz über „Feedback mechanisms and circular causal systems" in biological and social systems" erinnerten.[7] An dieser Konferenz nahm auch Kurt Lewin teil. Die Konferenz fand vom 8. bis 9. März 1946 statt. Auf der

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[6]An dieser Stelle sei eine Bemerkung zu den sozialtechnologischen Arbeiten von Kurt Lewin eingefügt, die Clemens Knobloch 2021 [this volume] skizziert hat: Lewin hat zwar in den USA weitgehend sein Wissenschaftsverständnis und sein empirisches Vorgehen aus Berliner Zeit beibehalten; jedoch veränderten sich seine Interessen merklich. Hatte er damals in Berlin auf Seiten der Arbeitnehmer gestanden, die Lage der Industriearbeiter(innen) erforscht und beschrieben, so stand er in den USA auf Seiten der Ministerien, Regierungseinrichtungen und Unternehmer (Lück 2011; Binder 2019). So stellte er u.a. die Interessen der Unternehmensleitung der Harwood Manufacturing Corporation nicht infrage. Maßnahmen im Interesse der Unternehmensleitung sollten mit Geschick umgesetzt und in ihren Wirkungen erprobt werden. Lewins angewandte Psychologie dieser Zeit legte auf diese Weise eine „stark manipulative Tendenz" an den Tag – „manipulativ" hier eindeutig negativ gemeint (Elteren 2007: 230). Erklärbar ist dieses Verhalten durch Lewins Bereitschaft zur Mitwirkung an einer nicht parteigebundenen Sozialtechnologie in einer demokratischen Gesellschaft. Lewin rechnete sich einer Bestrebung zu, die sowohl von der politischen Rechten als auch der Linken akzeptiert war. Da Kritik ausblieb, sind von ihm auch kaum Rechtfertigungen für die angestrebten Ziele zu finden. Der Historiker William Graebner hat dieses *Democratic Social Engineering* in den USA auf die Zeit 1917–1947 datiert und auch die Kleingruppen Lewins kritisch beurteilt, wobei Graebner den aus Deutschland zwangsemigrierten Kurt Lewin als „prominenten Vertreter der im wesentlichen amerikanischen Form der sozialen Kontrolle" ansah (Graebner 1986: 137). Miriam Lewin (1987) hat heftig gegen Graebners Auffassung protestiert, vor allem gegen den Vorwurf der Manipulation in der Aktionsforschung ihres Vaters. Es gibt aber gute Argumente, Graebner zu folgen, wie besonders Mel van Elteren (2007) gezeigt hat. Die Auswirkungen der Sozialtechnologie und Gruppendynamik Lewins auf die angewandte Psychologie, die Erziehungswissenschaften, Sozialarbeit und auf Unternehmensmanagement sind bis in die Gegenwart stark geblieben. Der Bezug dieser sozialtechnologischen Aktivitäten zur topologisch begründeten Feldtheorie ist dagegen insgesamt gering.

[7]Der Titel dieser Konferenz wird uneinheitlich angegeben. Der Begriff *Cybernetics* von Norbert Wiener war noch nicht im Titel. Er kam erst später auf. Die interdisziplinären Konferenzen wurden von der Josiah Macy, Jr. Foundation finanziert (Protokolle s. Pias 2003; 2004).

zweiten Macy-Konferenz im Oktober 1946 „Teleological Mechanisms in Society"
sprach Lewin über sein Konzept des Feldes und andere Begriffe der Gestaltpsy-
chologie und Sozialpsychologie. Auch während der 3. Konferenz im März 1947
sollte Lewin referieren. Er starb jedoch kurz zuvor im Februar 1947. Diese Abfol-
ge der Ereignisse ist möglicherweise bedeutsam, weil Lewin im Sommer 1946 in
einem Weiterbildungskurs mit Regierungsangestellten des Staates Connecticut
Techniken zur Reduzierung von Vorurteilen eingeübt hat. Miriam Lewin:

> Dort entdeckte Lewin zunächst die Wirkungen, die das Feedback auf Grup-
> penmitglieder hatte, indem am Ende des Tages das durchgegangen wurde,
> was in den einzelnen Sitzungen mit den Gruppenmitgliedern passiert war.
> Zur Überraschung führte solches Feedback zu starken Veränderungen in
> den nachfolgenden Einstellungen. (Lewin 1998: 17)

Die Begebenheit dieser Entdeckung ist immer wieder zur Geschichte der Ange-
wandten Gruppendynamik erzählt worden. Genauere Recherchen würden viel-
leicht erkennen lassen, dass Lewins Teilnahme an der Macy-Konferenz an der
Entdeckung einen nennenswerten Anteil hatte.[8]

Kurt Lewin selbst jedenfalls hat „die gewaltige pädagogische Wirkung tief be-
eindruckt, die diese für den Zweck der wissenschaftlichen Erfassung bestimmter
Evaluationsmeetings auf den Trainingsprozess hatten" (2009: 255). Genutzt, ver-
bessert, auch zum Allerweltsbegriff verwässert wurde *Feedback* als „kommuni-
kative Schlüsseltechnologie" allerdings erst nach Lewins Tod (s. Bröckling 2017:
197–221).

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[8]Die von Brand überlieferte, eher oberflächliche und spottende Erinnerung von Margaret Mead
an Kurt Lewin (s. Clemens Knobloch Knobloch 2021 [this volume]: 28, dem ich den Hinweis
auf diese Passage bei Bröckling verdanke), wirft Fragen nach der Art des Gesprächs und der
Qualität der Arbeit von Brand auf. Die Kulturanthropologin Margaret Mead kannte Lewin
seit 1935 persönlich und hat später mit einer umfassenden und positiven Darstellung ihre Zu-
sammenarbeit mit Lewin in verschiedenen Projekten die Fähigkeiten und Leistungen Lewins
differenziert bewertet (Mead 1967). Diese Darstellung lässt erkennen, dass Mead skeptisch be-
züglich der Topologie war, dass sie dagegen aber mit großer Begeisterung gemeinsam mit
Lewin verschiedene Projekte durchgeführt hat. In einer der experimentellen Untersuchungen
über die Umstellung von Ernährungsgewohnheiten übernahm die Kulturanthropologin sogar
die Rolle einer Versuchsleiterin. Mead (1967: 8): „Ich weiß, wenn Kurt noch leben würde, dann
würde ich aus meiner letzten Untersuchung ein Problem mitbringen, das für mich besonders
dringend wäre, dann würde er mir das Gefühl geben, dass dies genau das wäre, woran wir als
nächstes arbeiten müssten."

6 Beziehungen der Attributionstheorie Heiders zur Feldtheorie

Heiders Attributionstheorie auf der Grundlage seiner Naiven Handlungsanalyse wurde schon skizziert. In welchem Verhältnis steht diese zu Lewins Feldtheorie? Heider hat einen Gedanken des Lewinschülers Ferdinand Hoppe aufgegriffen. Hoppe untersuchte den Anspruch, den Personen an ihre eigene Leistung stellten, indem sie sich nach Erfolg oder Misserfolg einer neuen Aufgabe stellten. Der Handlungseffekt „Leistung" war nur so weit psychische Realität, als er nicht als „Zufall" erlebt, sondern der eigenen Person „zugerechnet" wurde (Hoppe 1930: 60f.). Diese Zurechnung ist es, die im Mittelpunkt der Attributionstheorie steht.

Heider unterscheidet zwischen Person- und Umweltfaktoren in der Bewertung von Ereignissen, Handlungsergebnissen usw. Er nimmt hier Bezug auf Lewins universelle Verhaltensgleichung (1936: Kapitel 5), in der das Verhalten (V) als Funktion (f) der Person (P) und der Umwelt (U) angesehen wird und in der Gleichung $V = f(P,U)$ zum Ausdruck gebracht wird. Heider sieht in sehr ähnlicher Weise das Ergebnis einer Handlung (x) als Ergebnis wirksamer persönlicher Kraft und wirksamer Umweltkraft. Es gilt nach Heider (1958/1977: 102) daher:

$$x = f(\textit{ff} \text{ Person}, \textit{ff} \text{ Umwelt})$$

Die persönliche Kraft oder Macht liegt nach Heider vor allem in der Fähigkeit der Person, auch in deren Temperament. Heider „ist versucht" (1958/1977: 103) die Beziehung von Person und Umwelt in dieser Gleichung als *additiv* zu beschreiben. Ein *Handlungsergebnis*, das ausschließlich Resultat einer der beiden Kräfte ist (wobei der andere Wert Null ist), ist für ihn immerhin vorstellbar. Heider nennt hier als Beispiel den Segler, der einschläft, aber vom Wind (= wirksame Umweltkraft) an Land getrieben wird.

Diese Darstellung entspricht in einem wichtigen Punkt nicht der Feldtheorie, denn Lewin nutzte einen Kunstgriff, um P und U in seine Formel zu bringen: Nach ihm werden P und U nicht addiert, multipliziert oder in anderer Weise verrechnet; Person und Umwelt sind nie vollständig zu separieren, da der Lebensraum (Lr) immer nur durch die Wahrnehmung der Person (P) bestimmt wird.[9]

Lewins topologische Darstellungsformen waren für Heider

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[9]Der Begriff „Lebensraum" wurde in der Nazizeit als Kampfbegriff verwendet. Diese Verwendung aus dem „Wörterbuch des Unmenschen" hatte mit Lewins Topologischer Psychologie nichts zu tun, macht allerdings die heutige Verwendung des Begriffs „Lebensraum" (*life space*) ein wenig missverständlich. In der Psychologie ist es allerdings üblich, bei dem ursprünglichen Begriff zu bleiben.

ein wunderbares Mittel, über verschiedenartige Handlungen und Lebenssituationen nachzudenken und sich mit anderen über sie zu verständigen. Sie erlaubten eine Sprache, die exakter und strenger als die normale war und zu neuen Formulierungen führte, die zu untersuchen waren. (Heider 1977: 75)

Doch benutzte Heider die topologische Psychologie nur in Teilen. Sie war für ihn „eine Art Leitbild" (1977: 155). Nach einiger Zeit kam er zu dem Ergebnis, dass die topologische Psychologie sich nicht zur Analyse von Problemen eignete, die mehr als eine Person betrafen.

Dies besprach Heider auch mit Lewin selbst. Lewin habe diese Begrenzung dann zugestanden und am Ende seiner Arbeit über psychologische Kräfte (Lewin 1938) als Einschränkung benannt. Tatsächlich endet diese Arbeit mit der Aussage:[10]

An adequate treatment of social problems, especially social conflicts, however, makes certain distinctions necessary, particularly that between "own" and "foreign" forces, which we have merely mentioned". (Lewin 1938: 210)[11]

Gern hätte Heider länger und eingehender mit Lewin über seine Ideen und über Lewins nicht vollendete Theorie gesprochen.

Es ist traurig, dass er sie nicht zum Abschluss bringen konnte, und er starb gerade, als er wieder zu theoretischeren Studien zurückkehren wollte. Ende 1946 war er wieder bereit, über grundlegende Fragen zu sprechen, nachdem er einige Jahre mit praktischen Angelegenheiten und angewandter Sozialpsychologie verbracht hatte. Nach 6 oder 8 Jahren hatte ich mit ihm wieder eine gute Diskussion, kurz vor seinem Tod. (Heider 1988: 40)

Die Frage drängt sich auf, was geleistet werden muss, um die topologische Theorie so zu gestalten, dass Konflikte zwischen zwei oder mehr Personen darstellbar sind. Sieht man den Lebensraum als objektiv gegebene Umwelt, so ist dies

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[10]Es ist dies die einzige, umfangreichere Schrift von Lewin, die bislang nicht ins Deutsche übersetzt wurde.

[11]Noch Jahrzehnte nach Lewins Tod hat sich Heider mit Lewins Theorien befasst. In den ersten Bänden seiner „Notebooks" (1987; 1988) wird Lewin besonders oft genannt. Ursprünglich waren diese Notizen nicht für die Öffentlichkeit gedacht. Heiders Notizen sind daher teilweise verkürzt oder aphoristisch, manchmal sind es nur Selbstaufforderungen, bestimmte Arbeiten oder Autoren zu lesen oder zu vergleichen.

kein Problem, wenn diese Umwelt für die beteiligten Personen die gleiche ist. Lewin hat selbst hierfür das anschauliche Beispiel von zwei sechsjährigen Jungen gegeben, die gemeinsam in der Badewanne sitzen und von denen einem das Toben des andren zu weit ging und er daher einen fiktiven Strich als Trennungslinie über das Wasser zieht, um die „Territorien" abzugrenzen (Lewin 1936: 43; 1969: 62). Dies ist ein hübsches Beispiel, um die Feldtheorie mit ihrer Jordankurve anschaulich zu machen. Es ist jedoch keine streng topologisch-psychologische Betrachtung, da es nach der Theorie nicht um die objektive Umwelt geht, sondern um den Lebensraum einer Person. Dieser ist immer durch die Wahrnehmung der Person definiert, und zwei oder mehr Personen unterscheiden sich in ihrer Wahrnehmung der gleichartigen Situation erheblich.

Lewin hat in entwicklungspsychologischen Arbeiten den sich verändernden Lebensraum einer Person dargestellt und für diese Person jeweils für verschiedene Phasen mehrere Jordankurven nebeneinander, auch verschränkt übereinander gezeichnet. Da die topologische Darstellung der Beziehungen von zwei Personen schwierig ist, ist in dieser Sichtweise auch die Entwicklung einer Theorie der Interaktionen durch Lewin unterblieben. (Seine Gruppendynamik kann zwar auch feldtheoretisch verstanden werden – vgl. Binder 2019 –, Gruppen waren aber nicht Heiders Forschungsinteresse.)

Ohne dass es ihm immer bewusst gewesen wäre, suchte Heider lange nach theoretischen Erklärungen von Beziehungen und Interaktionen zwischen zwei Personen. Hierzu studierte er die Philosophie von Baruch Spinoza, dessen Ziel es war, eine Moralphilosophie „nach den Regeln der Geometrie" zu entwickeln. Bei Spinoza fand Heider Aussagen, die ganz nah an die Balance-Theorie heranführten: z.B. die Aussage, dass wir Sympathie gegenüber Personen empfinden, die uns in unseren guten Zielen unterstützen. Doch wandte sich Heider von Spinozas Philosophie schließlich ab, weil ihm die *allgemeine* Gesetzmäßigkeit fehlte, die er suchte (Heider 1978: 114). Die für ihn befriedigende Lösung fand er schließlich, indem er auf Max Wertheimers einheitsbildende Faktoren (Nähe, Ähnlichkeit usw.) zurückgriff und diese auf soziale Beziehungen anwandte. So konnte er *unit-* und *sentiment-Beziehungen* zunächst als gleichartig behandeln und in sein Balancemodell aufnehmen.

Die These, Heider habe sich enger an sozialpsychologischen Fragen orientiert als Lewin, ist vielleicht überraschend. Wenngleich sich die empirische Mainstream-Sozialpsychologie nicht am Wissenschaftsverständnis von Lewin orientiert hat, so wird er doch als Mitbegründer einer experimentellen Sozialpsychologie angesehen und geradezu als Säulenheiliger verehrt (Danziger 1990a,b). Ein Vergleich, wer denn der wichtigere oder gar bessere Sozialpsychologe war, macht allerdings keinen Sinn.

Helmut E. Lück

Einen Vergleich der Theorien von Heider und Lewin hat Bernd Schlöder (1988) durchgeführt. Ihn interessieren die Strukturen der Theorien, nicht deren praktische Anwendung. Für die Feldtheorie nimmt er vier Ebenen der wissenschaftlichen Diskussion an: (1) die wissenschaftstheoretische Ebene, (2) die grundbegriffliche Ebene, (3) die Ebene der empirischen Theorie und (4) die empirische Ebene. So erinnert Schlöder an Lewins Herkunft aus dem Neukantianismus, seine naturwissenschaftliche Begriffsbildung, den angestrebten Fortschritt wissenschaftlicher Erkenntnisse und Lewins normativen Charakter seiner Methodologie. Analog zur Feldtheorie gliedert Schlöder für Heider vier logische Niveaus: (1) die phänomenologisch-analytische Ebene, (2) die Ebene der Grundbegriffe (Notation und Terminologie), (3) die Ebene der Hypothesen und (4) die Ebene der empirischen Prüfung.

Der Vergleich der beiden Ansätze führt Schlöder zu einer abschließenden Bewertung: Das gemeinsame Kennzeichen der Theorien von Lewin und Heider ist nicht in erster Linien ein Satz von empirisch prüfbaren Annahmen und Hypothesen, sondern vor allem eine umfassende begriffliche Systematik (Schlöder 1988: 240). Hierin folgt Heider Lewin. Heider kritisiert an Lewin die Begrenztheit der Topologie als problemadäquate Repräsentation psychischer Tatsachen. Dagegen findet Schlöder, dass Heiders Handicap komplementär ist: Es gelingt ihm nicht, seine *common sense*-Psychologie in ein wissenschaftliches System zu überführen, obwohl dies dem eigentlich naturwissenschaftlichen Erkenntnisideal Fritz Heiders entsprochen hätte.

Die gut begründeten kritischen Ausführungen Schlöders erklären zum Teil, warum die Ansätze beider Psychologen nicht unverändert fortgeführt, sondern immer wieder „handlicher" gemacht wurden.

Jüngere Kollegen griffen Heiders Ideen auf. Insbesondere die Theorie von Harold H. Kelley fand weite Verbreitung. Sie war eine Art dreidimensionale „Operationalisierung" der Ideen Heiders. Menschliches Denken bekam bei Kelley den Charakter einer Varianzanalyse von Wahrnehmung und Bewertung eines Sachverhaltes in der Zeit. Heider selbst begrüßte diese Weiterentwicklung seiner Theorie.

Da bald weit häufiger auf Kelley als auf Heider Bezug genommen wurde, haben sich Kritiker, wie besonders Betram F. Malle (2008), nach den Gründen dafür gefragt. Sie fanden, dass Heider missverstanden wurde, und dass es sich lohnen würde, auf Heiders eigentlichen Ansatz zurückzugehen. Dafür hat implizit auch Gerd Gigerenzer (1994) argumentiert. Er hat deutlich gemacht, dass die „plötzliche" (1994: 114) sog. kognitive Wende der amerikanischen Psychologie um 1960 von einer Bereitschaft begleitet war, frühere, eher phänomenologische

Ansätze in experimentelle Modelle mit unabhängigen und abhängigen Variablen umzusetzen. Die psychologische Methodenlehre mit experimentellem Vorgehen, Stichprobenvergleichen und inferenzstatistischen Prüfungen war Mitte der fünfziger Jahre zum methodischen Standard der amerikanischen Psychologie geworden. So wurde auch die neue sozialpsychologische Thematik der Ursachenzuschreibung diesem Methodenparadigma unterworfen. Aber Albert Michotte (1946), Fritz Heider und die Gestalttheoretiker waren nicht auf kausales Denken ausgerichtet gewesen (Gigerenzer 1994), auch Lewin war bewusst von positivistischem Denken entfernt geblieben. Nun, mit dem Beginn der kognitiven Psychologie wurden diese frühen Bestrebungen passend gemacht und zum Gegenstand einer experimentellen Psychologie. Den Vorstellungen von Heider mit seiner Einbeziehung einer *common sense*-Psychologie entsprach das nicht. Trotz der Wertschätzung für die kognitiven Richtungen der Psychologie und trotz seiner bescheidenen Art war Heider skeptisch. Zu Ulric Neisser, der als Mitbegründer der kognitiven Psychologie gilt und das erste Buch mit dem Titel „Cognitive psychology" verfasste, notierte Heider für sich: „*Neisser*: treats only half of cognitive (the Köhler half?), he left out *Lewin* life space, cognition of situations" (1987: 565).

7 Erinnerung an Kurt Lewin

Fritz Heider hat 1967 Erinnerungen an Kurt Lewin aufgeschrieben, die ein wenig mehr über Lewins Persönlichkeit und das Verhältnis der befreundeten Kollegen zueinander kurz vor Lewins Tod verraten:

> Seine Art zu arbeiten war spielerisch; nicht belanglos, aber nie pedantisch. Alles machte mehr Spaß, war lebendiger und farbiger, weil er sich in eine Sache einbrachte und andere einbezog. Er wurde immer hektischer, machte zehn Sachen auf einmal, und im letzten Sommer bevor er starb, 1946, waren die zwei Wochen, die wir zusammen auf Martha's Vineyard zusammen waren, etwas frustrierend, weil Lewin so hektisch beschäftigt war. (Heider 1967: 3)

> Das letzte Mal sah ihn meine Frau, im Januar oder Februar kurz vor seinem Tod, ich war im Krankenhaus. Kurt brachte sie mit dem Auto nach Hause und er war wütend, weil ich kein Projekt übernehmen wollte. Er sagte: „Ich kann ihm Geld besorgen, alles was er will. Er würde nur Mitarbeiter benötigen, er solle diese an die Arbeit setzen und dann könne er gehen und sein Buch schreiben". Das war die Art, wie Kurt arbeitete, und das war es, was ihn umbrachte. Er hatte seine Kerze an mehreren Stellen angezündet. (Heider 1967: 5)

Heiders Eindruck ist, dass Lewins Visionen

> einen Reichtum impliziter Bedeutung [haben], der noch nicht ausgeschöpft ist, und dass es daher die Aussicht auf weitere Entwicklung gibt. (Heider 1967: 9)

Danksagung

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Feedback, Sozialkybernetik, Democratic Social Engineering. Kommentar zu Helmut Lück

Clemens Knobloch

1. Helmut Lück 2021 [this volume] argumentiert in seinem Beitrag, Kurt Lewins topologische und feldpsychologische Axiomatik sei letztlich eher geeignet für die "monologische" Fragestellung einer Psychologie des (individuellen) Handelns. Fritz Heiders Axiomatik hingegen sei insofern besser für die Zwecke einer genuinen Sozialpsychologie zugeschnitten, als Heider das Feld, in welchem die Individuen sich orientieren und handeln, von vornherein mit anderen Akteuren (und deren Orientierungen) ausstattet – und nicht bloß mit Kraftfeldern, Vektoren, Hindernissen, Aufforderungscharakteren etc., weshalb sie in Teilen dem Anliegen der Sozialpsychologie besser entsprechen als Lewins Feldtheorie. Ich halte diese Argumentation für schlüssig, was die explizite Theorie- und Modellgeschichte der Psychologie betrifft. Ergänzungsbedürftig scheint mir diese Argumentation jedoch in einem anderen, eher rezeptionspraktischen Punkt: Lewins demokratiepolitisches Engagement setzt die gesamte Sozialpsychologie (oder vielleicht eher die Mikrosoziologie?) axiomatisch auf ein anderes Gleis. Die im Jahr 1938 mit Lippitt begonnenen Gruppenexperimente (am Institute of Child Welfare der University of Iowa) führen zur Entdeckung der „Gruppe als eigenständiger Gestalt" und als „Interventionsfeld" (in der Formulierung von Binder 2021). Um 1938 herum datiert auch Lewins Hinwendung zum Praxisfeld der „Demokratieexperimente", zum *democratic social engineering* im weiteren Sinne (vgl. Binder 2021, Bröckling 2017b). Das handelnde Individuum changiert mit einem Male vom eigenständigen Subjekt zu einer Funktion der Gruppenatmosphäre und des sozialen Kraftfeldes. Angelegt ist dieser Perspektivenwechsel in der experimentellen Praxis der Lewin-Gruppe. Helmut Lück berichtet von Tamara Dembos Versuchen, bei denen die Versuchspersonen mit unlösbaren Aufgaben konfrontiert wurden (und von den „Warte-Experimenten", bei denen die Versuchs-

personen ohne explizite Instruktion mit einer weitgehend undefinierten Situation konfrontiert waren, die sie nicht ohne Weiteres verlassen konnten). Diese Praxis erinnert bereits an Garfinkels *breaching*-Versuche. Es gibt auch Hinweise darauf, dass Garfinkel mit den einschlägigen Versuchen von Lewin und Dembo vertraut war, als er seine *breaching*-Experimente entwarf. Jedenfalls notiert er an einschlägiger Stelle in den „Studies" von 1967:

> I designed a procedure to breach these expectancies while satisfying the three conditions under which their breach would presumably produce confusion, i.e., that the person could not turn the situation into a play, a joke, an experiment, a deception, and the like, or, in Lewinian terminology, that he could not "leave the field". (Garfinkel 1967: 58)

Das war in der Tat auch eine entscheidende Neuerung in der experimentellen Praxis von Lewin und Dembo: dass die Versuchspersonen nicht „aus dem Feld gehen" konnten.

Anders als in der experimentellen Praxis der Mainstream-Sozialpsychologie gilt der Versuchsleiter mit *seinem* Verhalten als eine für die praktischen Resultate entscheidende Größe. Kurz: Das Experiment selbst wird als eine höchst spezielle soziale Situation reflektiert und gilt nicht mehr als „neutraler" Wahrheitsort.

2. In einer begriffsgeschichtlichen Skizze zum Schicksal des kybernetisch-technischen Begriffs *feedback*, der trivialisiert, aber höchst erfolgreich und resonanzstark in der Gruppendynamik und in der populären Kommunikationswissenschaft der Nachkriegszeit kontinuiert wird, berichtet Bröckling (2017a: 197f.) über ein Gespräch zwischen Margaret Mead und Gregory Bateson, in dem die beiden von der Teilnahme Kurt Lewins an der ersten Macy-Konferenz 1946 erzählen. Das Thema der Konferenz ist programmatisch einschlägig für die (sagen wir) „sozialtechnologische" Wendung kybernetischer Modelle im fraglichen Zeitraum. Es lautet: „Circular causal, and feedback mechanisms in biological and social systems". Kurt Lewin wird in diesem Gespräch zwischen Mead und Bateson (aus dem Jahr 1976) mit einer Mischung aus Parodie und Respekt erwähnt: Das Modell *feedback* sei von ihm, der keine fremde Sprache wirklich gut verstanden habe, reduziert worden auf die Idee, dass alles, was man in einen Gruppenprozess nachträglich verbal-kommunikativ einbringt, als „feedback" für diesen Gruppenprozess zu werten sei. Bateson fügt hinzu: „In the small group cult, feedback now means either telling people what they did, or answering" (zitiert nach Bröckling 2017a: 197). Lewin identifiziert den Begriff offenbar mit den verschiedenen Verhaltensstilen der Gruppenleiter in den „Democracy Experiments", die in den

Iowa-Studien von 1938ff auf ihre praktischen Ergebnisse hin verglichen worden sind.[1]

Bröckling (2017b) weigert sich, die Begriffskarriere von *feedback* in Kommunikation, Management und Gruppendynamik als Trivialisierungs- und Normativierungsgeschichte zu erzählen. Den steilen Aufstieg des regeltechnischen Ausdrucks motiviert er eben mit der zentralen Position, die *feedback* „in den Praxistheorien professioneller Menschenführung annahm" (Bröckling 2017a: 198). Lewins Übernahme des Begriffs in die Gruppendynamik könnte durchaus das Schlüsselereignis für diese Begriffskarriere gewesen sein. Bröckling notiert darüber hinaus, dass Lewins Interesse an der sozialtechnologischen und sozialreformerischen Implementierung von Rückkopplungsprozessen in der Gruppendynamik auch von seiner Bekanntschaft mit Norbert Wiener herrühren könnte, den er 1945 am MIT getroffen hatte (2017a: 200ff.).

Aus heutiger Sicht und vor dem Hintergrund hoch professioneller massendemokratischer Manipulationstechniken wirken Lewins drei Phasen des demokratischen Gruppenprozesses höchst manipulativ. In der ersten Phase (*unfreezing*, "Auftauen") werden eingeschliffene Erwartungszusammenhänge irritiert. Sie dürfte durch Dembos Warte-Experimente inspiriert sein. Der Gruppenleiter gibt in dieser Phase eben kein *feedback*, er erzeugt ein soziales Vakuum durch Schweigen (Bröckling 2017a: 205). In der zweiten Phase (*changing*) wird die Gruppe auf einem neuen Niveau kalibriert und stabilisiert – durch die Vorgaben und Anregungen des demokratischen Leiters. Eigentlich bildet sich die Gruppe *als Gruppe* erst in dieser Phase. Im dritten Schritt (*refreezing*) schließlich wird die Gruppe auf einem neuen Verhaltensniveau stabilisiert. Es ist das feedback des demokratischen Leiters, das die Gruppe als solche kalibriert.

Dass Bröckling (2017b) seine feedback-Studie in einem Band über „Pastoralmacht" veröffentlicht, hat insofern seine Richtigkeit. Die kommunikativen Techniken der „demokratischen Gruppenpolitik" sind durchweg pastoral in dem Sinne, den bereits der Anreger moderner Diskursanalysen, Kenneth Burke, herausgestellt hat. Das Pastorale (so Burke 1973 [1941]: 422) besteht im (kommunikativen) Kern in einer Umkehrung der Werte. Die Schwachen, Unterworfenen, Hilfsbedürftigen werden gefeiert und aufgewertet für die unermüdliche Tapferkeit, mit der sie sie ihnen auferlegten Verpflichtungen erfüllen. Darin besteht ihre innere Größe. Es ist natürlich auch der Krieg, der eine solche (verpflichtende und

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[1]Das Gespräch zwischen Mead und Bateson ist dokumentiert in Pias (2003: 301-312). Dass Lewins Verständnis von *feedback* Jargon geworden sei und sich die optimistischen Erwartungen der Zeit in die (kybernetische) Perfektionierung der Kommunikation nicht erfüllt hätten, notiert Mead auch in Mead (1964: 272f.).

einbindende) Aufwertung der kleinen Leute nahelegt. Carrier schreibt, auf Burkes eher literarische Analyse des Pastoralen verweisend:

> When the pastoral moves out of literature and into the group's orientation, it provides a nobility for the subordinate by showing that in their subordination to the hierarchical principles they become equal to the powerful in society, who are powerful only because they too subordinate themselves to those principles. Thus the pastoral provides a kind of unity and ironic equality across social classes, while confirming the subordinated in their subordination and the dominant in their domination. (Carrier 1982: 55)

Der demokratische Gruppenprozess ist, so gesehen, Bestandteil einer Machttechnologie, die in den USA der 1940er Jahre Gestalt annimmt und sich seither ausgebreitet hat. Was Bröckling (2017a: 221) zu der Schlussfolgerung bringt, das trivialisierende Missverständnis von *feedback*, das Mead und Bateson Lewin unterstellen, sei rezeptionsgeschichtlich keineswegs „falsch", sondern im Gegenteil der Auftakt zu all den höchst demokratischen Rankings, Evaluationen, Selbstevaluationen und Akkreditierungen, von denen wir heute umgeben sind.

3. Margaret Mead, die 1976 im Rückblick über Lewins simplifizierte Version des feedback spottet, hat freilich im Jahr 1942 (unmittelbar nach dem Kriegseintritt der USA) selbst zusammen mit Lewin eine gruppendynamische Studie für das „Committee on Food Habits" (und im Auftrag des NRC) durchgeführt (vgl. Binder 2021). Wie sehr diese Studie vom sozialtechnologischen Optimismus und vom Pathos einer Wissenschaft im Dienste des demokratischen Wandels getragen war, erhellt aus den Formulierungen Lewins in einem Memorandum an das „Committee on Food Habits", aus dem Binder (2021) zitiert. Da geht es um „efficient means for democratic actions in various fields" und um die Implementierung „for the much needed translation of expert knowledge into social action". Konkret entwickelten Mead und Lewin eine effektive Technik zur Beeinflussung von Hausfrauen, welche die Ernährungsgewohnheiten ihrer Familien auf den Verzehr von Innereien einstellen sollten – für den Fall kriegsbedingter Versorgungsengpässe. Auf ihre transformatorische Wirkung untersucht werden autokratische und demokratische Stile der Gruppenführung (wie auch schon in den Kindergruppen in Iowa) – mit dem Ergebnis der praktischen Überlegenheit demokratischer Führungsstile, auch über den später hinzugenommenen „anarchischen" laisser-faire-Stil, bei dem sich der Gruppenleiter weitgehend bedeckt hält.

4. Das Programm mit dem label „efficient democracy" dürfte auch Einfluss gehabt haben auf David Riesmans (zuerst 1950 erschienene) soziologische Studie

über den Wandel in den Sozialcharakteren der USA, *The Lonely Crowd*, in der er eine quasi-evolutionistische Abfolge von Traditionsleitung, Innenleitung und Außenleitung postuliert. Der massendemokratische Charakter des Außengeleiteten ist in seiner Vorherrschaft die Voraussetzung dafür, dass die sozialtechnologischen Gruppenpraktiken Lewins und vieler seiner Zeitgenossen effizient funktionieren. Ein gewisses Maß an sozialer „Feldabhängigkeit" ist Vorbedingung dafür, dass Gruppenprozesse Verhaltensänderungen bewirken können. Wer als innengeleiteter Gewissens- und Überich-Mensch in eine Gruppe eintritt, der wird seinen inneren Kompass nicht so leicht neu adjustieren. Der soziale Typus des *other-directed* hingegen ist auf die Erwartungen der anderen Gruppenmitglieder eingestellt und sucht deren Anerkennung.

Die Kleingruppen der optimistisch-demokratischen Sozialtechnologen (so argumentiert Bröckling 2017b) sind experimentelle Gemeinschaften aus Individuen, die in der US-Migrationsgesellschaft nicht darauf rechnen können, durch vorgängige und geteilte kulturelle Traditionen verbunden zu sein. Während Gemeinschaftsideologien in Deutschland (von F. Tönnies bis zu H. Plessners textueller Warnung vor den „Grenzen der Gemeinschaft"; Plessner 1981 [1924]) nostalgisch eine vormoderne Vergangenheit verklären, sind Gemeinschaften in einer Migrationsgesellschaft ein demokratisches Projekt – oder sie können es sein. Es versteht sich auch vor diesem Hintergrund, dass der sozialtechnologische Demokratie-Optimismus Lewins bei den exilierten Soziologen der Frankfurter Schule auf Skepsis und Misstrauen stoßen musste. Lewins sozialpsychologische Wirkungsgeschichte wurzelt weniger in der Feldtheorie als in der „science of democracy" der 1940er Jahre (Rose 1998).

Ist die Handlung in Lewins topologischer Feldpsychologie ein Produkt der aktuellen Feldkräfte – so wie sie in der Orientierung des Handelnden repräsentiert sind (entsprechend dem *scene-agent-ratio* bei Kenneth Burke 1969 [1945]), so verschiebt der Gruppenprozess die Akzente dergestalt, dass der Akt selbst ins Zentrum rückt, der für die Gruppenmitglieder das Kraftfeld der Orientierungen neu definiert (*scene-act-ratio* bei Kenneth Burke 1969 [1945]). In den Sozialtechniken der Gruppendynamik wird sichtbar, dass der individuelle Akteur zugleich marginalisiert, und, sofern er gruppenkonform agiert, auch aufgewertet wird. Diese theoretische Schwerpunktverschiebung spiegelt sich begrifflich im Wechsel der Leitterminologie von *attitude* (= dem einzelnen Akteur zugeordnet) zu *atmosphere* (= der Gruppe zugeordnet); vgl. hierzu erneut Binder (2021).

5. Helmut Lück weist darauf hin, dass Fritz Heider psychologiehistorisch in enger Verbindung mit Attributions- und Balancetheorie steht. Vor allem die Studie von Heider & Simmel (1944) ist attributionstheoretisch einschlägig und wirkungsgeschichtlich stark. Wenig untersucht ist meines Wissens dagegen, ob der

Attributionskomplex, der ja beansprucht, die naiven Praktiken zu rekonstruieren, mittels derer sich die Individuen eigene und fremde Handlungen und Ereignisse erklären, nachweisliche Wirkungen auf den *accountability*-Komplex bei Garfinkel hatte. Allerdings nimmt die akademische Attributionspsychologie in den USA dann alsbald eine andere Richtung und entfernt sich von der Axiomatik Heiders, nach der die Alltagspraktiken der Teilnehmer Basis sowohl der naiven als auch der wissenschaftlichen Psychologie zu sein haben.

Wiewohl Fritz Heider in seinen Hamburger Jahren (von 1927 bis 1930) bei William Stern ebenfalls in engem Kontakt mit den Anfängen der „angewandten" Psychologie in Deutschland war, bleibt er, Sohn einer liberalen Grazer Bürgerfamilie, im Vergleich zu Lewin ein philosophisch-grundsätzlicher Theoretiker, ein bescheiden gewordener Gelehrter der deutschen bildungsbürgerlichen Professorentradition. Lewin hingegen, aus einfachen Verhältnissen stammend und mit seinen (frühen) sozialistischen Neigungen zur praktischen Umwälzung der Verhältnisse hingezogen, greift in den USA begierig nach einer demokratischen Wissenschaft, die wirksam und eingreifend werden möchte. Die allgemeine Stimmung, der Lewin in den Jahren nach dem Kriegseintritt der USA folgte, ist greifbar auch bei Garfinkel (2019 [1942]), dessen „Kriegseinsatz" in der aus dem Boden gestampften Sphäre von Luftwaffe und Flugzeugreparatur den gleichen Geist der demokratischen Sozialtechnologie atmet.

Man mag aus heutiger Sicht gar nicht daran erinnern, dass die bedingungslose Mobilisierung der Wissenschaften für den patriotischen Krieg auch eine Erfindung der Nazis war, eine Erfindung übrigens, die auch die Human- und Geisteswissenschaften einbezog – freilich ganz ohne den pragmatisch-interventionistischen Dreh, der den demokratischen Modernitätsfortschritt der USA gegenüber dem „Alten Europa" anzeigt (vgl. zum Kriegseinsatz der Geisteswissenschaften im NS Hausmann 2007).

Was aus meiner Sicht Heider und Lewin als Theoretiker und Axiomatiker der Psychologie sehr eng verbindet (und beide mit Karl Bühler), das ist der Umstand, dass sie sich zentral für das „Mediale" interessieren, für die vermittelnden Instanzen, in denen sich die Erfahrungswelt der Akteure herausbildet. Medial in diesem Sinne konzipiert sind Lewins Gruppenprozesse ebenso wie sein topologischer Handlungs- und Orientierungsraum, und medial in diesem Sinne denkt Heider seit seinen theoretischen Anfängen bei Meinong. Heiders fachlicher Erstling trägt nicht zufällig den Titel „Ding und Medium" (vgl. zu diesem Komplex Wieser 2018).

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Kapitel 2

Kurt Lewin und Fritz Heider in der Vorgeschichte der US-Kommunikationswissenschaft

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Taking as its point of departure Helmut Lück's thesis that Kurt Lewin's "topological psychology" is well adapted to the purposes of analyzing and explaining individual action in the life space of a person, but much less so for the purpose of understanding group action and settings with several actors, this chapter argues that Lewin was much more enthusiastic about democratic social engeneering and the practical prospects of furthering the aims and values of democracy in the USA. These "pastoral" (Bröckling) motives were strong in the US social sciences during the 1930s and during World War II. However, from the very beginning Fritz Heider based his naive psychology on settings with several actors, a difference that is mirrored in Heider's way of analyzing concepts from everyday language as indicators of social relations between "selves" and "others". Both Heider and Lewin focus on the "medialization" of social action, but Lewin's impact in social psychology is based mainly on his democratic group projects (rather than on his topological psychology).

1 Vorab

In den 1960er Jahren war Kommunikationsforschung in der BRD-Sozial- und Humanwissenschaft ein eher exotisches Thema. Paul Watzlawicks populäre *Menschliche Kommunikation* wurde zwar, als das Buch 1969 auf Deutsch erschien,[1] wie eine Offenbarung gefeiert, aber das Bonner Institut für Kommunikationsforschung

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[1]Ursprünglich 1967 als *Pragmatics of Human Communication* erschienen.

und Phonetik (unter Gerold Ungeheuers Leitung) war mit seiner breiten sozial-wissenschaftlichen und philosophischen Fundierungsarbeit ziemlich allein auf weiter Flur. Vollkommen anders war die Lage in den Humanwissenschaften der USA, wo der Begriff *communication* in den 1950er und 60er Jahren zum hoch umkämpften Grund- und Leitbegriff einer breiten interdisziplinären Szene avancierte. Den Entstehungszusammenhang dieser Entwicklung um 1950 herum, mit dem (vorwiegend deutschen) Wissenschaftsasyl und mit den Kriegserfahrungen der Wissenschaften, expliziert Schüttpelz (2002). Wer einen Eindruck von der US-Diskussion der 60er Jahre sucht, der findet ihn vor allem in den von Lee Thayer (1967a,b; 1970) herausgegebenen Bänden und Arbeiten wie Smith (1966). In der fraglichen Szene tummelten sich Sozialpsychologen, Kybernetiker, Rhetoriker, Psychoanalytiker, Ethnologen, Ethnolinguisten, Politologen, Massenkommunikationsforscher, Spezialisten für das nonverbale Geschehen etc. Eine bemerkenswerte Ausnahme bildet das Fach Linguistik, das sich (jedenfalls in der rasch dominierenden generativ-grammatischen Richtung) für unzuständig erklärte für alle Fragen der Kommunikation.

Ganz allmählich tauchten zu Anfang der 1970er Jahre dann in der BRD die ersten Übertragungen konversationsanalytischer, ethnomethodologischer, symbolisch-interaktionistischer und ethnolinguistischer Arbeiten in der deutschen Szene auf, allen voran die beiden Bändchen der „Arbeitsgruppe Bielefelder Soziologen" (1973). In der Rolle des philosophischen Anregers trat zunächst hauptsächlich der im US-Exil an der New School for Social Research tätige Husserlschüler und Sozialphänomenologe Alfred Schütz in das fachliche Bewusstsein. Dass freilich mit der modernen US-Kommunikationswissenschaft weit mehr von dem über den Atlantik zurückkommen würde, was 40 Jahre vorher in die entgegengesetzte Richtung vertrieben worden war, blieb dem fachlichen Bewusstsein lange Zeit verborgen.

In der sprachwissenschaftlichen Szene der BRD war die (sprachvölkisch-muttersprachideologische) Sprachinhaltsforschung (Leo Weisgerber) bis weit in die 1960er Jahre hinein die herrschende Lehre, die dann aber um 1970 herum sehr rasch ihre Vorrangstellung verlor. Whorfs Variante der Ethnolinguistik wurde (theoriegeschichtlich vollkommen zu Unrecht) mit der sprachvölkischen Richtung in eins gesetzt und ging mit ihr (reputativ) unter. Gewiss, es gab auch in der späten Weisgerber-Zeit strukturalistische Inseln im akademischen Meer der Sprachwissenschaften (Hansjakob Seilers Kölner Institut für Allgemeine Sprachwissenschaft dürfte die wichtigste unter diesen Inseln gewesen sein). Auf diese Szene traf dann mit aller Macht, zeitgleich mit den populären US-Kommunikationslehren, die Generative Grammatik Chomskys. Und deren (in unserem Zusammenhang) vorstechender Zug war die konsequente Marginalisierung aller

Verbindungen zwischen „Sprache" und „Kommunikation". Das ging auch vielen Sprachwissenschaftlern zu weit, was sich in der bald einsetzenden „pragmatisch-kommunikativen Wende" äußerte.

Ziel des Projektes ist es, die esoterischen und exoterischen Bedingungen zu rekonstruieren, unter denen ein audiovisuelles Medienformat zum selbst kooperativen Werkzeug der wissenschaftlichen Modellbildung werden konnte. Unter den Projekten des SFB ist es (als historiographisches) sicher eher gestaltungs- und praxisfern, was dadurch kompensiert wird, dass es erstmals die Probleme sozial- und kommunikationswissenschaftlicher Denkstil-Genese (im Sinne von Fleck 1980 [1935]) praxistheoretisch und methodologiehistorisch reflektiert. Mediale Mittel, die sich selbst organisierende Sequenzialität mikrosozialer kommunikativer Abläufe sichtbar und erfahrbar zu machen, stehen im Zentrum.

2 Kurt Lewins handlungstheoretischer Aktualismus

Lewins „topologische Psychologie" ist keine Kommunikationstheorie, sie ist im Kern „monologisch", und auch die sozialpsychologischen Untersuchungen von Gruppenprozessen in Lewins späten, den 1940er Jahren, sind keine echten Kommunikationsanalysen im Sinne der späteren Kybernetik. Sie bahnen diesen aber den Weg. In seinen gruppendynamischen Arbeiten aus den 1940er Jahren entdeckt Lewin die kommunizierende Gruppe als ein Kraftfeld, das sich von dem des individuellen Akteurs unterscheidet:

> Er stellte sich damit explizit gegen die zu jener Zeit in der Psychologie verbreitete Ansicht, Individuen als von der Umwelt isolierte, vornehmlich von der Vergangenheit geprägte Elemente zu betrachten. Stattdessen nahm er systemische Zusammenhänge und aktuelle psychosoziale Kräftefelder in den Blick, wie sie in familiären, arbeitsorganisatorischen, therapeutischen oder militärischen Gruppenkonstellationen zu finden waren. Der Wunsch, sich zu einer Gruppe zugehörig zu fühlen und mit dieser zu interagieren, stellte für Lewin ein grundlegendes anthropologisches Bedürfnis dar, entsprechend konnte auch individuelles Handeln nicht losgelöst vom sozialen Umfeld verstanden werden. (Mareis 2018: 205)

Was ist zu verstehen unter Lewins „Handlungstheoretischem Aktualismus"? Nun, so etwas wie eine strikt synchrone Perspektive auf die „Analyse der Gesamtsituation"; in Burkes Terminologie: „scene, as perceived and structured by the actor" – „action as a result of interaction between actor and perceived scene" – Was Kenneth Burke als *scene-act-ratio* benennt, fungiert im Alltagsdenken als

Maß für die situative Angemessenheit einer Handlung. Wir sehen und berücksichtigen von Fall zu Fall Hindernisse, Umwege, Einsichten, Gestaltschließungen. Aber für unsere aktuelle Handlungsorientierung spielt weder die historische Tiefendimension der Szene noch unsere eigene historische Tiefendimension eine (bewusstseinsfähige) Rolle. Wir handeln im Hier und Jetzt. Erscheint jedoch einem anderen Teilnehmer (oder einem Beobachter) unsere Handlung seltsam, unangemessen etc., so greift er gerne zurück auf diachrone Deutungsmuster, also auf die „Vorgeschichte" des Akteurs als Determinante der Handlung. Psychoanalytische Deutungen sind anders gar nicht denkbar. Und sie beherrschen in den USA durchaus einen Teil der sozialpsychologischen Szene.

Lewins „Aktualismus" ist darum ein modelltheoretischer Affront sowohl gegen die Psychoanalyse, die alle aktuellen Handlungen „motiviert" durch ihre Fundierung in der Vor- und Frühgeschichte des Handelnden, als auch gegen die behavioristische Psychologie, die auf gelernte und wiederholte Stimulus-Response-Konstellationen zählt.

Brunswicks Kritik an Lewin lautet: der aktuelle Lebensraum sei *post-perceptual and pre-behavioral*, was die Sache insofern trifft, als die sequentielle Dynamik interaktiven Geschehens nur schwer zu repräsentieren ist in Lewins Formaten. „Gegenwärtigkeit" und „Gerichtetheit" sind die strukturierenden Faktoren im Handlungsfeld, das den Akteur in der je aktuellen Situation ausmacht (Blankertz 2017). Lewins Modell verortet die Handlung in einem Zwischenraum zwischen der wahrgenommen und gedeuteten Szene mit ihren Valenzen und den Zielen des Handelnden.

Auch zu dieser strittigen Modellkonstellation hat Kenneth Burke in seiner *Grammar of Motives* bereits einiges gesagt: Für unsere (alltägliche oder wissenschaftliche) Modellierung „enthält" die Szene sowohl den Akteur als auch die Handlung. Das lässt viel Spielraum für Variation im *scene-act-ratio*. Wir können das Verhältnis *deterministisch* deuten (die Szene bestimmt die Handlung) oder *programmatisch* (die Handlung verändert die Szene). Es ist nicht schwer zu zeigen, dass die alltäglichen Zurechnungspraktiken in diesem Verhältnis äußerst beweglich sind. Von „in dieser Lage konnte ich nicht anders als […]" bis zu „in dieser Lage wollte ich vor allem […]" gibt es zahlreiche Mischvarianten. Ich komme darauf zurück (Heider widmet sich solchen Konstellationen penibel und gründlich!).

Offenkundig ist die Herkunft der Veranschaulichungsstrategien aus der phänomenologischen und gestaltpsychologischen Tradition. Das Modell Lewins ist allenthalben die optische Wahrnehmung, und daraus folgt die idealisierte Verbildlichung und Veranschaulichung des Psychischen in topologischen, mathematisch rein darstellbaren Schemata. Es gibt insofern eine Parallele zwischen Lewins

Filmarbeit und seinen Veranschaulichungstechniken, Lewins Filme illustrieren und „verlebendigen" wiederum die topologischen Schemata. Wieser (2014) konstatiert im Werk Lewins eine konsequente Abfolge von Verbildlichungs- und Veranschaulichungsstrategien: Von der (traditionell philosophischen) Schrift über den szenischen Film als Verbildlichungsmittel hin zu den abstrakt topologischen Schemata der Feldtheorie – Heiders Filme (vor allem die Dinge zur phänomenalen Kausalität) sind bereits Entdeckungsprozeduren für naiv-alltägliche Deutungsmuster.

Im Kern geht es bei Lewin um die mathematische Veranschaulichung (perspective by incongruity!) der Größen und Kräfte, die den Orientierungs- und Handlungsraum des Einzelnen strukturieren, um Valenzen und „goals", um Hindernisse, Barrieren, die (vielleicht) umgangen oder überwunden werden können. „Kommunikativ" strukturiert ist dieser Raum aber nur in dem Sinne, dass seine Gegebenheiten eben nicht als (positivistische) Gegebenheiten wahrgenommen werden, sondern im Blick auf das, was sie den Handlungen gegebenenfalls entgegensetzen, wie sie auf seine Aktionen antworten, wie sie als Mittel in sie eingebaut und als Gefahren umgangen werden können. Das ist die Lektion der „Kriegslandschaft". Es ist aber zugleich auch die Lektion der Schimpansenversuche von Wolfgang Köhler auf Teneriffa. Das Handlungsziel (*goal*) wird zum Mittel für die perzeptive Organisation der Szene, und das Problem ist gelöst, wenn ein dynamisches Schema gefunden ist, das beide zu einer Handlungsgestalt verbindet.

Was im mathematisch-topologischen Modell nur schlecht berücksichtigt und repräsentiert werden kann, ist das, was die spätere Systemtheorie (Luhmann) als „doppelte Kontingenz" fasst: die manifeste oder latente Anwesenheit anderer Akteure (oder auch nur: Beobachter) im Handlungsraum, auf die man sich qua „Erwartungserwartung" einzurichten hat. Anders gesagt: Lewins *action space / life space* ist im Kern monologisch.

Lewins Tonfilm „Das Kind und die Welt" von 1931 illustriert, dass Kinder und Erwachsene in sehr unterschiedlichen (sagen wir: Uexküllschen) „Umwelten" / Handlungsräumen leben. Nicht allein die kindlichen Akteure sind weniger komplex, auch der Handlungsraum, wie er sich ihnen darstellt, hat andere Ziele, Hindernisse, Strukturen. Und das wird dem Zuschauer des Films deutlich und bildlich daran, wie sich das Kind in seinem Handlungsraum bewegt.

Lewins späte Arbeiten über Gruppenprozesse sind hingegen eher sozialtechnologisch und pädagogisch inspiriert. In ihnen geht es um die Optimierung sozialer Lernprozesse (vgl. hierzu Nora Binder 2021). Was von Lewins späten Arbeiten zur Gruppendynamik bekannt geworden ist (etwa durch das Bändchen *Resolving social conflicts* bzw. *Die Lösung sozialer Konflikte,* das Lewins späte sozialpsychologische Arbeiten zwischen 1939 und 1947 umfasst), sieht auf den ersten Blick aus

wie eine dankbare Verbeugung des exilierten Forschers vor der Überlegenheit der US-Demokratie, die ihn aufgenommen hat. Aber was unser Thema „Vorgeschichte der systemischen Kommunikationstheorien" betrifft, sind definitiv weitere Dinge zu berücksichtigen.

Als Schlüsseltext für das gewandelte Selbstverständnis des späten Lewin kann eine Art Nachruf gelten, den er 1946 anlässlich des Todes von Ernst Cassirer auf diesen verfasst hat (und der erst 1949, zwei Jahre nach Lewins Tod, in den USA erschienen ist; vgl. jetzt Werke I, Lewin 1981–1982: 347–364). Er handelt in der Hauptsache von der Vorbildwirkung, die Cassirers Analyse der naturwissenschaftlichen Methode und Begriffsbildung für Lewins eigene psychologische und sozialpsychologische Methodenauffassung hatte. Dass Gruppen gegenüber den sie formierenden Individuen eine reale (und vor allem: real folgenreiche) Existenz haben, illustriert er zunächst am naturwissenschaftlichen Vergleich: Moleküle haben reale Eigenschaften, die den Atomen, aus denen sie sich zusammensetzen, nicht zukommen. Und dann mit einem sehr einfachen sprachlich-kommunikationswissenschaftlichen Vergleich: Der Ausdruck „die blonden Frauen in der Stadt X" steht zunächst bloß für eine sprachlich zusammengefasste Menge von Individuen. Sobald diese Menge von Individuen aber (freiwillig oder durch äußeren Druck genötigt) in gegenseitige praktische Beziehungen tritt, wird aus der Menge von Individuen eine real existierende Gruppe, über deren Realität nicht die Individuen entscheiden, sondern die praktischen Beziehungen zwischen ihnen (Lewin 1981–1982: 357). Und dieser Zusammenhang zwischen Individuum / Atom und Gruppe / Molekül ist bei Lewin höchst dynamisch. Die (filmisch reich illustrierten; etwa bei Gesell, Spitz & Wolf) Bindungstheorien der frühkindlichen Entwicklung sieht das Neugeborene ganz selbstverständlich nicht als „Atom", sondern als Teil der „molekularen" Mutter-Kind-Dyade. Die Individuen beginnen ihre Laufbahn eben nicht als „Atome", sie hören nie ganz auf, Bestandteile sozialer „Moleküle" zu sein.

Eine Art Fazit der späten sozialpsychologischen Entwicklung Lewins in der Gruppendynamik-Phase: Seine Begriffs- und Denkbewegung führt ihn vom monologischen Lebens- und Handlungsraum des einzelnen Akteurs allmählich zu einer „systemischen" Perspektive, die sich gegen den einzelnen Akteur gewissermaßen auf die Hinterbeine stellt und ihm ihre eigenen Bedingungen vorschreibt. Während Heider eigentlich Sozialpsychologe bleibt (vgl. den nächsten Abschnitt), bewegt der späte Lewin sich in Richtung einer genuin soziologischen Disjunktion von „Person" und „Sozialsystemen" (wie bei Parsons, Garfinkel, Luhmann).

Dass auch schon der frühe Lewin der späten 1920er Jahre eine Affinität zu dem hatte, was man heute als Praxistheorie bezeichnet, erhellt aus einem zentralen

methodisch-theoretischen Text von 1927 (*Gesetz und Experiment in der Psychologie*). Da heißt es: Was der Forscher als reflektierender Philosoph für Meinungen vertritt, ist unwesentlich. Was zählt, ist, „welche Thesen in den tatsächlich zur Anwendung kommenden Methoden der Forschungsarbeit implizit enthalten sind" (Lewin 1927: 378). Die (experimentelle) Praxis des Forschers prägt seine wissenschaftliche Begriffsbildung. Induktive Schlüsse laufen nicht von vielen auf alle Fälle, sondern von einem exemplarischen Fall auf alle *gleichartigen* Fälle. Für die Aporien, die in der Versprachlichung empirisch-experimenteller Befunde lauern, hat er ein feines Bewusstsein. Die Aussage, ein anderer Vertreter desselben konzeptuellen Typus habe die gleichen Eigenschaften, sei eine einfache Tautologie, heißt es da (Lewin 1927: 391). Es geht beim exemplarischen Einzelfall um diejenigen Veränderungen der Konstellation, die für das Ergebnis faktische Folgen haben (hier lauert schon die spätere Formel von der „difference that makes a difference" bei Luhmann und seinen systemtheoretischen Gewährsleuten). Phasenfolge, Sequenzierung, Korngröße in der Handlungsanalyse werden in diesem Schlüsseltext traktiert. Lewin notiert: Je kleiner man die Korngröße der Handlungssegmentierung wählt, desto „kausaler" erscheint uns die Abfolge der Segmente. Je größer wir die Korngröße ansetzen, desto mehr motivationale und intentionale Kontingenzen müssen wir annehmen. Wissenschaft ist immer „der nächste Schritt" in der Überschreitung dessen, was wir bereits wissen, und dieser nächste Schritt belehrt uns darüber, was alles beim Alten lässt und was einen Unterschied macht.

3 Fritz Heider oder: Film als Heuristik und Projektionsfläche für sozial-aktionale Muster

Fritz Heider schafft den Durchbruch zu Konstellationen, die mehr als eine Person betreffen, er trägt der Tatsache Rechnung, dass die wichtigsten Größen im Handlungsraum des Einzelnen andere Akteure sind (was sich in den topologischen Schemata nur schwer darstellen lässt; Lück 1996: 76ff.). In Heiders sozialpsychologischem „Formular" fungieren immer „p" und „o", *person* und *other*, die durch Gleichgewichte, geteilte oder nicht geteilte Zu- oder Abneigung, gemeinsame Relationen und Bewertungen verbunden sind.

Units, relations, balance im Spiegel des Gebrauchs alltagssprachlicher Konzepte, die expliziert werden – was ihn entschieden einen Schritt näher an die ethnomethodologische Alltagssoziologie heranbringt; Heiders *life space* ist kein mathematischer Kräfte- und Vektorraum, sondern er besteht aus naiver Psychologie, minutiöser Analyse selbstverständlicher Alltagssprache (Modalverben: *kann,*

soll, muss, darf, will und ihre Wechselbeziehungen); wie legen die Akteure sich selbst und anderen ihre Einstellungen, Motive, Handlungen zurecht? Man muss deutlich unterscheiden zwischen Heiders eigenen Überlegungen, die offenbar bereits viele Jahre vor der Veröffentlichung (1958, deutsch 1977) informell bei zahlreichen US-Psychologen zirkulierten. Das erklärt, warum die von Heider inspirierten Attributions- und Dissonanztheorien (Festinger 1957) noch vor Heider selbst an die Öffentlichkeit kommen. Die in den US zu ziemlich schlichten Attributionstheorien umgebauten Gedanken Heiders illustrieren genau den Bruch zwischen dem Szientismus der frühen US-Sozialpsychologie und der deutschen Gestaltlehre. Ich komme darauf zurück.

In den Analysen alltagssprachlicher „Zurechtlegepraktiken" nähert sich Heider (ganz ähnlich wie Whorf von der ethnolinguistischen Seite mit seiner Bestimmung von *common sense*) dem *accounting*-Gedanken der Gesprächsanalytiker. Permanent reflektiert Heider das Verhältnis der alltagsweltlichen sprachlichen Zurechnungspraktiken und ihrer wissenschaftlichen Explikation in Theorien und Modellen.

Und wenn die anderen Handelnden die wichtigsten Größen im Handlungsfeld der Akteure sind, dann gerät die Schnittfläche zwischen Handelnden in den Blick; seit knapp 20 Jahren erobert in der Sozial-, Kommunikations- und Entwicklungspsychologie ein Leitgedanke Terrain, der unter dem Stichwort *theory of mind* (TOM) läuft: Die Repräsentation der Orientierungen des einen in den Orientierungen des anderen. Wir wissen, dass ganz kleine Kinder zunächst in praxi davon ausgehen, dass alle anderen genau das gleiche wissen, was sie selbst auch wissen, was sie selbst in ihre Handlungsfeld wahrnehmen und ansetzen; im Piaget'schen Sinne einrechnen, dass andere Teilnehmer notwendigerweise Dinge nicht wissen können, die sie selbst wissen, können Kinder etwa mit 4;0. Zwischen beiden Extremen gibt es viel Spielraum. Die TOM-Psychologie bringt ins Spiel, wie der Handlungs- und Orientierungsraum der andern im „monologischen" Handlungs- und Orientierungsraum des fokussierten Akteurs repräsentiert ist; die TOM-Perspektive ist „psychologisch" im Sinne von Heider: Sie fragt nach der Repräsentation der Orientierung von anderen in der eigenen Repräsentation. Die Kommunikationssystem-Perspektive ist dagegen soziologisch, sie banalisiert die Repräsentationen der Individuen zugunsten emergenter Systembildungseffekte. Der späte (gruppendynamische) Lewin erfasst eher derartige Systemeffekte (unter der Überschrift: die reale und wirksame Existenz der Gruppe). Was in Heiders peniblen und kleinteiligen Rekonstruktionsversuchen der naiven Teilnehmerpsychologie und ihrer Versprachlichung mitläuft, das ist die Erkenntnis, dass es sich bei diesem Komplex alltäglicher Praktiken gewissermaßen

um die „Realität" nicht allein der psychologischen, sondern der gesellschaftlichen *theory of mind* handelt (vgl. Malle & Ickes 2000). Es versteht sich, dass von Anfang an auch der „monologische" Orientierungsraum des Individuums sozialisiert ist durch die von andern übernommen symbolischen Perspektiven. Die TOM-Perspektive ist diesen gegenüber *reflexiv*. D.h. sie handelt von der Möglichkeit, die Differenz der Perspektiven kalkuliert einzusetzen.

Heiders Kritik an Lewin kulminiert in dem Satz: „Es ist schwierig oder unmöglich, mit topologischen Begriffen zu beschreiben, wie der Lebensraum der einen Person im Lebensraum der anderen Person repräsentiert wird" (Heider 1977: 24f.). So nähert sich Heider deutlich dem Wechselspiel von kognitiven und kommunikativen Faktoren in den Alltagsmethoden der Akteure. Demgegenüber wirken Lewins Verbildlichungsversuche für interpersonale und Gruppendynamiken (vgl. 1953 [1948]) unbeholfen.

Lewin, in seinen pragmatisch-gruppendynamikbezogenen Arbeiten aus der Kriegszeit, läuft direkt durch zu Verfahren und Empfehlungen, die in den heute aus gutem Grund misstrauischeren Zeiten als höchst manipulativ empfunden würden (Binder 2021) – aber die junge Sozialpsychologie verstand sich als „science of democracy". Die Maxime ist: Man muss – das ist Demokratie – den Leuten die Aufgabe stellen, für ein moralisch akzeptiertes Ziel praktisch zu kooperieren. Der *democratic leader* spricht über Ziele und Notwendigkeiten, und das Fußvolk handelt dann entsprechend selbstverantwortlich. Das hätte auch in Chinas Kulturrevolution nicht schlecht gepasst.

Für die systemische Kommunikationstheorie beginnt an diesem Punkt das „rhetorische" bzw. legitimatorische Problem der US-Nachkriegswissenschaft. Es lautet (mit Burkes *rhetoric of motives*):

> Yet, willy nilly, a science takes on the moral qualities of the political or social movements with which it becomes identified. (Burke 1969 [1950]: 31)

Das illustriert Burke mit der „bösen" Naziwissenschaft, der hoch legitimen und „guten" wissenschaftlichen Unterstützung des US-Kriegseintritts durch die US-Wissenschaften (von der sowohl Lewins späte Arbeiten zeugen wie auch Garfinkels Gulfport Field-Studie) – und mit der Entwicklung der Atombombe, einer Kriegstechnologie, mit der dann doch viele Wissenschaftler nicht umstandslos identifiziert werden wollten. Begriffsgeschichtlich gespiegelt wird diese Spaltung in der Entgegensetzung von „Technologie" (= abhängig von den Zwecken, zu welchen sie eingesetzt wird) und „Wissenschaft" (= „reine" Erkenntnismotive konnotierend):

> Any purely secular power, such as the application of technology, would not be simply "good", but could become identified with motives good, bad, or indifferent, depending upon the uses to which it was put, and upon the ethical attitudes that, as part of the context surrounding it, contributed to its meaning in the realm of motive and action. (Burke 1969 [1950]: 30)

Erst der Vietnamkrieg wird das öffentliche Vertrauen in die „demokratischen" Sozialtechnologien nachhaltig erschüttern. Das kann man belegen am plötzlichen Ende der *General Semantics*-Bewegung in den späten 1960er Jahren (mit ihrem Programm, alles werde gut, wenn man nur ordentlich, konkret und mit den richtigen Worten darüber spricht; vgl. Rapoport 1970) oder an wachsender Zustimmung für radikale Kritik an den „neuen Mandarinen" (Chomsky). Lediglich der (immer ziemlich weitsichtige) Burke warnt schon 1950 vor einem drohenden „Kultus" der angewandten Wissenschaft und Technologie, in dem sich Elemente von Religion, Politik und Ökonomie zusammenschieben (wie wir ihn heute – allerdings bereits in einem krisenhaften Modus – haben).

Bereits in den 1920er Jahren reflektiert Sapir (1924) den schwierigen Start von Linguistik (und Ethnolinguistik) in den USA mit dem völligen Fehlen einer wie auch immer gearteten Anwendungsperspektive in diesen Disziplinen. Die kompetitive Vielsprachigkeit Europas (so Sapir 1924) erkennt er als Nährboden auch für sprachtheoretische Unternehmungen. Über die US-Psychologie und -Soziologie, bei der die Linguistik seiner Zeit (Sapir selbst eingeschlossen) Rückhalt sucht, schreibt er:

> If psychology and sociology are popular sciences in America today, that is mainly due to the prevailing feeling that they are convertible into the cash value of effective education, effective advertizing, and social betterment. Even here, there is, to an American, something immoral about a psychological truth which will not do pedagogical duty. (Sapir 1924: 149)

Ich denke, dieses Stimmungsbild Sapirs (zu dem auch der von allen praktischen Menschen als Pedant verunglimpfte Grammatiker gehört) umreißt einigermaßen genau die Szene, in welche die aus Deutschland exilierten Humanwissenschaftler getaucht wurden – die selbst (zumal Lewin und Heider) aus immer noch sehr anwendungsfernen, theorie- und philosophielastigen Forschungstraditionen stammten. Traditionen, in denen der praktische Nutzen einer wissenschaftlichen Erkenntnis viel weniger Gewicht hatte als ihre theoretische Überzeugungskraft.[2] In den USA zeigt sich die Lage genau entgegengesetzt. Ich komme darauf zurück.

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[2] Allerdings verändert sich auch in Deutschland die humanwissenschaftliche Szene nach dem

4 Heider und/oder Lewin?

Beide, Heider und Lewin, eint das „Unmittelbarkeitspathos" in der Modellierung der Situation: Alles, was es braucht, um Ordnung für sich und die anderen Teilnehmer herzustellen, liegt vor unseren Augen, *seen, but not noticed*. Wir brauchen, um praktisch Ordnung herzustellen, keine „Geschichtskenntnisse", weder über die handelnde Person noch über das Zustandekommen des aktuellen Handlungs- und Orientierungsfeldes. Es verdient Beachtung, dass auch Bühlers (1933; 1934) Handlungsmodell in diesem Sinne aktualistisch ist. Es ist gebaut und zentriert um die beiden Pole „Bedürfnis" und „Gelegenheit" (vgl. Knobloch & Schallenberger 1993).[3]

In diesem Zusammenhang verdient auch Heiders Arbeit mit kleinen Trickfilmen, die geometrische Figuren (Kreise, Dreiecke etc.) in Bewegung zeigen, wie sie sich berühren, stoßen, verfolgen etc. (selbst über bewegte geometrische Objekte sprechen wir gerne mittels Verben, die zur "dramatistischen" Perspektive von Handlung und Motivation gehören!). Was die Versuchspersonen aus der (ganz und gar nicht menschlichen) Figurendynamik herauslesen, das existiert psychologiegeschichtlich unter dem höchst missverständlichen Namen „phänomenale Kausalität": Die Figuren werden als Handelnde mit Eigenschaften, Intentionen und Wechselbeziehungen gedeutet, wenn sie in Abläufe verwickelt sind, die eine solche Deutung ermöglichen. In der Terminologie Burkes: Die Filme zeigen eine kausale Welt verursachter Bewegungen unbelebter Objekte, der Zuschauer verwandelt aber diese *motion*-Welt in eine symbolisch-intentionale *action*-Welt mit Motiven und Akteuren (vgl. hierzu Hörmann 1976: 426ff.). Bruner (1986: 18) schließt aus den Befunden von Heider & Simmel (1944), dass wir die terministischen Parameter von Burkes „dramatistischer" Pentade (*agent, scene, act, agency, purpose*) in alle Erfahrungskonstellationen projizieren, denen sie überhaupt als rahmender Hintergrund dienen kann. Sie sind (noch einmal mit Burkes Worten) „necessary forms of talk about experience" und keineswegs „necessary forms of experience", sie drängen sich auf, wenn wir über Erfahrungen kommunizieren (Burke 1969 [1945]: 317).[4]

Diese Befunde zur „phänomenalen Kausalität" im Film sind gewiss nicht leicht zu deuten. Das Geschehen auf der Leinwand kann ebenso gut dem Experimentator als intentionale Kommunikation zugerechnet (und solchermaßen in eine Art

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Ersten Weltkrieg grundlegend, durchaus auch hin zur Anwendung. Die bleibt indes weithin eher ideologisch als szientifisch (vgl. für die Sprachforschung Knobloch 2005).

[3]Und damit eigentlich nicht sehr verschieden von Kurt Lewins Leitgedanken.

[4]„In der Sprache wird alles ausgetragen", schreibt Wittgenstein (1973: 143) in der *Philosophischen Grammatik*.

Parabel verwandelt) werden. Darauf deuten z.B. neuere Replikationsversuche, bei denen die Versuchspersonen weit weniger Bereitschaft zeigten, die geometrischen Figuren als handelnde Personen zu dramatisieren (vgl. den Überblick bei Lück 2006). Wie auch immer man die Befunde zur trickfilminduzierten „phänomenalen Kausalität" deuten mag, sie sprechen in jedem Falle für ein ziemlich müheloses Hin-und-Her zwischen einer kausal attribuierten Bewegungs- und Ereignislogik und einer intentionalen Motiv-, Ziel- und Akteur-Logik.[5]

Während die ausdruckspsychologischen Filme aus der Wiener Bühler-Schule (vgl. Czwik 2018) ihren Ausgang nehmen von Bühlers darstellungstechnischer Analyse, die darauf setzt, dass der Film durch Schnitte, Einstellungen Perspektiven den (unbeweglichen!) Rezipienten nahezu unbegrenzt versetzen kann in beliebige Zeigeräume (in denen der Rezipient stets die Nullstelle der Origo einnimmt), setzen die späteren Filmarbeiten aus der Bühler-Schule (etwa von Käthe Wolf und René Spitz) darauf, die affektiven Empathieangebote der Bilder (von beziehungsdepravierten Säuglingen und Kleinkindern) durch zusätzliche sprachliche *framings* auszurichten.

Nach den Ausführungen von Czwik (2018: 40ff.) war es vor allem Käthe Wolf, die das kommunikative Setting ausdruckspsychologischer Filme reflektiert hat. Dass der Film mit seinen Möglichkeiten der Verlangsamung, Wiederholung, Mehrfachbeobachtung ein gutes, aber nicht widerspruchsfreies Mittel gegen die unverifizierbaren Erlebnisbeobachtungen der europäischen Psychologie sei, steht im Vordergrund. Zu den feinen Beobachtungen von Wolf (1938) gehört, dass der Film den gefilmten Ausdruck dadurch verfälscht, dass der Zuschauer kein Teilnehmer des flüchtigen Ausdrucks-Eindrucks-Geschehens ist und ergo gezwungen, ein anderes Bezugssystem in Stellung zu bringen – und zwar ein sprachlich explizietes. Die Praxis der humanethologischen Säuglings- und Kleinkindfilme Käthe Wolfs (zusammen mit René Spitz), den Filmsequenzen gewissermaßen sprachliche Beobachtungsinstruktionen mitzugeben, könnte dadurch motiviert sein. Man denkt sofort an Kenneth Burkes *terministic screens* (Burke 1966: 44–62), mit der These, dass erst terminologische Netzwerke (seien sie alltagsweltlich oder „wissenschaftlich") Beobachtungen erzeugen. Die illustriert Burke ausdrücklich mit der Frage, was ein bindungstheoretischer Humanethologie wie Bowlby sieht, wenn er einen Säugling beobachtet, und was ein Behaviorist wie Watson. Ähnlichkeiten und Unterschiede, Kontinuitäten und Diskontinuitäten, Grad- und Artunterschiede sind „verkörpert" in unseren terminologischen Wahlen und deren Impli-

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[5]Die Deutungsprobleme spiegeln sich bereits in den Verlegenheiten der Bezeichnung: *apparent movement* heißt es in der Heider-Simmel (1944)-Studie, *perception of causality* bei Michotte (1963), *phenomenal causality* später bei Heider selbst etc.

kationen, Inferenzen, Wechselbeziehungen. So gesehen sind „obervations implicit in terms", die Nomenklatur kanalisiert unsere Aufmerksamkeit, sie gibt uns „*different* photographs of the *same* object" (Burke 1966: 45).

Käthe Wolfs Reflexion des Films als einer Verbildlichungs- und Veranschaulichungstechnik hat ihre große Stärke darin, dass sie für den Zuschauer stets eine kommunikative Matrix annimmt. Schauspieler im Spielfilm etwa stilisieren und übertreiben den Ausdruck rhetorisch. In alltäglicher Interaktion verarbeiten und notieren wir vor allem Abweichungen gegen eine neutrale Nulllinie des Ausdrucks. Der professionelle Spielfilm (und seine Schauspieler) bindet Aufmerksamkeit durch notorische Abweichung des „Helden" von dieser erwartbaren Nulllinie. Die Unmittelbarkeitssuggestion der filmischen „Sehprothese" bedarf der mehrfachen Relativierung: Die Platzierung der Kamera bringt eine externe Perspektive ins Spiel, und der Beobachter bringt seine „terministischen" Kategorien mit.

Unter den wenigen Autoren, die auf die geistige Verwandtschaft zwischen Heiders *common sense*-Sozialpsychologie und Garfinkels Ethnomethodologie verweisen, ist Bruner (1990) zu nennen. „Balance", Gleichgewicht, und „Trouble", normalisierungsbedürftiges Ungleichgewicht, sind die beiden Pole, zwischen denen die Ordnungspraktiken des Alltags aufgehängt sind und zwischen denen sie oszillieren. Für die kanonische Psychologiegeschichte gilt dagegen eher Schlöders (1984; 1988) Aussage über die Auswirkungen und Folgen von Lewin und Heider für die Geschichte der Sozialpsychologie:

> Die fundamentalen methodologischen Reflexionen, insbesondere auch ihre Überlegungen zum Verhältnis von Wissenschaftssprache und Umgangssprache, mit denen beide ihr Theorieprogramm fundieren, haben in der neueren sozialpsychologischen Literatur keine Fortführung gefunden. Der methodische Bezugspunkt der modernen experimentellen Sozialpsychologie ist ein abstraktes, inhaltlich nicht mehr ausgewiesenes empiristisches Methodenideal. (Schlöder 1988: 241)

Mit anderen Worten: Bereits die in den USA klassisch gewordene sozialpsychologische Attributionstheorie hat den (phänomenologischen) Anspruch aufgegeben, die fachliche Methodologie in den praktischen Aktivitäten und Methoden der Teilnehmer zu fundieren. Sie verfällt damit in den klassischen Denkfehler der szientifizierungslüsternen Human- und Sozialwissenschaften, der darin besteht, die symbolischen Ordnungspraktiken der Akteure selbst als bloße Fehler, falsche Ideologien und Rationalisierungen zu marginalisieren. Man kann es

auch in Jerome Bruners konzise Formel packen: „Folk psychology needs explaining, not explaining away" (Bruner 1990: 32). Und zwar nicht nur, weil sie den Sozial- und Kulturwissenschaften die Fundamente liefern, sondern auch, weil die „wissenschaftlichen" Typisierungen und Modelle über institutionelle und diskursive Machtpraktiken (und über die interdiskursive Autorität der Wissenschaften) in den Alltag erneut eingefüttert werden. Bruner (1986) nennt Dawkins und den Neoevolutionismus mit ihrer konsequenten Naturalisierung des „Egoismus" als Beispiel. Versehen mit den szientifischen Weihen evolutionistischer Autorität, erfahren solche Motive ein legitimes *reentry* in die *accounting*-Praktiken der gesellschaftlichen Kommunikation.

Heiders Praxis, alle Vorverständnisse einzuklammern, sich dumm zu stellen, alles einstweilen zu ignorieren, was wir an stillschweigenden Voraussetzungen machen, wird erst sichtbar, wenn wir diese Weiterungen stören, an der Entfaltung hindern, einklammern. Zurück bleiben dann: Inkrementalismus und Sequentialität als Ordnungsprinzipien der solchermaßen reduzierten Verfahren – Sedimentierung in sprachlichen Formen ist das phänographische Ergebnis.

Es war im Übrigen ebenfalls Bruner (1986), der uns darauf hinweist, dass Fritz Heiders sozialpsychologische Balancelehre (mit ihrer ausgewogenen Mischung von Deutungs-, Zurechnungs- und Bewertungsbalancen) für Garfinkel nicht das letzte Wort war. Bruner berichtet anekdotisch über eine Begegnung mit Garfinkel in seinem (Bruners) Seminar:

> Harold Garfinkel, now a distinguished sociologist, once took a "reading and research" course with me in order to find out what psychologsts were up to. He hit on a very interesting experiment. Borrowing a dozen trait names from one of the standard lists, each with a positive and negative pole – like lazy and energetic, honest and dishonest – he selected at random combinations of negatives and positives. He presented these combinations on cards and asked his subjects for a general description of the persons being depicted. (Bruner 1986: 51)

Merkwürdig genug hielt keine der Versuchspersonen jemals Personen für unvorstellbar, die solchermaßen kontradiktorische Eigenschaften in sich vereinen. Offenbar braucht es deutlich mehr *imbalance*, um Alltagsakteure in Vorstellungs- und Erklärungsnot zu bringen. Man kann Garfinkels kleinen Denormalisierungstest allerdings auch verstehen als Hinweis darauf, dass sich Teilnehmer in ihren symbolischen Normalisierungspraktiken durch einfache semantische Widersprüche zwischen Merkmalen ein und desselben Akteurs nicht aus der Ruhe brin-

gen lassen.[6] Eben das hat offenbar damit zu tun, dass attribuierte Weil-Motive und attribuierte Um-zu-Motive selbst so etwas wie ein Fließgleichgewicht bilden. Was auf der einen Seite nicht unterzubringen ist, wandert attributiv auf die andere. Die angemessen deutende Reaktion auf widersprüchliche Verhältnisse ist selbst widersprüchlich (darauf besteht Kenneth Burke auf Schritt und Tritt!). Anrüchig und verdächtig sollten eher symbolische Aktivitäten sein, die den Widerspruch systematisch verdunkeln, zudecken, die Handeln eindeutig machen. Und das sind, *horribile dictu*, in der Hauptsache die „wissenschaftlichen" *accounts* der Sozial- und Humanwissenschaften, deren Ambiguitätstoleranz deutlich geringer ist als die der Alltagsakteure. Sozialtechnologisch wieder eingefüttert in den Deutungsalltag der Akteure fungieren die Befunde der Sozial- und Humanwissenschaften als autoritative „Zusatzsteuerung" der Zurechnungspraktiken. In Umrissen erkennbar wird hier, was Heiders naive Psychologie, Burkes rhetorischen „dramatism" und sekundäre „linguistic ideologies" (Silverstein) mit einander verklammert: Das Sprechen selbst lernen wir durch Mittun und es „repräsentiert" in seinen Praktiken und Routinen unsere naive Psychologie – unsere Sprachideologien lernen wir durch Schule, Unterricht, Schreiben. Und eben auch durch „Rückwirkungen" naiver und fachlicher Wissensbestände in unsere alltäglichen *accounting*-Praktiken hinein.

Von einem guten Verständnis des ethnomethodologischen Anliegens zeugt auch Bruners eigener Kommentar zu dieser Episode von semantischer Unvereinbarkeit:

> Now, perhaps there *can* be every kind of person. Or perhaps the better way to say it is that we can create hypotheses that will accommodate virtually everything we encounter. (Bruner 1986: 51)

Und hier wäre tatsächlich noch einmal an das zu erinnern, was die wirkmächtige US-Attributionstheorie, die sich stets auf Heider beruft, sowohl von diesem selbst als auch von Burke und Garfinkel unterscheidet: Dass Heider die Attributionspraktiken der Teilnehmer als eine Art „naiver Faktorenanalyse" bezeichnet hat, wird gerne zitiert. Diese Formulierung ist aber so etwas wie ein semantisches Grenzobjekt. Für die fachliche *community* der Sozialpsychologen signalisiert sie, dass die Attributionspraktiken der Teilnehmer unzureichend sind. Nach der Teilnehmerseite signalisiert sie, dass deren Attributionspraktiken so sind „wie die wissenschaftlichen".

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[6] „[E]ven antagonistic terms, like parry and thrust, can be said to 'cooperate' in the building of an over-all form", schreibt Burke (1969 [1950]: 23).

Die Attributionstheoretiker haben daraus den (irrigen) Schluss gezogen, sie müssten nur die Teilnehmerpraktiken auf ihre „Wissenschaftlichkeit", auf ihre Vorurteile, auf ihre Verfälschungstendenzen, auf ihre Widerspruchsfreiheit etc. untersuchen (vgl. statt vieler den Forschungsbericht von Sillars 1982). Die Teilnehmer erscheinen aus dieser Sicht als unvollkommene Wissenschaftler, und die Wissenschaftler sehen sich berufen, die Vorurteile der Teilnehmer zu korrigieren. Diesen ganzen (sagen wir) epistemischen Komplex wischen Burke und Garfinkel einfach beiseite. Er spielt keine Rolle. Die „Wahrheit" von Zurechnungsfiguren ist unentscheidbar. Es geht um die dramaturgisch-terministischen Netzwerke, aus denen die Akteure ihre *accounts, inferences, glosses* spinnen. Und die müssen füreinander *anschlussfähig* sein. Das gilt für die alltäglichen wie für die wissenschaftlichen Zurechnungspraktiken. Wenn sie übereinstimmen würden, gäbe es kein Gespräch. Das in sich zirkuläre Netz der Zurechnungsadressen (*agent, scene, act, purpose, agency*) sorgt dafür, dass wir in allen Lebenslagen über die Details der Zurechnung streiten können. „There are objections to any decision", paraphrasiert Burke die Alltagsrhetorik des Aristoteles, und er erinnert an die bekannte Fabel von Vater und Sohn, die gemeinsam einen Esel auf den Markt bringen, um ihn zu verkaufen. Gleich, wer auf dem Esel reitet, immer gibt es ernstzunehmende Einwände bei den Mitmenschen, die ihnen begegnen, so dass die beiden am Ende entnervt gemeinsam den Esel auf den Markt tragen – was aber offenbar auch keine Lösung ist! Noch einmal mit den Worten Burkes:

> Given the world as it is, with its jangling variety of imputed motives, most often one merely assumes that there is a well-rounded philosophic, scientific, or theological rationale to justifiy the censorial weighting of his terms. (Burke 1969 [1950]: 98)

Wissenschaftler wie Alltagsakteure gehen davon aus, dass es „letztlich" eine vernünftig-rationale Erklärung für ihre Wahrnehmungen und Handlungen gibt. Bezeichnend ist auch, dass die zeitgleiche Rezeption der ethnolinguistischen Boas-Sapir-Whorf-Tradition exakt dem gleichen Muster folgt. Man hat sie durchweg interpretiert, als ob die „Relativität" der sprachlichen Weltsichten epistemisch an der einzig richtigen „wissenschaftlichen" Weltsicht zu messen sei (wie die Alltagsattributionen an den „wissenschaftlichen") – während Boas, Sapir und Whorf (das ist freilich keineswegs fachlicher Konsens) der Ansicht waren, dass uns unvertraute Ordnungsprinzipien und Musterbildungen im Sprache-Kultur-Interface zur besseren Erkenntnis auch unserer eigenen Ordnungsprinzipien beitragen könnten. Sie sind produktive Verunsicherungen unserer eigenen „linguistic ideologies" (Silverstein 1979; 2000).

Wissenschaftler haben einen Horror vor Ambivalenz, vor Uneindeutigkeit. Die Alltagsakteure müssen freilich damit leben, wiewohl sie permanent damit befasst sind, Dissonanz und Ambiguität zu reduzieren. Für sie reicht es freilich, wenn die Dissonanzreduktionspraktiken für alle praktischen Zwecke hinreichend sind, so dass man weiter handeln kann. Die Wissenschaftler wollen die Dinge ein und für alle Male klären! Burke (1969 [1945]: xviii) schreibt dazu:

> What we want is not terms that avoid ambiguity, but terms that clearly reveal the strategic spots at which ambiguities necessarily arise.

Das dramatistische Schema für die Zurechnung von Handlungsmotiven etabliert „Adressen", auf die alltäglich und/oder wissenschaftlich zugerechnet werden kann, was geschieht, gemacht wird etc.

5 Doppelte Kontingenz

In Garfinkels Werk gibt es zahlreiche Anzeichen dafür, dass die Auseinandersetzung mit Fritz Heider und dem Attributions- und Balancekomplex für ihn eine wesentliche Rolle gespielt hat, insbesondere auch das bewegliche Wechselverhältnis zwischen Kausalitäts- und Motivzuschreibungen. In Ann Rawls (2019: 58) können wir lesen:

> The *Third* problem that Garfinkel says Parsons solves is that of *the actor as agent* (see p. 156). In everyday situations, questions of causation and agency are typically answered through the assignment of *blame* and *responsibility*: "In their everyday use, such categories or procedures are the morally equivalent categories of cause". But, the theorist is concerned with matters of causation as it pertains to actors, not persons. By this Garfinkel means that social actors within a social contract, or definition of the situation, are not natural individuals in the natural world operating with natural reason. Social actors are not persons for Parsons in that sense. That is, they are not the individuals assumed by utilitarian theory. For Parsons, actors are courses-of-action oriented to an environment of objects within a specific definition of the situation, or social contract. As such, the same person can project multiple actors, or selves, even in a single situation. This complicates questions of causation. The self is a social object that changes in relation to other objects (including other actors) across time and sequence. Thus, Garfinkel explains, the meaning of causation will vary depending on the

kind of actor, or course-of-oriented-actions – e. g. a role, role set, collectivity, subsystem, etc. – in question (see pp. 156–157). What Parsons calls the "voluntaristic" character of action provides for a specification of how *actors themselves* handle attributions of causation. But, the theorist needs to talk about causation differently.

In diesen Dingen war Heider offenbar weiter als Parsons! Heiders direkte attributionstheoretische Nachfolger (Festinger, Thibaut, Kelley) haben *seinen* Pfad an einer entscheidenden Stelle verlassen: Da nämlich, wo er (phänomenologisch geschult) seine wissenschaftlichen Modelle abgestützt und fundiert hat auf den naiv psychologischen Praktiken der Teilnehmer selbst, die er in den alltagssprachlichen Konzepten fand, mittels welcher die Teilnehmer selbst sich ihre Wahrnehmungen und Handlungen zurechtlegen und erklären.

Just as Lewin and Asch before him, Heider recognized that a psychology of social interaction must chart out the subjective concepts and perceptions of the social perceiver, "studying interpersonal relations at the level of their meaning for the participants" (Ickes & Harvey 1978). (Malle & Ickes 2000: 203).

Während seine szientistischen Rezipienten Heiders bewegliches und lokal für ad hoc-Praktiken konzipiertes Material umstandslos in Kausalitätsattribution und Persönlichkeitszüge umbauten, blieb in der Rezeption weitgehend unbemerkt, dass es ihm um die lokalen Dynamiken der praktischen Bearbeitung dessen ging, was bei Parsons dann „doppelte Kontingenz" heißt. Ganz wie Kenneth Burke gliedert auch Heider das Geschehen doppelt: in eine Ebene, auf der kausal wirksame Ursachen attribuiert werden, und eine (symbolische) Ebene, in der wirksame Motive, Absichten, Ziele zugerechnet werden. Und beide Gliederungen gehören (in beweglichen und wechselnden Mischungsverhältnissen) zu den Akteuren selbst, die manches als kausal bedingten „Zwang der Verhältnisse" zurechnen (Weil-Motive bei Alfred Schütz) und anderes auf Absichten und Motive der Teilnehmer zurechnen (Um-zu-Motive bei Alfred Schütz). Burke spricht im gleichen Sinne von einer (kausalen) *motion*-Dramaturgie und einer (motivationalen) *action*-Dramaturgie (vgl. Burke 1966 in Thayer 1967a; und hält den Versuch, letztere auf die erstere zu reduzieren, für den Geburtsfehler der US-Sozialwissenschaften).

Das Einleitungskapitel von Heiders publiziertem Hauptwerk (Heider 1985, dt. 1977) handelt ganz ausdrücklich vom Verhältnis zwischen den „oberflächlichen" Alltagspraktikern und den (vermeintlich) „tiefen" Erkenntnissen der Psychologie. Und es enthält für den US-Szientismus so provozierende Formulierungen wie die,

dass man alle expliziten Erkenntnisse der wissenschaftlichen Psychologie aus der Welt entfernen könnte, und gleichwohl könnten alle zwischenmenschlichen Probleme genauso gelöst werden wie *mit* dem expliziten Fachwissen. Das ist freilich insofern übertrieben, als die Wissenschaften ja (wie Lewin *und* Burke notieren) selbst wieder als legitimatorische *accounts* in die Teilnehmerpraktiken eingefüttert werden, wenn sie (interdiskursiv popularisiert) die Teilnehmer erreichen.

Und hier komme ich zurück auf die eingangs zitierten Überlegungen von Burke (1969 [1945]) zum *scene-act-ratio*. Vom Akteur, der von der Szene eingeschlossen wird, erwarten wir, dass er sich in dieser Szene definiert, sei es deterministisch als Gezwungener oder Getriebener, sei es programmatisch als zielorientierter Veränderer der Szene. Im letzteren Extremfall gilt die Handlung als „frei" und „verantwortlich", als dem Akteur motivational zurechenbar. Im ersteren Extremfall gilt sie als kausal verursacht durch die szenischen Sachzwänge, die den Akteur von Zurechnungszumutungen entlasten. Zusammen bilden beide Bezugssysteme den Verschiebebahnhof zwischen Um-zu-Motiven und Weil-Motiven. Bereits Heiders penible Bemerkungen zum Gebrauch der Modalverben (*can, must, should* etc., seine Reflexionen zur „naiven Theorie vom Können"; vgl. Heider 1977: 121ff.) sind nur verständlich, wenn man begreift, dass der Gebrauch solcher Modalverben mit alltäglichen Attributionspraktiken aufs engste zusammenhängt. Modale Konstruktionen dienen der alltagspraktischen Verteilung und Gewichtung von Attributionen auf die Faktoren, in deren Netz wir Handlungen bestimmen (und das sind im Kern die der Burkeschen Pentade). Jedes *ich muss* bremst Attributionen auf das handelnde Ich und lenkt sie auf szenische Zwänge oder andere (mächtigere) Akteure ab. Es gibt eine nicht-personale Ordnung des „Sollens" (ebenso wie eine personale), als „Ereignis" behandeln wir, was wir nicht beeinflussen können. So lauern hinter zahllosen alltagssprachlichen Ausdrücken vorgeordnete Attributionspotentiale. Heider selbst notiert auch die weniger offensichtlichen modalen Optionen der Alltagssprache wie Gerundiva, Verbalnomina, die ein „Sollen" fixieren, (*ceterum censeo carthaginem esse delandam*), oder Dispositionsadjektive wie *brennbar* oder *verletzlich*, die „Möglichkeiten" versprachlichen.

Es ist nicht die Aufgabe von Sozialwissenschaftlern, die Zurechnungspraktiken der Teilnehmer zu korrigieren oder zu verbessern, sie müssen vielmehr die Prinzipien erklären, von denen diese Praktiken angeleitet und gesteuert werden – alle ihre Ambiguitäten eingeschlossen

Und einen weiteren Punkt möchte ich abschließend noch erwähnen, an dem Heiders penible Rekonstruktionen der naiven Teilnehmerpsychologie *und* seine heuristische Filmarbeit gemeinsam gesehen vielleicht sogar ein Stück über den

Medienoptimismus der Sequenzanalytiker hinausführt: Im ersten Band der (notorischen) *Notebooks* gibt es ein Kapitel zum Stichwort „Ökologie" (Heider 1987 I: 274ff.), in dem Heider die praktischen *constraints* für Teilnehmerorientierungen und Anschlusshandlungen in Interaktionslagen reflektiert. Da gibt es den paradoxen Gedanken, dass der enorme Zeitdruck des online-Prozessierens die Anschlusshandlungen konditioniert (und dass nichts den Analytiker und Film-Beobachter so meilenweit von den tatsächlichen Handlungsbedingungen entfernt wie die Möglichkeit, alles mehrfach und in aller Ruhe zu betrachten!): die handlungsökologischen Bedingungen könnten für Teilnehmer und Beobachter nicht unterschiedlicher sein![7] Interaktiv und „orat" in der Kooperation verfertigte Texte (vgl. Maas 2010) löschen alle ihre formalen Eigenschaften und stellen die Aufmerksamkeit der Teilnehmer auf strikt lokale und indexikalische Bündigkeiten.

Und noch einen allerletzten Punkt, der vielleicht nur für Sprachwissenschaftler Sinn macht: Garfinkels umständlich-genauer, den Leser immer ins Stolpern bringender, alle Automatismen des Verstehens unterlaufender schriftlicher Duktus liest sich für mich immer so, als hätte man einen deutschsprachigen phänomenologischen Philosophen (Brentano oder Husserl oder Schütz) beinahe wörtlich ins Englische gebracht. Den gleichen etwas eckigen und unbeholfen anmutenden Duktus finde ich in Heiders *Notebooks*. Hier nur eine Kostprobe:

> Naive science. Maybe naive science never asks questions about events which are once expressed in terms of fixed invariants. – „that is settled", for instance […] things are conceived as such, what has to be explained are the exceptions. That is important for practical purposes; it doesn't help for predictions if one „explains" what one can predict anyway. (Heider 1987 I: 376)

Bei einer solchen Passage kann man sich schwerlich dem Eindruck entziehen, dass hier von den Angelegenheiten der Ethnomethodologie gehandelt wird, von begrenzter Explizierbarkeit, idealisierter semantischer Fixierung des Geteilten, trotz indexikalischer Vielfalt der Beziehbarkeiten etc.. Prioritätsfragen sind vorläufig unbeantwortet. Von Heiders *Notebooks* wissen wir in der Hauptsache, dass die erste Phase nach 1958 angelegt worden ist – und die zweite nach 1978, also durchaus dann schon nach dem Aufkommen der Ethnomethodologie.

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[7]Da die zeitliche Einordnung von Heiders Notizen ausgesprochen schwierig ist, kann ich nicht sagen, ob diese Bemerkungen vor Batesons *Ecology of Mind* liegen oder vielleicht auch erst dadurch angeregt sind.

6 Schlussfolgerungen und Affiliationen

Was sich in den 1930er und 1940er Jahren in locker verknüpften Szenen exilierter deutsch-jüdischer Sozial- und Humanwissenschaftler zusammenschiebt, das sind die ersten (aber keineswegs die einzigen) Bausteine eines neuen fachlichen „Denkstils" in den Kultur- und Humanwissenschaften (Fleck 1980 [1935]). Der Kontext dieser neuen Formation ist (wie die Projekte von Heider, Lewin, M. Mead, Bateson, Garfinkel demonstrieren) in den USA durchaus praktisch und auch sozialtechnologisch. Man möchte etwas bewirken und identifiziert sich mit den Zielen und Werten der US-Demokratie. Allerdings entfernt man sich, unter dem Einfluss von Sozialphänomenologie, Gestalttheorie, Ethnologie und Kybernetik, rasch vom naiv-szientischen und positivistischen Pragmatismus der damals in den USA herrschenden Lehren. Was die exilierten Psychologen und Humanwissenschaftler aus der deutschen Tradition mitbringen, ist freilich ganz im Gegenteil zutiefst „antipraktisch", philosophisch, grundsätzlich, theoretisch – und keineswegs vorab auf Anwendung abgestellt.

Dieser Unterschied ist freilich weniger klar, als er aussieht. Es steht fest, dass auch im deutschen Wissenschaftsraum der sozial- und psychotechnische Einsatz der Humanwissenschaften um diese Zeit (und auch bereits im Ersten Weltkrieg) in vollem Gange war. Nicht zuletzt die sozialpolitischen und arbeitspsychologischen Aktivitäten des Bühler-Instituts selbst belegen das unmissverständlich. Und Hugo Münsterberg, ein Pionier der Wirtschaftspsychologie und „Psychotechnik" wechselt bereits vor dem Ersten Weltkrieg zwischen deutschen und US-Wirkungsstätten. Es ist also keineswegs das Fehlen technokratischer Anwendung im deutschen Sprachraum, das einen Unterschied ausmacht. Es ist vielmehr der akademische und politische Widerstand der bildungsbürgerlichen „deutschen Mandarine", der die Anwendung misstrauisch begleitet und in Schach zu halten versucht. Die deutsch-jüdischen Exilanten stehen für eine neuartige Verbindung des Theoretisch-Grundsätzlichen mit dem Modern-Technokratischen.

Im Gegenzug gibt es in der nur wenig später in den USA virulenten Kommunikationsdebatte, in der „Kommunikation" als neuer Grund- und Programmbegriff der Human- und Sozialwissenschaften (und auch der kybernetischen Regulations- und Gleichgewichtslehren) verhandelt wird, das erkennbare Motiv, sich endlich auch theoretisch von den importierten europäischen Sozialtheoretikern zu lösen – und ebenso auch vom kruden Szientismus des zählenden und messenden US-Mainstream. Duncan (1967) kann als Analyse dieser Konstellation und als Programm ihrer Überwindung gelesen werden (also durchaus als Konkurrenz- und Begleitprogramm zu Garfinkel 1967): Er fordert, die (aktualistischen) Theorietraditionen der sozialen Handlungsanalyse (von Mead, Cooley, Burke) in Stel-

lung zu bringen gegen die europäischen Importe. Der handlungstheoretische Ak-
tualismus gilt als ihre Markenzeichen:

> The present is the locus of an act in society; images of the past and future
> are used to organize actions in a present. (Duncan 1967: 198)

Die von innen heraus erfassten und sezierten Praktiken alltäglicher Sinnge-
bung legen das symbolische Fundament der Kulturen frei. Szientische Sozial-
wissenschaftler glauben erklären zu können, was die Menschen *wirklich* bewegt,
sie verstehen aber nicht, dass auch ihre fachlichen Problemstellungen auf all-
täglichen Fundamenten errichtet sind, die sie vergeblich abzustoßen versuchen.
Die Wissenschaftskritik des späten Husserl formuliert diese „Entfremdung" der
Wissenschaften von ihren lebensweltlichen Grundlagen theoretisch. Im US-Exil
wird diese Einsicht praktisch. Und dass sich die Exilierten auch praktisch mit den
Zielen der US-Demokratie identifizieren können, ist mehr als verständlich. An-
ders gesagt: Im US-Exil reüssiert nur, wer Anschluss findet an den pragmatisch-
wirkungsorientierten Szientismus der dortigen Szene.

Vor diesem Hintergrund ist und bleibt es erstaunlich, dass gerade die Angehö-
rigen der Berliner und Grazer Gestaltpsychologie (Koffka, Köhler, Lewin, Heider)
in den USA so erfolgreich waren. Dass etwa die Arbeiten ihrer Lehrergenerati-
on (Carl Stumpf, Alexius von Meinong) in den USA hätten reüssieren können, ist
schlechthin unvorstellbar. Und auch Heiders wissenschaftlicher Duktus ist in ho-
hem Maße „unamerikanisch". Was also prädestiniert die „Gestaltisten" (aus der
in Deutschland als rückständig und neoscholastisch geltenden Brentanoschule)
für ihren Erfolg in den USA?[8]

Beide, Lewin und Heider, reflektieren in ihrem Werk die Wissenschaftsszene,
aus der sie kommen, und die Szene, in der sie wirken. Heider in seiner Autobio-
graphie und in den *Notebooks*, Lewin zuletzt im erwähnten posthum veröffent-
lichten Text über Cassirer sowie in einer (ebenfalls erst posthum gedruckten)
Studie über „Frontiers in Group Dynamics" (Lewin 1947). In der letzteren heißt
es eingangs über die Auswirkungen des Zweiten Weltkriegs:

> It is an important step forward that the hostility to theorizing which do-
> minated a number of social sciences ten years ago has all but vanished. It
> has been replaced by a relatively widespread recognition of the necessity
> for developing better concepts and higher levels of theory. The theoretical

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[8] Auch Whorfs späte Arbeiten stehen unter dem starken Einfluss des Gestaltdenkens, überdeut-
lich im Programm mit dem Namen „configurational linguistics" (vgl. Lee 1996). Das gleiche gilt
für Sapir, der vor allem mit dem Werk Koffkas gut vertraut war (z.B. Koffka 1935).

development will have to proceed rather rapidly if social science is to reach that level of practical usefulness which society needs for winning the race against the destructive capacities set free by man's use of the natural sciences. (Lewin 1947: 5)

Das ist eine Paraphrase von Lewins oft zitiertem Wahlspruch, nichts sei so praktisch wie eine gute Theorie. Im erwähnten Nachruf auf Cassirer hebt Lewin dessen Analysen der Methode und Begriffsbildung in den Naturwissenschaften hervor (Lewin 1981 [1949]: 347), und er spricht selbst sozialen Phänomenen den gleichen Realitätsgrad zu wie physischen Gegenständen. Auf Schritt und Tritt geht es um den Vergleich zwischen alltäglichen und „wissenschaftlichen" Praktiken und Verfahren. Beide verstehen „Daten" als bloße Symptome, die sie mit ihren Deutungsmustern „lesen" müssen. Und es ist die Abhängigkeit der praktischen (und wissenschaftlichen) Erfahrungen vom gesamten sozialen Feld, die klare und deutliche, theoretisch systematisierbare Einsichten verhindert (1981 [1949]: 359f.). Was alltäglich, sozialwissenschaftliche und naturwissenschaftliche Praktikern und Verfahren verbindet, das ist ihre Verwurzelung in sozial-kooperativen Handlungsfeldern.

In diesem Punkt ist Heider nachgerade obsessiv. Seine Position könnte man als „explikationistisch" bezeichnen. Sie nimmt vorweg, was Brandom (1994) viel später logisch zu systematisieren versucht. Während unsere intuitiven Alltagsakte immer mehrere Gesichtspunkte synkretisch und gleichzeitig verarbeiten, operiert fachlich explizierendes „reasoning" mit „*one* factor at a time" (Heider 1987 I: 372) – aber immer gegründet auf der naiven Psychologie des Alltags. Auch das Experiment folgt dieser Logik, Faktoren zu vereinzeln und explizit zu formulieren (Heider 1987 I: 377). Die anekdotische und labyrinthische Form der *Notebooks* (zweifellos Heiders „Hauptwerk" und den veröffentlichten Arbeiten des Autors überlegen) spiegelt die improvisierten Deutungs- und Attributionspraktiken, mittels derer alltägliche Akteure und Wissenschaftler ihre kommunikative und kognitive Welt provisorisch ordnen (und die Begründungsverpflichtungen, die sie dabei eingehen). Wir finden in den *Notebooks* viele Fragen (und nur wenige Antworten). Das Verfahren gleicht ein wenig dem Wittgensteins (mit dem sich Heider 1987 I: 361 auch selbst vergleicht). Programm ist: zu ordnen, was wir in praxi bereits „wissen", aber nicht explizieren können. Leicht fällt ihm der Nachweis, dass manche psychologische Theorie nicht mehr ist als eine formalisierte Ausarbeitung von *common sense*-Annahmen. Sein Beispiel: die behavioristischen Lehren vom *reinforcement* explizieren lediglich das, was der naive Alltagsverstand über Belohnung und Strafe „weiß" (Heider 1987 I: 353). Nicht expliziert ist auch, was wir als alltägliche praktische Sprecher über unsere Sprache wissen.

Die Schemata des alltäglichen Sprechens vergleicht Heider (1987 I: 363) mit inter-personalen Schemata – und seine anekdotischen Beobachtungen setzt er gegen rein auf das Medium Sprache bezogenen Überlegungen ab mit dem Satz: „The linguistic philosophers study the net – I am studying the fish that are caught in the net" (Heider 1987 I: 550).

Was die handlungstheoretischen Verschränkungen von *agent–act–scene–pur-pose* betrifft, so denkt Heider die von Lewin (und Burke) skizzierten dialekti-schen Linien weiter: Einerseits besteht die Szene für den Handelnden aus Ob-jekten mit positiven Valenzen, Aufforderungscharakteren etc.: Wasser lädt zum Schwimmen ein und der Stuhl sagt: „Setz dich!" (Heider 1987 I: 282). Andere sze-nische Gegebenheiten werden zu Barrieren, Grenzen etc. Im Gegenzug färben aber auch die Ziele und Zwecke der Handelnden ein, wie die gegebenen Bezü-ge interpretiert (und welche Szenen aktiv aufgesucht) werden. Heiders Sinn für alltägliche Illustrationen fasst das in Beispielsätze wie „if you have a need for killing, find yourself a dragon", „fit the object to the act" (Heider 1987 I: 291). Das böse Objekt rechtfertigt den aggressiv-feindseligen Akt. Das symbolische Mittel-Paradox, von dem Burke (1969 [1945]) sehr ausführlich handelt („the war to end all wars" etc.), illustriert Heider dialogisch-interaktiv:

(1) *You're always complaining about things!*

(2) *And what are you doing right now?* (Heider 1987 I: 343)

Die unendliche Vielfalt von triadischen Person-Person-Sache-Konstellationen, die wir in unserem Alltag gewöhnlich problemlos ordnen und managen, versucht Heider durch eine ganz einfache, aus wenigen Buchstaben und Relationszeichen bestehende Kalkülsprache formal zu ordnen (hierzu hauptsächlich Heider 1988 IV).

Die Berliner Gestaltschule und Bühlers Wiener Institut waren harte Konkur-renten um die Meinungsführerschaft in der deutschen Psychologie der 20er und 30er Jahre. Sie sind das in gewissen Grenzen auch in den USA geblieben, wie-wohl die Wirkung der „Gestaltisten" vermutlich weiter reicht. Dabei ist zu be-rücksichtigen, dass Heider als Meinong-Schüler ebenso wenig zum harten Kern der Berliner Gestaltpsychologenschule gehört wie Lewin. Bei den „harten" Ge-staltpsychologen wie Wolfgang Köhler galt Lewin nicht als einer der ihren, Köh-ler verhinderte Lewins Berufung an die New School for Social Research. Heider dürfte zudem einer der wenigen gewesen sein, die beide Institute, das Berliner *und* das Wiener Institut, gut kannten, ihm war 1927 eine Assistentenstelle bei Bühler angetragen worden. Lewin hingegen galt auch in der Bühler-Schule als Konkurrent, vor allem in Angelegenheiten der Modellierung des Handelns (vgl.

Knobloch & Schallenberger 1993), obwohl Lewins topologisch-vektorielles Modell und Bühlers Dualismus aus „Bedürfnis" und „Gelegenheit" aus heutiger Sicht sehr ähnlich wirken, und vor allem: gleichermaßen aktualistisch (vgl. die Beiträge in Friedrich 2018). Hier sind viele Zusammenhänge nach wie vor unklar – und möglicherweise ergiebig für ein besseres Verständnis kooperativer Praxis.

Zu den im US-Exil ausgesprochen erfolgreichen Psychologen aus der Wiener Bühler-Schule darf man wohl neben Charlotte Bühler selbst (kindliche Entwicklung und Jugend) Paul Lazarsfeld, einen der Mitbegründer der modernen Wirtschafts- und Marketingpsychologie und der Massenkommunikationsforschung, rechnen. Beide eint die reiche Erfahrung in der praktischen und angewandten (politischen) Szene der Wiener 20er und 30er Jahre, zweifellos eine wichtige Vorerfahrung für den Pragmatismus der US-Wissenschaften. Die Arbeit an der Wirtschaftspsychologischen Forschungsstelle im Roten Wien war eng verbunden mit dem Bühler-Institut, wiewohl sozialpolitisch und interventionistisch orientiert. Lazarsfelds US-Tätigkeiten (hierzu ausführlich Fleck 2015: 333–374) entfernen sich freilich rasch von den Theorien und Axiomen der Bühlers.[9] Darüber, wie man in der US-Szene als europäischer Psychologe wahrgenommen wurde, schreibt er in einem undatierten (laut Fleck 2015: 349 wahrscheinlich 1934 an Karl Bühler gerichteten) Brief etwas kokett:

> Man wird hier als europäischer Psychologe im Grunde genommen entweder für einen Narren oder für einen Zauberer gehalten, aber ein systematischer Gedankenaustausch ist ausgeschlossen. (zitiert nach Fleck 2015: 350)

In diesem Passus steckt der auch nach den Ansehensverlusten aus dem Ersten Weltkrieg offenbar immer noch „sagenhafte" Ruf der deutschen Wissenschaft in den USA, ebenso wie das nach wie vor ungebrochene Selbstbewusstsein der „Deutschen Mandarine" (auch in der zweiten Generation). Aber man erwartete in den USA sicher auch, der deutsche „Gelehrte" sei wahrscheinlich unpraktisch und ein wenig weltfremd – was Lazarsfeld und Lewin sicher nicht waren, wohl aber Heider.

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[9]Lazarsfeld war schon 1933 als Stipendiat der Rockefeller Foundation in die USA gekommen.

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Kurt Lewin's wide-ranging influence on the history of qualitative research. Response to Helmut Lück and Clemens Knobloch

Andrea Ploder

Helmut Lück 2021 [this volume] and Clemens Knobloch 2021 [this volume] show that Kurt Lewin was a traveler between continents and disciplines. His work was received in numerous contexts – in and outside of academia – and had a long-lasting impact in a number of fields. Knobloch and Lück point out the impact of Lewin's early work on the development of sequence analysis and the early use of audio-visual data in the social sciences. Both methodological elements play a decisive role in contemporary qualitative research, and it is fair to say that Lewin's role in this development has been underestimated so far. But a closer look reveals that Lewin also influenced several other central elements of contemporary qualitative research, especially in the German-speaking countries. This observation is not new (see e.g. Lück 1996a: 128–132), but it deserves to be recalled in a book about the history of research methodologies. In the following, I want to highlight Lewin's influence on group discussion, participatory research, and qualitative case studies, and suggest adding him to the list of key figures in the history of qualitative research.

In his later work, Lewin had a keen interest in groups, and his work on group dynamics is probably the most widely received part of his œuvre. It was also an important foundation for the methodology of "group discussion", which emerged after World War II and has been further developed ever since. In 1950, shortly after their return to Germany, Friedrich Pollock, Theodor W. Adorno, and Max Horkheimer started the famous *Gruppenexperiment* at the Institut für Sozialforschung (IfS) in Frankfurt am Main (Pollock 1955; Perrin & Olick 2011). Starting off with only a small research team, by the end the project involved at least 37

researchers. In different parts of Germany, they brought research participants together in groups and confronted them with a stimulus which triggered a discussion that was chaired by one of the researchers. The discussions were recorded on audio tapes, which were transcribed and interpreted by members of the research team, under the guidance of Hertha Herzog and Helmuth Plessner (Adorno & Horkheimer 1955: VI). The members of the IfS had a clear idea of their aims: continuing their work on "prejudice" (Adorno et al. 1950), they wanted to understand the political climate in post-World War II Germany. Methodologically, on the other hand, they did not walk well-worn paths. The group experiment was a typical "pilot study", and the development of a novel research method was one of the declared goals of the project (Pollock 1955: 3).

The authors of the *Gruppenexperiment* claimed that they did not build on any existing studies and that the team in Frankfurt had developed the approach mostly on their own (Pollock 1955: 4). Even Werner Mangold, a former student of Adorno who wrote a dissertation on the methodology of the *Gruppenexperiment*, dedicates only one footnote to Lewin (Mangold 1959: 63, regarding the normative character of informal group opinions). Lück (1996b: 130), Fleck (2007: 390) and others have argued that the members of the IfS owe a lot to Lewin's ideas, but that they conceal this connection because they saw Lewin as a competitor. This view is supported by correspondence between Adorno and Horkheimer (see e.g. Wiggershaus 1986: 412, 415), but the story is probably more complicated. In his 1953 preface to Lewin's collected papers (published after Lewin's death), Horkheimer claims an "intimate relationship" between Lewin's work and his own and calls Lewin's work "indispensable" for German academic research. Either way, it is striking that Lewin, whose work on group dynamics was well known among the members of the IfS, was not quoted in any of their major publications on the *Gruppenexperiment*.

Throughout the 1950s, the members of the IfS conducted several studies based on the group discussion method (see e.g. Braunstein & Link 2019). In the 1970s, the approach was taken up by Ralf Bohnsack, who used it in a project on police work (Schütze & Bohnsack 1973: 278). In 1977, Bohnsack embarked on a collaboration with Mangold and – over the course of ten years – developed a new methodological foundation for group discussion based on Karl Mannheim's sociology of knowledge (Mangold & Bohnsack 1988; Bohnsack 1989). Further variants of the group discussion, also based on Mangold's work, were developed by Thomas Leithäuser and Birgit Volmerg (1979), Manfred Nießen (1977) and others. In these reinterpretations of the method, group discussion became one of the central approaches to data production in German-speaking qualitative research.

Another line of Lewin's late work, which was more openly credited in the literature on social research methodology, is action research. The German movement of *Aktionsforschung* started in the late 1960s and used social research to foster "democratic values" and social emancipation of underprivileged groups. Drawing explicitly on Lewin (1946), its proponents argued that action research could help to find solutions to social problems and support social engineering (Lewin 1946: 202; Lück 1996a: 128–130; Unger et al. 2007: 10). During the students' movements of the 1960s, this idea became very popular in Germany. Through the active involvement of research participants in the research process and systematic reflection of the relationship between researchers and research subjects, action research promised to be an answer to both political and methodological questions (e.g. the question if social research can ever be free of value judgements, Adorno et al. 1969). The first German action research projects were initiated by young scholars in the fields of social work and education. Pioneer studies were conducted in Hamburg in 1969, Berlin in 1970, and in Wiesbaden and Marburg in 1971 (see Altrichter 2008: 33). In these projects, each local group developed their own distinctive approach and later published methodological as well as empirical reports on their work (e.g. Fuchs 1970; Haag et al. 1972; Heinze et al. 1975). After a few years of high visibility (about 400 publications in ten years), action research lost popularity in the early 1980s. According to Hella von Unger et al. (2007: 19), it disappeared quickly and thoroughly from the methodological landscape in the mid-1980s. In the last fifteen years, it was rediscovered and substantially revised under the label *Partizipative Forschung* ("participatory research"; Unger 2014) and is widely used in German-speaking qualitative research today.

One more line of influence is worth mentioning: Lewin's methodological argument for an in-depth analysis of individual cases. Valuable scientific generalizations or laws, Lewin (1930/31) argues, are not the result of abstraction and quantification from a large number of cases. On the contrary, only the detailed and context-sensitive analysis of concrete, individual cases and situations brings about valuable generalizations (Lewin 1930/31: 455–456). This idea, which Lewin framed as a transition from an *Aristotelian* to a *Galilean* way of thinking (Lewin 1930/31, see Lück 2021 [this volume]: 5), left deep traces in contemporary qualitative research. It was particularly important in psychology, where quantitative approaches are dominant up to the present day (Schulze 2020: 605; Tateo 2013). But we find it also in textbooks for readers from all disciplines, such as the widely read introduction to sociology by Gabriele Rosenthal (2008). Rosenthal refers to Lewin (1927; 1930/31) for an epistemological justification of biographical case studies (1995: 210) and – more generally – for social research with small case numbers (Rosenthal 2008: 75–76).

Sequence analysis, audio-visual data, group discussion, action research, and case studies: these are five (relatively independent) lines of reception of Lewin's work in qualitative research, and there might be more to find. It must be assumed that reconstructing the impact of Lewin on research methodologies in a more detailed and systematic way will add an important dimension to the history of the social sciences.

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Kapitel 3

Hans Hermas Überlegungen zur Bildhaftigkeit des Films im Vergleich mit anderen Darstellungsformen – Vorstellung einer Arbeit aus den Forschungen zum Film am Wiener Institut für Psychologie in den 1930er Jahren

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This chapter examines the doctoral dissertation of Hans Herma (Johann/John Leopold Herman, * 1911 in Wien, † 1966 in New York), *Die Bildhaftigkeit des Films*. This dissertation was part of an extensive research program at the Vienna Institute for Psychology in the 1930s. Herma's work focused on a specific aspect of film: its ability to take the viewer out of their immediate perceptual environment and place them in the scene represented by the film. According to Herma, film goes beyond the deictic possibilites of a novel through its ability to offer an *ad oculos* demonstration. Taking the pictorial character of film as his starting point, Herma provides in his dissertation a detailed and empirically based description of the perceptual psychological foundations of film and how they interact with the technical features of the medium.

1 Einleitung

Im Folgenden wird die Dissertation *Die Bildhaftigkeit des Films* (1938) von Hans Herma (Johann/John Leopold Herma, * 1911 in Wien, † 1966 in New York) vor-

gestellt. Sie war Teil umfassender Forschungen zum Film am Wiener Institut für Psychologie in den 1930er Jahren. Basierend auf Karl Bühlers Arbeiten, erforschten mehrere seiner Studierenden und MitarbeiterInnen das Medium Film im Vergleich mit sprachlichen und anderen Darstellungsformen. Dabei betrachteten sie Film als Mittel zur Darstellung einer im weitesten Sinne *anschaulichen* Wirklichkeit, also von Gegenständen, physikalischen und sozialen Sachverhalten, vor allem aber Handlungen, die einem Publikum filmisch kommuniziert werden sollen. Im Rahmen dieser Forschungen ging es um die Klärung der Funktionsweise und der Möglichkeiten des Mediums selbst, auch wenn die Idee, menschliche Mimik auf Filmstreifen kontextuell zu fixieren und wiederholbar zu machen, als ein projektinitiierendes Moment im Kontext der Ausdrucksforschungen am Institut angenommen werden kann (vgl. Czwik 2018: 34f.). Käthe Wolf (Katherina/Katherine M. Wolf, * 1907 in Wien, † 1967 in New York) leitete die Forschungen. In deren Zentrum standen vor allem Spielfilme, die in den Wiener Kinos gezeigt wurden, andere Genres fanden jedoch auch Berücksichtigung. Von einer phänomenologischen Basis ausgehend, wurde die individuelle Wahrnehmung von Film mittels empirischer Studien erforscht, um filmspezifische Regelmäßigkeiten in Abläufen des filmischen Kommunikations- und Interaktionsprozesses nachzuweisen.

Die Ergebnisse wurden niemals, wie ursprünglich geplant, publiziert.[1] Viele beteiligte WissenschaftlerInnen und Studierende waren 1938 gezwungen, das Land zu verlassen. Sie trugen das in Wien Erarbeitete nicht mehr zielgerichtet in weitere medienpezifische Forschungen – ihre Arbeiten wurden international nicht wahrgenommen und gerieten in Vergessenheit. Die unveröffentlichten Ergebnisse, liegen uns heute großteils nur in Form von maschinengeschriebenen Dissertationen in der Universitätsbibliothek Wien vor. Sie sind durch diesen Umstand einer breiteren Wahrnehmung entzogen und stellen gewissermaßen einen *missing link* im Schriftgut dar. Darüber hinaus bieten sie Kommentare, Vertiefungen und Interpretationen zu Karl Bühlers eigenen Arbeiten durch sein unmittelbares Arbeitsumfeld – seine zeitgenössischen GesprächspartnerInnen, InstitutsmitarbeiterInnen und Studierende vom Fach – die gerade in Hinblick auf die magere Quellenlage zu Bühlers Forschungen in den 1930er Jahren Relevanz erhalten.[2]

Hier wird ein Überblick der Forschungsergebnisse Hans Hermas gegeben. Seine Arbeit fokussiert auf eine spezifische Möglichkeit des Films: Die Fähigkeit, die

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[1]Bühler (1938: 201) spricht von einem „[...] druckreifen Buch über den Film aus dem Wiener Institut. Käthe Wolf und ihre Mitarbeiter werden es veröffentlichen."

[2]Die Verfasserin arbeitet im Rahmen eines Dissertationsprojektes über die Forschungen zum Film am Bühler-Institut.

ZuseherInnen aus dem sie unmittelbar umgebenden Wahrnehmungsraum (Kinosaal) zu versetzen und sie filmisches Geschehen tatsächlich *erleben* zu lassen. Die romanähnlichen deiktischen Möglichkeiten von Film sind Herma zu Folge durch eine *ad oculos*-Demonstration erweitert, die vom Publikum als nahezu Wirklichkeit wahrgenommen wird – eine vom Regisseur geschaffene Wirklichkeit. Er beschrieb in seiner Arbeit, ausgehend vom *bildhaften* Charakter des Films, detailliert und empirisch fundiert die wahrnehmungspsychologischen Grundlagen des Mediums im Zusammenspiel mit seinen technischen Mitteln.

Die Dissertation Hermas wurde 1938 nicht mehr von Karl Bühler beurteilt. Bereits im Mai des Jahres begutachteten Otto Tumlirz und Richard Meister die Arbeit. Karl Bühler befand sich vom 23. März bis zum 7. Mai 1938 in Schutzhaft. Er wurde von der Universität Wien entfernt, in Ruhestand versetzt und mit dem 31. Juli 1938 auch von der Stadt Wien gekündigt, für die er im Kontext ihres Pädagogischen Institutes tätig gewesen war (vgl. Stumpf 2018: 72).

Herma war am Wiener Institut für Psychologie als Bibliothekar angestellt und wurde, eigenen Angaben zu Folge, 1937 in der Nachfolge von Egon Brunswik als Assistent in Betracht gezogen (vgl. Benetka 1995: 248–248 und Pakesch 2019: 167). Ein Kontakt zwischen Herma und Bühler bestand auch in der Zeit nach Bühlers Inhaftierung. Dies geht beispielsweise aus einem Schreiben Ingeborg Bühlers an Charlotte Bühler aus dem Jahr 1938 hervor, das Markus Stumpf im Kontext der Schwierigkeiten um die Einlagerung von Bühlers Möbeln und Büchern zitierte:

> Aber wenn im Juli alles eingestellt ist und er – wahrscheinlich mit Herma od. einem anderen Schüler irgendwohin aufs Land geht um zu arbeiten [...] (AUW, NL Charlotte und Karl Bühler, Schreiben Ingeborg Bühler an Charlotte Bühler, 8.6.1938; zitiert nach Stumpf 2018: 79)

Herma flüchtete wie weitere MitarbeiterInnen von Karl und Charlotte Bühler zunächst nach Genf und arbeitete im Umkreis Jean Piagets am Rousseau Institut. Im Mai 1940 verließ er die Schweiz und emigrierte nach Amerika, wo er trotz zahlreicher Empfehlungen bei seinen Versuchen sich neu zu etablieren auf Schwierigkeiten stieß (siehe Fleck 2015: 233–234). Eli Ginzberg, Professor für Wirtschaftswissenschaften an der Columbia University, mit dem Herma als Assistenzprofessor an der New York University zusammenarbeitete, fasste Hermas wissenschaftliches Leben zusammen:

> I would like to talk with you about Hans Herma, my friend of two decades – of his childhood on a farm in Slovenia; of his secondary and university education in Vienna, where he had the finest of academic training in philosophy under world-famous leaders of symbolic logic, and in psychology

under Karl and Charlotte Buehler; of his first exposure to psychoanalysis; of his witness to the erosion of Austrian socialism and its replacement by Nazi imperialism; of his break with his homeland and flight to Switzerland, where he had the good fortune to join the research group of Jean Piaget; of his immigration to the United States, through the intercession of Lawrence Kubie; of his initiation to American academic life at St. Lawrence University, and his retreat therefrom; of his war service, both military and civilian, during which he was a collaborator of Ernst Kris; of his years of teaching at New York University; of his active participation as a faculty member in various psychoanalytical training institutions – for the past 14 years at the National Psychological Association for Psychoanalysis – and of his years in the private practice of psychotherapy (Eli Ginzberg über Hans Herma in Ginzberg & Bergmann 1966: 173–177).

2 Was ist ein Bild? – Die Bildkriterien nach Hans Herma

In seiner Dissertation *Die Bildhaftigkeit des Films* (1938) wählte Herma den bildhaften Charakter filmischer Darstellungen als seinen Ausgangspunkt – er nahm eingangs einen Vergleich von Bild und Film im Sinne Lessings vergleichender Unterscheidung von Dichtung und Malerei in seinem Laokoon vor, versuchte Bildkriterien zu bestimmen und festzustellen, inwiefern Filme diesen entsprechen, also Bild bzw. bildhaft sind (Herma 1938: 1–22). Darauf aufbauend arbeitete er Spezifika filmischer Darstellungen heraus.[3] Lessing räumte der Malerei nur in eingeschränktem Maße die Möglichkeit ein, einen Handlungszusammenhang darzustellen:

> Die Malerei kann in ihren coexistierenden Kompositionen nur einen einzigen Augenblick der Handlung nutzen, und muss daher den prägnantesten wählen, aus welchem das Vorhergehende und das Folgende am begreiflichsten wird. (Lessing 1876: Kapitel XVI, 167, Zeile 4–7)

Herma schloss sich in diesem Punkt Lessing an. Die Malerei wäre tatsächlich in der Lage räumliches, aber nicht oder nur ein andeutungsweises zeitliches Nebeneinander darzustellen. Allerdings hätte Lessing mit dieser Feststellung kein „Bildkriterium" oder ein Kriterium der Malerei gegeben, sondern ein allgemeines Kriterium bildender Kunst, welches für den Film jedoch nicht gelte, obwohl

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[3]Herma stützt sich hierbei vor allem auf Karl Bühlers Schriften, die filmtheoretischen Arbeiten von Rudolf Arnheim (1932), Béla Balázs (1930; 2001 [1924]), Sergej Eisenstein (2006 [1923–1948]) und Wsewolod Pudowkin (2004 [1926]; 1933).

dieser bildhaft zweidimensional ist. Da Herma gerade die *Zweidimensionalität* als entscheidende Übereinstimmung von Bild und Film ins Auge gefasst hatte, war das Lessing'sche Kriterium zur Bestimmung eines Bildes für ihn nicht ausreichend relevant. Bildliche zweidimensionale Darstellungen können, Herma zu Folge, Lessings Kriterium unterliegen, sie müssen es aber nicht. Herma suchte nach anderen Charakteristika (Herma 1938: 2f.). Er griff auf Bühlers Kapitel zur Gemäldeoptik in seiner *Erscheinungsweise der Farben* (1922) zurück. Bühler schrieb darin über Unterschiede von Malerei und Plastik:

> Das Werk des Architekten und des Bildhauers tuen sich im realen Wahrnehmungsraume auf, psychologisch präziser gefasst, der Sehraum der Beschauer ihrer Gebilde trägt die Wirklichkeitsprägung, und wenn die Illusion hineinspielt, so ist es in vielen Punkten wie mit der Verwandlung des Theaterpodiums, dass eben das Ganze verwandelt wird. Es soll nicht bestritten werden, dass sich der Beschauer eines Gemäldes ebenso versetzt fühlen kann aus der Wahrnehmungssituation des Standortes hinüber in den Raum des Künstlers. [...] Aber davor liegt in der nachschaffenden malerischen Vision das Andere, Spezifische, daß der binokularen parallaktische Wahrnehmungsraum am Farbenblatt abgeschlossen ist und trotzdem eine anschauliche Erweiterung erfährt. Um es noch einmal zu sagen: Wie der Bildraum als solcher d.h. mit seiner eigenartigen Unwirklichkeitsprägung aus dem Sehraum herauswächst, dies ist das psychologische Sonderproblem der Gemäldeillusion. (Bühler 1922: 205-206)

Bild und Plastik werden also durch ihr Verhältnis zum Raum des Betrachters unterschieden. Plastik, bzw. das dreidimensional Dargestellte, ist in unseren Wahrnehmungsraum direkt mit einbezogen, was Herma unter anderem an der Gültigkeit der Bewegungsparallaxe aufzeigte. Das im Bild Dargestellte befindet sich hingegen nicht in unserem Sehraum – die Verdeckungsverhältnisse bleiben gleich, auch wenn wir uns bewegen (Herma 1938: 4f.).

Damit setzte Herma ein erstes für seine weiteren Ausführungen relevantes Bildkriterium fest. Ohne Dreidimensionalität, die unserem Sehraum eigen ist, ohne die Gültigkeit der Bewegungsparallaxe, muss sich Malerei anzeichenhafter Momente bedienen. In solchen machte schon Leonardo da Vinci die Überlegenheit der Malerei gegenüber der Plastik aus (Kuhn 1988). Die Malerei bediene sich hierbei der Zusammenstellung von Farben in einem bestimmten Kontext und wandle entlang der Funktionsweise unserer Wahrnehmung von Gegenständen ihren *Palettenwert* in einen *Bildwert* um (Bühler 1922: 188). Der Maler oder

die Malerin entwickelt einen eigenen (einheitlichen) *Bildraum*, eine eigene Bild-Perspektive, eine eigene Beleuchtung, die dem menschlichen Sehraum nachempfunden sind. Indem die Farbelemente auf die Leinwand gesetzt werden, wird diese zu einem Feld. Die Ordnung innerhalb des Feldes entspricht einer *Relationstreue* der einzelnen Elemente unter einander im Verhältnis zur Wahrnehmungswirklichkeit – Herma beschrieb es als *Zweiklassensystem* indirekter Darstellungsmittel (Herma 1938: 6. Vgl. Bühler 1934: 73f. und 188f.).

Das zweite Bildkriterium war für Herma der Rahmen oder die Begrenzung – abgeleitet aus dem Geltungsbereich der Feldgesetzlichkeit des Bildes und ihrem Ende. Der Rahmen bestimme auch das Verhältnis des Dargestellten zu ihm, den Vorder- und Hintergrund, den Maßstab und die Blickrichtung, senkrecht und waagrecht im Bild sowie die Lage der Figuren im dargestellten Raum. Er ersetze den Koordinatenausgangspunkt subjektiver Orientierung im realen Raum des Betrachters (Herma 1938: 10f.).

Ausgehend von der für Herma (und Bühler 1922 [1918]: 144–153) zentralen Rolle einer tief verankerten räumlichen Orientiertheit in der Wirklichkeit als nicht bewusste Schemata des Menschen untersuchte Herma nun die Frage, wie ein illusionärer Raum (Bild als erscheinungstreue Abbildung) mit dem Wahrnehmungsraum der wirklichen Wahrnehmungswelt des Betrachters *hic et nunc* kooperiert, um ein drittes Bildkriterium herauszuarbeiten. Hierzu strich er die Bedeutung des *Zeigens (Deixis)* im kommunikativen Prozess und seine Rolle im Rahmen der *Steuerung* eines Gegenübers heraus,[4] für das jedenfalls eine gemeinsame Orientierung in einem intakten Wahrnehmungsraum erforderlich sei (Herma 1938: 14ff.). Karl Bühlers Ausführungen zur *Deixis im Sprechverkehr* (vgl. Bühler 1938) folgend, ist hierzu aber keine Orientierung in einem wirklichen Raum *ad oculos* nötig, vielmehr kann sowohl Fiktives in den präsenten gemeinsamen Wahrnehmungsraum lokalisiert werden (*dramatische Deixis*), als auch ein gänzlich fiktiver, nur vorgestellter Raum zur Orientierung herangezogen werden. Der aktuell präsente Raum wird hierbei nahezu ausgeschaltet: *Epische Deixis* – wir begeben uns in den vorgestellten Raum (Herma 1938: 16). Bilder erfordern von Betrachtenden genau dieses epische Versetztsein in Bühlers Sinne.

1. Bildkriterium: Die Bewegungsparallaxe wird aufgehoben.

2. Bildkriterium: Ein Bild ist immer etwas Begrenztes (Umrahmtes). Eine Bereicherung gegenüber der Wirklichkeit. Der Rahmen/die Begrenzung gibt die räumlichen Relationen vor und ersetzt das Koordinatensystem der subjektiven Orientierung mit dem Ausgangspunkt *hier*, *jetzt* und *ich*.

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[4]Zu Steuerung vgl. Friedrich (2018: 149ff.)

3. Bildkriterium: Die *epische Deixis*; Der Betrachter wird in den Bildraum versetzt. (Bei Statuen findet laut Herma eine echte *demonstratio ad oculos* statt.)

3 Der Film ist Bild – Der Film ist kein Bild

Herma beschrieb nun die *drei Antinomien des Films*: Auf den ersten Blick scheinen die herausgearbeiteten Bildkriterien auch für Filme zu gelten. So bleiben etwa die Verdeckungsverhältnisse gleich, egal von welchem Sitz im Kino man den Film ansieht, eine rahmenhafte Begrenzung scheint der Film ebenso aufzuweisen. Dies gilt allerdings nur für ZuseherInnen, die weiterhin ihre Orientierungspunkte in der Wirklichkeit des Kinoraumes finden. Für das wahrnehmungsmäßig versetzte Filmpublikum werden die Bildkriterien aufgehoben. Genauso wie im alltäglichen Wahrnehmungsraum gibt es im Filmraum eine Bewegungsparallaxe. Ermöglicht wird sie durch Veränderungen des Standpunktes der Kamera und des Bildausschnittes als Koordinatenausgangspunkte der Orientierung. Ebenso entzieht sich bei einem konzentrierten Verfolgen der Handlung der Rahmen unserer Wahrnehmung und das zweite Bildkriterium wird aufgehoben (Herma 1938: 23–37). Der Rahmen hinterlasse jedoch Spuren im Erlebnis: Durch ihn würden wir davon abgehalten, die Filmwirklichkeit für unsere eigene Wahrnehmungswirklichkeit zu halten, und stellten uns als bloße ZuschauerInnen nicht so handelnd ein, wie zu unserer Wirklichkeit. Er bliebe auch als Maßstab für alle Relationen im Bild erhalten (Größe, Entfernung). Gerade seine Wirksamkeit ermögliche die verschiedenen Kameraeinstellungen, meinte Herma (1938: 127–128). Er habe die Funktion Wesentliches gegenüber Unwesentlichem abzuschließen, bzw. das Unwesentliche auszuschließen – zugleich ermögliche er ein über das Einzelbild hinausgreifendes räumliches Erlebnis, von dem das sichtbare Einzelbild nur ein Teil wäre. Der Rahmen wirkt im Film also als selektives Prinzip und Instrument, er selektiert das, woran uns der Film interessieren will und ist für Herma (1938: 145–149) ein Instrument des Zeigens,

> das aus einem Kontinuum eines potentiellen Raumes das jeweils Wichtige zu bezeichnen und hervorzuheben hat. (Herma 1938: 148)

Durch diesen Funktionswechsel zwischen bildhaftem Abschließen und Hervorheben einer Auswahl verschwinde der Rahmen für das Publikum.

Um festzustellen, ob die Ausschnitthaftigkeit des Filmbildes im Erlebnis der ZuseherInnen bleibt oder ob diese versetzt sind und die Ausschnitte tatsächlich

nicht bemerken, führte Herma empirische Studien durch: Den Versuchsperso-
nen wurden Zeichnungen vorgelegt, mit der Frage, welche von sieben Einstel-
lungen im Film ganz allgemein die drei häufigsten wären. Dies wurde für Fil-
me, die in Wiener Kinos liefen und dem Publikum gut bekannt waren, wieder-
holt. Hermas Auswertung der Versuche ergab eine nur sehr grobe Übereinstim-
mung mit den tatsächlichen Kamera-Einstellungen. Die ZuschauerInnen täusch-
ten sich den richtigen Verhältnissen gegenüber. Sie überschätzten etwa die Zahl
der den Gesamtraum zeigenden Totalaufnahmen deutlich. Aus dieser Überschät-
zung schloss Herma, dass das Publikum jedenfalls in den Filmraum versetzt und
der Rahmen in ihrer Wahrnehmung weitgehend ausgeschaltet war (Herma 1938:
126–145).[5] Im Gegensatz dazu steht die Beobachtung, dass auch eine überdeut-
liche Wahrnehmung der Nahaufnahmen festgestellt wurde. Herma meinte, dies
geschehe, da eine Nahaufnahme so stark wirkt, dass sie durch das intensive Er-
lebnis von den Versuchspersonen überschätzt wird. Sie zeige mehr von dem, was
etwas über die inneren Vorgänge der Personen aussagt (Ausdrucksresonanz der
Nahaufnahmen des Gesichtes – wir reagieren resonanzmäßig; Herma 1938: 143-
144; siehe auch Wolf 1938: 496-497).

Auch das dritte Bildkriterium, die *epische Deixis*, fand Herma filmspezifisch
verändert und darin eine Begründung für die Aufhebung der ersten beiden Krite-
rien: Dem Publikum werde das gezeigt, worauf im vorangegangenen Bild hinge-
wiesen wurde. Der Film übernehme die Rolle eines *steuernden Gegenübers* in der
Wahrnehmungssituation. Zugleich benehme er sich aber auch, als ob er gesteuert
würde: er wende sich selbst dem Gezeigten zu. Eine gemeinsame Orientierung
in einem Wahrnehmungsraum ist normalerweise die Voraussetzung des Zeigens
(Herma 1938: 29). Nun saßen die KinobesucherInnen der 1930er Jahre im Kino-
saal, nicht aber in der Aufnahmesituation des Filmes, wo der Schauspieler oder
die Schauspielerin der Kamera *zeigte*. Bühler, mit dem sich Herma laufend aus-
tauschte, kam auf diesen wesentlichen Aspekt filmischer Zeigetechnik in seinem
Vortrag zum 11. Internationalen Kongress für Psychologie 1937 zu sprechen:

> Die Schauentfernung von meinem Sitzplatz im Kino bis zur Leinwand än-
> dert sich nicht [...] Geboten aber wird mir etwas, was in immer wieder
> wechselnder Entfernung und in wechselndem Aufblick auf das dargestell-
> te Objekt von der Linse aufgefangen worden ist. Das geschieht natürlich
> nicht, hat aber eine wichtige Konsequenz, dass ich mich zu dem Geschehen
> auf der Leinwand ähnlich verhalte wie der Beschauer eines Bildes an der
> Wand welcher im Aufhängungsraum des Bildes umhergeht und bald nah,

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[5]Mit Abbildungen und detaillierter Auswertung.

bald fern, bald rechts, bald links vor dem Bild stehenbleibt. Im Kino wechsele nicht ich den Platz aber es springt der Aufnahmeort des Objektes. Und das bedingt trotz aller Tiefe in einem entscheidenden Punkte die prinzipielle Bildhaftigkeit des auf der Leinwand gebotenen. (Bühler 1938: 201)

Die Kamera wendet sich dorthin, wo sich das Gezeigte befindet. Das versetzte Publikum im Kino fasst nun laut Herma den Standpunkt der Kamera als seinen eigenen auf und wird somit zum gesteuerten Gegenüber. Die Orientierung der Kamera im realen Raum und die Orientierung der ZuschauerInnen im Bildraum sind nahezu identisch. Herma sah das filmspezifische Zeigen vor allem darin begründet, dass es wie in der wirklichen Wahrnehmungswelt, gelinge, einen gemeinsamen Wahrnehmungsraum zu schaffen – die eigentlich *epische Deixis* wird beim Film als *demonstratio ad oculos* erlebt (Herma 1938: 34-65).[6] Nur Film ist Herma zufolge hierzu in der Lage – mit Hilfe der Zeit, die er anders als Bilder tatsächlich darstellen kann. Im Bild bleibt das Zeigen episch und für nicht versetzte ZuseherInnen, die sich noch im Kinoraum orientieren, bleibt auch der Film Bild. Für die Versetzten ist er jedoch *scheinhaft aktuelle Wirklichkeit* (Herma 1938: 32). Der Bildcharakter des Filmes und alle Bildkriterien sind aufgehoben.

4 Versetzung und Orientierung im Filmraum

Methodisch versuchte Herma das Phänomen der Versetzung und Orientierung in einem anderen Wahrnehmungsraum mit empirischen Studien zum Bewegtsehen aufzuzeigen, um subjektive Beschreibungen des eigentlich untersuchten Erlebnisses *Orientierung* selbst zu vermeiden (Herma 1938: 36). Ausgehend von der Feststellung der Gültigkeit der Bewegungsparallaxe im Zustand der Versetzung ging er der Frage nach, ob ein bestimmter Gegenstand im Film bewegt erschien oder nicht, bzw. in welcher Weise er bewegt erschien. Zentral für Hermas methodisches Vorgehen ist die Abhängigkeit des phänomenologischen Bewegtsehens von räumlicher Orientierung. Herma stützte sich dabei auf die gestaltpsychologischen Untersuchungen von Karl Duncker (1929), Wolfgang Metzger (1936), Erika Oppenheimer (1935) und Walter Krolik (1935). Er griff Karl Dunckers These zu Bedingungen der Wahrnehmung von Abstandsänderungen als Bewegung auf – seien es auch Scheinbewegungen (induzierte Bewegungen). Karl Duncker:

Die phänomenalen Bewegungen der an einer Abstandsänderung beteiligten Objekte (ihre Bewegungsanteile) bestimmen sich in erster Linie nach

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[6]Mit exemplarischen Filmauswertungen.

> Art und Grad der gegenseitigen „Lokalisiertheit" der Objekte [...] Auf die
> kürzeste Formel gebracht: phänomenale Bewegung ist Verschiebung im na-
> türlichen Bezugssystem. (Duncker 1929: 246)

Es erscheint immer das Objekt bewegt, das zu einem anderen Objekt lokalisiert
wird und wir lokalisieren immer das umschlossene Objekt zu einem umschlie-
ßenden Objekt (Herma 1938: 38; vgl. Duncker 1929: 246-248). Das *Umschließungs-
gesetz* wirkt laut Herma im Film auch dann, wenn im Bild eigentlich nichts Um-
schließendes zu sehen ist und zwar sogar stärker, als der an sich für das Publi-
kum sichtbare Rahmen. Ein bereits hervorgerufenes Raumschema reicht aus –
es kann aus einer zuvor gezeigten Weitaufnahme stammen und muss nicht im
aktuellen Bild sichtbar sein. Gelingt es einmal nicht, dieses Bezugssystems im
Film zu erhalten, so tritt der Rahmen plötzlich hervor und ein ruhendes Objekt
scheint sich, z.B. zur Kamera hin, zu bewegen (Herma 1938: 48-50). Eine geeigne-
te empirische Studie zur Untermauerung seiner für den Film entwickelten These
über Bewegung und räumliche Orientiertheit der ZuseherInnen fand Herma in
Walter Kroliks Arbeit über *Erfahrungswirkungen beim Bewegtsehen* (Krolik 1935),
in der unter anderem gezeigt wurde, wie die orientierungsgebenden Raumver-
hältnisse gleich einem abrufbaren Schema auch durch *Anzeichen* (z.B. ein Haus
für eine ganze Landschaft) jederzeit aktualisiert werden können (Raumzeitlicher
Horizont aller Handlungen; Herma 1938: 42; vgl. Krolik 1935).

Anhand zahlreicher Filmbeispiele und Kamerasituationen wendete Herma die
Duncker'schen Bewegungsgesetze auf filmische Szenen an und illustrierte seine
These, dass Bewegungserscheinungen im Film und die Ausschaltung des Rah-
mens als Bezugssystem ohne eine bildsystemgerechte Orientierung (Versetzung)
nicht zu erklären sind – umgekehrt Bewegungseindrücke als Beweis für die tat-
sächliche Versetzung der ZuseherInnen dienen können (Herma 1938: 37-65).[7]

Um das intensive Versetzungserlebnis weiter herauszuarbeiten führte Herma
zusätzlich Publikumsbefragungen durch, in deren Rahmen es ihm um eine ver-
gleichende Erlebnisreproduktion von Film- und Theatereindrücken ging. Er stell-
te die These auf, dass sich bei geringerer Versetzung die ursprüngliche Orien-
tierung im Wahrnehmungsraum rückschauend deutlicher erhalten haben muss.
Das Theater wählte er, weil die äußere Situation dem Kino ähnlich ist, zugleich
aber alle Momente, die für die Versetzung im Film zutreffen, beim Theater ent-
fallen. Tatsächlich konnten sich die Befragten zwar an Theaterräume, nicht aber
an Kinosäle erinnern (Herma 1938: 106-109).

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[7] Als Material dienten ihm zahlreiche zeitgenössische Kinofilme.

5 Sieht das Publikum den Film auf die vom Regisseur vorgesehene Weise?

Ist nun der Filmraum aus den Bildern des Films einmal aufgebaut, die KinobesucherInnen hineinversetzt, die Realität weitestgehend ausgeschaltet und der Film mit dem Charakter aktueller Wirklichkeit versehen, so stellt sich die Frage, wie dies von den Zusehenden verarbeitet wird, in weiterer Folge, ob der Regisseur tatsächlich in der Lage ist, sie *seine* Wirklichkeit erleben zu lassen.

Herma erwähnte in seiner Dissertation Vorversuche zur Wiederspiegelung der psychologischen Situation der Versetzung im Bewusstsein der ZuschauerInnen, die den Umfang einer eigenen Arbeit hätten. Hinsichtlich des Erlebens verglich er die Filmwirklichkeit mit Träumen und strich einige Ähnlichkeiten hervor: Im Traum würden Vorstellungen vom Träumenden als Wahrnehmungen aufgefasst, was nur durch die Sprengung des Systems Traum und das Erwachen beendet werden könne. Hierin zieht Herma eine Parallele zum Film. Auch bei diesem müssten sich die ZuseherInnen aus der Versetzung befreien, um sich der Steuerung ihres Intellekts durch die Anschauung entziehen zu können. Ähnlichkeiten sah er auch darin, dass die Steuerung nicht bewusst von den Zusehenden/Träumenden abhängt (Herma 1938: Schluss. I–V). Ein wesentlicher Unterschied zum Traum wäre Herma zu Folge, dass das Phantasma des Films zugleich reale Wahrnehmung sei. Es findet eine tatsächliche *demonstratio ad oculos* statt – die Augen sehen wirklich. Die filmische Deixis ist für Herma daher verschieden von jener in der Vorstellung (Herma 1938: 30). Unterschiede bestünden auch in der Art der Steuerung: Film könne nur so lange steuern, als die Steuerung nicht auf einen Widerstand stoße. Darüber hinaus komme das Steuern beim Traum von innen, beim Film von außen. Anschließend an Josef Gregor verglich Herma daher das Filmerlebnis mit einem hypnotischen (Herma 1938: Schluss. I-V; vgl. Gregor 1932).

Über die Macht der Versetzung schrieb Herma:

> Der Regisseur verlegt sozusagen seinen Zuschauerwillen in den Aufnahmeapparat, indem er ihm seine Orientierung verleiht, und der Apparat zwingt seinen Willen dem Zuschauer auf, indem er ihm das Koordinatensystem des Apparates als das des Zuschauers erscheinen läßt. (Herma 1938: 155)

Diese Beschreibung erinnert an Ausführungen von Béla Balász:

> Die Bilder enthalten in ihrer Einstellung die Einstellung des Regisseurs zum Gegenstand […] Daher die propagandistische Gewalt des Films. Denn er

braucht einen Standpunkt nicht zu beweisen; er läßt ihn nur optisch einnehmen. (Balázs 1930: 33f.)

Dennoch ist Herma der Ansicht, beim Film käme die Steuerung nicht in dem vollen Maß von außen, wie es zunächst scheinen mag. Für ihn ist Film ein vom Regisseur für viele geträumter Traum und zugleich ein Abkömmling der Persönlichkeiten der Zusehenden (Herma 1938: Schluss. I-V). Die ZuschauerInnen bleiben Handelnde und bauen aus den Bildern des Films einen Raum auf – nur in ihrem jeweils *individuellen* Erlebnis werden sie in einen nur für *sie* existierenden Raum versetzt:

> Ein weiteres Argument dafür, dass der Film eine im höchsten Grade subjektive Kunst ist, eine Kunst, die so subjektiv ist, dass es fast paradox erscheint, wenn man seine technischen Grundlagen bedenkt. (Herma 1938: Schluss. V)

Hermas Auffassung bezüglich der räumlichen Orientierung im Film scheint prinzipiell der *topomnestischen Orientierung* bei Bühler zu entsprechen (Bühler 1934: 131, 146–147).

Er versteht die Raumschaffung durch ZuseherInnen in einem intrinsischen Sinne. Das Publikum ist versetzt in den Filmraum, den es sich – subjektiv – vorstellt. RezipientInnen erschaffen, als an der Kommunikation Beteiligte, eine Art *mental map*, eine mentale Repräsentation des Filmraumes (siehe Figur 1). Ohne ein Gelingen dieser aktiven Verarbeitung und Syntheseleitung (Eisenstein – siehe etwa: Eisenstein 2006 [1923–1948]: 115f.) könne Film nicht wirken.

Herma geht davon aus, dass Film Wahrnehmung, Emotion und Denken des Publikums durch seine Möglichkeit *ad oculos* zu zeigen, tatsächlich steuern kann, meint aber zugleich, nicht nur formale Mittel wären ausschlaggebend, sondern eine bestimmte psychische Situation der ZuseherInnen. Form und psychische Situation müssten auf einander abgestimmt sein, um das Interesse des Publikums zu erhalten oder zu steigern (Herma 1938: 151).

6 Steuerung durch die Kamera

Die Steuerung des Publikums setzt Herma bei der Kamera an: Sie benehme sich so, wie die Zusehenden sich benehmen würden bzw. sollen. Der Regisseur denkt in Filmbildern.[8] Er denkt als Publikum seines eigenen Filmes (Herma 1938: 153).

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[8]Vgl. Pudowkin (2004 [1926]: 265–274): der Regisseur stellt sich Ereignisse in jener Form vor, in der sie, aus Einzelstücken zu einer Bilderfolge komponiert, auf der Leinwand erscheinen werden.
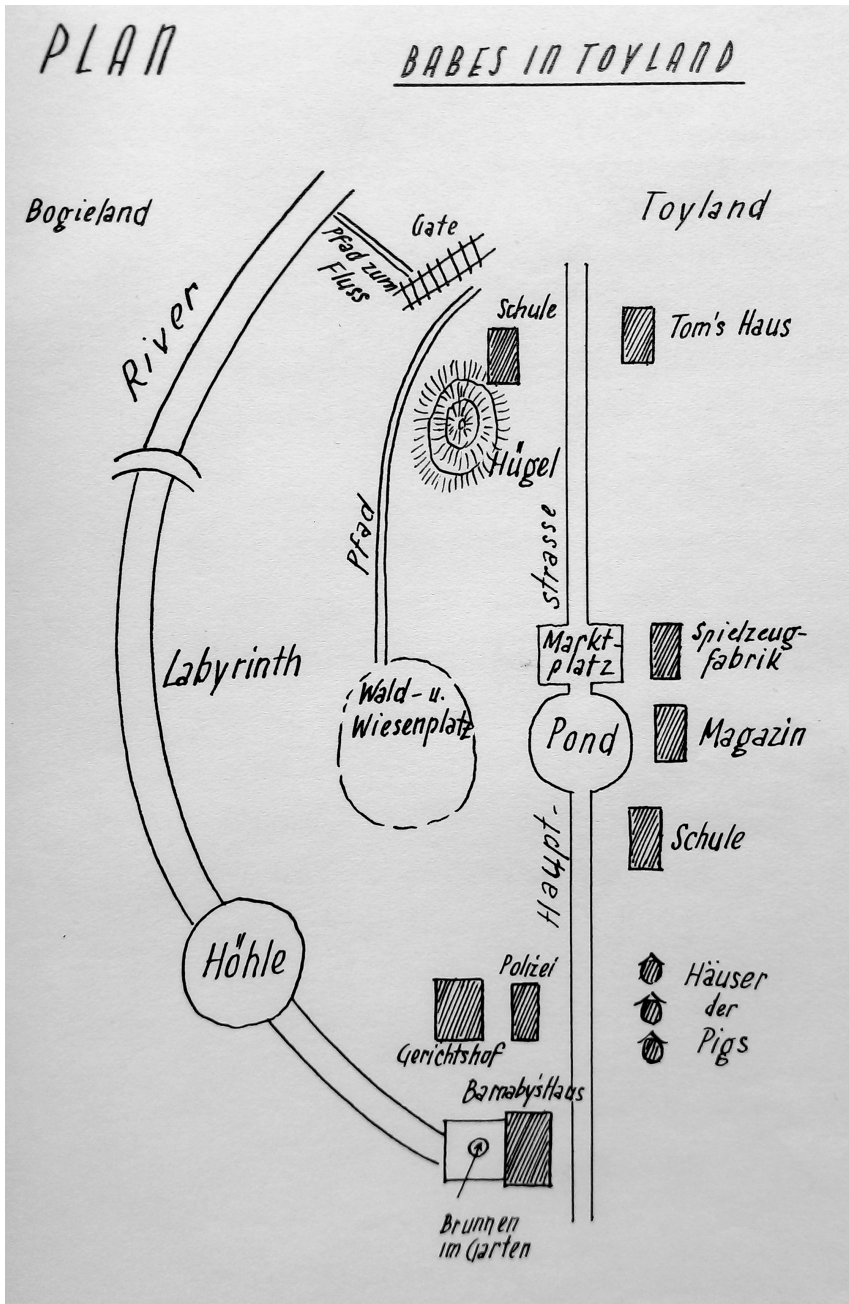
Abbildung 1: Topomnestischer Filmraum des Filmes „Babes in Toyland/ Rache ist süß". Quelle: (Herma 1938)

Durch Beleuchtung, Schärfe der Einstellung, Linienführung oder Bewegtheit der Objekte wird das Auge des Publikums gelenkt. Unwesentliches wird durch die Kameraeinstellung ausgeblendet. Auch Mimik und Gesichtsausdrücke (Nahaufnahmen) lassen in Bewegung alles andere in den Hintergrund treten. Ereignisse sind aus den Kamerabewegungen (Sprüngen) aufgebaut, die in irgendeiner Weise mit der natürlichen Transferierung der Aufmerksamkeit fiktiver ZuseherInnen korrespondieren müssen, welche schließlich durch ein reales Publikum ersetzt werden. Jedes Bild gibt der Aufmerksamkeit einen Impuls (Herma 1938: 155–158).

Den Möglichkeiten der Kamera – vor allem den *Kamerasprüngen* – widmet Herma in seiner Arbeit relativ viel Raum (Herma 1938: 66–104). Er wertet Versuche aus, die Karl Bühler schon in seiner Sprachtheorie (1934) erwähnt hat:

> Wir haben an einigen gerade laufenden Filmen die Kamerasprünge abgezählt und die unerwartet große Anzahl von durchschnittlich 500 gefunden. Etwa 80–90% davon gehören zu den bereits erwähnten Perspektivensprüngen. [...] Sorgfältig gewählt sind im Film die Größensprünge. (Bühler 1934: 393-394)

Die ProbandInnen hatten die Kamerasprünge zumeist gar nicht bemerkt. Herma zählte durchschnittlich 440–800 Sprünge bei einem Film mittlerer Länge (exkl. Bildmontagen und kontinuierliche Kamerabewegungen).[9]

Das Tempo des Filmes könne bis zur „Grenze" der Apperzeptionsgeschwindigkeit der ZuseherInnen hinaufgesetzt werden. Einheiten würden dabei nicht gänzlich verschwinden, wie es etwa bei einem Mosaik der Fall wäre. Dennoch störe auch ein rascher Bildwechsel die Zusehenden nicht, denn durch die verschiedenen Blickrichtungen wären sie damit beschäftigt ihren eigenen Filmraum aufzubauen (Herma 1938: 74). Dieser Filmraum bestehe aus allem, was vor dem gerade sichtbaren Bild schon einmal gezeigt wurde, was wirkend eingriff oder an Zeichen erkennbar war. In ihn könnten die Einzelbilder eingeordnet werden, ohne die Orientierung zu stören, weil sie nicht als Einzelelemente aufgefasst würden, sie wären nur immer weitere Details in dem schon vorhandenen Raum. So wie das Sehfeld nur ein aktueller Teil eines potentiellen Raumes ist, der für ein orientiertes Individuum als Ganzer präsent, aber nicht sichtbar ist. Beim Aufbau des Bildraumes durch Blickpunktwechsel und Distanzsprünge der Kamera müsse der Film nur auf die Wahrung eines *einheitlichen* Raumgefühls bei verschiedener

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[9] Ausgewertet wurden etwa: *Die Kameliendame*: 6500 Sekunden/108 Minuten/538 Sprünge; *Romeo und Julia*: 7324 Sekunden/122 Minuten/791 Sprünge; *Fury*: 5323 Sekunden/88 Minuten/521 Sprünge; *San Francisco*: 6492 Sekunden/108 Minuten/918 Sprünge. Siehe Herma (1938: 69).

Entfernung der Gegenstände Rücksicht nehmen, um die Größenkonstanz zu erhalten (Herma 1938: 94-96).[10]

Schwierigkeiten für den Film stellen, Herma zu Folge, Unterbrechungen der Raumkontinuität dar, wie sie etwa durch Schauplatzwechsel hervorgerufen werden können. Generell gelte: Für den Film ist jedes Nacheinander in Bildern auch ein Nacheinander in der Zeit. Die Größe des verfließenden Zeitabschnitts ist mit einer wahrgenommenen räumlichen Distanz verbunden. Der Film kann nicht in der Zeit herumspringen (vgl. auch Spiel 1935: 14). Dies unterscheide ihn von anderen Darstellungssystemen wie der Sprache. Herma bezieht sich auf Béla Balász:

> Das erste Problem der Bilderführung entsteht durch die Unkonjugierbarkeit der Bilder. (Balázs 2001 [1924]: 84)

Film lasse mit dem eigenen Raum auch eine eigene Zeit entstehen, die sich aus inhaltlichen Momenten wie vorgestellten räumlichen Verhältnissen verschiedener Orte aber auch aus dem Verhältnis der Handlungen zueinander aufbaue. Gestützt auf Publikumsbefragungen beschrieb Herma die Zeitsteuerung im Film als eine Interferenz aus Filmsteuerung und Selbststeuerung der Zusehenden. Film brauche die reale Zeit des Publikums und nutze sie, um darauf seine Zeit aufzubauen. Diese bliebe immer an die Realzeit gebunden. Da die Filmzeit in dieser Weise auf der erlebten Gegenwartszeit der ZuschauerInnen aufbaue, gebe es für den Film auch nur die Gegenwart, nur ein Nacheinander, kein früher als das *Jetzt* im Erleben, woraus sich Herma zu Folge der Aktualitätscharakter des Filmes ergebe (Herma 1938: 168–179). Karl Bühler beschrieb dies als ein Charakteristikum der *ad oculos*-Demonstration, die sonst eigentlich die *dramatische Deixis* auszeichnet (Bühler 1938: 202).

7 Schluss

Da Hermas etwa 200 Seiten umfassende Arbeit, wie eingangs erwähnt, bislang nur als unveröffentlichte Originalquelle in Wien vorliegt, dürfte sie relativ unbekannt und wenig diskutiert worden sein. Die Hauptergebnisse Hermas liegen in der Klärung der *deiktischen Möglichkeiten* des Films, der Beschreibung der *Versetzung*, der *Orientierung im Filmraum* und der *Steuerung* im Zusammenspiel eines gezielten Einsatzes technischer Mittel mit der psychischen Einstellung des in seiner Wahrnehmung geführten raumaufbauenden Publikums. Seine Dissertation

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[10]Er verwies hier auf Versuche und eine eigene Arbeit über Größenkonstanz auf Photographien, die im Archiv für die gesamte Psychologie erscheinen sollten (Beitrag VIII der Unters. über Wahrnehmungsgegenstände v. Egon Brunswik).

wurde hier herausgegriffen, da sie die vermutlich klarste Einsicht in die Inhalte des Gesamtprojektes zum Film liefert.

Die Wiener Ergebnisse wirkten auch in späteren Projekten der Beteiligten weiter, bei denen Film als Analyseinstrument genutzt wurde. Dies zeigt sich beispielsweise in Zusammenhang mit den Arbeiten der Projektleiterin Käthe Wolf. 1952 sprach sie auf einer Konferenz in New York in der Diskussion einige Schwierigkeiten bei der filmischen Beobachtung von Kleinkindern an, die sofort an die Wiener Arbeiten der 1930er Jahre denken lassen:

> K. Wolf: […] It seems important to have the possibility of moving one's eye while observing the infant. […] The motion picture focuses on the child from a definite angle which the observer of the motion picture cannot modify or supplement.
>
> I. A. Mirsky: But you are trying to get a formal expression of the movement. I can't imagine why a motion picture of such movement wouldn't be very helpful.
>
> K. Wolf: I can only tell you it is a fact.
>
> I. A. Mirsky: The observer is a variable in the experiment under your conditions.[…]
>
> K. Wolf: […den Film betreffend] I really think that the limited view is the most inhibiting factor. Please do not misunderstand me, I don't say that the movie wouldn't be a good check on one's observation. The ideal situation would be to observe the baby and then observe the movie and compare them. […] (Aus Wolf 1953: 104)

Es war Hans Herma, der in seiner Dissertation die Ausschnitt- und Bildhaftigkeit des Films erforschte und die immersive Kraft des Films beschrieb, dem es gelingt, die Zusehenden über das „vor Augen führen" in seine eigene Narration zu versetzen, welcher sie sich nur mehr schwer entziehen können.

Hermas Arbeit bietet einige sehr innovative Ansatzpunkte. Abschließend soll hier ein kurzer science-fictionaler Ausblick Hermas auf eine mögliche Erfindung von plastischem Film erwähnt werden: Auch für diesen bliebe, so Herma, die Versetzung das wesentliche Kriterium – ohne sie würde selbst ein solcher zukünftig Bild bleiben (Herma 1938: 23). Anhand der Entwicklungsgeschichte von *Virtual Reality*, innerhalb der ein möglichst hoher Grad an *Immersion* als ein Ziel verfolgt wurde und wird, kann diese visionäre Annahme Hermas gut nachvollzogen werden. Zur Steigerung des Eintauchens dient nicht unbedingt Dreidimensionalität, sondern eben der nun fehlende Rahmen, das räumliche Bewegungs- und

Orientierungserlebnis und die hinzugetretenen Interaktionsmöglichkeiten. Wie von Herma für den Film beschrieben, wird bei VR-Erlebnissen die Darstellung von Raum- und Zeitsprüngen, etwa zu einem *Früher*, durch den noch stärkeren *hic et nunc*-Charakter weiter erschwert. Durch die starke Immersion sieht sich VR jedoch auch mit neuen Fragestellungen hinsichtlich möglicher Narrationen konfrontiert (siehe etwa Ryan 2015).

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Das *nichtsprachliche* Darstellungsmittel Film – Diskussionen am Wiener Psychologischen Institut. Kommentar zu Maria Czwik

Janette Friedrich

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Der Text von Maria Czwik 2021 [this volume] gibt Einblick in eine Forschungspraxis, die selten Gegenstand von Psychologiegeschichtsschreibung bildet. Diese ist oft auf die *Maîtres* des Faches konzentriert oder interessiert sich in externalistischer Weise für die sozialen bzw. institutionellen Bedingungen und Praxisformen von Forschung. Dissertationen spielen meistens nur dann eine Rolle, wenn ihr Autor zu einem wichtigen Vertreter seiner Disziplin avancierte.[1] Wissenschaft wird jedoch seit der Wende vom 19. zum 20. Jahrhundert immer mehr zu einem kollektiven Unternehmen. Dies kann gerade auch in den sich neu konstituierenden Disziplinen wie der Psychologie beobachtet werden. Die Gründung von psychologischen Instituten und Laboratorien ermöglichte eine verstärkte Einbeziehung von Studentinnen, Doktorandinnen und jungen Forscherinnen vor allem in die Durchführung empirischer Studien. Im Rahmen der Wissenschaftsphilosophie thematisierte Lakatos in den 1960ern die Entwicklungsprozesse in den Naturwissenschaften mit dem Begriff des *Forschungsprogramms* und reinterpretierte die Geschichte der Wissenschaften ausgehend von diesem Gesichtspunkt. Ein Forschungsprogramm verfüge notwendigerweise über einen *harten Kern (hard core)*, d.h. eine Reihe von theoretischen Postulaten, Hypothesen, Forschungswegen, die als Grundlage und Orientierung für die weiteren Forschungen unumstritten angenommen werden (sogenannte negative Heuristik). Ihre konsequente Anwendung und Umsetzung bei der Analyse schon bekannter oder

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[1]Hier kann exemplarisch auf Karl Popper verwiesen werden, der 1928 bei Karl Bühler zu Fragen der Denkpsychologie approbierte, eine Dissertation, die nicht nur in die deutschsprachige Werkausgabe aufgenommen wurde, sondern auch Übersetzungen in andere Sprachen fand.

neu entdeckter Phänomene führe dann zu detaillierteren Kenntnissen, sowie zur Weiterentwicklung des Programms selbst, ohne dieses jedoch in Frage zu stellen. Lakatos zeigt ausführlich wie dies abgesichert wird. Dazu führt er den Begriff des *Schutzgürtels* (*protective belt)* ein: Hypothesen, die entwickelt werden, um den *harten Kern* zu komplettieren und die Erkenntnisse zu erklären und damit zu neutralisieren, die dem Forschungsprogramm widersprechen. Genau diese Verfahrensweisen ermögliche, Lakatos zufolge, eine relative Autonomie der theoretischen Wissenschaften (Lakatos 1982: 49). Er verweist auf solch fundamentale Forschungsprogramme, wie die mechanistische Theorie des Weltalls, illustriert es aber auch an spezifischeren Programmen wie das von Bohr. Sein Modell scheint mir auch für die Sozial- und Geisteswissenschaften interessant und zwar gerade dort, wo ein besonderes Augenmerk auf die Entwicklung von theoretischen Modellen (von Begriffswelten) gelegt wird. Das war bei Karl Bühler der Fall. Seine Bücher zur Sprache und zum Ausdruck tragen nicht zufällig den Begriff *Theorie* im Titel. Auch seine letzten Arbeiten in Wien, die er in den USA fortzusetzen suchte, galten der *Theoretischen* Psychologie. Theorie heisst für Bühler Entdeckung von Axiomen, bzw. Entwicklung von Modellgedanken, mit denen die zu analysierenden Phänomene theoretisch erfasst werden. Dieses Forschungsprogramm dann zu „realisieren", d.h. in der Analyse der Phänomene zu benutzen, zu komplettieren, Widerlegungen zu neutralisieren, dazu ist ein Kollektiv notwendig, eine strukturell und hierarchisch gut organisierte Forschungseinheit. Dies traf auf das Psychologische Institut der Universität Wien zu. Es gehörte zu den dynamischsten Forschungseinrichtungen der Psychologie in Europa. Durch von der Rockefeller-Stiftung über mehrere Jahre gezahlte Fördergelder konnten vier Forschungsgruppen mit einer relativ hohen Anzahl von Mitarbeitern finanziert werden. Das erklärt auch die grosse Zahl von Dissertationen, die damals am Institut unter Leitung von Karl Bühler approbiert wurden. Zwischen 1923 und 1939 waren es ca. 200, die Mehrzahl davon (ca. 150) wurden zwischen 1930 und 1939 eingereicht (siehe Friedrich Im Druck). Karl Bühler und seine Frau Charlotte verfügten über einen grossen Kreis von Mitarbeitern, die an der Betreuung der Dissertationen teilnahmen.

An den Dissertationen lässt sich das Einschreiben in ein Forschungsprogramm und der Versuch es zu nutzen, zu testen, zu bestätigen und zu erweitern, gut beobachten. Der Text von Czwik zeigt diesen Aspekt, besonders dort, wo sie den Dialog zwischen Herma und Bühler zum Gegenstand macht. Nun stellt sich die Frage, um welches Forschungsprogramm handelt es sich? Czwik bezieht sich auf einige der Hauptwerke Bühlers: auf die *Ausdrucks-* und *Sprachtheorie* und sie resümierende und erweiternde Texte, aber auch auf die 1927 veröffentlichte *Krise der Psychologie.* Das von Bühler hier skizzierte Forschungsprogramm ist, so wür-

de ich es formulieren, dem Problem der „Steuerung durch Medien" gewidmet. Beide hier verwendeten Begriffe sind zentral für sein Denken. In der *Sprachtheorie* gibt er am Beginn des dritten Kapitels, das dem Symbolfeld gewidmet ist, eine Definition von Sprache, die selten wirklich ernst genommen wird. Er schreibt:

> Und diese zweite Einsicht lautet in prägnantester Einkleidung so: das sprachliche Darstellungsgerät gehört zu den *indirekt* Darstellenden, es ist ein *mediales* Gerät, in welchem bestimmte *Mittler* als Ordnungsfaktoren eine Rolle spielen. Es ist nicht so in der Sprache, dass die Lautmaterie kraft ihrer anschaulichen Ordnungseigenschaften direkt zum Spiegel der Welt erhoben wird und als Repräsentant auftritt, sondern wesentlich anders. Zwischen der Lautmaterie und der Welt steht ein Inbegriff medialer Faktoren, stehen (um das Wort zu wiederholen) die sprachlichen Mittler, steht z.B. in unserer Sprache das Gerät der indogermanischen Kasus. (Bühler 1982 [1934]: 151)

Bühler analysiert in der *Sprachtheorie* das Funktionieren von Sprache und das, was er dabei entdeckt, lässt ihn Sprache als etwas *Mediales* vorstellen. Nun kann man den Terminus *medial* auf das gerade zu Anfang des 20. Jahrhunderts in der Psychologie und Parapsychologie so eifrig diskutierte *Medium* beziehen, eine Person, der besondere (oft übernatürliche) Kräfte und Fähigkeiten zugeschrieben werden.[2] *Medial* bezieht sich aber auch auf eine Raumposition, etwas was in der Mitte liegt, mittig ist. Beides ist treffend, denn Bühler interessiert, was Sprache *macht*, wenn man sie verwendet, worin ihre Leistung besteht, d.h., was ein Sprecher oder Hörer tut und tun muss, wenn er spricht und versteht, ob er es will oder nicht. Was passiert, wenn man Sprache benutzt (ein Medium im Raum ist), worin bestehen ihre Kräfte (ihre Leistung)? In seiner Antwort auf diese Fragen akzentuiert Bühler den Aspekt des Steuerns, des Geleitet- und Orientiertwerdens durch die Sprache, dabei kommen die von ihm sogenannten Mittler (Ordnungsfaktoren) ins Spiel, die Sprache zu einem indirekten Darstellungsmittel machen. Diese Mittler zu identifizieren und zu beschreiben steht im Zentrum seiner Forschungen (vgl. Friedrich 2009: 21–58). Neben dem Begriff des *Medialen*, sind noch zwei andere Begriffe zentral für sein Forschungsprogramm: der Begriff der *Steuerung* und der der *Orientierung*. Beide werden von Czwik auch in Hermas Dissertation nachgewiesen. Während Bühler den ersten Begriff 1927 in der *Krise der*

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[2]Hier könnten eine Reihe von Beispielen aus der Geschichte der Psychologie bzw. der Sprachwissenschaften zu Beginn des 20. Jahrhunderts angeführt werden, die sich der Untersuchung der damals in den Salons des Bürgertums praktizierenden Medien widmeten. Man denke an die beiden berühmten Genfer Gelehrten: den Psychologen Théodor Flournoy, der in seinem Buch *Des Indes à la Planète Mars* ausführliche Studien und Überlegungen zum Genfer Medium Hélène Smith veröffentlichte und den Linguisten Ferdinand de Saussure, der die von Hélène benutzte Sprache zu identifizieren bzw. zu erklären suchte. Siehe u.a. Fehr (1997: 478 ff.).

Janette Friedrich

Psychologie im Zusammenhang mit Steuerungsphänomenen in der Tierwelt diskutiert, scheint der Begriff der *Orientierung* erst in den 30er Jahren zum Grundelement seines Forschungsprogramms zu avancieren. Besonders in den Arbeiten und Vorlesungen zur Theoretischen Psychologie, die sich auf Fragen der biologischen Psychologie konzentrieren und um den Begriff des *Lebens* kreisen, gibt er der *Orientierung* eine zentrale Bedeutung. Davon zeugt auch ein Briefwechsel zwischen Herma und Bühler aus 1940 der meine These von einem existierenden *Forschungsprogramm* untermauert. Der Brief von Herma zeigt auch, dass Forschungsprogramme etwas sehr Emotionales sind; man ist überzeugt, dass das theoretische Modell funktioniert, dass das Erklärungspotential der Begriffe uneingeschränkt wirkt, man fiebert den Weiterentwicklungen entgegen:

> Ich war sehr froh, von Frau Dr. Wolf, die im Sommer in England war, zu hören, dass Ihre Aufenthaltsformalitäten in Amerika nunmehr endgültig geregelt sind und Sie nun ruhig arbeiten können. Ich bin davon überzeugt, dass die „Theoretische Psychologie" inzwischen schon solche Fortschritte gemacht hat, dass wir hier in dem kleinstädtischen Betrieb schon längst nicht mehr auf dem Laufenden sind und bei unserer Ankunft neu werden umlernen müssen. Ach, wenn es nur schon so weit wär, wie gerne würden wir wieder auf der Schulbank sitzen und uns über die „Orientierung" neu orientieren lassen. Nur drüben sollte man schon sein![3]

Nun einige Bemerkungen zu dem von Czwik detailliert herausgearbeiteten originären Überlegungen Hermas zum Thema Film. Ich möchte den in ihrer Studie aufgezeigten Dialog zwischen dem Forschungsprogramm Bühlers und Hermas Resultaten und Schlussfolgerungen punktuell fortsetzen. Czwik unterstreicht zu Recht, dass Bühler kaum zum Thema Film publizierte, ihn diese Fragen jedoch stark interessierten, wovon nicht nur die approbierten Dissertationen zeugen, zu denen aus den Forschungen von Czwik mehr zu erfahren sein wird (siehe Czwik Im Druck(b) und Czwik Im Druck(a)).

1. Bühler bezog sich schon in der *Sprachtheorie* 1934 vereinzelt auf das nichtsprachliche Darstellungsmittel Film, das erklärt sich aus seinem methodischen Vorgehen. Bei der Entwicklung seiner Zwei-Felder-Lehre und insbesondere bei der Diskussion des sprachlichen Symbolfeldes, benutzt er ein *übergreifendes Vergleichsverfahren*. Durch Gegenüberstellung von sprachlichen und nichtsprachlichen Darstellungsmitteln sucht er zu klären, wie sprachliche Darstellung vor

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[3]Brief Hans Herma an Karl Bühler, Genf, 2. Februar 1940, in: Bühler, Charlotte und Karl; Teil-Nachlass („Exil-Nachlass"), Universitätsarchiv Wien, AT-UAW/131.147.2.2.20, Schachtel 1439.

sich geht. Dabei werden Ähnlichkeiten zwischen dem Funktionieren von Gemäl-
den, Tabellen, Notenschriften, Landkarten, Schauspielbühnen auf der einen Seite
und der menschlichen Sprache auf der anderen Seite herausgearbeitet. Das Inter-
essante an diesem Verfahren ist, dass von Nichtsprachlichem auf Sprachliches
geschlossen wird. Ein Maler, ein Komponist, ein Kartograph benutzen zur Dar-
stellung von Wirklichkeit immer zwei Elemente: (nichtsprachliche) Zeichen (Far-
ben, Noten, Kartenzeichen) und ein Feld (eine durch Umrisslinien vorstrukturier-
te Leinwand; ein durch Gruppen von je fünf waagerechten, gleichabständigen
und parallelen Linien organisiertes Notenblatt; ein durch gerade und gekrümm-
te Linien aufgeteiltes Kartenblatt). All die, die mit Darstellungsmitteln arbeiten,
führen dementsprechend folgende zwei Operationen durch: 1) die Organisation
und Verwandlung eines physischen Gegenstands (z.B. ein Blatt Papier oder eine
physische Malfläche) in ein Darstellungsfeld und 2) die Eintragung von Zeichen
in dieses Feld. Sehr gut lässt sich das an der Malerei zeigen, wo ein durch den
Künstler vorbereitetes Malfeld die Eintragung der Farben steuert, die auf diese
Weise jeweils einen bestimmten *Bildwert* erhalten (Bühler 1982 [1934]: 165, 182).

> Nun angenommen: ein Maler mischt auf der Palette aus schwarz und weiss
> ein bestimmtes Grau und setzt mit gleichem Pinselzug dreimal an verschie-
> denen Stellen physisch den gleichen Farbfleck grau. Der nimmt (passend
> ausgesucht) drei recht verschiedene Bildwerte an: – als Schmutzfleck auf
> dem Tischtuch, – als Schatten irgendwo das zweite Mal, – als aufsitzender
> Reflex ein drittes Mal.[4]

Die Farben, wie alle anderen verwendeten Zeichen, haben Bühler zufolge ei-
nen feldfreien Darstellungswert (die Noten bezeichnen den Notenwert, d.h. die
Tondauer; ein Kreuz bezeichnet eine Kirche). Dieser wird bei der Eintragung auf
das Darstellungsfeld durch feldeigene Bestimmungen ergänzt. So ist die Positi-
onsangabe der durch das Kreuz präsentierten Kirche auf einer Landkarte Auf-
gabe der Feldwerte. Bühler leitet aus dieser Beschreibung der nichtsprachlichen
Darstellungsmittel eine Verpflichtung für den Sprachtheoretiker ab: Er „muss
imstande sein zu zeigen, wie einer, der sich anschickt, mit Sprachzeichen darzu-
stellen, überhaupt ein Feld oder Felder im Pluralis braucht und was sie leisten.
Dass man sie haben *muss*, um darzustellen, ist eine sematologische Grundein-
sicht" (Bühler 1982 [1934]: 182). Wie nun diese Artikulation von Zeichen und Feld

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[4] Karl Bühler Nachlass, Sp.38, in: Alexius Meinong-Institut / Forschungsstelle und Dokumenta-
tionszentrum für Österreichische Philosophie an der Karl-Franzens-Universität Graz, Inv. Nr.
11602-11609; Abschrift in: Bühler, Charlotte und Karl; Teil-Nachlass („Exil-Nachlass"), Univer-
sitätsarchiv Wien, AT-UAW/131.147.3.10.2.2, Material von Achim Eschbach, Schachtel 1561.

in der menschlichen Sprache vonstattengeht, die ebenfalls ein Zwei-Faktoren-System ist, nämlich aus Nennwörtern (lexikalischen Zeichen) und der Syntax (Satzschemen, Platzordnung der Worte im Satz) besteht, wird von Bühler ausführlich in der *Sprachtheorie* analysiert. Auch Czwik macht darauf aufmerksam, dass sich Herma beim Vergleich der Malerei mit dem Film auf Bühlers Bestimmung der Malerei als Zwei-Klassen Darstellungsmittel bezieht und sie verweist auf die von Bühler verwendeten Begriffe des *Bildraumes* und *Bildwertes* (Czwik 2021 [this volume]: 73–74). Dieser Frage müsste jedoch genauer nachgegangen werden, so steht eine explizite Beschreibung des Films als Darstellungsmittel im Sinne Bühlers noch aus. Ansätze findet man in Hermas Überlegungen zum *Filmraum*, die Czwik am Ende ihres Textes anführt. Demzufolge wäre der Filmraum (wie der Malraum) etwas nicht durch den Künstler, sondern durch den Zuschauer Konstruiertes. Der Zuschauer baue sich mit Hilfe der verschiedenen, von der Kamera gegebenen Perspektiven und Blickrichtungen, mit Hilfe dessen, was vor dem jetzigen Bild sichtbar war und dem, was gegebene Zeichen anzeigen, einen „eigenen Filmraum". In diesen werden die verschiedenen Einzelbilder, die sich sukzessive folgen, eingetragen, denn Film ist ja immer etwas zeitlich Ablaufendes. Czwik schreibt: „So wie das Sehfeld nur ein aktueller Teil eines potentiellen Raumes ist, der für ein orientiertes Individuum als Ganzer präsent, aber nicht sichtbar ist" (Czwik 2021 [this volume]: 82). Der Filmraum würde also garantieren, dass der Film vom Zuschauer nicht nur als eine zusammenhangslose Folge von Bildern wahrgenommen wird, sondern als ein Ganzes (eine zusammenhängende Handlung). Dies scheint mir eine interessante Ergänzung der von Bühler entwickelten Ideen. Der Filmraum funktioniert auf Basis der Gestaltung eines Feldes (Raumes), das sich von dem Wirklichkeitsraum, in dem der Zuschauer lebt und agiert, unterscheidet. Der Filmraum gibt vor, wie feldfremde Elemente, in diesem Fall Einzelbilder, eingetragen werden können. Ergänzt werden müsste wahrscheinlich, dass nicht erst der Zuschauer diesen Filmraum konstruiert, sondern dies die eigentliche Aufgabe des Filmemachers ist, man denke an die Technik des Filmschnitts bzw. der -montage.

2. Trotzdem scheint es mir, dass Bühler und auch Herma bei der Diskussion von Film an einem anderen Problem interessiert waren. Davon zeugt der Titel von Hermas Dissertation, der von *Bildhaftigkeit* spricht und Argumente für und gegen das Gleichnis „Film ist Bild" anführt. Kehren wir nochmals zu Bühlers *Sprachtheorie* zurück. Bühler zeigt nicht nur Gemeinsamkeiten zwischen sprachlichen und nichtsprachlichen Darstellungsmitteln, sondern verweist auch auf Unterschiede. Dabei geht es fast immer um den *Bildbegriff*: „Nein, die menschliche Sprache malt nicht, weder wie der Maler noch wie der Film malt, sie ‚malt' nicht einmal wie das Notenblatt der Musiker" (Bühler 1982 [1934]: 191). Seine Reformulierung dieser Behauptung macht klarer, was er unter *Bild* und damit unter *malen*

versteht: nämlich ein unmittelbares, direktes Zuordnungsverhältnis. Er spricht von „direkte(n) Feldabbildungen zwischen den sinnlich manifesten Feldmomenten und dem Darzustellenden" (Bühler 1982 [1934]: 192). Das von ihm angeführte Beispiel macht deutlich, was er meint. Es ist etwas anderes, *zu sagen* „der Kölner Dom hat zwei erst in der Neuzeit ausgebaute Türme" oder es *zu malen* (Bühler 1982 [1934]: 191). Dem ersten fehlt die anschauliche Gleichheit, die dem zweiten eignet. Trotzdem ist auch die anschauliche Gleichheit des Bildes nur relativ, denn Bühler zeigt ja, dass auch das Gemälde (das Bild) durch Feldwerte wiedergibt, d.h. einen eigenen Bildkontext aufbaut. Die erfassten Dinge und Gegebenheiten werden durch die anschauliche Ordnung des Gemäldes vermittelt und damit stellt letzteres indirekt dar. Trotzdem, es bleibt bildlich und wenig willkürlich. Bühler insistiert in der *Sprachtheorie*, dass die durch die Sprache produzierten Zuordnungen um einiges willkürlicher und damit indirekter seien, denn Sprache arbeite mit Mittlern oder Ordnern, die ihren Ursprung nicht in der anschaulichen, sondern in der *begrifflichen Ordnung* haben. Diese begriffliche Ordnung ist die vom Menschen geschaffene Denkordnung, sie wird produziert, wenn die Nennwörter in das Darstellungsfeld der Sprache, in das Satzschema eingetragen werden. Denn dann wird nicht die Wirklichkeit dargestellt, sondern eine bestimmte Art diese zu denken (siehe dazu ausführlicher Bühler 1982 [1934]: 190–195, 236–251).

Der Vorteil des indirekteren Darstellens besteht darin, so Bühler, dass es *Struktureinsichten* ermöglicht, die durch Anschauung, durch Bilder nicht zu haben sind. In der *Sprachtheorie* benutzt er als Beispiel die Ziffernsprache der Mathematik. Stellt man sich eine Skala der Darstellungsgeräte vor, die von den bildhaften bis zu den rein symbolischen (willkürlichen) verläuft, dann befände sich die Ziffernsprache unter den bedingt willkürlichen Darstellungsmitteln, sehr weit am rechten Ende der Skala. Er illustriert dies an der Zahl *3824*. Sie bildet in ihrer räumlichen Anordnung eine bestimmte Ordnung oder Konvention ab, die für alle zählbaren Mengen gilt: von rechts nach links haben die Zahlen den Wert von Einern, Zehnern, Hunderten usw. An dieser Abbildungsform können nun mathematische Struktureinsichten gewonnen werden, z.B. dass die Reihe 0–9, sich in den Zehner-Reihen an rechter Stelle wiederfindet und auch in allen anderen Werttypen (den Hundertern, den Tausendern) wiederauftaucht; oder dass der Sprung zwischen den Ziffern jeweils ein Zehnfaches des vorhergehenden Zifferntyps bildet, usw., usf. Dies wird durch die Zahlen nicht direkt gezeigt, sie malen nicht, aber sie ermöglichen es herauszulesen, zu „sehen". Dabei wird auch etwas Anschauliches benutzt, nämlich die räumliche Organisation der Ziffern, weshalb Bühler eben von einem *bedingt* willkürlichen Darstellen spricht.[5] Wo wäre nun der Film auf dieser Skala zu platzieren?

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[5]Wittgenstein (1995: 311–316) weist ebenfalls auf solch eine Möglichkeit, ‚Wissen' zu produzieren, in seinem Beispiel zur Lehre des dezimalen Zahlensystems hin.

3. Eine Antwort darauf findet man in einem anderen Buch Bühlers, das leider nie fertiggestellt wurde. Nach der *Sprachtheorie* hat Bühler an einem „kleineren" Buch über Sprache gearbeitet. Seine Struktur war konzipiert und Teile schon geschrieben, davon kann man sich in den Nachlassmaterialien überzeugen. Dort finden sich auch mehrere Entwürfe für ein Vorwort:

> Der umfangreichen ‚Sprachtheorie' von 1934 folgt hier ein kurzes Buch; es ist einfacher im Aufbau und wird allen Liebhabern der Sprache verstehbar sein. Neu ist der ganze dritte Teil über die Sprachkunst und die Fragestellung im zweiten Teil über den Sprech-verkehr; geblieben sind die Abschnitte im ersten Teil über Funktion und Bau der Sprache.[6]

Für unsere Diskussion sind die beiden neuen Teile interessant, denn hier setzt Bühler die begonnene Unterscheidung der Darstellungsmittel fort. Er spricht von *Sprachwerk* und stellt dieses dem *Bildwerk* gegenüber. Er formuliert auch eine Aufgabe, nämlich „die Struktur des sprachhaften Werkes vom nichtsprachhaften Bildwerk, dem ruhenden und dem bewegten Bilde abzuheben".[7] Einen Bezugspunkt bildet dabei Lessings Unterscheidung zwischen Poesie und Malerei, oder wie Bühler auch sagt, der Vergleich der Sprache mit dem Bilde des Malers, die Bühler zwar teilt, aber deren Kriterien er für inadäquat hält. Während Herma in seiner Dissertation zu zeigen sucht, dass der *Film kein Bild* ist, steht für Bühler wohl eher im Mittelpunkt, dass der *Film keine Sprache* ist. Aber erfährt man bei diesem Perspektivenwechsel mehr und anderes als das, was Herma herausarbeitet?

4. Um darauf zu antworten, muss eine weitere Bühlersche Unterscheidung in den Blick genommen werden, die er bei der Analyse des Sprechverkehrs ausführlich entwickelt. Sie wird ebenfalls bei Herma diskutiert und ermöglicht eine Klassifizierung zwischen den Darstellungsgeräten, die eher der Gattung Dramatik zugehören und denen, die man zum Epos zählen sollte. Jede Produktion sprachlicher Mitteilung beinhaltet, so Bühler, ein Zeigen und ein Nennen. Wir zeigen mit Hilfe der Zeigwörter und bezeichnen mit Hilfe der Nennwörter, beide Operationen koexistieren in einer sprachlichen Mitteilung. Für Bühler existieren drei Formen des Zeigens mit Sprache: 1. Das Zeigen im Wahrnehmungsfeld (der Koordinatenausgangspunkt ist das *ich*, *jetzt*, *hier*); 2. Das Zeigen am Phantasma;

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[6]Karl Bühler Nachlass, Sp.15-1, in: Alexius Meinong-Institut / Forschungsstelle und Dokumentationszentrum für Österreichische Philosophie an der Karl-Franzens-Universität Graz, Inv. Nr. 11526; Abschrift in: Bühler, Charlotte und Karl; Teil-Nachlass („Exil-Nachlass"), Universitätsarchiv Wien, AT-UAW/131.147.3.10.2.2, Material von Achim Eschbach, Schachtel 1558.

[7]Karl Bühler Nachlass, Sp.34-2, in: Alexius Meinong-Institut / Forschungsstelle und Dokumentationszentrum für Österreichische Philosophie an der Karl-Franzens-Universität Graz, Inv. Nr. 11764-11768 (III: Gemälde, Film und Sprachwerk).

3. Das syntaktische Zeigen. Das Zeigen im Wahrnehmungsfeld wird von ihm als dem Drama eigen identifiziert. Das Drama befindet sich auf dem Niveau der *demonstratio ad oculus*, denn Abwesendes wird in die Gegenwart gebracht, vor die ‚äusseren' Augen der Zuschauer gesetzt und dort zeigend behandelt. In der *Ausdruckstheorie* diskutiert Bühler dies ausführlich mit Hilfe des Theaterkritikers Johann Jakob Engel, der solch Präsentieren (Versinnlichen, Vergegenwärtigen) an Beispielen aus der Bühnenkunst nachweist (Bühler 1933: 44–46). Das filmische Darstellen dagegen wird von Bühler als Deixis am Phantasma diskutiert, deren Hauptcharakteristikum das Benutzen von *Versetzungen* ist. Geht es um eine Handlung, die an einem anderen Ort abläuft, wird also über etwas erzählt, das sich nicht im präsenten Wahrnehmungsfeld befindet, dann müssen Zuhörer oder Zuschauer an den Ort versetzt werden, an dem die Handlung spielt (*Mohammed geht zum Berg*, wie es Bühler ausdrückt). Dort, „am Berg", wird dann gezeigt, können die Zeigwörter benutzt werden. Damit wird der Film von Bühler mit dem Epos und den historischen Erzählungen gleichgesetzt. Wichtig ist für ihn, dass diese Versetzungen sowohl im Epos wie auch im Film meistens ganz unkompliziert und unbemerkt geschehen, obwohl beide ganz andere Mittel benutzen. Im Epos wird mit Eigennamen gearbeitet, Zeiten und Orte werden benannt, z.B. „Paris, Revolution, Napoleon der I.". Der Leser/Hörer versetzt sich selbst in seiner Phantasie an den Ort, in die Zeit, wo es passiert: „Der General Napoleon aus Italien zurück in Paris. Wir sind bei ihm und verstehen das weiterhin Erzählte von seinem ‚hier, jetzt, ich' aus" (siehe Bühler 1982 [1934]: 374). Maria Czwik findet genau diese epischen Dimensionen des Films bei Herma diskutiert. Beim epischen Zeigen wird versetzt, weg aus der Gegenwart und dem Standort (dem Kinosaal) hin zum Handlungsort. Der Film realisiert diese Versetzungen durch Szenenschnitte, Standpunktwechsel, Perspektivensprünge, Grössensprünge, Szenenfolgen. Diese betreffen nicht nur einen Wechsel zu den Handlungsorten hin, sondern auch ein *Sehen* der Handlung vor Ort. Folgt man Czwik dann betont Herma dabei eine Besonderheit des Films, die ihn vom Epos unterscheidet und dem dramatischen Zeigen annähert. Denn im Film sieht man die Handlung mit dem äusseren Auge, sie wird in einem Wahrnehmungsraum gezeigt, nicht im Wahrnehmungsraum *hier*, aber im Wahrnehmungsraum *dort*. Dagegen bemüht die Erzählung eher das innere Auge des Lesers, es wird in einem Vorstellungsraum gezeigt, ich sehe *Paris* vor meinem inneren Auge. Man könnte auch sagen, dass das Bild im Film wahrgenommen, das Bild in der Erzählung vorgestellt ist. Nun macht Bühler in seinem 1938 veröffentlichten Kongressbeitrag eine interessante Feststellung, die das eben Gesagte nochmals anders wertet. Er schreibt: „das bewegte Bild in manchen Punkten weit undramatischer und noch epischer vorgeht als das Epos" (Bühler 1938: 200). Was meint er?

5. Zum einen verweist er auf einen *Entsubjektivierungsschritt*, den man bei der in der Erzählung verwendeten Deixis am Phantasma beobachten kann. Wenn man die Nennworte *Paris* und *Napoleon* mit Sinn erfüllt hat, braucht man nicht mehr die Verständnishilfen der konkreten Sprechsituation (man muss sich nicht vorstellen wer Napoleon ist, noch wo sich Paris ausgehend von meiner lokalen Position bzw. auf der Landkarte befindet), beide Wörter bekommen den Status eines Satzsubjektes, sie bringen die Situation zur *Exposition* und damit kann das Zeigen am Phantasma beginnen.

> Wenn ich ohne Präludien höre ‚es regnet‘, so nehme ich dies Wort als eine Wetterdiagnose in der Sprechsituation; es regnet im Augenblick dort, wo sich der Sprecher befindet, […]. Durch die beigefügte Exposition ‚am Bodensee‘ erfolgt ein Enthebungsschritt: ‚es regnet am Bodensee‘; dies erweiterte Wort kann irgendwo gesprochen sein, sein Sinn ist weitgehend abgelöst von der engsten to-Deixis im Rahmen der Sprechsituation. (Bühler 1982 [1934]: 375)

Das hat für Bühler eine wichtige Konsequenz: „wer im Phantasma zur Sache versetzt ist, *kann vergessen, von wo aus er hinversetzt wurde*" (Bühler 1982 [1934]: 375). Man muss sich das *Origo* (*ich, hier, jetzt*) nicht vergegenwärtigen, um dem Zeigen folgen zu können, genau das scheint Bühler mit Entsubjektivierung zu meinen. Folgt man Herma dann sieht die Versetzung im Film etwas anders aus. Czwik gibt Hermas Standpunkt so wieder:

> Die Orientierung der Kamera im realen Raum und die Orientierung der ZuschauerInnen im Bildraum sind nahezu identisch. Herma sah das filmspezifische Zeigen vor allem darin begründet, dass es wie in der wirklichen Wahrnehmungswelt, gelinge, einen gemeinsamen Wahrnehmungsraum zu schaffen – die eigentlich *epische Deixis* wird beim Film als *demonstratio ad oculos* erlebt. (Czwik 2021 [this volume]: 77)

Das scheint mit Bühlers Behauptung, der Film ist epischer als das Epos, in Widerspruch zu stehen, identifiziert Herma doch dramatische Elemente im epischen Zeigen. Doch wenn man es genauer betrachtet, sagen beide vielleicht dasselbe. Denn die Versetzung im Film bleibt ja eine Versetzung am Phantasma, der Zuschauer kann nicht wirklich in den Raum treten, in dem das Geschehen spielt, deshalb benutzt Herma ja auch den Begriff des Bildraums. Trotzdem begibt sich der Zuschauer in diesen Raum, denn ihm bzw. der Kamera wird gezeigt. Czwik führt diese Idee Hermas immer wieder an, der Zuschauer nimmt gewollt oder

ungewollt die *Origo* der Kamera ein. Man könnte auch sagen, dass auf diese Weise das *ich, hier, jetzt* des Zuschauers im Bildraum nochmals produziert wird, der dadurch seine Bildhaftigkeit verliert. So interpretiere ich die von Czwik wiedergegebene Argumentation Hermas. Damit scheint die Versetzung, die durch den Film realisiert wird, radikaler als die in der Erzählung. In der Erzählung interveniert die Sprache und diese braucht das wahrnehmende und selbst das vorstellende Subjekt nicht wirklich, um dieses zu versetzen. Der Versetzungsbegriff wird, wie Bühler es sagt, entsubjektiviert. Beim Film ist das Gegenteil der Fall und der Verweis auf die *Virtual Reality,* den Czwik bei Herma anführt, verstärkt diesen Eindruck. Der Clou der *Virtual Reality*, vor allem der dreidimensionalen, ist ja, dass man wirklich in einen anderen Raum versetzt wird und in diesem rumspaziert, hinter die Mauer schaut, auf die Terrasse springt…usw., usf. Die von diesen Verfahren beabsichtigte Immersion in den Raum, in dem dann etwas passiert, geht mit wirklichen Bewegungs- und damit auch Orientierungserlebnissen einher und die setzen das *Origo* voraus. Herma scheint nun aus dieser Besonderheit des Films – der irreale Raum wird durch mein Sein in ihm real – zu schlussfolgern, dass der Bildraum verschwunden sei. Er zitiert dies als ein Argument für die These „der Film ist kein Bild". Mit Bühler könnte man aber auch sagen, die Versetzung ist im Film vollkommen, denn das Subjekt wird „wirklich" versetzt und genau deshalb ist der Film epischer als das Epos. Und wie sieht es Bühler zufolge mit der Bildhaftigkeit des Films aus? Das Erleben der epischen Deixis als *demonstratio ad oculus* bedeutet doch nicht, dass der Raum, in dem dies stattfindet, kein Bildraum ist. Auch in der *Virtual Reality* ist der Raum ein Bild, auch wenn es sich um ein der Wirklichkeit sehr ähnliches dreidimensionales Bild handelt. Übrigens läuft man in diesem Bild nicht mit Hilfe der Beine, sondern mit Hilfe von Steuerhebeln herum. Bühler sagt in einem seiner Manuskripte: „Die Bildhaftigkeit des modernen Films ist ein ebenso wichtiges Merkmal des im Kino gebotenen wie die Bewegtheit und bis heute wenigstens ist technisch nicht abzusehen, wie unter Beibehaltung von Kamera und der Leinwand[8] als Projektionsfläche die prinzipielle Bildhaftigkeit je abgestreift werden könnte".[9] In dem Sinne bleibt der Film doch Bild!?

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[8] In der *Virtual Reality* würde die Leinwand sich in der Brille befinden oder auf dem Monitor oder auf dem Display des Handys (z.B. *street view* bei Google Maps) und die Kamera wird zwar wirklich vom Benutzer gesteuert, aber eben muss jemand davor mit der Kamera schon das, was man selbststeuernd sieht, aufgezeichnet haben. Man bleibt dem Willen der Kamera oder genauer dem Konstrukteur der *Virtual Reality* ausgeliefert.

[9] Karl Bühler Nachlass, Sp. 34-1, in: Alexius Meinong-Institut / Forschungsstelle und Dokumentationszentrum für Österreichische Philosophie an der Karl-Franzens-Universität Graz, Inv.Nr.12607; Abschrift in: Bühler, Charlotte und Karl; Teil-Nachlass (ˮExil-Nachlassˮ), Universitätsarchiv Wien, AT-UAW/131.147.3.10.2.2, Material von Achim Eschbach, Schachtel 1556.

6. Schlussbemerkung: Ziel meines Kommentars war es, die von Czwik gründlich herausgearbeiteten Thesen Hermas mit einigen Postulaten und Bemerkungen Bühlers zu ergänzen, zu konfrontieren und zu diskutieren. Es ging nicht darum, eine andere Lösung der Filmfrage vorzuschlagen, es ging überhaupt nicht um Lösungen, sondern um das Andeuten einer Debatte, die damals in Wien begonnen und nie wirklich fortgeführt werden konnte, jedenfalls nicht von ihren Protagonisten. Untermauern wollte ich, was der Text von Czwik zeigt, nämlich dass der von Bühler entwickelte Begriffsapparat relevante Forschungswege für die Analyse sprachlicher und nichtsprachlicher Darstellungsmittel bereitstellt. Schliessen möchte ich mit einem Eindruck, den die Filmfrage verstärkt hat. Auch wenn Bühler das Funktionieren sprachlicher Darstellung aus dem nichtsprachlicher herleitet, sucht er doch immer die Stärke des Sprachwerks zu beweisen. So lässt eine in den Manuskripten gefundene Bemerkung aufhorchen und stellt einmal mehr rein situative Lesarten des Sprechens in Frage:

> [...] dass das Sprachwerk nicht nur dem Grad nach, sondern prinzipiell die Welt, die es bietet, begrifflich durchkonstruiert und aus dem Vollen schöpft, wo es die innere statt die äussere Anschauung mobil macht. Zur Dignität einer neuen Einsicht wird diese triviale Erkenntnis in dem Maße, wie wir es dem <u>Film</u> nachrechnen können, was es bedeutet, wenn ein Darstellungsmittel trotz aller Bewegtheit der Bilder (in der Zeit) im Nichtbegrifflichen und in der Schilderung oder Erzählung von aussen nach innen hin verhaftet bleibt. Die begriffliche Durchkonstruktion der Welt, das ist eine Eigentümlichkeit der Sprache, die ihr auch dort nicht verlorengeht, wo sie <u>anschaulich</u> schildert oder erzählt. Dies Anschauliche aber bedeutet deshalb etwas beträchtlich anderes als das, was das Gemälde und der Film vorzeigen, weil es gar nicht wahr ist, dass die Vorstellung nur ein Abklatsch der Wahrnehmung ist.[10]

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Part II

From Palo Alto
to New York and Pennsylvania

Chapter 4

Perception, awareness, and film practice: A natural history of the "Doris Film"

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This chapter takes a close look at the film at the center of the influential inter-action research project the Natural History of an Interview. The film, commonly known under the alias of its main character "Doris", was made in May 1956 by the anthropologist and cybernetician Gregory Bateson and the cinematographer David Myers. It was subsequently studied in detail by an interdisciplinary group of psychiatrists, linguists, and anthropologists brought together by the psychiatrist Frieda Fromm-Reichmann. Tracing the film's production history and analyzing its cinematic techniques, this chapter considers the film's integration into emerging research procedures, documentary film practices, and experimental film discourse. It is argued that the film formed a transitional object, marking a turning point in approaches to research filming, but also a critical intersection between research film, communication theory, and emerging observational styles in documentary film.

"[…] intelligibility, after all, is an extremely loose concept."
– Hollis Frampton, Annenberg School for Communication, March 28, 1972

1 Creating a Specimen

One afternoon in May 1956, the anthropologist Gregory Bateson and his camera-man David M. Myers visited the home of a woman in Palo Alto who came to be known under the alias of "Doris". They wanted to film an interview between Bateson and Doris for a research project on interaction and communication in family situations. Yet, at the appointed time Bateson and Myers "found the house

empty" (Bateson 1971b: 1). When they phoned Doris a quarter of an hour later, she had returned from a session with her psychiatrist, picked up her five-year-old son "Billy" at a friend's house, and reluctantly invited the researchers into her home. In principle, she had agreed to being filmed. A few weeks before, Doris and her husband, "Larry", had attended a lecture by Bateson on nonverbal communication and family interaction; Doris later told Bateson that she "was quite fascinated with the films we saw in your lecture" (quoted in Birdwhistell et al. 1971: 27). Following the lecture the couple contacted Bateson to express their interest in participating in the project. Doris was aware of the general direction of Bateson's research, relating to him that she had spoken to a group of women after his lecture, who had talked about "how they did or didn't approve of what some mother or other was doing with [...] her child", but failed to "understand what nonverbal communication was" (Birdwhistell et al. 1971). Still, she was obviously surprised when the researchers showed up at her doorstep that afternoon. Bateson ascribed her flustered appearance to the haste in which she prepared for being filmed, "exhibiting the expectable response of a housewife unprepared to receive her visitors – let alone cameras and lights" (Bateson 1971b: 1). During the interview, Doris was "markedly distressed" by the noise of commuter trains passing by the house (Bateson 1971b: 3).

Forming the core of the influential research project the Natural History of an Interview (NHI), this fraught encounter resulted in one of the most intensely analyzed documents in the history of social interaction studies (and film studies, for that matter). With some delay, Myers eventually filmed Bateson interviewing Doris and Billy playing nearby, occasionally interrupting the two adults. A few weeks later, Bateson and Myers again visited Doris' house, filming Billy playing in the garden and being bathed, as well as an informal party with neighbors. On a third occasion, they also filmed an interview with Doris' psychiatrist Robert Kantor. Rather than getting used to being filmed, Doris' discomfort grew. Her reluctant consent to the initial interview became something of a pattern. In the film of the party at her house, she asked Bateson about his plans to film her in a therapy session with her psychiatrist. She had heard about "this new project" from Kantor and expressed her doubts, since it "didn't seem very feasible" to have two other people, Bateson and Myers, in the room during the session. Bateson assured her that there wasn't "any need for that". Doris then agreed, "Yeah, it's okay with me". When the therapy session was actually filmed shortly after, she was, however, "very upset" (*Therapy session, July 28* 1956). Bateson and Myers indeed left Kantor's office after having set up and started the camera. But this did not mitigate her anger. She "felt worse than she had for a long time" and she

was "angry at Bateson for turning up at this point when she did not want him" (*Therapy session, July 28* 1956).

In July, Bateson screened the original interview film and the additional films to a multidisciplinary group of fellows at the Center for Advanced Studies in the Behavioral Sciences (CASBS) at Stanford University.[1] Formed on the initiative of the psychiatrist Frieda Fromm-Reichmann, the group had previously studied the sound recording of a psychiatric interview. Besides Fromm-Reichmann, the initial group included another psychiatrist, Henry Brosin, and the two linguists Norman McQuown and Charles Hockett.[2] Their analysis resulted in an article, authored by McQuown, on "Linguistic Transcription and Specification of Psychiatric Interview Materials" that was published in 1957. Collaborating with linguists (and anthropologists), Fromm Reichmann hoped "to make psychiatry more scientific" by understanding the mechanisms underlying "psychiatric intuition" (Fromm-Reichmann, *My year's fellowship*: 8). She explained:

> As a practicing psychiatrist and psychoanalyst specializing in analytically oriented psychotherapy with psychotics, and as a teacher of psychotherapy, I have been interested for years in the investigation and understanding of those elements effective in the psychiatrist's psychotherapeutic endeavors which have so far defied rational formulation: I may call them, for lack of a better term, "intuitive" processes. (Fromm-Reichmann, *Tentative statement*: 1)

With the analysis of the sound recording almost completed, Fromm-Reichmann invited the anthropologist Ray L. Birdwhistell to join a meeting of the group held in February 1956. Her intention was to extend the initial focus on linguistic and paralinguistic data.[3] Birdwhistell, who at this time taught at the University

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[1]The follow-up film was shot in May or early June 1956, the footage of the interview with the psychiatrist was filmed in June 1956 (Brosin 1971b: 1–2). The dates given by Brosin approximately match with Robert Kantor's statement that the first film (the Doris film) had been made in the fifth month of Doris' therapy, which had started in November 1955 (*Therapy session, July 28* 1956: 1, notes by Kantor). The films were made with funds provided by the Macy Foundation for Bateson's research on the etiology of schizophrenia at the Veterans Administration Hospital in Palo Alto (Fromm-Reichmann, *My year's fellowship*: 8).

[2]Two anthropologists, who, like the others, were fellows at the CASBS at this time, at first also contributed to the project. These anthropologists were Alfred Kroeber and David M. Schneider (Leeds-Hurwitz 1987: 5).

[3]According to Ray Birdwhistell, Fromm-Reichmann "was losing her hearing and knew she needed to see with more control" (quoted in Leeds-Hurwitz 1987: 5).

of Louisville, Kentucky, had done extensive research on body motion behavior, working toward establishing what he termed the science of "kinesics". Responding to this invitation, Birdwhistell suggested that Gregory Bateson, who worked on a project on the etiology of schizophrenia at the nearby Veterans Administration Hospital in Palo Alto, might also be invited to join the group of researchers. Fromm-Reichmann, Birdwhistell and Bateson already knew each other from the Macy Conferences on Cybernetics and the Macy Group Processes Conferences (Leeds-Hurwitz 1987: 21). Bateson and Fromm-Reichmann, moreover, shared an interest in understanding group processes involved in causing schizophrenia. It seems that Bateson was not present at the meeting in February. By July, however, when the next meeting took place, he had the films of Doris and her family at hand to show to the group. Bateson may also have screened earlier films made in the context of his research on family interaction and schizophrenia.

The addition of Birdwhistell and Bateson to the group marked the moment when the project shifted from linguistic and paralinguistic analyses of tape recordings, that is acoustic signals, to audiovisual data and sound film. Whatever else Bateson may have shown at the July meeting, the group quickly settled on the film of him interviewing Doris, which may have been made (besides its original function in the project at the Veterans Administration Hospital) with the incipient project at the CASBS in mind. This choice brought about another change in the direction of research, since, as Bateson observed, it "shifted the project from a study of linguistics and kinesics in psychotherapy, to a study of the natural history of these phenomena in the family constellation" (Bateson 1957: 5). Bateson, of course, was not a psychiatrist, nor was Doris his patient.

Analysis of the film commenced almost immediately. Repeated viewings of the Doris film by the whole group of researchers, a process they termed "soaking", led to a selection of scenes which appeared most promising, or most suitable, for the initial microanalysis (Fromm-Reichmann, *My year's fellowship*: 9). Birdwhistell stayed at the Center for the remaining three months of Fromm-Reichmann's fellowship, instructing the group in methods of microanalysis and working on the kinesic transcription. At the same time, McQuown and Hockett began to analyze the linguistic material. By the end of the summer, the group was able to summarize preliminary results. During the next meeting at the University of Buffalo in October 1956, the researchers began to collate their findings and they also devised a plan for a book publication. It would, however, take several more, increasingly scattered, meetings and another ten years until a final version was completed. Changes in the composition of the group, resulting from internal conflicts and changing research preferences of individual members, caused setbacks. But it was above all the extremely time-consuming work of microanalysis and

microanalytic transcription that caused the delay.[4] In the end, the resulting, massive manuscript, including hundreds of pages of highly technical linguistic and kinesic transcriptions, turned out to be all but unpublishable. A microfilm version of the manuscript was finally deposited at the Joseph Regenstein Library at the University of Chicago in 1971.

Despite its unfortunate publication history, the NHI exerted a substantial influence in such diverse fields as interaction studies, family therapy, sociology, and visual anthropology. As Wendy Leeds-Hurwitz observes in her 1987 social history of the project, it had, by the 1980s, "become well known through the academic grapevine" (Leeds-Hurwitz 1987: 1). Leeds-Hurwitz points out four major innovations: its character as a model for sustained multidisciplinary collaboration, its extensive use of microanalysis and film in research on communication behavior, its seminal contribution to the development of kinesics and paralinguistics, and its role in establishing "the structural approach to communication" (Leeds-Hurwitz 1987: 2). Adam Kendon, whose own research built on and extended the natural history approach and was based on the use of film and video, points out that the NHI marked a turning point in the use of film in communication interaction studies. The project initiated a shift from film as a means of capturing individual expression to a perspective, informed by cybernetics and systems theory, and based on filmic "specimens", that regarded the actors in human communication "as participants in complex systems of behavioral relationships instead of as isolated senders and receivers of discrete messages" (Kendon 1979: 69).

Procedures of highlighting and coding features in the filmic record established an influential form of what Charles Goodwin (1994) has termed "professional vision". This professional vision was, as I will discuss in this chapter, connected to other ways of seeing – specifically, ones that emerged in documentary and experimental modes of filmmaking. In recent years, the NHI and related projects have received renewed scholarly attention across a range of disciplines, including sociology, film and media studies, and literary studies. One reason for this is that the project suggests a connection between media epistemologies, observational practices and interaction theory that appears to speak to present digital culture. Bringing together human observers, technical apparatuses and notational procedures in an "integrated process" (Watter 2017: 37), and looking at social actors as

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[4]Estimates by various group members of the ratio between screen time and analysis time vary from six hours to 100 hours for a one-second segment (Fromm-Reichmann, *My year's fellowship*: 9; Birdwhistell 1970: 12). Birdwhistell remarks that, with improved techniques of analysis, the latter number could eventually by reduced to "less than one hour" per second of screen time (Birdwhistell 1970: 12).

"mere nodal points within a wider matrix of continuous communications" (Watter 2017: 45) resonates with contemporary debates on algorithmic agency and posthuman subjectivity. It also has reverberations in sociological practice theory as well as the turn toward "surface reading" in literary studies. "[F]oregrounding observation and description" brings into awareness, as Heather Love argues, "the complex links between texts and social worlds" (Love 2013: 412).

Such a crossing-over between social science methods, humanistic/aesthetic concerns, and "social worlds" was already, if implicitly, inscribed in the analytic procedures and the film practice of the NHI. In the interstices between professional vision, filmed document, and microanalytic practice, there emerged an aesthetics and an epistemology of film, connecting social research with concerns of film practice but also contemporaneous film theory. This is another aspect of the media archaeological implications of the NHI – an aspect that brings into view interrelations between practices of analyzing body motion interaction and filmic bodies, temporalities and identities. It also affords a look at the intersecting histories of research film and film studies. But back to the Doris film, on which my considerations are based.

2 Watch and Learn!

The use of film was crucial for the NHI, because film was to bring into awareness what was always already in plain view – continuously unfolding, and intuitively registered by us all – yet too complex and too fleeting to be consciously apprehended. Film thus responded to one of the principal theoretical tenets of the project, succinctly stated in Bateson's introduction:

> It is necessary again to insist upon the *unconscious* character of most communication. We are mostly totally unaware of the process by which we make our messages and the processes by which we understand and respond to the messages of others. We are commonly unaware also of many characteristics and components of the messages themselves. (Bateson 1971a: 24)

Countless repeated viewings of brief film segments at various speeds were necessary to perceive, separately, one at a time, the multiple "channels" through which interaction signals were constantly exchanged, revealing ever more complex interrelation patterns. Simultaneously exhausting and intriguing, the process of analyzing and transcribing the filmic record in different linguistic and kinesic registers became, as the group members described it, a source of perpetual discovery:

> Each succeeding listening or viewing will bring additional features to [the observer's] attention, and, if he attempts to tie his transcription to auditory or visual reception, he will be forced to bring into conscious focus items of which he, without such aids, would only be subliminally conscious, or, in the extreme case, totally unaware. (Birdwhistell et al. 1971: 3)

Despite their focus on microanalysis, the researchers did not pay equal attention to all clues and all levels of signals the film encompassed, nor did they assign equal weight to all types of contextual information. Curiously, Doris' discomfort and subdued anger are never discussed, even though Bateson, at least, was keenly aware of these emotions and they are clearly, if intuitively, noticeable in the footage. This is all the more striking, since placing signals in context formed the core of the natural history method. Context was crucial in moving from microanalytic findings to ever larger patterns of behavior from which, eventually, something like meaning might emerge: "As we climb the hierarchic ladder of Gestalten from the most microscopic particles of vocalization towards the most macroscopic units of speech, each step on this ladder is surmounted by placing the units of the lower level *in context*" (Bateson 1971a: 16). If context was a communication theoretical concept that described how messages of different logical types interrelate, it also manifested itself in the filming situation: "the context of a signal emitted by Doris is not merely those other signals which she has recently emitted plus those which she emits soon after; it is also the room in which she is speaking, the sofa on which she is sitting, the signals emitted by Gregory with whom she is talking, and by the little boy Billy, and the inter-relationships among all of these" (Bateson 1971a: 20).

Doris' effective inability to refuse being filmed obviously reflected contemporary gender roles, including her actual or perceived powerlessness facing the researchers. The fact that they "found her quite flurried" (Bateson 1971b: 1) did not discourage Bateson and Myers from proceeding with their plans that afternoon. We can see in this another manifestation of the configuration described by Geoghegan, in which "family therapy's emphasis on the home as the site for nurturing personality and the role of 'the schizophrenogenic mother' in producing mental illness aligned it with a coterie of postwar technologies of gender that produced the home as a site of feminine care and semipublic 'workplaces' as a site of masculine labor" (Geoghegan 2017: 84). It would, perhaps, be too strong to assume that Bateson and Myers inadvertently put Doris in a "double bind", leaving her with no real choice. A certain pressure is, however, undeniable. But ignoring Doris' misgivings also had to do with the researchers' wish to exclusively study *filmed* interaction. The film, in fact, became their prime object of

study. Their focus was on looking at this film as if it preserved traces of "naturally" occurring behavior. In principle they acknowledged that the situation they studied included "two identified persons in the presence of a child, a camera and a cameraman" (Bateson 1971a: 6). Their attention, however, was directed to what was going on *in front* of the camera. This corresponded with the "modest" descriptive goals of the inquiry (Brosin 1971a: 3), its limited concentration on certain types of information with the aim of establishing parameters for future research on interaction communication. "Context", in this sense, only included what was captured in the film image. This required the researchers to downplay the camera's role in shaping the interaction situation, and also Doris' reluctance to being filmed. The film was viewed as containing what Adam Kendon would later call "*specimens* of behavior" (Kendon 1979: 67).

The concentration on what was going on in front of the camera, and particularly between Bateson and Doris, entailed the omission of yet another aspect: the technical and aesthetic features of the film itself. To be sure, it is not my intention to criticize the project for something it never intended to do, or to somehow complete the analysis of the interaction scene.[5] What I wish to consider is how the separated practices of filmmaking and analysis were still entangled with each other, and how, in the perceived gap between these practices, epistemological assumptions about interaction and film developed. References to Myers and to the camera are not entirely absent from the notes on project meetings nor the final report. But cinematographic aspects are usually mentioned only when they present obstacles to analyzing the film images for their interactional content, such as temporal gaps in the recording, out-of-focus shots, too narrow framing, or low image resolution. These aspects, as well as Myers' presence at the scene, however, are part of the media history, sociology and theoretical practice of the NHI. This is reason enough to give them some attention here.

The separation of film practice/data gathering from analysis is interesting in itself, since it points to another paradox inherent in the project: reflexively paying attention to techniques of analysis was, on the one hand, an essential element. On the other hand, some aspects, such as the practices involved in obtaining data, could not be (fully) included in this reflection. Not only would they have exceeded the researchers' capacity to process the already vast amount of data,

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[5]Leeds-Hurwitz (1987: 18) notes that an appendix on "the techniques of manipulating taped and filmed materials" had been planned, but was never written. Kendon rejects Williams and Feld's proposal that research filmmakers should produce "footage that shows the filmer's through-the-camera experience of the event he is filming" (Feld & Williams 1975: 31, quoted in Kendon 1979: 76).

but they would also have marred the presumed autonomy (purity?) of the filmic specimen.

When Myers and Bateson entered Doris' home, Myers immediately started to set up his Auricon 16mm camera on a tripod, facing the living room couch at a slight angle. This enabled him to cover most of the room when using the wide-angle setting of his zoom lens. Although the camera recorded a synchronous optical sound track, Myers also installed a magnetic tape recorder – its microphone, placed on the coffee table, is prominently visible in the film. Since the camera could only hold 100ft magazines, it had to be reloaded every 3½ minutes, resulting in temporal gaps in the film (which are diligently noted in the transcript). The tape recorder ran continuously so that a full audio transcript of the conversation between Doris and Bateson could be made. The researchers treated the gaps in the visual record as mere inconveniences. For the purpose of analysis, they broke down the film into "scenes", "incidents or sequences with beginnings and ends psychologically meaningful to the participants" (Bateson 1971a: 13). Typical titles were "pillow scene", "toy gun scene", "playmate scene", "little green ears scene", or "cigarette scene".

If the initial set-up of the camera suggests, in principle, the intent to obtain a straight record of as large a visual field as technically possible, Myers in practice constantly deviated from this approach. He often zoomed in on Doris and occasionally used pans. Neither the zooming nor the panning were random movements. Rather, Myers' camera appears to react to, and participate in, what is going on in the room. In one instance, panning movements were obviously triggered by Bateson's gaze and then reinforced by eye contact with the boy, Billy. This interaction started in what the researchers called the "second airplane scene", where Billy briefly plays with his toy plane. Most of the time, Myers keeps the camera on Doris and Bateson, who are seated on the sofa. When Billy leaves the frame, Bateson follows him with his gaze. Upon reentering, the camera similarly stays on the boy, who aimlessly ambles toward the terrace door on the left. Billy then turns around and from up close looks at Myers and his apparatus. The camera briefly follows him, but eventually settles on Doris and Bateson, allowing the boy to again leave the frame. Shortly after, Billy reappears once more, continuing to curiously stare into the camera. This time, the camera follows him all the way to the terrace door. When he exits through this door, Myers starts to pan back to Bateson and Doris just before the film roll runs out. Myers here is obviously picking up clues from his surroundings on where to point his camera. In one register, Bateson's looking at the boy could be described as functioning

somewhat like a stage direction, which is then confirmed by the subsequent eye contact between Myers and the boy.[6]

At other times, Myers' camera movements seem to be motivated more by the attempt to expand the field of vision of the camera lens and to connect the film's characters – Doris and Bateson on the sofa, Billy playing in another corner of the room. Trial and error is also evident. Myers follows Doris getting up to look after Billy, who has dashed out of the terrace door. The pan continues toward a window with the blinds down, blocking the view of Billy's outdoor activities; the camera then rapidly swings back to Bateson, alone on the sofa. Apparently noticing the error, Myers turns the camera off. When it is turned on again, Doris and Bateson are once more seated together on the sofa. Zooming in on Doris also appears to have been motivated by interactional clues. It usually occurs during phases when she displays heightened gestural activity or raises her voice to give her speech emotional stress. Sometimes the framing is so close that only her face and upper body are shown. In fact, for long stretches Doris is the only person in the frame, underlining her role as the main character of the film that came to be known under her (alias) name. The one roll of footage shot of her husband, Larry, differs from this material in that Larry is sitting opposite Bateson, his back turned toward the camera, his face only partially visible. He thereby somewhat eschews the role assigned to Doris as the subject of the inquiring gaze of the camera.

3 A Transitional Object

It has often been noted that the Doris film was not a perfect document for interaction analysis, but that it was used because it was readily available (e.g. Birdwhistell 1970: 228). The film's main problem, from the perspective of the researchers, was that Myers' camera too often focused on individual actors rather than the whole scene. This hampered their attempt, crucial to the project, of analyzing interaction between multiple agents rather than individual expressions. Bateson claimed that he and Myers made the Doris film before he "had any contact with micro-kinesics or micro-linguistic analysis" (Bateson 1971b: 2). This should, however, not be taken as evidence that he had not heard of the project developing at the CASBS. It is unlikely that Birdwhistell, whom Bateson may already have consulted regarding earlier films on family interaction, did not inform him about

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[6]Bateson's looking at Billy rather than Doris is interpreted in McQuown's chapter "Collation" (chapter 9 of the NHI manuscript) as "evidence for occasional disorientation" (McQuown 1971: 23).

the meeting in February and the opportunity to participate in the project. Bateson also spoke with Fromm-Reichmann (Bateson et al. 1956: 264). As mentioned earlier, it seems probable that when Bateson made the films about Doris and her family he already had their potential usefulness for this project in mind.[7] What he did not and could not know were the methodological details of how the material would actually be analyzed. In fact, many microanalytic methods and transcription procedures were only developed over the course of the analysis of this film. Commenting on his methodological ignorance, Bateson stressed that he himself had not been aware at the time of the filming that his own "smallest movement[s] and intonation would later be examined" and hence "was not in a position to communicate any self-consciousness" concerning the methods of analysis (Bateson 1971c: 2). He is, in other words, implying that he behaved "naturally".

What, from the perspective of the researchers, appeared as flaws of the Doris film were, however, not merely the result of the lack of a clear-cut cinematic method. Myers did have a distinct method, but he filmed in a style that differed fundamentally from the one that would eventually develop out of the analysis of his film. This latter filming method, to which I will return, was characterized by the requirement to fully and unobtrusively cover whole bodies and complete movement "phrases". Myers, on the other hand, participated in the action. He and his camera joined in the interaction ritual between Doris, Bateson and Billy. But the film also reveals his self-consciousness, particularly when he occasionally turns off the camera on realizing a pointless or mistaken shot. There is an experimental, probing quality to Myers' attempt at capturing what is going on between the actors in the room. This approach, ultimately, left the researchers enough room to select material suited for their purpose, especially since they primarily intended to work with brief interaction sequences. Already during the initial viewings in July 1956 they homed in on two short segments, one 10 seconds long, the other 20 seconds long, that appeared particularly promising (Fromm-Reichmann, *My year's fellowship*: 9). The first one was the "pillow" scene from the beginning of the film; the longer one was the much discussed "cigarette" scene.

With Bateson and Doris in full frame for a relatively extended period of time, this scene provided a somewhat larger context on the dynamics of interaction between the two actors. The scene, roughly, unfolds like this: Doris picks up a cigarette from a pack lying on the coffee table, she taps it on the table, appears to tentatively raise it to her mouth, but then lowers the hand holding the unlit

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[7]This was definitely the case when Bateson asked Doris (recorded, as mentioned above, in the follow-up film) for her consent to filming a therapy session with Kantor.

cigarette again to rest it on her thigh. All the while Bateson, somewhat hesitantly, fiddles with a book of matches. Eventually, Doris lifts her head, turns her face toward Bateson and determinedly puts the cigarette in her mouth. Bateson reacts by striking a match and lighting the cigarette. What the researchers began to see in this interaction over repeated film viewings was the "ritual dance-like lighting of Doris' cigarette" (McQuown 1971: 6). The closely coordinated movements of the two "appeared to mark a critical point in the interviewer-interviewee relationship" that highlighted "the dynamic aspect of their relationship" (McQuown 1971: 6). It was here "that Doris and Gregory achieved the greatest intimacy as evidenced by the adjustments required during the lighting of the cigarette" (McQuown 1971: 8). When Bateson finally makes a hand and arm movement in which he wields his own cigarette "as an orchestral baton", this brief moment of "male-female centered reciprocal" is terminated and the interviewer-interviewee relationship is re-established (McQuown 1971: 8).

From early on in the project, the researchers regarded this scene as hinting at the wider potential of their method. Viewing the segment in slow motion, they observed that Bateson, before eventually lighting Doris' cigarette, had briefly withdrawn the match. He had been unaware of this, but, as Frieda Fromm-Reichmann reports, retrospectively explained his behavior as having been motivated by "the feeling that [Doris] may withdraw and kick any moment, so how would anybody like to light her cigarette?" (Fromm-Reichmann, *My year's fellowship*: 10). Fromm-Reichmann cites this to demonstrate how microanalysis brings into awareness things that could not be known "from listening to a recording of the interview" or "just from observing the scene" (Fromm-Reichmann, *My year's fellowship*: 10). One may also perceive in this exchange a mutual awareness of the awkward situation created by the unwanted visit. One of the initial tasks of the project consisted in sifting the film for similar scenes of interaction that not only promised insights into the psychodynamics of the participants but that also unfolded more or less uninterrupted by camera actions. The researchers hoped that "the spot-intensive analysis" of such scenes could be extrapolated to establish "a topography of interaction for the whole interview" (McQuown 1971: 7). They also hoped to chart "self-regulatory mechanisms" – such as Bateson's use of the cigarette "as orchestral baton" to readjust the interactional relationship (McQuown 1971: 8).

But again, this was only feasible for segments, like the "pillow" and "airplane" scenes, that covered two, if not all three of the actors, for at least a few seconds. In this sense, the whole project hinged on and was, to some extent, shaped by Myers' camerawork. Not fully accommodated to the requirements of microanalytic interaction research, the Doris film formed a transitional object, marking a

turning point in approaches to research filming, but also a critical intersection between research film, emerging observational styles in documentary film and communication theory.[8] Before considering these intersections, a look at Bateson's previous psychiatric research films seems in order.[9] These films provide an often overlooked context for the Doris film, and they can help us understand its transitional role.

What, from the outset, made Bateson's earlier films, all produced between 1951 and 1955, unsuitable for use in the NHI was their lack of sound. At this time, Bateson did not have access to a sound-on-film camera. Instead, he and his then cinematographer Weldon Kees used simple spring-wound cameras with a limited shot length of about 20 seconds. Lack of synchronous sound was not a problem, since the researchers wanted to study nonverbal communication. (The use of an additional magnetic tape recorder for the interview with Doris, allowing for good quality speech-recording, hints at the possibility that this film responded, in part, to the needs of the group at the CASBS.) The earlier project was a direct precursor of Bateson's subsequent research on the role of family interaction patterns in the etiology of schizophrenia. Bateson worked with the psychiatrist Jurgen Ruesch at the Langley Porter Clinic in San Francisco, with whom he co-authored the influential book *Communication: The Social Matrix of Psychiatry* (1951). Starting in 1951, Bateson and Kees visited families which, presumably, had a history of psychiatric problems. They concentrated on recurring daily activities, especially feeding and bathing routines, as in *Hand-Mouth Coordination* (1951). This focus was prompted by the hypothesis, already in place by then, that psychological disturbances resulted from repeated instances of misdirected communication (Ruesch & Bateson 1951: 19). In contrast to the interview with Doris, Kees and Bateson went to great lengths to edit their footage. Some films, such as *Communication and Interaction in Three Families* (1952), had an added soundtrack with an

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[8]I am here obviously using the term "transitional object" in a different sense from how it was introduced in children's psychology in the 1950s by the pediatrician Donald Winnicott. For Winnicott the term described an object that bridges a child's imagination with external reality. It should be noted, though, that Winnicott's work – and specifically his focus on the home as an important scene for psychological study – unfolded in a context not entirely unrelated to the NHI. And one might even see in the Doris film an object that, in the words of the film scholar Annette Kuhn (2010: 83), "inhabit[s] an intermediate position between fantasy and reality".

[9]It might be rewarding for further research to include in this discussion the extensive footage shot by Bateson during his field trip with Margaret Mead to Bali between 1936 and 1939. The overly didactic film series "Character Formation in Different Cultures", although based on footage shot in Bali and New Guinea by Bateson and Jane Belo, is not directly relevant here. The series was made independently by Mead (assisted by film editor Josef Bohmer) without Bateson's participation between 1951 and 1953 (Jacknis 1988: 172).

expository narration. Others, such as *A Problem Child Before and After Therapy* (1955), were silent.

Bateson and Ruesch used these films in academic talks and public lectures – like the one where Doris and her husband saw *Communication and Interaction*. The function of the films and the data gathered from them for the research project, on the other hand, is somewhat unclear. Bateson, in particular, often developed his arguments deductively from theoretical premises, while at the same time suggesting a (tenuous) link with empirical observations (Harries-Jones 1995: 87). He may, as I have suggested elsewhere, have used film and filmmaking as a material aid – or a model – for thinking through theoretical problems (Engelke 2014: 233). The films also allowed him to search for patterns and analogies, and to make comparisons, as stated in *Communication and Interaction*, between "each family's own language of action".[10] If the focus on family interaction overlapped with the Doris interview, Bateson, Kees and Ruesch at this time had neither the methodological skills nor the conceptual apparatus to systematically analyze communication behavior. Kees and Bateson made up for the lack of a systematic approach through aesthetic intuition, spending long hours on editing the research material to tease out interaction patterns. In Weldon Kees, Bateson had found a collaborator who brought his artistic sensibilities to the project while at the same time developing a profound understanding of communication theory.

An artistic polymath who had gained recognition as a poet and as an abstract expressionist painter, Kees quickly became a member of Ruesch and Bateson's research group, eventually co-authoring with Ruesch the influential photographic study *Nonverbal Communication: Notes on the Visual Perception of Human Relations* (1956). Kees not only sustained intense contacts with the San Francisco experimental film scene, but himself made, with equipment borrowed from the research project at the Langley Porter Clinic, the experimental film *Hotel Apex* (1952). Bateson's research on communication systems closely resonated with Kees' own artistic interests, evident in his filmmaking and poetry, in shifting signifiers, contradictory signals and glitches in communication (Engelke 2018: 405–407).

It is revealing that the earlier films reflexively addressed concerns downplayed in the more "scientifically rigorous" film on Doris. In films like *Hand-Mouth Coordination* (1951), Kees and Bateson almost obsessively dwelled on the problem of the cinematographer's inclusion in the scene, only fleetingly touched upon in

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[10]Such patterns and analogies formed an important element in Bateson's theoretical work as outlined already in his article on "Experiments in Thinking about Observed Ethnological Material" (1941).

the analysis of the material on the Doris interview. Kees remarked, "Our picture is so damned documentary that the cameramen (Bateson and me) are always getting into the picture, partly to emphasize that it is a picture about people being photographed, and not something 'spontaneous', that just happened" (quoted in Reidel 2003: 240). If these films were conceived as research films, providing somewhat unspecified observational data, they were also edited in such a way as to illustrate ideas on feedback, circular causality, and metacommunication in interaction systems that included the filmmakers/observers. In *Communication and Interaction* we also get to see what it looked like when Bateson, tape recorder and lights in hand, arrived at the home of one of his subjects – duly recorded by Kees, who even throws in a few shaky shots from a first-person perspective of the stairs leading up to the house, evoking the researcher's expectant mood. We also get to see, in tracking shots taken from a car on a freeway, the sprawling suburbs where most of Bateson and Kees' subjects lived.[11]

After Bateson moved on to conduct research on schizophrenia at the Veterans Administration Hospital in Palo Alto, Kees and Ruesch continued making films such as *Children in Groups* (1954) and *Approaches and Leavetakings* (1955). The latter is a brilliant illustration of how Kees' aesthetic imagination could fathom, even if unsystematically and intuitively, intricacies of interactional behavior. Kees wittily makes use of serendipitous correspondences in everyday scenes, such as the encounter of a nun and a leftist "radical" on a busy San Francisco street; status-rituals on the campus of the University of California at Berkeley; the complex interactional dance between a newspaper vendor, a little girl and a pigeon; or the way an African-American sailor becomes conspicuously invisible to white passers-by. Kees, in this film, clearly went beyond the focus on white middle-class families characteristic for much psychiatric research at this time. There is, moreover, a sense of playfulness and self-irony in almost all of the earlier films that is notably absent from the seemingly unaltered footage presented in the Doris film. In this film, Bateson obviously aimed at a more straightforward recording. The Doris film also departs from earlier films on interaction in families in that it depicts an interview situation.[12]

Bateson had already worked extensively with audio recordings of psychiatric interviews in his collaboration with Ruesch (Ruesch & Bateson 1951: 12), even

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[11]The significance of the suburban family for Bateson's research is comprehensively discussed in Geoghegan (2017). For an account of how microanalytic procedures contributed to reconfiguring conceptions of maternal labor, see Joice (2020).

[12]Several films by Bateson on "structured family interviews" are catalogued in both the Bateson Papers at UC Santa Cruz, Special Collections and Archives, and the Don D. Jackson Archive, University of Louisiana at Monroe. These films were made in 1959, that is, after the Doris film.

though this interest was not reflected in the films he made with Kees (which were made after the publication of *Communication: The Social Matrix of Psychiatry*). The general exploration of levels of communication in psychotherapy and of the role of nonverbal communication in family interaction in the work with Ruesch and Kees took on a more specific direction in Bateson's subsequent research on schizophrenia. At the core of the double bind hypothesis which Bateson developed lay the conception, already outlined in the earlier book with Ruesch, that communication unfolds on several levels of abstraction. Higher – metacommunicative – levels frame the lower levels, as when they indicate that an utterance is to be understood as metaphorical or that a certain kind of behavior is to be perceived as play. If, however, messages on different levels continuously falsified each other in sequences of habitualized behavior in vitally important relationships, such as between children and their parents, this would lead to the disturbances observed in schizophrenic communication. The hypothesis was "that sequences of this kind in the external experience of the patient are responsible for the inner conflicts of Logical Typing" (Bateson et al. 1956: 252).

While the double bind was ostensibly formulated in a psychiatric context, it simultaneously addressed broader communication theoretical issues. Importantly, the hypothesis stressed the crucial role of "nonverbal media of posture, gesture, facial expression, intonation, and the context" for the higher level framing of communication (Bateson et al. 1956: 252). Even though the role of empirical data in the formulation of the hypothesis remained ambiguous,[13] the concern with bodily and paralinguistic signals provided a rationale for producing and studying audio recordings as well as "taking sound motion pictures of mothers and disturbed, presumably preschizophrenic, children" (Bateson et al. 1956: 262). From both the audio material and the sound film footage the researchers hoped to obtain "a clearly evident record of the continuing, repetitive double binding which we hypothesize goes on steadily from infantile beginnings in the family situation of individuals who become schizophrenic" (Bateson et al. 1956: 262). In addition to family interaction, the study also reflected back on psychotherapy itself, its potential for creating double bind sequences and its form as "a context of multi-level communication, with exploration of the ambiguous lines between the literal and metaphoric, or reality and fantasy" (Bateson et al. 1956: 262). Again, audio recordings became important tools because, unlike written transcripts, they promised to preserve the intricacies of therapist-patient interaction:

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[13] According to John Weakland, "the idea of a double bind came out of a very mixed background. We mixed in a little bit of direct contact with patients, a good deal of thinking about communication and its complexities and its different levels, Russell's Theory of Logical Types, and how things fitted together and what might lead to what. Lord knows it was a strange combination of observation and speculation" (quoted in Harries-Jones 1995: 136).

> [W]e prefer exact records since we believe that how a schizophrenic talks depends greatly on how another person talks to him; it is most difficult to estimate what was really occurring in a therapeutic interview if one has only a description of it, especially if the description is already in theoretical terms. (Bateson et al. 1956: 263)

Filming interviews, therapeutic or otherwise, would obviously have made perfect sense within the framework of Bateson's project on the etiology of schizophrenia. It is not clear, though, if Bateson made other interview films prior to the Doris film. He and Myers shot the Doris film shortly before "Toward a Theory of Schizophrenia" was submitted for publication in June 1956. While this makes it unlikely that any last-minute observations derived from the film entered into the article's argument, it underlines Bateson's concern, at this time, with the intersection of interviews, psychotherapy and the family constellation. This concern strongly resonated with the problems Frieda Fromm-Reichmann and her co-fellows at the CASBS sought to address. Finding out "what was really occurring in a therapeutic interview", after all, aptly summarizes the initial motivation for the NHI. Bateson may have viewed both projects as complementing each other. "Toward a Theory of Schizophrenia" concludes with an extensive description, based on a personal conversation with Fromm-Reichmann that had taken place shortly before, of how Fromm-Reichmann had intuitively created a "therapeutic bind" to reach a withdrawn schizophrenic patient (Bateson et al. 1956: 263–264). Echoing the conception of the NHI, the article looks forward to a time "when such strokes of genius will be well enough understood to be systematic and commonplace" (Bateson et al. 1956: 264).

The Doris film would contribute to this transition. Emerging, like the films Bateson made with Kees, in a research context where it provided general observational data that loosely intersected – by bringing into awareness patterns and analogies – with the formation of hypotheses, it turned into an object systematically scrutinized by linguists, kinesicists and psychologists. In effect, it became the keystone in these researchers' efforts at developing systematic procedures for describing the multi-channel process of interaction-communication. Bateson's ideas for using the Doris film may have differed from his earlier efforts from the outset, because it was made with different problems in mind. But, of course, he also worked with a different cinematographer.

As late as 1954, Bateson and Kees finished *The Nature of Play – Part 1: River Otters*, based on material shot in 1952/53 at the San Francisco Zoo. The film's observation "that animals other than man can exchange two orders of message" (Bateson & Kees 2017 [1954]: 112) became an important element in developing the

double bind hypothesis. The collaboration between Bateson and Kees came to an abrupt end when Kees, tragically, vanished in July 1955, his car parked on the Marin County side of the Golden Gate Bridge. Myers, Bateson's new cinematographer, came from a similar artistic background. The same age as Kees, he had not yet achieved the latter's artistic recognition, but was about to embark on a long and highly successful career as a documentary filmmaker. Myers had studied still photography at the California School of Fine Arts (CSFA), and had only recently turned to filmmaking when Bateson recruited him. Among his credentials was his work as a photographer at a mental hospital in Spokane, Washington, during the Second World War, where he took pictures of incoming patients. Unlike Kees, who, besides his involvement in experimental filmmaking, had started a documentary film company shortly before his disappearance, Myers was drawn more unambiguously to documentary formats. Around the time of his collaboration with Bateson, he was making a film on the photographer Ansel Adams, one of his teachers at CSFA. A few years later he made *Ask Me, Don't Tell Me* (1960), a pioneering portrait of a social work project for youth gangs from different racial backgrounds in San Francisco. His reliance in this film on techniques that came to be associated with direct cinema, cinéma vérité, and observational cinema is already evident in the Doris film – especially the free-roaming, spontaneous camerawork, reacting to what is going on in front of the camera. In addition, the use of synchronous sound in the Doris film, unusual in documentary filmmaking at this time, directly anticipated one of the most prominent features of the later approaches.

These correspondences were not accidental, but rather emerged from interrelated media practices and epistemologies in documentary filmmaking and social research. In the 1950s and 1960s, practices and technologies of research filming often intersected with those of the emerging direct cinema and cinéma vérité approaches. Such interrelations were particularly close in ethnographic film, but they extended to other fields (see MacDonald 2013). Long audiovisual sequences of uninterrupted interaction were appealing to both documentary filmmakers and social scientists because they gave "the impression of lived experience by being there as events happened" (Ruoff 1992: 218). Synchronized sound evoked an immediacy and spatio-temporal unity that distinguished this approach from the tradition of expository documentary with its reliance on didactic montage and authoritative narration. Much of this development was driven and financed by television, and films often focused on events with implicit storylines and "crisis structures" (Mamber 1974: 114).

Especially in ethnographic film, there were also instances more closely related to the concerns of the NHI. One might think of the "sequence films" Timothy

Asch and John Marshall started making in the early 1960s out of footage shot by Marshall over the course of several years among the Ju/'hoansi of the Kalahari Desert. Concentrated on single, more or less continuous "events", sequence films such as *A Group of Women* (1961) or *A Joking Relationship* (1962) aimed to present "unrehearsed social interaction" (Asch 1971: 41). In contrast to the rigorous procedures deployed by the NHI researchers, Marshall relied on his familiarity with the people he filmed as well as aesthetic techniques to convey an understanding of what was going on. He remarked: "Our thoughts and feelings are the invisible drama of our daily lives, and, in any film, angles and distances create some feelings and perceptions experienced by the audience. [...] With my camera, I was trying to let the audience share what people were really thinking and feeling instead of projecting my interpretations on events" (Marshall 1993: 43).

Marshall's terms "thinking" and "feeling" are certainly at odds with the NHI group's cybernetically informed perspective on communication systems and observable interaction behavior. Still, he too – despite invoking an "invisible drama" – assumes that it is possible to capture (and convey) on film the intricacies of interaction behavior, and his observational method resembles the natural history approach. Ironically, Marshall sought to achieve this through precisely the kind of filmic participation that appeared as a major flaw in Myers' film. Marshall and Asch's concept of "sequence films" also shares aspects of what Michael Lempert has described as the "epistemological longing" for the "indexical real" that permeated the use of film and other audiovisual media in mid-20th century communication/interaction studies. For the psychiatrists, linguists and anthropologists involved in these studies, the use of recording technology was driven by "the hope that one could put one's finger on the nerve of unconscious interpersonal life". Ultimately unattainable, even by "the kaleidoscopics of sound-film", the wish was to move "toward the interpersonal real for which no media was a substitute" (Lempert 2019: 29). We may find reverberations of this epistemological longing, transposed to such notions as "immediacy" and "spontaneity", not only in ethnographic sequence films, but across a variety of practices and discourses of contemporaneous documentary film.

Writing about films made by Richard Leacock, Donn Allan Pennebaker, Robert Drew and Albert Maysles, the film critic and experimental filmmaker Jonas Mekas pointed out that they "caught scenes of real life with unprecedented authenticity, immediacy and truth" (Mekas 1960: 11). For Leacock himself, writing in the same issue of *Film Culture*, this new aesthetic of "spontaneity" held profound implications for the art and politics of cinema. It paved the way for what he termed "an uncontrolled cinema", allowing "the filmmaker as an observer and perhaps a participant" to capture "the essence of what takes place around him, selecting,

arranging but never controlling the event" (Leacock 1960: 25). "Uncontrolled" here also meant freedom from commercial sponsors (because of the relatively inexpensive means of production) as well as social conventions. Mekas hoped that this would turn film into an instrument of social revolution, ultimately overcoming the separation between art and life. To not control the filmed event, evidently, was also essential for the natural history method. Jacques Van Vlack, who worked as a cinematographer with Ray Birdwhistell at the Eastern Pennsylvania Psychiatric Institute, observed that "the process of viewing and selecting scenes will seem familiar to conventional film editors particularly those working in *cinéma vérité*" (Van Vlack 1966: 5). There were also concerns, similarly surfacing in discourse on direct cinema, about the research cinematographer unconsciously trying "to censor out disturbing sequences by momentarily diverting his camera" (Scheflen, *Principles of film recording*: 2). But we are skipping ahead. These concerns were, in effect, a *result* of working with the Doris film.

4 "Socially Organized Ways of Seeing"

"Warm up that Movi-ola, Ray – we expect *it* to do all the work for us!" (McQuown, *Letter to NHI contributors*). Norman McQuown knew perfectly well that the film viewer, normally used by film editors, would not relieve him and his colleagues from the mind-crushingly exhausting tasks of microanalytic transcription. What his joking invocation of the Moviola, in a February 1957 letter to the group members, hinted at was how strongly their research was entangled with technological apparatuses: film cameras, tape recorders, film viewers and customized projectors, film negatives and duplicate prints, audio tapes and – in a later phase of the project – a "B-roll" process for numbering individual film frames. This machinery was, as Seth Watter (2017: 52) has argued, not something external to the project, but it inscribed itself, through a "formalized chain of technical operations", into the understanding and conceptualization of the linguistic and kinesic aspects of interaction behavior. To handle the complexity contained in even a one-second segment of the film, the kinesic researcher started by repeatedly looking at isolated body regions. The findings were then, step-by-step, connected to each other: "For the goal was really to work one's way back from the atomized fragments to the total mise-en-scène – to so connect part to part, part to whole, whole to part, and one moment to the next, until the image became a great tapestry of human communication" (Watter 2017: 59).

The process of piecing together started even before the actual analysis began. This had to do with the film's sound. The film's low-quality optical soundtrack

was important during the initial process of "soaking", since it evoked an immediacy that allowed the researchers to immerse themselves in the filmed situation. But its quality was not sufficient for detailed analyses of linguistic and paralinguistic features. Using the separate magnetic tape-recording required Birdwhistell, Hockett and McQuown to carry out "the grueling process of synchronization" manually, frame by frame (Bateson 1971a: 19). Synchronization thus had a double meaning: automatically achieved in the original film by the camera's recording apparatus, it had then to be repeated manually in order to combine the image with the higher quality magnetic sound. Similar adjustments and reconfigurations of the film went on for some time over the course of the project. A crucial step was the addition of frame numbers in 1962. The procedure of adding frame numbers had been devised by Jacques D. Van Vlack, who had become Ray Birdwhistell's cinematographer in residence at the Eastern Pennsylvania Psychiatric Institute in Philadelphia in 1960. Van Vlack used a "B-roll" of clear leader, imprinted with consecutive numbers that was superimposed in the film lab onto the original footage. Facilitating references to individual frames and film segments, this procedure was later used by a number of researchers, including Paul Byers, Adam Kendon and Albert Scheflen. The frame numbers marked a further step in the development of microanalysis as a "a fully integrated phenomenon, with special procedures regulating the film object from its initial production to its classificatory status and, finally, to its use in a body of comparative research" (Watter 2017: 52). Modifications such as the frame numbers and the manual synchronization of the magnetic sound track served to turn the Doris film into an object of "professional vision", as Charles Goodwin has termed it. They integrated the film into "socially organized ways of seeing and understanding events that are answerable to the distinctive interests of a particular social group" (Goodwin 1994: 606).

Goodwin's practice theoretical perspective on professional vision echoes, without explicitly referencing this, procedures of body motion analysis developed in the NHI and subsequent projects inspired by it: "To analyze how practice is organized as a temporally unfolding process encompassing both human interaction and situated tool use, I require as data records that preserve not only sequences of talk but also body movements of the participants and the phenomena to which they are attending as they use relevant representations" (Goodwin 1994: 607). To conduct his studies, Goodwin used the audiovisual medium of videotape. Similar to Goodwin's approach, part of the procedures of the NHI group focused on reflexively reconfiguring its own body of knowledge. If the interview Bateson conducted with Doris was not in itself a psychiatric interview, the analysis was nevertheless intended to lay the groundwork for understanding therapeu-

tic interaction. Of course, more general patterns of interaction/communication behavior were also a concern; and for some researchers, such as Bateson and Bird-whistell, they were the primary concern. Much of the research influenced by the NHI focused directly on reflexive assessments of professional procedures, such as Albert Scheflen's work on the communicational structure of a psychotherapy session (1973) or Paul Byers and Margaret Mead's study of *The Small Conference* (1968).

Goodwin's specific focus on professional vision allows us to go one step further and to consider what lay outside the demarcations established by the researcher's practices, by bringing into view the "asymmetry in interaction" these practices established (Goodwin 1994: 626). Specifically, it affords us with a more comprehensive perspective on why Doris' anger (as a behavioral phenomenon, not an internal state) was excluded from the analysis. Her misgivings about being filmed on that afternoon were, as I have suggested earlier, certainly part of the filming situation/context and, by extension, also of the procedures of knowledge production forming around the filmic record of her encounter with Bateson. But her perspective carried no weight for the project, because it did not fall into the category of professional vision. She was, in effect, the subject to be scrutinized – Bateson, although also exposed to the camera, less so.[14] Diligently noted in the transcript, Bateson continuously asserted his professional role, as when he brandished his cigarette like a conducting baton to re-establish the interviewer-interviewee relationship.

It is worth noticing that Kees' artistic rendering of interaction patterns in *Approaches and Leavetakings*, for all its sarcasm, evokes a much more inclusive vision. In a sense, his artistic approach also depended on "socially organized ways of seeing". But not only was such aesthetic practice more open-ended, it simultaneously challenged those established ways of seeing, constantly rearranging the figure-ground relationships of perception and awareness. What we are looking at, though, is a complicated situation – a situation that cannot be understood by resorting to simple art versus science dichotomies. An aesthetic undertow, "a motor of aesthetics with a dividend of pleasure" (Watter 2017: 61), runs through the specific practices and protocols of professional vision developed in the work of the NHI group with the Doris film. We have already noted that the film, by restricting the number of scenes suitable for interaction research, contributed to shaping the researcher's perspective. One could speculate that Myers' camerawork, though detrimental to microanalytic procedures, helped to intuitively

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[14]Bateson, however, reports "moments of considerable pain when the others were interpreting my actions, and I was forced to see those actions on the screen" (Bateson 1958: 8).

evoke the sense, in the group's joint film viewings, that the film was indeed a record of spontaneously occurring social behavior, confirming its status as a valid document. Heather Love has pointed out that, choosing an epigraph from Rainer Maria Rilke's "Sonette an Orpheus" for his introduction, Bateson suggests that "the division between scientific and aesthetic activity is not absolute" (Love 2013: 429).

It is from this perspective that Love reclaims "surface reading", as facilitated by the natural history approach, for literary studies and, more broadly, the humanities. She concludes that "an expanded definition of reading might return the text to the context of communication as a whole and to make visible the fact that history includes what happened, thought includes thinking, and culture includes behavior. […S]uch practices might help us reframe reading as a social science, one that along with more traditional social scientific methods can contribute to the project of showing 'what the real world is really like'" (Love 2013: 430). The Doris film, as we can now see, was already enmeshed in a nexus that connected procedures of professional vision and aesthetic techniques. Besides occupying a place in both the development of microanalysis and direct cinema, it also formed a critical node for emerging techniques of research filming, archival policies, and filmic epistemologies. Notably, in the early 1970s it also produced reverberations far beyond its initial context of social and psychiatric research in aesthetic and discursive practices of artistic experimental films.

Drawing directly on the experience of working with the Doris film, Ray Birdwhistell and Jacques Van Vlack developed elaborate methods for filming interaction behavior at the Eastern Pennsylvania Psychiatric Institute (EPPI) in Philadelphia. Van Vlack, who had been hired as cinematographer in residence at the EPPI in 1960, devised the already mentioned B-roll procedure for adding frame numbers to research films. Collaborating with Birdwhistell, he also installed a film studio at EPPI that was specifically designed to record interview situations. Outfitted with lights, microphones and a "living room" set, it allowed the cinematographer to leave the room after having started the camera, which was installed on a table, and thereby to presumably minimize his influence on the filmed interaction. Avoiding any camera action, such as the pans and zooms Myers had used in the Doris film, it sought to capture whole people and whole interaction events in long camera takes; it thus "made a virtue of the most boring cinematography possible" (Davis 2001: 44). While he was engaged in the – at this time well-funded – project of creating filmic "specimens" of interaction behavior, Van Vlack also worried about the preservation of these records. In a paper given to the Society of Motion Picture and Television Engineers in 1963, he addressed the difficulty of categorizing what he called "data films". Uncomfortably squeezed in

between "terms such as 'documentary,' 'educational' and even 'experimental'", he perceived the danger that these unedited "film clips" might be lost to film history. Preventing this potential loss was all the more urgent since "[w]e cannot hope to know with any accuracy what data in our films will be important to the world of tomorrow" (Van Vlack 1963: 2).

Van Vlack's assertion that these films "do contain a wealth of unexplored information" (1963: 2) chimed in with efforts undertaken at the same time by the anthropologist E. Richard Sorenson and the neurophysiologist Carlton Gajdusek to establish an archive of research films at the National Institute of Neurological Diseases and Blindness, NIH, in Bethesda, Maryland.[15] Like Van Vlack, they viewed films of "non-recurring phenomena" as "preserving data", which they sought to make permanently accessible through carefully outlined procedures for filmmaking, editing, annotation and archiving. If the focus on "the programming of the human nervous system of unique subjects" aligned their archival project with programs in ethnographic film that sought to preserve records of presumably "disappearing cultures" (Sorenson & Gajdusek 1963: 112), it also echoed the cybernetic and systems theoretical foundations of the NHI. The interrelations between the two projects were even more specific, as when Sorenson and Gajdusek supported Allison Jablonko's ethnographic filmmaking and research on body motion behavior among the Maring of Papua New Guinea in 1963. In preparing for her fieldwork, Jablonko took classes with Birdwhistell and Van Vlack, and her research footage was eventually placed in Sorenson and Gajdusek's archive (Jablonko 1968: xiii). Protocols and practices developed in the NHI were integrated into and shaped the discourse of the emerging discipline of visual anthropology. One might think of Alan Lomax's research on choreometrics, or Margaret Mead's contributions to debates on ethnographic research film, where she references kinesics and choreometrics as well as Gregory Bateson's filmic studies of interpersonal behavior (Mead 1971: 34).

This direction in visual anthropology was soon to be contested as too scientistic by observational filmmakers, such as David MacDougall, who sought to make narrative ethnographic documentaries in an interactive film style that echoed – even if unintentionally – Myers' probing camerawork in the Doris film. Some experimental filmmakers, on the other hand, were more receptive to the communication theoretical conceptions and microanalytic procedures developed by the NHI researchers. This was particularly true of Hollis Frampton and Stan Brakhage, who, in the early 1970s, both, in different but interrelated ways,

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[15]The National Institute of Neurological Diseases and Blindness was renamed the National Institute of Neurological Disorders and Stroke in 1988.

saw in microanalysis and body motion interaction research something that contributed to their understanding of film aesthetics. Their interest was most likely sparked by the publication of Birdwhistell's book *Kinesics and Context* (1970) and Birdwhistell and Jacques D. Van Vlack's film *Microcultural Incidents in Ten Zoos* (1971). The film was shown widely outside the circles of interaction researchers (in 1976 it even screened at the Berlinale Film Festival). In a general way, the use of slow motion, repetition and freeze frames in this film, emulating the practice of microanalytic film viewing, resonated with techniques used by many new formal/structural filmmakers to probe and reflect on elements of cinematic motion, materiality and illusionism. Brakhage's and Frampton's interest, however, was motivated by more specific problems of filmic temporality, the photographic basis of film images, and questions of how film elucidated processes of perception, awareness and consciousness. The aesthetic undertow of filmic research procedures and theoretical concerns of the NHI here intersected with and was activated in artistic reflections on the historical, material and perceptual conditions of the film medium.

Frampton explicitly referred to this overlap during the discussion period after a screening of his films at the Annenberg School of Communication in Philadelphia in March 1972. He found himself in the curious position of having to defend Ray Birdwhistell, who was not present, against the accusation, made by an audience member, that kinesics was "not scientific". Shortly before, Frampton had cited Birdwhistell to counter simplistic conceptions of communication (and artistic meaning) as a one-way process. Birdwhistell, Frampton claims, had shown that "we are, all of us, communicating all of the time, non-stop on fifty different channels, and we are receiving, sending and receiving in all directions, all the time" (Frampton, *Annenberg School for Communication*). A bit later, an interlocutor challenged this approach as "not belonging to science". Frampton responded that "[Birdwhistell] has organized his body of knowledge in such a way that it has been useful to me in my search for further knowledge" (Frampton, *Annenberg School for Communication*). In a letter to Stan Brakhage written a few months later, Frampton explained that he had discovered "a crosslight in Ray Birdwhistell's stuff", illuminating "a large portion of the whole snapshot 'problem'" (Frampton, *Letter to Stan Brakhage, September 2, 1972*).

Brakhage and Frampton had begun to correspond in late 1971, when Brakhage enthusiastically wrote Frampton in response to having seen the latter's film *Zorns Lemma* (1970). Often perceived by critics as embodying contrasting approaches to filmmaking (with Brakhage representing subjective vision and Frampton conceptual filmmaking), the two filmmakers engaged in a long-lasting and productive exchange. What Frampton described as the "snapshot 'problem'" originated

in their joint discussion of the photographic basis of the filmic image. Early in their exchange, Brakhage had asked Frampton for his thoughts on "Document" (Brakhage, *Letter to Hollis Frampton, November 22, 1971*).[16] Prompted by the experience of filming *Eyes* (1971), *Deus Ex* (1971) and the yet unfinished *The Act of Seeing with one's own eyes* (1971) in Pittsburgh, he had turned away from the visual metaphors, evoked through techniques of editing and superimposition, characteristic of his earlier films. His new "observational films" (Kase 2012: 2) depicted the work of the Pittsburgh police, medical procedures in a local hospital, and autopsies in the Allegheny County morgue. Brakhage conceived them as "a gathering of images [...] which refers to its source" (Brakhage, *Letter to Hollis Frampton, November 22*). He contrasted his effort "to make all reference terminate in the film" (Brakhage, *Letter to Hollis Frampton, November 22*) with traditional documentaries which sought to steer their viewers toward ideologically preconceived conclusions.

Brakhage's conception of film as "Document", his striving for "indexical directness" (Kase 2012: 6), evidently resounded with the way the NHI researchers conceived and constructed the Doris film as a document of interaction behavior, and also the natural history approach with its attempt to avoid theoretical preconceptions. At first, Brakhage seems to have been unaware of these similarities. Shortly after, though, he acquired the comprehensive collection of Ray Birdwhistell's published and unpublished writings on body motion interaction that is today archived in his papers at the University of Colorado at Boulder.[17] These writings (perhaps suggested to him by Frampton) helped him to articulate more precisely what was at stake in the new direction his work had taken and also how to integrate it with his broader artistic goals. In a lecture on *Das Cabinet des Dr. Caligari* (1920) from October 1972, Brakhage cited Birdwhistell to explain how silent film highlighted and depended on "the particularities of people's motion and speech", revealing how bodily gestures were interrelated with cinematic technology (Brakhage, *Lecture on Caligari*). Filmic meaning, and linguistic meaning, too, should be viewed as emanating, like breath, from bodily, "cellular" activity (Brakhage, *Lecture on Caligari*).

This was as much about his own work as it was about the historical film he was ostensibly speaking about. Most of Brakhage's films, including the films from the Pittsburgh trilogy, were intentionally silent to avoid distraction from their visual structure. And from the late 1950s on, he had sought to evoke in his filmmaking

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[16]For a discussion of Brakhage's use of "Document", see Nesthus (2001).

[17]Wendy Leeds-Hurwitz has suggested that Birdwhistell himself might have sent Brakhage a selection of his papers upon the latter's request, as he often did with academic colleagues (email to the author, October 15, 2020).

an embodied vision. The document films, and especially *The Act of Seeing with one's own eyes* with its viscerally shocking images of the dissection of dead bodies, reflected on these aspects to reveal, as Carlos Kase observes, "the fault lines between human bodies and the technologies that circumscribe them in art" (Kase 2012: 13). Viewed as "a delicate, nuanced work about transcription and observation" (Kase 2012: 12), *The Act of Seeing* simultaneously elucidates a blind spot inscribed in the analytic procedures of the NHI: the way in which the professional vision of the researchers was shaped by the cinematic dispositif which mediated their encounter with – and constituted a document of – the "reality" of interaction behavior.

Hollis Frampton picked up the "Document" thread, but he developed it in a somewhat different direction. At this time, he was working on his *Hapax Legomena* film series and was simultaneously embarking on the monumental *Magellan* project that would occupy him for the next decade and a half until his untimely death in 1984. He was also deeply engaged in writing a series of articles on film, still photography, history, art, and consciousness. Across these texts he unfolded a dense web of reflections on the connections between these areas.[18] One aspect that is particularly relevant for our present discussion is his assumption, shared with Brakhage, that photographic images presented "a virtually perfect *continuum*" of sensory data (Frampton 1971: 34). Their "ultimate structure seems to elude us at the same rate as the ultimate structure of any other natural object" (Frampton 1971: 34). Even though Frampton stressed the mediated nature of photographic "illusions" (Frampton 1971: 34), we may perceive in this an echo of the interaction researchers' longing for the "indexical real" (Lempert 2019: 29). Commenting on *The Act of Seeing with one's own eyes*, Frampton praised Brakhage for his decision "to *stand aside*", to let the camera do its work as seemingly "perfect Eidetic Witness", in order "to see, with your own eyes, what coherence might arise within a universe for which you could decree only the boundaries" (Frampton, *Letter to Stan Brakhage, January 26*).

Themselves the product of the mechanistic world view of 19th century science, and always in danger of reinforcing it, photographic images simultaneously promised to become "the subversive restorer of *contextual* knowledge seemingly coterminous with the whole sensible world" (Frampton 1971: 34).[19] This also un-

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[18] For a comprehensive discussion of Frampton's theoretical articles and their complex interrelations among themselves and with Frampton's films, see Eisenstein (2016).

[19] It is noteworthy that Frampton's view of the interrelations between film images and processes of consciousness overlaps with Bateson's epistemological claim "that the laws and processes of our perception are a bridge which joins us inseparably to that which we perceive, – a bridge which unites subject and object" (Bateson 1957: 1).

dermined mechanistic conceptions of the relationship between still photography and film, bringing to the fore the problem of how "snapshots" as well as single filmic frames were cut from and referred back to a spatio-temporal continuum. If film resurrected "bodies in space from their dismembered trajectories" (Frampton 1971: 34), it released, rather than just setting still images in motion, the latent movement and the microtemporalities always already inscribed in still photographs.[20] These technological, perceptual, and historical intersections between film and still photography formed a persistent theme of Frampton's films, most notably in *(nostalgia)* (1971). The film links – through the interplay of images of still photographs being burned on a hot plate with a quasi-autobiographical (and temporally offset) narration – questions of stillness and motion with reflections on memory and entropy. *Critical Mass* (1971), on the other hand, foreshadowed the intersection of Frampton's aesthetic explorations with microanalytic procedures: a heated argument between a young woman and a young man is broken up into brief fragments that are partially repeated, producing a stuttering pulse that highlights, similar to a microanalytic viewing, the details of the body motion communication process going on between the actors.

It was these aesthetic and media-theoretical concerns with time experience, filmic movement and still photography that drew Frampton to the research procedures and theoretical conceptions of body motion research. In four consecutive articles (Frampton 1972e,b; 1973; 1974), he approached questions of image technology, time consciousness and historical time from the perspective of the history of photography, retracing, in a sense, his own turn in the mid-1960s from photography to film. But the concern with still photography was also inspired by microanalytic film viewing. In a letter to Brakhage (the same in which he had mentioned Birdwhistell in connection with the "snapshot 'problem'") he expressed the hope that "these pieces on still photography [...] will nourish the attentions [sic] of other film-makers, from the admittedly nominal & (sometimes fruitfully) arbitrary point of view that one can learn something about the nature of images one frame at a time" (Frampton, *Letter to Stan Brakhage, September 2, 1972*).

Frampton expanded this idea in his article "Incisions in History/Segments of Eternity" (1974), where he discussed how Ray Birdwhistell had detected in a film about a mother and her baby, through careful "frame-by-frame analysis", an instance of assumedly double bind producing interaction.[21] What intrigued Frampton was the chasm between the extremely short interaction sequence, merely

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[20]For a detailed discussion of Frampton's concern with microtemporalities, see Hansen (2011: 61–63.

[21]The text discussed is "The Age of a Baby" (Birdwhistell 1970: 11–23).

one sixth of a second, or four film frames, and the huge amount of time needed for "rigorous examination" – "hundred hours per running second of real time" (Frampton 1974: 48). Our lived experience, more unsettling even, emerged from "thousands of such brief, wordless exchanges" (Frampton 1974: 48). There was "a monster in hiding here" which "has cunningly concealed itself in time" (Frampton 1974: 48).[22] If the NHI researchers had used film as a tool to bring to awareness and to systematically describe the details of interaction behavior, microanalytic viewing procedures, for Frampton, illuminated two interrelated aspects that were central to his understanding of film and his aesthetic practice: the threshold between still and moving images, and the intersection, "in the reaches of temporality" (Frampton 1974: 48), of the technological gaze of the camera with human perception and time consciousness.

What these examples demonstrate is how the Doris film and the procedures of professional vision developed in working with it and on it were enmeshed in a broader matrix of film historical and theoretical practices. We have seen how this matrix contributed to shaping this specific form of professional vision, and how this vision, in turn, reflected back on emerging film practices and aesthetic discourses. The Doris film is also an example of the asymmetrical power relations inscribed in professional vision. This vision, though, was not monolithic: it could be adopted and exploded, for instance, by inclusion in artistic film practices. Looking into these twisted relationships opens up a media archaeological perspective that makes visible interlocking epistemological and aesthetic practices across a broad film historical field that includes intersections between "useful film", archiving practices, film art, and what has long been understood as the proper domain of cinema, theatrical feature films. This may also afford us with a new look at the sociological inflection as well as the entanglement in filmmaking practices of film theory and media studies.

Acknowledgements

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[22]Hansen (2011: 61–63) discusses this passage from Frampton in a media historical/theoretical context.

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A question of perspective. Response to Henning Engelke

Wendy Leeds-Hurwitz

To begin, I want to thank Henning Engelke for sharing his "media archaeo-logical perspective" (Engelke 2021 [this volume]: 110, 133) to NHI, as it is very different from my own approach, and thus quite interesting to read. Clearly, as we come from distinct backgrounds and research interests, what we bring to the analysis of NHI will be quite different, and so what we take away from it will also be quite different. That leaves a lot of room to learn new things. So, let me respond to a few comments, and also mention a few things I learned by reading this manuscript.

I have a little more information about the filming to contribute, based on what Birdwhistell told me. Writing about himself in the third person, because he was critiquing a draft of my 1987 paper, he wrote: "Birdwhistell had been out to Bate-son, Jackson et al. as a consultant a number of times in relation to Bateson et al. filming. He knew of footage [the various films Bateson was making] and sug-gested to McQuown (the real organizer) that Bateson might be interested [in participating in the NHI seminar]. [Here he switches to first person] I went to see him [Bateson] – he was interested and attended nearly all of group review sessions" (Birdwhistell, undated letter to WLH, received April 23, 1984). Support-ing this, McQuown says, "In the search for suitable materials, Birdwhistell per-suaded Gregory Bateson to show the seminar some of his sound-filmed family-interviews" (1971: 1).

Although we are largely analyzing the same materials, we come to the NHI with few overlapping resources. For example, there's a reference to Heather Love's 2013 article on the values of thin description, which was new to me. Thank you for the unintended introduction – she and I have now corresponded about overlapping interests. And, although I had read Zabor's (1978) dissertation (based in large part on research conducted while at EPPI with Birdwhistell), I had not thought to look for a list of other students who took classes at EPPI with him

and/or Van Vlack. This is a good reminder that there are often additional points of view needing to be captured for any historical research.

The focus on Doris' anger (Engelke 2021 [this volume]: 106, 126) surprised me. Emotions, like thoughts, were not what interested the NHI group given that their project was not focused on the individual level. Rather, they wanted to understand interactions and relationships between persons, what people do and say and how they react to one another's words and actions. Of course, they also wanted to understand the ways in which everyone's words and behaviors interrelated during communication, what today is typically called multimodality. None of this would have led to a study of anger, especially presumed, rather than explicitly expressed (and responded to) during the interaction being examined.

But I particularly appreciate the felicitous phrase "transitional object" (Engelke 2021 [this volume]: 105) for the Doris film, given that it marks the intersection between research film, documentary film, and communication theory. All in all, an interesting read.

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Film as observation and experiment. Response to Henning Engelke

Seth Barry Watter

Admirable in Henning Engelke's (2021 [this volume]) paper is the way it shows us how footage of two people on a couch, talking for ten minutes, had such a lasting impact on so many fields: on the study of interaction, of course, but also on the development of documentary film, and indirectly – but no less importantly – on the aspirations of certain experimental filmmakers. Admirable, too, is the visually sensitive analysis Engelke performs on this unassuming film. But perhaps the most remarkable aspect of the paper is its critique of the assumptions that underpin research filming. This critique is measured and lucid, never exaggerated. Nor does it lapse into an anti-scientific skepticism that is even more naive than the objectivity it suspects. Rather, one could argue that Engelke tries to hold the film accountable to a higher standard of objectivity: a standard that would acknowledge and proceed in full awareness of the Doris film's status as an artifact of experiment.

Of course, the team of specialists who analyzed the film would never have described it in terms such as these. Experiment is precisely what they tried to avoid. Otherwise, they would never have called their project the "Natural History of an Interview". Natural history is a science of observation, not experiment. But it is hard to know what to call a recording of a visit to a woman's house, at a time that was inconvenient, perceived by her as an intrusion, and which in any case would require her to wait while the apparatus of recording was set up in her living room – it is hard to know what to call this if not an experiment. For an experiment is really a form of intrusion. In the words of Claude Bernard, it is "a variation or disturbance that an investigator brings into the conditions of natural phenomena" (1957 [1856]: 8). The body of films made in the 1960s at EPPI under Birdwhistell and Scheflen's guidance are much like what Bernard called "*experiments to see*, because they are intended to make a first observation emerge, unforeseen and undetermined in advance, but […] with the object of bringing to birth an idea"

(ibid.: 21, italics in original). Observation and experiment are not mutually exclusive terms. Experiment always involves observation. And when experimenters see the results of experiment, their vision must be all-inclusive; they must be, that is, "photographers of phenomena" (ibid.: 22). Writing in 1865, Bernard was being metaphorical. But even then his words were on their way to being literal. He saw these two activities, observing and experimenting, as ideally distinct – capable of separate and successive execution by two distinct agents, one passive and one active. He had in mind cases like that of the naturalist who, though blind, successfully devised experiments. Their physical performance and the reporting of their results just happened to be done by the naturalist's servant. The servant, "for his part, had not a single scientific idea" (ibid.: 23). So too with the social scientists who build themselves a studio, mic it, light it, turn the camera on, then leave to have coffee. They devise an experiment, which the camera observes.

That the camera inevitably alters the situation of its subjects if the subjects are aware of it – are aware of being filmed – is an oft-repeated, perhaps pedestrian notion. But that does not make it any less true. It is especially true of a film like that of Doris where, as Engelke shows, the exchange of signals between cameraman and subjects is so marked and dramatic as to structure the whole document.

Oddly, this experimental quality is admitted more openly in the work of fine artists inspired by science. Engelke quotes the filmmaker Hollis Frampton as saying that Birdwhistell "has organized his body of knowledge in such a way that it has been useful to me in my own search for further knowledge." If we interpret the phrase *experimental film* as experimental in the strong sense – as inducing material for observation – we can learn a great deal about how Frampton used his medium. Leaving aside whether he knew who Birdwhistell was by the time he completed *Critical Mass* (1971), the film is clearly conceived as an experiment to induce observations of human interaction. Frampton chose two students from his class at SUNY Binghamton and asked them to bicker in front of his camera. He did not choose at random but was guided by hypothesis, since these students had just ended their romantic involvement – and were judged the most likely to produce the most sparks. He does not attempt to hide this experimental quality but rather exaggerates it by a number of means: the neutral background behind the figures, the high-contrast photography that simplifies their features, the hieratic poses they assume by default within the constraints of a medium two-shot. Above all there is the powerful key light whose heat on their bodies probably made their interaction even more volatile. "The editing process," said Frampton, "became a process of decoding, or reading, the footage and the recorded sound" (quoted in MacDonald 1988: 66–67). He is in the position of the microanalyst,

trying to make sense of a mass of filmed particulars. He has taken a specimen; now he puts it beneath the microscope. The NHI team, too, often used a language derived from microscopy. But a microscopic science is not a telescopic science. It requires a lot of handling of the thing to be observed – perhaps even intrusion to make the thing into a specimen. Then the specimen must be placed on a slide of glass or plastic, then dried, fixed with fire, and covered with a stain. Only then does it pass beneath the observer's lens.

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Chapter 5

The Natural History of an Interview and the microanalysis of behavior in social interaction: A critical moment in research practice

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The Natural History of an Interview (NHI) began in 1955 at the Center for Advanced Study in the Behavioral Sciences. NHI was an applied project, as well as multi-, inter-, and transdisciplinary, involving a core group of well-established researchers, all of which were central to its results and influence. The result was a new framework for thinking about communication in face-to-face interaction, including the development of new tools, and a set of principles for analysis. The natural history method focuses on fine-grained observation and analysis of observable behavior during social interaction. Originally involving half a dozen researchers for an academic year, then several dozen scholars over a decade, the assumptions and methods of NHI contributed significantly to the assumptions and techniques used to study interaction today.

1 Introduction

The Natural History of an Interview (NHI) was the name given to a project established in the academic year 1955–1956 at the Center for Advanced Study in

the Behavioral Sciences (CASBS), located in Palo Alto, California.[1] Its aim, as it finally emerged, was the investigation of communication processes in face-to-face (co-present) interaction, although it began with a focus on analyzing particular moments in a psychiatric interview.[2] The outcome of this project was a new framework in terms of which communicative processes in interaction could be thought about, and the formulation of principles for a new methodology that this framework implied. This new framework and methodology, which entails fine-grained observation and analysis of the details of the full range of observable actions of participants in social interaction made possible by the close analysis of films of social interaction, was to throw new light upon the nature of human communication and had a part in shaping the later development of human interaction studies. A study of this project, the NHI project, as it will be called, the scholarly network it established, and how the new approach it developed came to have a wider influence, provides an interesting illustration of how new ideas in scholarly communities can be generated and diffused.

The work accomplished by the NHI group during 1955–1956 was consolidated and written up in the years following. It was never published, but the material, edited in readable form, was finally made available to the public in 1971 (Mc-Quown 1971g). The ideas and methods first outlined at CASBS were extended and elaborated in later meetings and small research teams, drawing in students and other interested colleagues, that were established in the home institutions of some of the original participants. Some of those who had joined these research teams, though not involved in the CASBS meetings, made significant further contributions to the theoretical and methodological framework that had emerged from NHI. The collaboration gave rise to an informal network of scholars, whose participants shared an interest in communication during social interaction which they approached with theoretical outlook and methodological procedures of considerable novelty at the time. Murray (1994) refers to the kind of network established as a "theory group"; it is also an example of an "invisible college" (Crane 1972). What is important is that these terms refer to a group of scholars who are not all based in a single place (Murray's focus), and who are not one another's students or professors (Crane's), but who still pursue a common research agenda.

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[1]CASBS was funded by the Ford Foundation and built on land in Palo Alto leased from Stanford University, but it only became affiliated with Stanford University (even then only as an independent research center) in 2008. For a brief history, see Thackray (2018; 2019).

[2]As Bateson explains: "We call our treatment [...] a 'natural history' because a minimum of theory guided the collection of the data" (1971b: 4). It is a term rarely used in Communication, but then Bateson's training was as a naturalist. For a longer explanation of the development and the use of the term natural history than is possible in this chapter, see Leeds-Hurwitz (2005).

In this case, the NHI group also used a common data set, and common analytic techniques to work with that data, which should strengthen the group. As is standard for other theory groups or invisible colleges, the NHI group also frequently referred to one another's publications, have some joint publications, and show up in one another's acknowledgments.

The participants in the CASBS meetings, who will be described in more detail in section II, included Frieda Fromm-Reichman, an interpersonal psychiatrist, Norman McQuown, a linguist, Henry Brosin, a psychiatrist, Charles Hockett, a linguist, and also two anthropologists, Clyde Kluckhohn and David Schneider (these two withdrew before the end of that academic year). Ray Birdwhistell, an anthropologist (also a product of Chicago) who founded the systematic study of the communicational significance of bodily action, or "kinesics", and Gregory Bateson, also an anthropologist as well as more general human communication theorist, both joined in as consultants at the beginning of 1956. All participants were scholars already well established in their fields. Those who were Fellows of the Center had come for the academic year, each with their own separate projects. Their collaboration in the NHI project arose as a result of their encounters with one another at the Center. Although, apparently to some extent, there was a deliberate effort to put together several fellows who knew each other and who would at the least combine psychiatry with linguistics, any specific project that they would do together had not been planned in advance. Brosin wrote a letter in 1991 to Philip Converse at the Center, explaining:

> I was brought in as part of a package deal by Ralph Tyler, Franz Alexander and Frieda Fromm-Reichmann, all well known to each other. Franz Alexander and Frieda Fromm-Reichmann were psychiatrists who pushed for improved study of linguistics in the study of Human Behavior – psychiatry. I was a psychiatrist well-known to anthropologists and had an interest in linguistics à la Edward Sapir, who wrote inspired essays on the subject. Ralph chose Norman McQuown (Chicago) and Charles Hockett (Cornell) as the linguists. They were absolutely *superb*. McQuown was the leader, who kept the "group" together. Actually Alexander left in December of '55 and Frieda was relatively inactive. "Chas" was brilliant but highly individualist! We were joined by Gregory Bateson informally – he worked at Palo Alto VA – and Ray Birdwhistell, who came to visit sporadically. Ray was our kinesics man. (Brosin letter to Converse, December 12, 1991, emphasis in original)

To make sense of these comments, it helps to know that Tyler was Director of the Center at the time, and Alexander had been a fellow the prior academic year, in the first ever class of fellows.

Once the fellows were in residence at the Center, the project came into being as a result of conversations between Frieda Fromm-Reichman, who had questions about the nature of the interaction process, and Norman McQuown. Fromm-Reichman wanted to understand those moments in her therapy sessions in which her patients gained useful insights. She wanted to understand better the details of what happened in those exchanges which might have brought these moments about. She asked McQuown to help her analyze the speech in some audio recordings of actual Chestnut Lodge therapy sessions of these moments. McQuown was sufficiently interested in this to set aside the work he had planned for his fellowship. He became fully absorbed in Fromm-Reichman's question. Soon it was recognized, however, that much more than just the analysis of speech would be needed. As a result, several other colleagues who were Fellows at the Center that year were invited to join in. This included Charles Hockett, who was already a fellow at the Center at that time, and Henry Brosin, who arrived at the Center several months after the others. Somewhat later, when it was realized that the body movements of the participants in the therapy interviews should be studied, Frieda Fromm-Reichman invited Ray Birdwhistell to join as a consultant and it was he who then persuaded Gregory Bateson to join in too. Bateson did so, making available to the group films he had made as part of his project on families with a schizophrenic child which he had organized with John Weakland and Jay Haley at the Veterans Administration Hospital in Palo Alto. As the specimen of interaction they would analyze in great detail, the group eventually settled on a film of Bateson in conversation with a family undergoing therapy as part of that separate project.

The participation of Birdwhistell and Bateson in the CASBS group proved to be of great importance. They both made crucial contributions to the theoretical and methodological approach that developed, and Birdwhistell became very important in enabling the continuation of the work and in maintaining the "invisible college" network that continued after the meetings at the Center came to an end.

The theoretical framework that arose from this collaboration supposes that communication in face-to-face interaction is a continuous process and it is as much about the establishment, regulation, and maintenance of necessary behavioral interrelations as it is about the transmission of new information. It supposes that for all participants any aspect of behavior could be communicatively relevant, and it is because of this that a new methodology developed. This methodology required that, in studying occasions of interaction, one could not assume in advance which kinds of participant actions could be ignored or which should be included. Careful attention needed to be paid to everything the participants did.

Only subsequent analysis could show what was, and also what was not, relevant. This could only be done, however, if an inspectable record of the interaction was available which would enable repeated close examinations. This is why the use of sound synchronized cinematography came to be a crucial element in the new methodology.

It is to be noted that this theoretical framework and its attendant methodology may be seen as a synthesis of the different disciplines represented by the participants. As already indicated, these disciplines were: interpersonal psychiatry, represented by Frieda Fromm-Reichman and Henry Brosin; structural or descriptive linguistics, represented by Norman McQuown and Charles Hockett and also by Ray Birdwhistell to a lesser extent; information theory and cybernetics, represented by Gregory Bateson; and cultural anthropology, also represented by Gregory Bateson and Ray Birdwhistell. The incorporation of sound-synchronous film in the methodology was largely due to Gregory Bateson, who had been a pioneer in its use in his earlier field research.

We may note how each of these disciplines entered into the new synthesis. The idea that the focus of the CASBS group should be on the interrelationships between the actions of the participants in the interaction examples studied, and so upon the communicative systems they were a part of, rather than focusing upon how these acts might be symptomatic or expressive of the inner states of the individuals, reflects the perspective of interpersonal psychiatry. Ideas about how the units of communicative behavior, whether verbal or not, could be identified and analyzed, and how they were to be understood to be participating in the communicative process at different organizational levels, were developed in the light of the method and theory in descriptive or structural linguistics; new thinking inspired by developments in information theory and cybernetics played a major role in shaping the way the processes of communication being studied were conceived; ideas from cultural anthropology influenced how the members of the project came to see how much of communicative behavior is culturally patterned; and the employment of sound-synchronized cinematography as the means by which inspectable specimens of interaction could be examined and analyzed allowed the recognition that, in co-present interaction, details of the visible behavior of the participants were as an essential feature of the communication process as vocal behavior. It was recognized that communication in co-presence was a continuous and unceasing process that operated at several different levels simultaneously and those aspects of these processes that served in the establishment, maintenance and regulation of the interactional relationship were just as important as those aspects deemed to be involved in the transmission of new

information. The concept of communication as a *multimodal* process (as it is fashionable to say nowadays) thus finds an early expression in this project.

The work started at the Center led to the writing of several chapters and the presentation of much of the transcription, both linguistic and kinesic, in what was hoped would be a publication. It turned out that publication was never realized, but chapters written by McQuown, Brosin, Hockett, Birdwhistell and Bateson, both separately and jointly, were brought together into a single multivolume document under the editorship of Norman McQuown and made available in the Regenstein Library at the University of Chicago in 1971, under the title *The Natural History of an Interview.*[3] How often it was consulted there we cannot know, but the NHI project had considerable influence on later researchers and has played a significant role in the later development of research on social interaction, as we shall see.

In what follows, we first provide more details on each of the participants in the 1955–1956 seminars at CASBS, clarifying why it was they were able to bring about the synthesis they achieved. Following this we will discuss some further developments in the methodology and theory that took place in the post-Center research groups, giving attention to the work Albert E. Scheflen in Philadelphia and that of William S. Condon in Pittsburgh. Details of the methodological approaches that were formulated by the NHI group then follow. We will close with a general evaluation and an assessment of some of the later outgrowths from this work and the influence it has had on later developments in interaction studies.

2 The NHI core group members: The original collaborators at CASBS

In this section we will explain in more detail who the original members of the NHI were, something of their backgrounds, and, where we can, indicate the extent to which they had known each other before gathering at the Center. We hope this may throw light upon how the collaboration itself developed and in what ways it was successful. As we have already noted, there was no single collective publication, in the end. However, an "invisible college" or "theory group" came into existence which persisted for some years which was important, if somewhat diffuse and often unacknowledged, as an influence in shaping much that we now understand of communication processes in co-present human social interaction.

James Gair in his obituary of Hockett published in *Language*, wrote that he had "a first-rate intelligence, a lively intuition, and a conscious commitment to

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[3]Now available in digital format.

rigor and precision" (2003: 611). This could have been said of any of the original NHI participants. This matters: bringing that many brilliant people together for a project will either succeed wonderfully, or quickly fall apart, depending largely on their ability to work together. NHI as a project succeeded because it changed the ways people could think about, document, and study human social interaction. However, it never took off as a driving force of any prominence in interaction studies as they were to develop more widely. It might be interesting to compare this to what happened with "conversation analysis", which developed a decade later, and which, in some important respects, had some methodological characteristics not unlike those of the NHI group's approach. It retained a distinct identity for quite a long period and has often been explicitly acknowledged as an influence (see the Introduction to Streeck et al. 2011, which provides a useful history of the development of interaction studies in recent decades, but does not mention any of the contributions of the NHI project and its aftermath).

This failure of the NHI work to have a more prominent place in the later development of interaction studies derives from a number of factors. Important, for sure, was the fact that its work was never published as a unified document. Also important, we may suppose, was the difficult methodology that was proposed. This required the use of sound synchronized films as specimens for analysis. This was something quite new in the social sciences at the time and few research projects at that time would have budgets that could afford either the expensive equipment or the necessary researcher time.[4] Further, techniques by which such specimens might be usefully analyzed were not then available and the techniques and apparatus needed for the kinds of the detailed analyses of human behavior advocated by the NHI group had hardly been developed, and such as were developed in the Center's seminar were as yet in embryonic form. It would take more than a decade for the methods for the microanalysis of films (and later video-recordings) of human interaction to be worked out and more widely understood. Finally, the new theoretical framework for thinking about communication that was developed was also not then widely recognized. Thus, the Natural History approach may have appeared to be too exotic or esoteric for it to be easily appreciated and also the importance of the kinds of questions that were being asked were also not yet widely appreciated.

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[4]Birdwhistell (1963) estimates that the apparatus they used for analyzing the film at EPPI, a PerceptoScope, cost $2000 at the time (this would be $17,000 in 2021 dollars, so the cost was clearly beyond the budget of most research projects, then or now). Both he and Scheflen talked about the enormous amounts of time spent viewing film clips in order to analyze them. More on that below.

The NHI project as undertaken in its first years, though successful for the new ideas it demonstrated, did not have a conspicuous lasting impact because it was ahead of its time. The subsequent wider influence that it has had resulted partly from the continued work by later investigators who became associated with some of the original members (as we shall see when we discuss the post-CASBS research teams that were set up), but also after the phenomena of communication in interaction became better appreciated as worthy of investigation by others, who were not connected to NHI.

Let us turn, now, to the individuals who were the original participants in the project, explaining their backgrounds, whether and how they were connected with one another beforehand, their roles with the NHI group, and whether and how they continued with the project after the group broke apart in 1956.

Frieda Fromm-Reichmann (1889–1957) was a psychiatrist at Chestnut Lodge in Bethesda, Maryland, where she worked with Harry Stack Sullivan.[5] Like the others selected for fellowships, she had prior experience talking across disciplinary boundaries at both the Foreign Service Institute (FSI) of the US Department of State (see Leeds-Hurwitz 1990) and the Josiah Macy, Jr. Foundation Conferences (Leeds-Hurwitz 1994).[6] In consequence of this, she already knew most of the other fellows who became involved in NHI for several of them also had worked at the Foreign Service Institute or had been involved in Macy Foundation Conferences (Leeds-Hurwitz 1994).

When Fromm-Reichman arrived at the CASBS she had a practical concern. Although known for her insightful analysis of schizophrenic patients, according to Bateson, "she felt insufficiently conscious of the actual non-verbal cues from which she arrived at her conclusions," and hoped that understanding these

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[5]Sullivan is well-known today for many things, but in this context his work with linguist Edward Sapir stands out: together they are known for work inventing what came to be called the "Culture and Personality" approach. For more on both Sullivan and Sapir, see Kendon (1990); for more on Sullivan's ideas, see Sullivan (1940); for an account of Sullivan's life and work, see Perry (1982).

[6]In 1946, the US Congress passed the Foreign Service Act establishing the Foreign Service Institute within the Department of State in order to train diplomats prior to travel abroad to take up posts as Foreign Service Officers and other positions, as well as to provide periodic in-service training. The focus was on language and culture, so they hired linguists and anthropologists, including many of those who were or became part of the NHI project at various stages (this included Birdwhistell, Hockett, and McQuown of the original cohort at CASBS, as well as Trager and Smith, who come into the story a bit later). Even those who worked nearby (such as Fromm-Reichmann) became part of the FSI extended network, as will be explained. See Leeds-Hurwitz (1990) for further discussion of the history of FSI and its role in the development of intercultural communication as a topic of study.

"would provide an enormously valuable tool for the teaching of Psychiatry" (1958: 96–97; see also Fromm-Reichmann 1955).[7] In addition to her first role of setting the problem for the group, Fromm-Reichmann's second role was to minimize arguments among group members, and soothe hurt feelings, keeping everyone on track (Birdwhistell 1959b). Finally, her third role was serving as an informal therapist for Bateson, who sometimes found it difficult to handle his reactions to others critiquing his performance in the film they were analyzing: "What she did was to lend that strength which enabled one to receive the comment" (Bateson 1958: 99). The difficulties that Bateson had with these comments arose because the film that the group analyzed in detail was of a conversation between Gregory Bateson and a woman known as "Doris".[8] These last two are roles that remain unfulfilled in most group projects and may perhaps be credited with the solid basis for the NHI group established while together at the Center. Unfortunately, Fromm-Reichmann became ill, and participated in only one small group meeting in the year following. Her untimely death in 1957 meant that she does not appear as author or co-author of any chapters in the final document, since that was only begun at the Center, not finished there.

Norman A. McQuown (1914–2005), a Sapir student in Linguistics at Yale based at the University of Chicago, was one of the first fellows at CASBS to become interested in joining a collaborative project. At Fromm-Reichmann's request, he set aside the project he had intended for his fellowship and worked with her to prepare an analysis of psychiatric interview materials during the first seminar (published as McQuown 1957). The interview he analyzed had been previously analyzed by Otto Will, also at Chestnut Lodge, and "supplied through the good offices of Frieda Fromm-Reichmann" (1957: 79). Although labeled a linguistic tran-

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[7]There was a larger context for this shift from intuition to analysis. As Mead said, looking back in a talk presented in 1968: "The last half century has seen the development of a whole new way of looking at human cultures. It has seen the rise and fall of one method, the use of insight in the perception of pattern, and the slow development of another, the use of instrumentation for recording and analysis of the kinds of materials which we formerly had no way of reducing to order except by insight, the perceptive activity of single human minds" (1969: 13). And in the end, the goal was met, as Birdwhistell suggests: the NHI made it "possible to equip psychiatrists with sufficient insight into the nature of the communicational process to make their own intuitions explicit and thus more available to their colleagues" (1959a: 103).

[8]Bateson explains: "A therapist, who knew of my interest in collecting film data on family interaction, told Doris (who was his patient) about my project. It so happened that Larry and Doris had attended a public lecture which I had given some months previously and, therefore, were receptive to the idea of having some part in our research" (1971b: 1).

scription, the analysis actually provides an early example of a paralinguistic analysis. McQuown took on the leadership of the group when Fromm-Reichmann could no longer play that role.[9] Birdwhistell praised his "sensitive analytic mind and capacity for painstaking and creative work" (1970: xiv). McQuown prepared the majority of the linguistic and paralinguistic transcriptions and coordinated the efforts of the linguists (Hockett from the original CASBS group, George L. Trager and Henry Lee Smith from the small working group established later in Buffalo) across the length of their participation in the project. He joined virtually every meeting at every stage and was the acknowledged memory of the group, according to both Birdwhistell and Brosin. He not only edited the final manuscript (McQuown 1971g), but also wrote the foreword (1971e), the collation (1971d),[10] and the summary and conclusions (1971c); he co-authored two chapters, including the key Chapter 6 presenting the transcription (Birdwhistell et al. 1971; the other was on baselines, Birdwhistell & McQuown 1971); finally, he contributed two appendices having to do with transcription (McQuown 1971a,b). McQuown credits all contributors to the NHI work, but he names Starkey Duncan as an especially important colleague (Duncan was a graduate student who worked with him extensively at Chicago). Thus he writes: "Although this (9) and the following chapter (10) bear my name, they could not have been written except as the end-product of an on-going and extensive intellectual inter-change among all* the contributors." The asterisk leads to a second note, saying "including, for most of Chapter 9, Dr. Starkey Duncan, whose prior data-researching, and preliminary hypothesis-formulation made possible the sub-selection whose incorporation into this chapter has been my responsibility" (McQuown 1971d: 2). McQuown was supposed to prepare yet a third appendix, on machinery, but that was never written; it was intended to cover the "techniques of manipulating taped and filmed materials in order to facilitate [such] analysis" (McQuown 1971e: 2). A decade later, McQuown also published much of NHI in Spanish translation, to use in training his own students in Mexico (McQuown 1983).[11] He wrote a sympathetic commentary on Scheflen's development of the natural history method

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[9]"When administrative or editorial debates were inevitable, we all voted for McQuown over Birdwhistell to break any deadlock" (Brosin letter to Stephen Murray, 7 May 1991, quoted in Murray 1994: 221, n. 40).

[10]In the table of contents, Chapter 9 is listed as being co-authored by Birdwhistell, Brosin, and McQuown, but the cover page for that chapter lists only McQuown, so he is the one credited here with writing it.

[11]McQuown's Spanish version of the NHI only offers three chapters from the English original: Chapter 1 (Bateson 1971a), Chapter 3 (Birdwhistell 1971d) and Chapter 10 (McQuown 1971c), plus the Foreword (McQuown 1971e) and Trager's earlier piece included as an appendix (Trager 1971). To supplement these, McQuown translated into Spanish the following: a paper by Scheflen (1966), a few pages from Zabor's dissertation (1978), entitled "Transcripción

(discussed further below) or "context analysis" as Scheflen called it (McQuown 1971f), taught that method to his own students (McQuown 1977; Zabor 1978), and encouraged the Chicago team members to publish related projects (Austin 1965; Duncan 1969; 1970; Duncan et al. 1968).

Charles F. Hockett (1916–2000) was a student of Sapir and Trager at Yale (in the same cohort as McQuown), known primarily as a linguist, and based at Cornell University (Hockett 1980). His strong national reputation (he was President of the Linguistic Society of America by 1964) was for work in structural linguistics (his 1958 introductory text was widely praised). In his interests he was not just concerned with issues regarding the structure of languages narrowly conceived. He was interested in the place of language in human life more broadly, much interested in the boundaries between spoken language and other modes of human communication, and he was rather unusual at that time for a linguist because he was interested in re-opening the question of language origins, a topic that, since the late 1860s most linguists had thought to be a waste of time (Hockett 1960c; Hockett & Ascher 1964). Hockett thought it would be useful to compare systematically features of what was then known of animal communication systems with features of human language and it was this that led him to formulate the "design features" of animal and human communication systems (Hockett 1960b). This was intended as a way of identifying just what features in human communication would have had to have evolved for language to be possible. Hockett's interest in the topic of language origins and the possible relationship of human language to communicative systems in other species meant that he shared interests with Bateson and came to be a contributor to the NHI group's insistence that *all* aspects of behavior in co-presence must be considered as having the potential for a role in the communication process. Earlier, Hockett had worked for the Department of State, so he had met Birdwhistell at FSI, although his was a different applied project.[12] Hockett worked with the Buffalo team once the NHI project divided into small groups. He received a grant to spend the summer of 1957 on a project with Fromm-Reichmann, but when she died, he joined another project just then getting started, "Linguistic-Kinesic Analysis of Schizophrenia", funded by the National Institute of Mental Health (see Watter 2017). Robert Pittenger and John Danehy were psychiatrists at Syracuse Univer-

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Kinésica Birdwhistelliana" [Birdwhistellian kinesic transcription], and three of his own papers (McQuown 1957; 1971f, and an otherwise unpublished paper, entitled "Modelo para la transcripción acústico-articulatorio-cinestética (Tragueriana y Pikeana)" [Acoustic – articulatory – kinesic transcription model (à la Trager and Pike)]).

[12]He prepared several handbooks for learning Chinese; Hockett & Fang (1944) was the first volume.

sity who had previously worked with Smith and Trager (e.g., Pittenger & Smith 1957), who were based at Buffalo with Birdwhistell, and all three of them served as consultants for the project (Birdwhistell, undated letter to WLH, received April 23, 1984). Together with Pittenger and Danehy, Hockett created a linguistic and paralinguistic analysis of an initial interview by a therapist of a new patient. The pages are cut, "Dutch door" style, with the words, phonetic and paralinguistic transcriptions appearing on the top portion, and commentary on the bottom; no kinesic analysis by Birdwhistell was included in the final publication.[13] *The First Five Minutes* credits the entire expanded NHI team:

> It is likely that we should never have been led to carry on the type of research in which we are now engaged had it not been for the stimulus all of us have had, over a number of years, from Gregory Bateson, Ray L. Birdwhistell, Henry W. Brosin, Norman A. McQuown, Henry L. Smith, Jr., George L. Trager, and the late Frieda Fromm-Reichmann. The occasional bibliographic credit given some of these seven in what follows is a totally inadequate identification of our debt to them. (Pittenger et al. 1960: ix)

Their research was an extension of the NHI project in terms of goals and techniques, incorporating different materials, with overlapping colleagues.

Of his departure from the NHI group, Hockett said: "I felt my theoretical orientation diverging from those of some of the other project members, and deemed it better for all involved if I developed my notions independently rather than running the risk of conflict within the project" (Hockett, letter to WLH, August 12, 1985). While he did not create his own research team to continue the project, choosing instead to join an existing group, he said he "regularly drew on the broadening of orientation the NHI work had given all of us" in his later teaching and writing (Hockett, letter to WLH, August 12, 1985). That impact can be found in Hockett (1960a), linking linguistics to psychiatry.[14] Despite his departure from the NHI group, Hockett wrote the chapter on vocal activity (1971b), prepared an

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[13]Of the project, Pittenger says: "A related development, which was not employed in the study under discussion, has been the work done in kinesics – the systematic study of body movements – by Ray L. Birdwhistell" (Pittenger 1963: 142). So, presumably Birdwhistell talked about what would have needed to be done to include a kinesic analysis, and the group decided not to include it.

[14]Specifically, Hockett says: "It was Birdwhistell's kinesics, Smith and Trager's paralinguistics, and the psychiatric-interview context that gradually rendered me uncomfortable with post-Bloomfieldian 'marble slab' grammar with its atomic morphemes and that forced me to try to look at language in action" (Hockett 1977: 107).

appendix on transcription (1971a), and co-authored the central chapter transcribing the interview serving as the focus of attention (Birdwhistell et al. 1971).

Henry W. Brosin (1904–1999) was a psychiatrist, like Fromm-Reichmann. They already knew each other and, as a psychiatric educator, Brosin took special interest in her project to improve the training of students. (He was chair of the Department of Psychiatry at the University of Pittsburgh, as well as Director of the Western Psychiatric Institute and Center, so particularly interested in education.) He was quite well-known nationally, both before and after NHI, serving as President of multiple organizations, including the American Psychiatric Association (Brosin 1968). Informally, like Fromm-Reichmann, he supported the group members during their time at the Center, but he was far more involved in the actual transcription and analysis and participated over a much longer period of time. He joined the CASBS fellows late, in December of 1955, but stayed with the project until the bitter end. On the actual process he said: "The enormously tedious work requiring hundreds of hours to do a microanalysis of even 120 seconds of film with twenty-four frames per second was beyond all of us except Norman McQuown for the linguistics, and Ray Birdwhistell during the summer of 1956 for the kinesics" (in his introduction to Leeds-Hurwitz 1989b: 97). In fact, he was one of the three who managed a group of researchers to continue the project's progress and, through the development of his research team in Pittsburgh, trained some of the next generation of researchers in microanalysis. Of the final document, he wrote two chapters related to psychiatry (1971c; 1971d) and two appendices documenting the references the group had found useful (1971b; 1971a). In his own chapters he credited the work of many of his team at Pittsburgh, and certainly encouraged them to publish projects related to NHI (Charny 1966; Condon 1970; Condon & Ogston 1966; 1967; Loeb 1968; Sarles 1974). He and his team continued writing about the value of film for psychiatry, as in Brosin (1959; 1964; 1966), Condon & Brosin (1969), or Condon et al. (1970).

Ray L. Birdwhistell (1918–1994), as already mentioned, was not one of the original CASBS fellows but was invited to join the group due to his invention of kinesics at FSI (Birdwhistell 1952; 1954; 1955), where he had met Hockett, McQuown, and Fromm-Reichmann. Although they were in different programs, Birdwhistell explained that Smith introduced McQuown to him, feeling they were "temperamentally suited to work with one another – i.e., equally compulsive about data – equally skeptical about explanatory schemes" (Birdwhistell, undated letter to WLH, received April 23, 1984). Birdwhistell specifically credits the work at FSI with setting up the necessary assumptions for NHI, as it "provided an atmosphere and the special guidance which encouraged the original formulation of kinesics as a science" (1971d: 22). He knew Fromm-Reichmann from her

visits to lectures at FSI. There's a story behind that too: anthropologist Edward T. Hall conducted research at the Washington School of Psychiatry while working at FSI.

> Because of my connection with the senior faculty members of the Washington School of Psychiatry and my close relationship with Dr. Frieda Fromm-Reichman, I was able to interest the major figures in the Washington psychiatric community in what we were doing at FSI. I felt that the communication process was at the core of psychotherapy. As a consequence, I used to invite most of the principal psychoanalysts to selected lectures at FSI. One of the spin-offs of this was Fromm-Reichmann's initiative in involving the linguists in her work when she was at Stanford. (Hall letter to WLH, November 13, 1989)

Birdwhistell again connected with Fromm-Reichmann at one of the Macy Conferences on Group Processes, and she thought to invite him to the Center when it became obvious that kinesics would be an essential part of the analysis. Birdwhistell knew Bateson (and Mead) from his time as a graduate student at the University of Chicago, and also saw them again at the Macy Conferences.[15] Conveniently, Birdwhistell happened to be physically in California in 1956, as he had been consulting with Bateson at the Veterans Administration in Palo Alto during several long visits a year since 1952 (Birdwhistell, undated letter to WLH, received April 23, 1984).

Once invited to join the CASBS fellows, Birdwhistell became a core member and maintained a central role until the very end. He created all of the kinesic transcriptions, and managed one of the continuing groups, training the next generation. He wrote the chapter on body motion (1971d, co-authored two chapters with colleagues, including the central chapter 6 (Birdwhistell et al. 1971); the other was on baselines (Birdwhistell & McQuown 1971). In addition, he wrote three appendices, all having to do with kinesic transcription techniques (Birdwhistell 1971a,b,c). In his chapter on body movements, he credits Bateson & Mead (1942) as providing "the most important anthropological contributions to the development of the study of body motion as a communicational system" (1971d: 18), referring to *Balinese Character* (1942). Birdwhistell managed the team at Buffalo (mostly linguists) and the one at the Eastern Pennsylvania Psychiatric Institute

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[15] As described by Davis (2001: 41–42): "Legend has it that Birdwhistell was a younger anthropologist listening to Mead and others comment on a Balinese film when he interjected something like, 'But did you see what the mother did with the baby after she took him out of the bath?' He then brought to their attention a fascinating medley of actions that occurred in a few seconds".

(EPPI) (relying heavily on Scheflen, and the film technician Jacques D. Van Vlack, with less participation by Raven McDavid, Jr., and William M. Austin).[16] He presented large numbers of conference papers, published journal articles and book chapters, most often about kinesics (Birdwhistell 1959b,a; 1960; 1961a; 1968a,b,c), and a book (1970, which includes 2 chapters from NHI: Chapter 3: Body Motion (1971d), and Appendix 6: Sample Kinesic Transcription (1971c), retitled "A Linguistic-Kinesic Exercise: The Cigarette Scene", and probably his best known single piece). As with Brosin, Birdwhistell encouraged his team members to publish as well (Scheflen 1963; 1964; 1966; 1968; Scheflen et al. 1970; Van Vlack 1966a,b). We shall have more to say about Scheflen in section III. He became much connected with the core NHI group members: Birdwhistell and Bateson served as consultants on Scheflen's later project (Scheflen 1960), especially during the final year of research when the film analysis was undertaken (1960: xv, 9, 269), and Scheflen (1973) bears a clear relationship to NHI in terms of both method and assumptions. As we shall see, Scheflen was important for later developments in the work initiated by NHI and he developed the theoretical framework in important ways, developing a focus upon the organization of occasions of interaction (such as psychotherapy sessions) showing how they can be regarded as self-regulating systems with developmental programs and processes by which they can adapt to changing environmental circumstances while maintaining their integrity.

Gregory Bateson (1904–1980) had prior experience with the Macy Conferences, and so knew several of the CASBS fellows (Fromm-Reichmann and Brosin) and he was already working with Birdwhistell. At the time the project started at the Center, Bateson was working at the Veterans Administration Hospital in San Francisco, and so he was nearby. He had previously worked with psychiatrist Jurgen Ruesch at Langley Porter Clinic in San Francisco, so he was already thinking about the ways in which communication played a role in psychiatry (Ruesch & Bateson 1949; 1951) and filming psychiatric interviews (e.g., Ruesch et al. 1955). The fact that he could supply relevant films for the group to analyze made everything move quickly (Bateson 1958: 97). For the final NHI volume, Bateson wrote the chapter on communication (1971a), and the chapter explaining the context of the data (Bateson 1971b). However, he only participated in the small group sessions and conference presentations outside of California sporadically, and ceased his involvement altogether by 1960, as he was then moving away from the study of people and into the study of animals, turning in his chapters

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[16]Austin was Research Linguist at EPPI across 1961–62, and then a professor at the Illinois Institute of Technology, so he was convenient to both McQuown's team and Birdwhistell's (McDavid 1972; Puech & Puech 2018).

before his departure from the group in 1960 (McQuown, letter to Yves Winkin, June 5, 1981). Like most of the others, he continued publishing on related topics (Bateson 1958; 1959), although less than some, given that his interests in animal communication (which he had begun to pursue in 1952, when he studied otters and raised questions about the nature of play; see Bateson 1956, summarized in abbreviated form in Bateson 1972) were beginning to overtake his concerns with human communication.

Bateson was the group member with significant prior experience in recording interaction. After graduating from Cambridge where he had studied zoology and botany, and then anthropology (at the urging of A.C. Haddon, at that time professor of anthropology at Cambridge), he embarked on fieldwork in New Guinea, an outcome of which was his book *Naven*, a study of coming-of-age rituals among Iatmul (Bateson 1936). In this work he had already become interested in communication processes in interaction, realizing their importance in the development, maintenance of differentiation of social roles and relationships. In this fieldwork he had also made some use of photography. After finishing *Naven* (which he wrote in Cambridge) he returned to New Guinea for further fieldwork, where he met and eventually married Margaret Mead. Together with her he undertook a study focusing on child rearing practices in Bali, using both still photography and cinematography extensively, and showed the value of these technologies for analyzing social interaction. Together they published *Balinese Character,* an extensive photographic analysis of many aspects of Balinese social behavior (Bateson & Mead 1942). As already mentioned, this book had a significant influence on Ray Birdwhistell (among many others) and proved to be of importance in developing interest in the analysis of the small details of behavior of interaction and how important it was to study them in order to understand how social relationships develop and are maintained. Some years after completing this book, Mead produced a set of short films from that research which were used in anthropology courses in the US for decades. Of these, *Bathing Babies in Three Cultures* (Bateson & Mead 1954) and *Trance and Dance in Bali* (Bateson & Mead 1952) are probably the best known (Henley 2013; Jacknis 1988).

Bateson had already given much thought to developing a theoretical account of communication processes but remained unsatisfied with his attempts at this until, in 1942, he first encountered ideas about feedback processes and the nature of self-regulatory systems as these were being developed by Norbert Wiener and others as *cybernetics.* Bateson participated in some of the Macy conferences which soon focused on this. After some years spent abroad, working for the US government through the Office of Strategic Services (war time precursor to the CIA), and then a temporary position at Harvard, he was put in touch with Jurgen

Ruesch, a Swiss psychiatrist who was setting up a program of research into communication in psychotherapy, who hired Bateson for this project. Bateson and Ruesch collaborated on a book which was published in 1951, in which ideas from cybernetics were used to understand human communication networks. Bateson soon began working on communication patterns in families with a schizophrenic member and eventually developed his theory of the "double bind", attempting to understand how conflicting communication with a family system could bring about schizophrenia (Bateson et al. 1956). As mentioned, Bateson had already become acquainted with Fromm-Reichman as a result of his participation in the Macy conferences, and when she arrived in 1955 for her fellowship at the Center, since they were now both in Palo Alto, it is not surprising that he should join her project at the Center as a consultant.

As already indicated, Bateson's contribution to the NHI seminar was important because he made films available to the group for discussion and analysis, but also important was his theoretical contribution. In fact, Bateson could become impatient with minute data analysis and he did not contribute much to the work of transcription and detailed discussions of specific observations. As Birdwhistell has stressed, Bateson's interest was mainly in broad theory, much less in the small details. Thus he commented: "In our every meeting, even though much of the detailed and necessarily minute data I manipulate often fails to excite him, he has supported my contention that communication is a social matter" (Birdwhistell 1977: 114).[17]

In addition to the six central members, one peripheral group member was included in the final NHI volume: **George L. Trager**'s article on paralanguage was included as an appendix (Trager 1971) since the content was so central to the project, despite the fact that it had been previously published in 1958.[18] Trager had been a colleague of Sapir's at Yale, he worked with Birdwhistell (and Smith) at FSI, so was very much a member of the theory group described here. He developed the notion of paralanguage while at FSI, although at the time that group was using the broader term "metalinguistics" to include the wide range of communication behavior beyond language (Smith 1952; Trager & Hall 1954; Trager & Smith 1951). The concept of paralanguage was only fully developed while working with the Buffalo group under Birdwhistell's direction (McQuown 1971c: 2). Trager names Smith, McQuown, and Birdwhistell as "virtual co-authors" (Trager 1958: 3), and Bateson is credited with suggesting the phrase "vocal segregates" (1958: 6), a term still in use today.

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[17]For an account of Bateson's life and work, see Lipset (1980).

[18]In the article he cites NHI, so to then have his article included in NHI seems oddly circular.

3 Further developments in methodology and theory, following the 1955–1956 CASBS seminars

As we noted in the introduction, some of those who had participated in the 1955–1956 seminars, once they had returned to their home institutions, continued to work on the NHI project and drew into this work new students and colleagues. These included Norman McQuown at Chicago, Ray Birdwhistell, first at Buffalo and later at EPPI in Philadelphia, and Henry Brosin in Pittsburgh. For two or three years following the Center seminars, the participants re-convened in Buffalo, Pittsburgh and Philadelphia, and at these meetings they continued their discussions, also with the involvement of some new participants. There were overlaps between the sets of meetings: Birdwhistell often joined the Pittsburgh group ("once a month for 3–4 days for 4 years", he says in an undated letter to WLH, received April 23, 1984). Team meetings led to a variety of conference presentations, mostly at psychiatric conventions, and publications mostly in related journals or books.[19] The final NHI manuscript was ready for publication in 1968 but proved to be unpublishable due to both length (it takes up five large volumes) and format (3 of the volumes are transcriptions of the data), so it was eventually made available through the microfilm series at the University of Chicago (McQuown 1971g) and it is now available as a CD-ROM or PDF.

We now consider in a little more detail the post-CASBS involvement of the original participants (except for Fromm-Reichman, of course, who had died), with some observations on the new participants who became part of the endeavor locally, at Chicago, Buffalo, Philadelphia, and Pittsburgh.

Norman McQuown, at the University of Chicago, as already described, continued to do much work on the transcription and also was involved in the writing of several chapters that became part of the final manuscript that he coordinated and edited. In this work he was aided a great deal by Starkey Duncan, as has also been noted. Duncan, in his own work, went on to analyze the kinds of cues that participants in conversation make available to one another which appear to play a part in coordinating the exchange of turns at talk. There were also other younger colleagues who worked with McQuown on research related to the NHI project. These included Raven McDavid, Jr. (a faculty member in linguistics at the University of Chicago then) and William Austin (faculty in linguistics at the Illinois Institute of Technology, located in the city of Chicago), and William Offenkranz

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[19]For example, Birdwhistell presented at the "Conference on Experimental Psychiatry" which Brosin organized with his team in 1959 and published (Brosin 1961); Birdwhistell's talk was on paralanguage (1961a).

(faculty in psychiatry at the University of Chicago). McDavid and Austin had worked with Smith, Trager, Hockett, and McQuown for the Army Language Section during the war, and so were already part of this extended network (McDavid 1980). Zabor (1978: 160) says that "McQuown also offered a course, 'Interview Analysis,' using the written, film, and audio tape materials of the NHI project as primary text material". Both Hockett and Birdwhistell had chapters published in a book that Austin edited (1960).

Ray Birdwhistell started a new position at the University of Buffalo after the NHI seminar ended, taking the post of an Associate Professor of Anthropology and Coordinator of an Institute for Human Communication. He continued work on the NHI materials and collaborated both with George Trager and Henry Lee Smith, who were in the Department of Linguistics (Trager, as already noted, contributed a chapter to the NHI collection, writing about paralanguage). Birdwhistell also hosted several get-togethers of the CASBS participants for continued work on the NHI materials. In 1959, however, Birdwhistell moved to Philadelphia to become a Research Scientist at the Eastern Pennsylvania Psychiatric Institute (EPPI). He was influenced to make this move by Albert Scheflen who had been studying patterns of communication in psychotherapy and they began a close collaboration. Scheflen and Birdwhistell organized a number of seminars and short courses on the study of human communication. Some of these were attended by Gregory Bateson and others from the original NHI project, as well as by Margaret Mead, but also others from elsewhere. These seminars and courses were important for making the insights of the NHI methods and theoretical framework more widely known.

Scheflen, as a result of his collaboration with Birdwhistell, made important further contributions. He applied methodologies he learned from Birdwhistell to work on communication in psychotherapy with very interesting results (representative is Scheflen 1973). Further, some of the papers he published in the early 1960s provided very clear and concise expositions of the method, findings, and the theoretical framework first developed in the NHI seminars. These were most valuable for others wanting to learn about this work (see Scheflen 1963; 1964; 1965 in the journal *Psychiatry).* He enriched the theoretical framework, for he made clear the nested hierarchical structure of communication processes, and widened the focus of analysis by developing ways to think about the patterned structure of occasions of interaction such as psychotherapy sessions, informal conversations, or greeting encounters. Kendon & Ferber's (1973) investigation is a good example of a work which is very much indebted to Scheflen's approach – see also Kendon (1981) and Kendon (1990). Scheflen also recognized the great

importance of spatial organization in interaction occasions, as witness his book with Ashcraft, *Human Territories* (1976).

Henry Brosin, whose home institution was the Western Psychiatric Institute and Clinic (WPIC), upon his return from California, set about assembling a small research team who were to pursue various issues, practical and theoretical, that had arisen from the Center's work. He also hosted some of the follow-up meetings and Ray Birdwhistell was a regular visitor there. The research team he assembled included E. Joseph Charny (faculty member in psychiatry at the University of Pittsburgh), William S. Condon (a doctoral student in Philosophy at the University of Pittsburgh), Felix F. Loeb, Jr. (a psychiatrist at WPIC), and Harvey Sarles (a faculty member in anthropology at the University of Pittsburgh), with Kai Erikson (Eric's son, a sociologist at the University of Pittsburgh) participating to a lesser extent. Both Charny and Loeb contributed research papers based on studies of their own psychotherapeutic work (Charny 1966; Loeb 1968), and Sarles published a number of theoretical papers (1974; 1975). Condon worked on the micro-organization of the flow of bodily movement in relation to speech and was important for certain advances he made in techniques of film analysis, as well as making important discoveries in regard to the way in which participants in face-to-face interaction often synchronized their action flows. As his contributions were directly relevant to extending methodologies in the NHI enterprise, we discuss his work a little more fully.

Condon had a background in philosophy and began his career teaching philosophy at Pittsburgh while still a graduate student. He became associated with WPIC when Brosin was director. He took a two-week course in linguistic and kinesic analysis with Birdwhistell and Scheflen (held at EPPI in Philadelphia). After this, he did not continue with philosophy but devoted himself to studying human communication, approaching it from the perspective he had learned about from the course at EPPI. He went to Chicago for a year to study linguistics with McQuown as a postdoctoral scholar, then returned as a researcher at WPIC under Brosin (Condon 1979). Here he began to investigate speech and body motion interrelations using sound film. Using a hand operated film analysis projector coordinated with a soundtrack reader, he developed microscopic techniques to investigate the flow of units of bodily movement and their coordination with speech, verifying and refining Birdwhistell's initial observations. He went on to examine how participants in co-present conversations often entered into synchronous relationships in their bodily movements. He termed this *interactional synchrony* (as explained in Condon & Ogston 1966). Continuing research on this, the nature and origins of interactional synchrony became his main research preoccupation. His methods of film analysis which Kendon, who studied with him in

1966–1967, has termed *movement phrase boundary analysis* (see Kendon 1977),[20] led to his ideas about what he called *process units* and their complex, multilevel overlapping organization (see Condon 1970; 1976; Condon & Ogston 1966; 1967).

Condon is a good example of someone who, upon encountering the NHI work through the course he took at EPPI, was sufficiently drawn to it to seek to continue working within that framework and, in doing so, not only contributed usefully to the methods of microanalysis that had already begun to be established, but then went on to investigate interactional phenomena that the NHI work had not dealt with. His trajectory also illustrates how the work originating with the 1955–1956 NHI seminar had matured enough for its methods and theoretical framework to be taught. And it is notable that this teaching was done, not only by one of the original participants (Birdwhistell) but also by someone who had fully absorbed the framework and then played a significant role in extending and elaborating it (Scheflen).

Lastly, it is appropriate to mention Kendon here, for he worked for the academic year 1966–1967 at WPIC, where he learned methods of micro-film analysis from Condon and then, in the Fall of 1968, joined Scheflen's project at Bronx State Hospital, in the Bronx, New York. Kendon had completed a thesis for the degree of D.Phil. at Oxford in 1963 on face-to-face interaction, using the methods of Eliot Chapple (with whom he worked, gathering the data for his thesis). After gaining the D.Phil. degree, he continued as a Research Assistant in the Institute for Experimental Psychology at Oxford (long since Department) where, with the assistance of E.R.W.F. Crossman, who had studied skilled action in operatives in manufacturing, he began to study films of two-person conversations. Dissatisfied with Chapple's insistence on measuring only the "actions" and "silences" of conversationalists without considering other aspects of their behavior, he proposed to examine, in relation to the spoken utterance exchanges, facial expressions, gaze direction, posture changes, change in head position, and hand movements in the conversationalists he filmed, believing that these things must play a role in the mutual coordination of actions in conversations. An outcome of this investigation was a publication on the apparent role of changes in gaze direction in the participants in regulating turn-taking (Kendon 1967). While engaged in this research, he had his attention drawn to Scheflen's article in *Psychiatry* of 1964 on the significance of posture in face-to-face communication. This article, a very clear and concise summary of the kinesic observations of Scheflen and of Birdwhistell, immediately struck Kendon as representing the kind of ap-

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[20]See the Appendix, pp. 225–240 for an account of "movement phrase boundary analysis", the method of microscopic film analysis as learned from William Condon.

proach to the study of interaction he was himself trying to develop. He thereafter got in touch with both Birdwhistell and Scheflen, receiving preprints and publications from them. In the summer of 1965, he was able to visit Scheflen in Philadelphia and showed him a preprint copy of his investigation into the functions of gaze direction in interaction. Scheflen was struck by this work and, eventually, arranged for Kendon to go to WPIC, where he worked with Condon, as already mentioned. Subsequently, in 1968, Kendon was able to join Scheflen's new project at Bronx State Hospital in New York. There he worked on studying greeting encounters and on the spatial-orientationa l structure of various kind of occasions of interaction (many of the essays in Kendon 1990 are a product of this work with Scheflen). He also did studies on how hand and head movements in speakers were co-ordinate with spoken utterances (Kendon 1972b; 1980), which were to be foundational for work in gesture studies as it developed later. For this work and for the work he undertook while at Pittsburgh, he made use of films made available to him by Birdwhistell, with whom he was in contact, though he never actually worked directly with him. He also published an extensive appreciation of Birdwhistell's work in kinesics (Kendon 1972a being an essay review of Birdwhistell's *Kinesics and Context*). Kendon, thus, coming to the study of social interaction with his own perspective which, so he discovered, was very compatible with the approach of the NHI project, is an example of someone who, so to speak, adopted himself into that network and to some degree has continued its tradition. A good example of how the NHI work has infected and modified the work of another who came to it from the outside.

4 The Natural History Method as Developed from the NHI Project

Here we describe the Natural History method, beginning with the way it was first formulated but then also incorporating later modifications and refinements due to the further work of Birdwhistell and Scheflen, also adding observations by Kendon who has discussed aspects of this methodology in several places as an outcome of his collaborations with Condon and Scheflen (see Kendon 1977; 1979; 1981). The NHI researchers spent an enormous amount of time preparing the NHI report and intended that "this manual may be used for the training of further adepts in the techniques of analysis and interpretation" (McQuown 1971e: 3). In fact, partially due to the final decision that the result was unpublishable, and partly to the fact that the "Doris" film used for analysis could not be made available in tandem with the transcription due to confidentiality concerns, it was

rarely so used, and then mostly by McQuown, either with his Chicago students, or with his students at Universidad Nacional Autonoma de Mexico, in the Spanish translation he prepared (1983). However, it is still useful to examine the method used to analyze their data due to the influence of the project on later researchers.

McQuown describes six steps (McQuown 1971e: 5):

1. Soaking (multiple viewing-listening),

2. Scene selection and intensive study,

3. Matching (and tagging with a frame number) of particular points in the kinesic record with their counter parts in the linguistic record,

4. Identification of symptomatic features,

5. Specification of clusters of symptomatic features, and

6. Uncovering of the interaction profile.

Before considering these steps, however, there is one step omitted from the list which is essential: acquiring a corpus of data to analyze.

Step 0: Obtaining a Corpus

The NHI team members at CASBS never intended to begin a long-term project, and so no one spent much time worrying about what data would be appropriate to use when beginning their second project in early 1956. After all, they were scheduled to be together for only a few more months. Birdwhistell knew that Bateson had been filming therapist/patient interviews and might be willing to permit their use, so it seemed simplest to use one of Bateson's films, and that is what they did. As Bateson describes it:

> We start from a particular interview on a particular day between two identified persons in the presence of a child, a camera and a cameraman. Our primary data are the multitudinous details of vocal and bodily action recorded on this film. We call our treatment of such data a "natural history" because a minimum of theory guided the collection of the data. The cameraman inevitably made some selection in his shooting; and "Doris", the subject of the interview, was selected for study not only because she and her husband were willing to be studied in this way but also because this family suffered from inter-personal difficulties which had led them to seek special psychiatric aid. (Bateson 1971a: 6)

Typically, the expectation (at least on Birdwhistell's and Bateson's parts) was that filming was itself still not the first step. Prior to recording should come observation; the intent was to document something already known to be potentially relevant and interesting. Bateson had already demonstrated this with *Balinese Character* (Bateson & Mead 1942), where the photographs used in that publication were taken in the light of understanding aspects of Balinese culture which they had arrived at through observation and with acquaintance with previous work by others in Bali. Clearly this position made even more sense to both Birdwhistell and Bateson as the years passed. For example, Birdwhistell made fun of the tendency of some "to go into the field, aim a camera or a microphone at a social occasion, and come back with the conviction that social convention or 'culture' has been ethnographically recorded for posterity" (1977: 111–112).[21] In class at the University of Pennsylvania in the 1970s, he used the metaphor of the vacuum cleaner to warn of the dangers of just gathering data with no idea of what it meant: "Once the bag is full, you then have to sort out the dirt you sucked in" (Yves Winkin email to WLH, May 20, 2020).

Some years after starting his work with Birdwhistell, Scheflen provided an explicit outline of the multiple steps ideally to be taken *prior* to recording any data (Scheflen 1973: 313–314):

1. Go to the site where the event being studied normally occurs.

2. Show up on the occasions at which it would happen anyway.

3. Observe experienced participants who already know each other.

4. Take all possible measures to avoid changing the situations.

5. Observe rather than participate directly.

Once it was clear what behavior was to be recorded, then the goal was to preserve that behavior so it could be viewed over and over again during analysis. Kendon

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[21]His many book reviews offer remarkably caustic comments in support of his views of what constituted adequate research, such as: "The little volume should be convincing as to the inadequacy of exclusively verbal data as a reliable instrument for measuring interpersonal adjustment. Students tempted to *substitute* the tape recorder for observation should read this before going into the field" (Birdwhistell 1964b: 486, emphasis in original) or "His attack is never burdened by data [...] he offers no behavioral data to support his conjectures [...] This book should be very useful as required reading for students who doubt the need for field work and for direct observation" (1964a: 1463-1464).

(1975: 7) highlights the need for recording, whether audio or video, prior to analysis: "Sound-film and videotape are thus the primary instruments because they are the only means available by which behavior may be 'fixed' and so made into a specimen that can be repeatedly examined" (see also Mead 1969). Brosin (1971c) points out the value to psychiatrists of having recordings: additional therapists could view an interaction after the fact and consult on its meaning. It becomes clear: first needed is some observation of a context, then and only then is it time for recording behavior. But again, before recording can begin, several decisions must be made.

David M. Myers was the technician who filmed and taped the interview with Bateson used in the NHI project (Bateson 1971b), but his comments on what he was doing are not available. However, the primary filmmaker at EPPI was Jacques Van Vlack, and he did leave a written explanation for others who might take the role of technician. For psychiatric interviews, he emphasized the importance of adapting to the needs of the research subjects, and recommended: studio quality lighting, clear high fidelity sound (wireless lavalier microphones and a spot microphone, supplemented by a separate audio recording), a camera set up to film the entire scene unattended so as to minimize interaction with an additional person, as well as a secondary camera for close-ups (Van Vlack 1966a). He stressed such details as having a second original copy in case of disaster, never permitting analysis of the original lest it be damaged, and absolutely preserving the confidentiality of the participants (1966a). Van Vlack also stressed the way in which "the sound camera[...] is a data-recording tool which circumvents an observer's cultural and psychological biases" (1966b: 5). And he described the value of assigning a number to each frame of a film, so that researchers could refer to them in the analysis, while using motion analysis and stop frame projectors (1966b). This he accomplished by creating a special frame numbering "B-Roll" – the films to be used for analysis were printed so that a frame number appeared at the top of each frame which could be seen when the film was viewed. Kendon (1979) also reviews some of these and other technical requirements. Among other additions, he stresses the importance of maintaining the camera angle: "choose the most comprehensive angle possible and then stick to it [...] so that all of the participants in a transaction can be seen all of the time" (1979: 75). And if possible, start filming before the event that is the focus starts, and continue after the participants have dispersed so as to "record the behavior by which the event was set up and by which it was brought to an end" (1979: 75). Only after all three of these preliminary activities (making observations to determine what to record, deciding how to record, actually recording) have occurred is it time to move on to the steps of analysis.

Step 1: Soaking

Once interaction has been recorded, analysis can begin. The first step in Mc-Quown's list is "soaking", which refers to repeated viewing of (and listening to) the film. Birdwhistell prepared a detailed description for all steps of the process, which McQuown included in the Collation (1971d: 5-10). Here are his comments on soaking:

> The film – with sound – was played through fourteen times in joint sessions before each of the analysts turned to his special medium. The linguists and the kinesicist again extensively reviewed the full collection of materials, each concentrating on those of his own medium. The psychiatrists joined them for listening or viewing, meanwhile continuing to gain perspective on the family being interviewed and on their associations with the researcher-interviewer, with the therapist, and with the neighbors who appear in several sections of the film not covered by the intensive analysis. (McQuown 1971d: 5)

Some clarifications may be useful. As a reminder, the linguists were Hockett and McQuown, the kinesicist was Birdwhistell (otherwise known as an anthropologist), the psychiatrists were Fromm-Reichmann and Brosin, and the researcher-interviewer was Bateson. The "full collection of materials" reviewed by team members refers to additional films of this family, as well as interviews with the psychiatrists who were actually treating Doris, the woman who serves as the focus in the film provided by Bateson (described in some detail in Bateson 1971b). These additional films of the family and interviews of the therapists served to provide context for the NHI core team and substituted for the more standard observation prior to recording. "The material from these subsequent filmings has all been a part of the background of the present study, though no part of it was actually used for micro-analysis" (Bateson 1971b: 4). There was also "a magnetic tape recording of the entire proceedings" (ibid.: 5), meaning they made both video and audio recordings simultaneously. Brosin (1971d) explains in some detail what material beyond the audio and visual recordings was available to the team: six film segments (created across two visits to the family home, involving Doris' son, husband, and various friends), plus Fromm-Reichmann and Brosin visited Doris and her son at home, they met separately with Bateson, and they met four times with Doris' regular therapist. Bateson explained to Doris his goal at the time:

> We're studying the disruption of communication between parents and children, trying to get some idea of the various gambits that the two sides use,

in trying to get together or, the degree to which the gambits separate them or bring them together. There's very little been done, actually, on the actual natural history of what does happen between parents and children. I mean a thing like that "Three Families" film. Nobody else has done a film like that that I know of. It's very obvious, it's very accessible, ready to do, and most of what is said about parents and children is on somebody's report of what happens. So, we're trying to get in and do the natural history of it a little. (Zabor 1978: 229-230)

Zabor transcribed this from the audio recording made during the interview; it was not caught on film and is not included in the NHI document itself.

Step 2: *Scene* selection and intensive study

The second step Birdwhistell outlined was to choose the critical scenes that seemed important to transcribe and analyze. As he described that step in NHI: "With the perspective gained through these experiences [those included in the full collection of materials listed above], the whole group collectively selected certain scenes for special consideration. The first of these chosen was the 'cigarette' scene […] which appeared to mark a critical point in the interviewer-interviewee relationship." Following that choice, "a variety of scenes within the interview were subjected to varyingly intensive analysis" (in McQuown 1971d: 6). Again, some clarification may be useful: the cigarette scene was the moment during their conversation when Bateson lit a cigarette for Doris. Once having sorted out which scenes would be the focus, Birdwhistell reports that the group returned to further soaking. "Repeatedly, during this research period, the team as a whole sat together for a full screening of the entire interview. Only in this way was it possible not to lose perspective" (in McQuown 1971d: 7). Based on his later experience in the project, Scheflen (1968) suggests that it may be necessary to view a film clip 50 to 100 times over the course of analysis. The need for it, of course, at least in those days, had partly to do with the fact that one was led to do this kind of close scrutiny of the film clip because one kept seeing new things – things that had never been seen before – in this way it was like looking in a microscope in early days of microscopy; it was revelatory. A very important instrument in this work was the use of hand-operated film analysis projectors which allowed one to look at very short stretches of film at close to normal speed. In this way one was able to see the movement segments and how these were interrelated. It is different from frame-by-fame analysis, also used. It was indeed a revelation to look at films of interaction in this new way. Specialized equipment was required,

including a Bell and Howell Slow Motion Analyser and a PerceptoScope. Bird-whistell called the former "reliable, sturdy, and easy to operate", and the latter was "designed for military use" with "complete time control of in-focus images," but much more expensive; even so, "as far as I know, it has no equal as a research or exhibition device" able to "stop and hold a given image for examination for a predetermined period of time" (Birdwhistell 1963: 58). As documentation that the PerceptoScope was intended for military use, in July 1956 an advertisement calling it "the new electronic aid for modern military training" was published in Air *Force: The Magazine of American Air Power* (Perceptual Development Laboratories 1956). Two years later, it was being advertised in the *Journal of the Society of Motion Picture Engineers*. This time the advertisement used a small photograph, with a detailed description of what it could do, including use of "a hand-sized electronic remote-control unit on a 25-foot cord" (Perceptual Development Laboratories 1958).

Step 2.5: Transcription

Again, there is a step which remains implicit in the description provided in the NHI compilation of 1971: transcription. Notice that transcription comes after soaking, not before. Through watching the visual record, "a conception of the structure of the event quite often emerges" (Kendon 1981: 479). Transcribing early, before one had thoroughly familiarized oneself with the specimen being studied, might mean making choices about what is important before the material was more fully understood. It is, of course, impossible to transcribe everything: "[…] no transcription, no matter how fine grained, is ever complete. One must inevitably make a selection. Thus the map one makes, the transcription one produces, is as much a product of one's investigation as a means of furthering it" (Kendon 1981: 479).

Birdwhistell credits McQuown with establishing the original design of the transcription process, before they had the Van Vlack frame numbering system in place:

> McQuown, as organizing editor of this multidisciplinary research, had insisted on fine-grained and exhaustive recording of both the linguistic and kinesic material. This recording was done as independently as possible: McQuown and Hockett working with tapes, while I recorded from the silently projected film. Later, McQuown and I, by careful listening and viewing, gave frame numbers (thus timing) to the material from the two modalities. (Birdwhistell 1970: 116)

To emphasize: the linguists created their initial linguistic and paralinguistic transcription only from the audio record, while Birdwhistell created the initial kinesic transcription only from the visual record, played without sound. Then they combined the separate transcriptions into a single whole, and this is what serves as the heart of the NHI report: Chapter 6, taking up three of the five total volumes of the printed version (Birdwhistell et al. 1971). This separation was not maintained once the researchers left California.

In his comments at a conference a few years later, Birdwhistell explained the basics for an audience unfamiliar with NHI:

> We are now recording from interactional behavior approximately one hundred forty-one lines of discrete information. Those one hundred forty-one lines are levels of abstracted material, separated carefully to make sure that we do not throw any future babies away in last week's bathwater! In final synthetic analysis, these must be put back together. However, you must first establish levels of behavior. When Scheflen and I work on kinesics, first we examine an incident, a piece of behavior, in a number of matrices. We ask: What was the response of others to this behavior? What does this allow us to discriminate as appropriate or inappropriate? (Birdwhistell comment in Hayes & Sebeok 1972: 173)

Like soaking, transcription was never expected to be a one-time activity (transcribe – once – and then move on to analysis using only the transcription). Instead, the transcription was repeatedly revised throughout the process of analysis. Kendon points out that the stages of transcription and analysis are interdependent, and so the process is both interdependent and cyclical: "A transcription system embodies a theory as to what constitutes the significant units of which the phenomenon being transcribed is made up" (Kendon 1979: 78). Thus, transcription does not occur prior to analysis, but rather should be understood as one element of theorizing about what is occurring during interaction, and how best to analyze it. Obviously, this technique takes far more time and effort than simply creating a transcription once and assuming it is reasonably correct. Even so, over time group members got significantly faster at the process. Birdwhistell proudly points out that "During the course of investigation, techniques were developed that reduced recording and analysis time [...] from about 100 hours per second to less than one hour per second" (1970: xi–xii). While he was pleased with the reduction, most others were only astonished that he had ever been willing to spend so much time. But the considerable time spent did not only apply to Birdwhistell's team at EPPI, and not only for NHI, but for others, and for later

projects. As an example, Duncan (part of the Chicago team) transcribed two 19-minute segments of film for a later study of turn-taking, and it took him "the better part of two academic years" (1972: 285). Much as he found these sorts of detailed transcriptions to be valuable, he acknowledged "A primary obstacle to research of this type is the laboriousness of making fine-grained transcriptions of multiple interaction behaviors" (1972: 291). Now with the availability of ELAN (an audio and video recording annotation software developed at the Max Planck Institute for Psycholinguistics in Nijmegen), and similar programs, the process has been somewhat streamlined and is now more standardized.

Step 3: Matching

In the original NHI, much effort was spent sorting out what behavior in which scenes was similar, or related in some way, so that that behavior might be granted additional attention. Birdwhistell explained it this way:

> It soon became evident that a topography of interaction for the whole inter-view might be worked out [...] As the team repetitively reviewed the film as a whole, it became clear that even though the trail of analysis, which the research problems themselves had imposed, had established some boundaries to the relations of the persons in interaction, the interaction itself contained self-regulatory mechanisms which required charting, if the interaction was to be understood and its topography established. (In McQuown 1971d: 7)

He added clarifications a few years later:

> When we do an analysis, we abstract particular events, search through our corpus until we find comparable events, and then look for larger frames within which they regularly occur. We ask whether there is anything in this which tells us that this piece has to be accommodated to somewhere else in the system. If you find a piece of behavior which is otherwise repetition and discover that it differs significantly in some respect, a search through the corpus usually reveals a cross-reference signal, often in the paralinguistic or tactile system, which handles the discrepancy or at least identifies it as especially worthy of attention. (Birdwhistell comment in Hayes & Sebeok 1972: 173)

The focus at this stage is to sort out what is important, "an initial delineation of structural units", as Kendon puts it (1979: 73). Each unit is examined in its context and compared with other examples of the same behavior. Kendon continues:

"Context analysis, thus, is so called because it insists upon always examining the patterning of units in their contextual relations with other units, and the interactive functioning of behavioural units is derived from the difference their occurrence or non-occurrence makes in otherwise similar contexts" (1979: 73–74). Observation provides one way of expanding an analyst's understanding of context, and soaking provides another, so matching provides the third element.

Steps 4, 5, 6

The focus on symptomatic features in steps 4 (identification of symptomatic features), and 5 (specification of clusters of symptomatic features) as presented by Birdwhistell in NHI are only relevant when the context is a psychiatric interview – so later publications drop that vocabulary, and it is not worth much time here. Step 6 (uncovering of the interaction profile) is explained this way by Birdwhistell:

> Throughout the analysis of the interview, the principal focus of interest of the group was on the abstraction of the relationship between the participants in it. Every attempt was made to frame the analyses, linguistic, kinesic, and psychiatric, in terms which would turn the attention of the analysts to the on-going social relationship and which would preclude the development of a set of parallel but separate biographies. A consistent effort was made to see in the individual responses not merely indices to personal systems but also indications of the developing interaction between such systems. (In McQuown 1971g: 52)

The important part here is to stress that the analysis did not focus on individuals: the goal was to understand *how interaction between people and within relationships works*, and so the parts (that is, individual utterances or movements) need to be seen as pieces of a whole. After all, although Fromm-Reichmann's original intent was to understand her patients, the eventual goal adopted by the NHI group was rather to examine the entire interaction, to see how multiple participants connect and mesh what they do when they are together. The separate pieces fit together in levels, as Kendon explains: "in examining the behavior in the specimens [recordings] gathered, one seeks out recurrent patterns in terms of units of behavior that are relevant for the communication system that is in operation […] the structural units of behavior which are being sought for the participants' behavior may be recognized at several different levels of organization. Units at one level […] may themselves participate as components of units at higher, more inclusive levels" (1990: 35-36).

Steps 3, 4, 5, and 6 are all parts of the analysis of the recorded data and so were most often combined once the researchers moved into small groups in Chicago, WPIC and EPPI. For example, Zabor combines these into a single stage of "analysis of the corpus" saying that "all perceptible behaviors are notated within the limits of the notation system and the researcher's perceptions" (1978: 208), separating out the technical paralinguistic and kinesic analyses as later steps. Essentially all of these steps are about gradually locating the various clusters of behavior that are related and coming to understand the ways in which they relate one to the other.

Scheflen added some features of the steps of the natural history method (which he came to call "context analysis"). One feature he emphasized which was not brought out in earlier formulations was his observation that communication during a therapy session is structured and can be analyzed. Indeed, one of Scheflen's innovative contributions to the method was that occasions of interaction tend to have a traditional overall structure or program. This he first pointed out in his studies of psychotherapy sessions, but he came to see that it applied to most kinds of occasions of interaction. In the light of this, the steps of analysis that he outlined are slightly different from those we have quoted above from McQuown. Here they are as presented by Scheflen in a chapter in a book on methods addressed mainly to a psychiatric audience (Scheflen 1966: 270–284):

1. Recording and transcribing using sound motion picture to record both linguistic and non-linguistic behaviors, providing a complete record of what occurred, so that a transcription might be made of everything ("We do not decide beforehand what is trivial, what is redundant, or what alters the system. This is a *result* of the research"; 1966: 270).

2. Ascertaining the structural units (what are the component parts, how are they organized and related to one another, what is the context in which they appear).

3. Synthesizing the larger picture to determine meaning or function (understanding the importance of context for understanding meaning).

4. Setting up the natural history experiment (checking to see what changes if a structural unit is changed or missing).

As noted above, one of Scheflen's innovations in his development of the method was his idea that interaction is *patterned.* As he put it: "Logically speaking, were it not that interactions were patterned, behavior would be unpredictable and

unreliable, and it would be impossible to sustain, mediate, and form human relationships, complete coordinated tasks, and transmit a common culture. Communication depends upon a common behavioral morphology of shared meaning" (1968: 47). Behaviors in interaction are also *context specific*: "each culture and subculture, each institution and each situational and social context has its own programs. In addition, there are multiple roles in each program [...] The point of the program concept is not to deny individual and social diversity, but to identify order" (1968: 47). And also *learned*: "behavior appears in standard units in any culture because the members learn to perform so as to shape their behavior into these molds so that it is mutually recognizable and predictable" (1968: 45). In addition, "[i]n learning the programs organisms come to be people of particular skills and social position and in performing them people make social relations and perpetuate culture" (1968: 48). With these concepts (pattern, context, learning) in mind, researchers examined the film over and over in order to decide what patterns appeared, and how they fit together. As put later by Kendon (1979: 72), "It is one of the principal [sic] aims of context analysis to discern and to give an account of the patterns into which behavior is organized which make communication possible". Others echo this approach: Birdwhistell (1970) stresses all these aspects, of course, but others as well; Duncan (1974: 161) says his research was "designed to discover elements of *structure* in the broader communication context" (emphasis added), structure and pattern being much the same.

Step 7: Creating an archive

Just as there was a preliminary step not made explicit in the NHI listing (here numbered 0), so there is a final step omitted from that list. Birdwhistell pointed out that observing and creating a recording (parts of step 0, as described earlier) and viewing and analyzing the data (steps 1–6) are still not complete.

> From the most technical point of view there are four cardinal steps in the development of valid and reliable social behavioral data: (a) learning to observe; (b) learning to record the component events and relevant context of that which is observed; (c) the organization, preservation, and preparation for analysis of stored data; (d) the development of relevant and efficient methods for the review and analysis of such data. (Birdwhistell 1967: 554)

If more than a few recordings are to be made, and if they are to be maintained over time and possibly re-used for later research studies, or shared with colleagues for their analyses, then there needs to be a system developed to organize an archive.

Each research team develops their own system, but it is the need for a system which is worth mentioning here. In Birdwhistell's time, no move to set up the sort of archive he had in mind was ever made. One attempt at creating an archive for storing films relevant to the kind of research on social interaction envisaged by Birdwhistell perhaps could be identified in the Human Studies Film Archive at the Smithsonian Institution in Washington, D.C., where Birdwhistell's own films are stored. More recently, archives for field linguistic research recordings have been set up in such institutions as the School of Oriental and African Studies (SOAS) in London, or at the Max Planck Institute for Psycholinguistics in Nijmegen in the Netherlands. Field linguists, many as anthropologists investigating and seeking to preserve endangered languages, in recent years are much more aware of and interested in looking at languages in their interactional contexts and many use video recordings in their field work and deposit their material in these newer language archives (the terms of their research grants often require them to do so). But these recordings are by no means always guided by interests in examining languages in their ecological and interactional settings, so how far the materials in these archives will prove useful in interaction research is not yet known.

5 Conclusion

The Natural History of an Interview was one specific project, originally developed by half a dozen people across an academic year, but in the end involving many more researchers and lasting over a decade. The research carried out by the group established at CASBS was never published, although the original team members struggled with that possible outcome for a long time – at one point McQuown described it as "the book with which we are still plagued" (comment in Hayes & Sebeok 1972: 173). The collaboration begun at the Center led to the establishment of a network of researchers into social interaction who shared a common style of research; many of the assumptions and features of their methods have had an extensive influence and have contributed significantly to the way interaction studies are pursued today. It may thus be regarded as a very fruitful and productive collaboration. On the other hand, for various reasons outlined at the beginning of this paper, the *specific* accomplishments of the collaboration, such as the actual analysis of the interaction specimen they used, were never published and this might be seen by some as an indication of a lack of success.

What can later researchers learn from the NHI project? First, NHI involved major scholars from a variety of disciplines, who had previously, in various combinations, collaborated and NHI can be regarded as something of a poster child

for combining disciplines in order to more adequately address real problems. Second, in order to answer the applied question Fromm-Reichmann initially posed in 1956 at CASBS, NHI developed specific tools and techniques, and a set of theoretical assumptions that serve as the grounding not only for that analysis, but also for the analysis of human interaction generally. Most of these tools and techniques and theoretical assumptions are today taken for granted as obvious.

The role of multi-, inter-, and transdisciplinarity in innovative research

NHI is variously described as multidisciplinary or interdisciplinary. There is a distinction between multidisciplinary, interdisciplinary, and transdisciplinary research, but the terms are not always used consistently, and significant distinctions in use appear across national or disciplinary borders. For the purposes of this discussion at least, the following assumptions will be made:

1. *Multidisciplinary* research just requires that people trained in different disciplines talk with one another and attempt to bring their different assumptions to bear on a common problem. NHI started as a multidisciplinary project because it involved psychiatrists, linguists, and anthropologists.

2. *Interdisciplinary* research requires that a new topic be studied, or that it be studied in a new way, achievable only by meshing what participants based in different disciplines take for granted. NHI became interdisciplinary, in that members developed a new question not typically framed as being inside any one discipline (how intuition reveals aspects of communication which might be codified), and then created something new and different in terms of how small details of interaction might be studied (microanalysis).

3. *Transdisciplinary* research either involves an applied focus, or participation of a larger public interested in a topic. The involvement of practicing psychiatrists who were attempting to resolve a practical problem (how to codify Fromm-Reichmann's use of intuition so that it might be taught to other therapists, thus improving patient care) means that NHI was also transdisciplinary.

All three of these approaches require participants to cross disciplinary boundaries, which is possible only because "Academic disciplines are made, not found. They are socially constructed, just like ideas, organizations, identities or relationships" (Leeds-Hurwitz 2012: 1). Moving from work within a single discipline to work entailing multiple disciplines can be difficult, and many scholars choose not

to go that direction. Then changing assumptions and techniques requires still more flexibility; moving from theoretical research to applied research requires more yet again. It should come as no surprise that most people are not willing to make these moves: at the very least they are challenging and time-consuming. However, they can be rewarding.

The fact that the NHI research team was multi-, inter-, and transdisciplinary was a central characteristic, and necessary to the results; it was not a chance occurrence. Birdwhistell quotes an unnamed executive at one of the major research foundations (likely Lawrence K. Frank at the Macy Foundation) with saying:

> We are at a critical stage in the history of science. We have learned to recognize problems so complex that no one discipline is able to solve them. At the same time we know very little about how to maintain productive collaborative research among scholars representing divergent disciplines. Out of some two hundred or so interdisciplinary projects with which we are associated I doubt if more than ten or fifteen will be sufficiently productive to reach publication. (Birdwhistell 1961b: 106)

Of course, it is ironic NHI was one of the projects that never saw actual publication. Nonetheless, the project had significant impact on research practices, or we would not still be talking about it over 60 years later.

That members of the NHI group had prior multi-, inter-, and/or transdisciplinary experience, mostly through various Macy Foundation Conferences, helped the group get moving quickly (McQuown 1971c: 3). As mentioned above, Fromm-Reichmann, Bateson, and Birdwhistell were all part of the Macy Conferences on Group Processes, while Brosin and Bateson were part of the Macy Conferences on Cybernetics (Leeds-Hurwitz 1994). The Macy Conferences were explicitly designed to provide a context for crossing disciplinary boundaries by inviting small numbers of scholars to sit around a table and listen to one another's newest ideas; the slogan was "novelty from interaction at interfaces of disciplines" (Brosin letter to WLH, August 26, 1991). As Mead points out:

> Such innovators as B. Ruml and Lawrence K. Frank experimented with various ways of breaking down the barriers between subject matter fields – psychology, sociology, anthropology, physiology, endocrinology – such as the establishment of longitudinal multi-disciplinary research projects [...] and the small substantive conference which specifically drew for its membership on many academic fields and many types of practice. (Mead 1968b: 10)

Mead specifically includes the NHI project as an example of time when this worked (Mead 1968a). She felt strongly that complex projects needed multiple disciplines involved in constructing a solution.[22] It is worth noting that she specifically refers to NHI as a "classic interdisciplinary study" (Mead 1975: 210).

Birdwhistell emphasizes a related issue, the importance of the main players knowing one another prior to beginning collaborative research.

> You have to remember that this was a much smaller world then – many fewer people and most of us knew or knew about one another […] This is part of the ferment out of which Macy was born, bringing together specialists who knew (and were recognized in) their own field and who were interested in ideas coming from other disciplines. *This is important*: People well framed in *particular* disciplines got together *as equals*. It was exciting and productive. (Birdwhistell, undated letter to WLH, received August 1991, emphasis in original).

Brosin once explained that the Macy conferences were the answer to the question: "how does a relatively small foundation (not Rockefeller, Carnegie, MacArthur, Johnson) capture the imagination of the intellectual world??" (Brosin letter to WLH, August 26, 1991). As Zabor (1978: 162) points out, for NHI "an immense amount of intellectual synthesis took place as a wide intellectual context and history was brought to bear on specific questions about audible and visible human social communication". Working together in multi-, inter-, and transdisciplinary groups can become accepted practice, but it still requires considerable effort to bring about and carry off successfully. Some of what worked at CASBS was unique to that context, and some transferred readily to the distributed research in the years following. Merton described CASBS as having "institutionalized serendipity": "It was thought possible to provide a microenvironment that would provide opportunity for sustained sociocognitive interaction between talents in different social science disciplines and subdisciplines that would prove

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[22]In a review of a *Darwin and facial expression: A century of research in review,* which contains four separate contributions by several different authors, as well as three pieces by Paul Ekman (known for disagreeing with Birdwhistell on the universality of facial expressions), who edited the volume, Mead argues that "[t]he narrowness and discipline-centric nature of the book is a continuing example of the appalling state of the human sciences, when members of each discipline treat their specialized approach as the only approach" (1975: 210), concluding "[t]aking all of the evidence into account would lead us towards a more comprehensive understanding of human behavior, to a human science instead of a series of one-track trains running parallel, meeting only in denigration of each other" (1975: 213).

to be symbiotic as talented individuals found themselves adopting new paradigmatic perspectives" (2004: 265). Given how strongly participants in the multiple stages of NHI felt about the need for conversations across disciplinary boundaries, it is unfortunate that today it is rare rather than expected to have a research project deliberately designed to be multi-, inter-, or transdisciplinary. This is a battle that has not yet been won.

Of the three disciplines that intertwined in NHI, linguistics and anthropology have a particularly long history of connection (the former having developed from the latter, as organized by Franz Boas in the early 1900s). Today connections between psychiatry and anthropology are rare, but they were more often connected in the 1950s. It was not chance that the applied context for NHI was a psychiatric interview. Aside from the fact that Fromm-Reichman brought a specific question related to her own context, there are several reasons why it made sense to the group. Earlier, Harry Stack Sullivan (who worked with Fromm-Reichmann) had particularly strong connections to Edward Sapir (Newman 1986; Perry 1982). Sapir had developed the 'Culture and Personality' school within anthropology (Sapir 1937). Equally important, both men were already known to others in the NHI group.[23] Brosin (1971c) mentions both and certainly knew Sullivan. Bateson was already working with Ruesch, as documented previously, so the psychiatric context was already one with which he was familiar. As Bateson says in his introduction to NHI, "Psychiatry was evolving away from the exclusive study of the individual patient towards the study of human relationships, most dramatically under the influence of Sullivan" (1971c: 4). Sapir's friendship with Sullivan specifically benefitted the NHI group beyond simply setting up a model of potential collaboration and establishing the elements which fit together: Sullivan founded and edited the journal *Psychiatry* (Murray 1994: 221, fn. 39), which explains why so many of the publications related to NHI appeared there, including Bateson (1958), McQuown (1957), and Scheflen (1963; 1964; 1965).

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[23]Both the anthropologists and the psychiatrists recognized the impact of the early Culture and Personality studies. Birdwhistell suggested that "the primary figure in the background of the micro-cultural analysis of communication is Edward Sapir" (1961a: 47), and Sapir is mentioned throughout the entire NHI manuscript. So it makes perfect sense that Sapir's well-known quote "we respond to gestures with an extreme alertness and, one might almost say, in accordance with an elaborate and secret code that is written nowhere, known by none, and understood by all" (Sapir 1959: 556), which appears in NHI (Birdwhistell 1971d: 21), serves as something of a touchstone for group members, and as a marker for membership in the NHI theory group (e.g., Duncan 1969). Markel, who studied with McQuown at Chicago and then Trager and Smith at Buffalo, says: "It is clear to me that Trager and Smith and McQuown, especially Trager, viewed their mission in this area of the paralinguistic and linguistic analysis of psychiatric interviews as a project assigned to them by Sapir" (in the Discussion section of Murray 1986: 288).

Psychiatry as a context was particularly valuable for the NHI project for several reasons. First, as an applied project, there was substantial interest on the part of the National Institute of Mental Health (NIMH), to support positions and grants for some of the NHI researchers (multiple publications mention grants from NIMH as supporting related research by team members in the 1960s, e.g., Duncan 1975, and Scheflen 1975; see Lempert 2019 for discussion). Second, there was an obvious audience interested in seeing results. For example, in reviewing a book on psychotherapy which includes a chapter by Scheflen on "Natural History Method", Seeman (1972: 287) writes: "Scheflen's paper on communicational research in therapy speaks to a theoretical perspective which is of increasing importance on the national scene […] I would have liked more of such papers" (see Wade 1999 for a more current evaluation of what NHI offered to psychiatrists). Third, a psychiatric interview typically involved very few people, they typically sit still when they talk, the interview is scheduled, with a limited time frame, occurring indoors. All these characteristics mean the context is a particularly easy one to film (Van Vlack 1966a,b). A fourth reason may well have been the high status of psychiatry in the 1950s, and specifically of psychoanalysis.

It is interesting to consider the links between the establishment of a new theory group and inter-, multi-, transdisciplinarity. After analyzing multiple examples of theory groups (which he also terms "research clusters"), Murray concludes that "[a]lthough geographic dispersion is not necessarily fatal to cluster formation, disciplinary dispersion may be. Interdisciplinary status makes cluster formation difficult, because advancement and prestige are determined intradisciplinarily and because education and professional socialization are primarily intradisciplinary" (1994: 485). So, while the combination of disciplines leads to new insights, at the same time, that very combination may make it difficult for any of the participants to gain adequate recognition for the significance of their work.

Developing original research tools, methods, and concepts

Kendon shows the connection between multiple disciplines and new methods, tools, and theoretical assumptions: "the behavior of face-to-face interaction is not adequately encompassed by any one discipline. Though the diverse skills and knowledge such a diverse range of disciplines can provide are needed, it seems that an adequate discussion of these phenomena demands new terms and new concepts which no existing individual discipline adequately supplies" (Kendon 1975: 6). Today, recording and transcribing small details from actual behavior filmed in context is very much taken for granted as the beginning point of most

research on face-to-face interaction, and in that, NHI has had an important influence. As Kendon (1981: 456–457) emphasizes, "Detailed studies of behavior structure [...] could not be undertaken without the availability of a recording technique that makes it possible to reinspect the behavior itself. Thus we could not have witnessed the emergence of the kinds of structural analysis of behavior in interaction that we are here concerned with if a recording technology such as cinematography had not developed" (see also Kendon 1990).[24] Although the term "soaking" is today rarely used, the idea that multiple viewings are essential, that behaviors do not have intrinsic meaning but convey meaning in context, and that communication is multimodal, are all taken for granted.

Kinesics, as Birdwhistell tried to develop it, guided as he was at first by concepts and terminology from structural linguistics, was not brought to any successful fruition, and Birdwhistell did not succeed in formulating a transcription system for body motion in interaction that anyone else could easily adopt. By 1974, Birdwhistell's attempt notwithstanding, Duncan (1974: 163) was lamenting that "[i]n contrast to paralanguage, there was for body motion no available transcription system which could be readily adapted to our purposes". And no one today would use "kinesicist" as their primary identification (Birdwhistell is so identified dozens of times in NHI, both by himself and by others, and occasionally in later publications, such as Watter 2017, or Zabor 1978). But even given a focus on kinesics, elements of success can certainly be found (see Kendon 1972a, for a positive, yet critical assessment of Birdwhistell's kinesics project; see also Kendon & Sigman 1996). Birdwhistell always stressed that kinesics was but one element in the larger communication system, that all the channels convey information through their interrelationships rather than singly, and so all of them need to be studied jointly. At least while at the University of Pennsylvania, Birdwhistell focused more on training students to become good observers and analysts of interaction than teaching them the mechanics of kinesics (Birdwhistell 1977; see Leeds-Hurwitz & Sigman 2010 for discussion). He certainly did publish on kinesics (1968c; 1970 being the best-known), but his larger concern was with communication more broadly understood, and he wrote several widely read more general publications (Birdwhistell 1968a,b; 1971e). Although today Birdwhistell is generally remembered for inventing kinesics, and the NHI project is most often

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[24]Erickson points out that NHI researchers used film, and special projectors which permitted viewing very, very slowly, but the invention of videotapes meant that this was lost. Only more recently, with the use of computers, could the technology again permit frame-by-frame viewing: "the close analysis of human social interaction cannot proceed without use of information storage and retrieval tools" (Erickson 2004: 206).

remembered for expanding our understanding of kinesics, paralanguage, and language, both Birdwhistell and NHI should be remembered more generally for the broader conclusions about communication behavior, specifically that it is patterned, learned, context-bound, multi-channel, multifunctional, and continuous (these terms appear throughout NHI, Birdwhistell uses them in his 1970 book, and they appear in many related publications by the various participants; for discussion see Kendon 1990, and Leeds-Hurwitz 1989a). So, while the details of recording body movements in the way that Birdwhistell proposed did not turn into accepted practice as he had initially intended, the underlying assumption "that bodily motion is patterned" (Kendon 1981: 456) has absolutely come to be taken for granted.

Ultimate influence of the NHI project

One final question to consider: What impact has the NHI project had on how researchers study interaction today? Despite the lack of publication, NHI had much influence on several basics that interaction scholars today take for granted.[25] NHI is an unavoidable "influence shadow" (or perhaps one might say it is like an "infusion", in the background), but it is striking how scholars today provide few acknowledgments of its influence. There is now a strong preference for the study of naturally occurring interaction, for recording that interaction, much of the time choosing videotape over audiotape (so that more than language and paralanguage can be examined), and for transcribing the results in order to show the examples analyzed. As Birdwhistell says in discussing the NHI project, "The advantages of working with naturalistic settings seemed to be demonstrated, too, by this devoted and concerted effort" (1970: xi). It is now accepted that interaction is patterned and structured, that it is learned, and that it varies by context, and that the focus should be on the relationship between participants. Duncan summarizes interaction as "highly structured, rule-governed social phenomena" (1974: 180), and Kendon emphasizes "people are seen as participants in complex systems of behavioural relationships instead of as isolated senders and receivers of discrete messages" (1979: 69). Equally, the assumption that communication behavior is continuous was not taken for granted before NHI yet is widely accepted

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[25]Many of the features of interaction studies listed here are also strongly reflective of the impact of conversation analysis, especially as this was refracted through the prism of Charles Goodwin and certain others. Goodwin himself, of course, encountered both Birdwhistell and Erving Goffman (who himself has been a very big influence on interaction studies) while at the University of Pennsylvania. And he, although a partial participant in the CA fraternity, differed from them in being much broader in his approach and his insistence of using video recordings, never just audio, as the CA researchers confined themselves to for so long.

today. As Kendon points out, "so long as one is in the presence of another, all of one's behaviour is a source of information for another, all of the time" (1979: 69). Putting the pieces together, Kendon explains that "communication in interaction is a continuous, multichannel process" (1990: 15).[26]

Since the collection of documents that comprised the "deliverable" (to use EU jargon) of the NHI project was never actually published, it might seem surprising that it should be chosen to illustrate how a collaboration can have influence on later researchers. Asked directly about its influence, Birdwhistell bemoaned the lack of credit, yet was still convinced the project had had major influence: "There are literally scores of works that are in some way derivative – but seldom by scholars who give credit." He was not talking about subtle, implicit connections, since he continued "at least 10 scholars have worked with the tapes and films" (undated letter to WLH, received April 23, 1984). He also concluded that:

> The final tragedy of this is that we could never find the money to train the people to carry on this work. Very few people continue in training after the Ph.D. and it takes at least five years supervised instruction to record both audible and visible behavior in a micro manner. What we need is some late maturing scholars who are not forced to hurry to publication or fund raising. On the other hand, any serious student of either audible or visible communicational behavior can test or expand our work. (Birdwhistell, letter to WLH, July 26, 1987)

Rather than judging the impact of the project solely based on either publication or acknowledgments, we can use an alternative measure, one which Birdwhistell suggested (discussing someone else's research) in a book review: "Its merit will emerge as its effect on other researchers can be measured" (Birdwhistell 1961b: 108). We think the indications are that its merit has well emerged, using this kind of measure.

Similarly, when Winkin asked permission to translate several chapters into French (for the book published as Winkin 1981), McQuown wrote: "It is regrettable that there has been no follow-up of the many openings to research presented in NHI (generally available since 1971) but that is an oft repeated phenomenon of research initiatives which are 'out-of-phase' with other work in the field, or fields, of the particular period" (McQuown letter to Winkin, June 5, 1981). A decade later Brosin provided a far more positive evaluation: "I think the study of micro-linguistics-kinesic human behavior, which was born at CASBS with

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[26]Continuous behavior is also called a "stream" repeatedly in NHI, and often in later publications, such as Condon & Ogston (1967).

McQuown / Hockett / Birdwhistell, will reach fruition when dedicated talented people continue these very difficult studies. Current research has not yet caught up to 1956 in the study of synchrony – three cheers for CASBS!" (Brosin letter to Philip Converse, December 12, 1991). It is perhaps relevant to remember that he was speaking as a psychiatrist, someone who benefitted from the analysis of therapist interviews, rather than as the one analyzing the data and worrying about passing on analytic techniques, in the same way that McQuown and Birdwhistell did. Were Birdwhistell and McQuown right that the NHI had little follow-up? The answer depends on what sort of follow-up is meant. If follow-up would imply large numbers of scholars who now study kinesics, proxemics, and paralanguage, then clearly the answer is not so much. Kinesics never took off the ground, although the study of gestures, substantially developed by Kendon, a member of the NHI theory group, certainly did; body movements are today studied as parts of a larger whole, often by those using the term "multimodality". Paralanguage is often included in conversation analysis or discourse analysis, and in those same studies of multimodality. Clearly the study of language has greater numbers of followers, whether in linguistics or communication. But there are far more studies of naturally-occurring behavior, and virtually all of these record interaction in order to study it in detail, even if not quite the level of microanalysis that NHI proposed, and most take for granted that interaction is patterned, learned, context-bound, multichannel, multifunctional, and continuous, just as the NHI researchers did. While the early term for the type of analysis used in NHI was "natural history" (starting with the title), later terms included both "structural analysis" (preferred by Duncan) and "context analysis" (preferred by Scheflen); see Kendon (1981; 1990), and Leeds-Hurwitz (1987; 2005), for further information about who used which vocabulary when. While the phrase "natural history" has occasionally been reprised (e.g., McDermott & Raley 2011), today none of these terms is widely used; instead, "microanalysis" (another term often used in NHI) seems more common (e.g., Erickson 1992; 2004; Goffman 1983; Gordon 2011; Kendon & Sigman 1996; Rampton 2013; Wieder 1999).

In the process of developing their research techniques and theoretical assumptions, the core NHI members trained the next generation of researchers. Once the project devolved into small groups working with McQuown at Chicago, Birdwhistell at EPPI and Brosin at WPIC, there was time and opportunity for both. McQuown brought in Starkey Duncan; Birdwhistell brought in Albert Scheflen;[27] and Brosin brought in William Condon. But this is too rigid: in fact, there was

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[27]Technically Scheflen brought in Birdwhistell to EPPI, but Birdwhistell returned the favor by bringing Scheflen into the NHI theory group.

enormous overlap between the groups. For example, Sarles, who studied with Smith and Trager at Buffalo and McQuown at Chicago, and who was part of the team at WPIC, specifically mentions Birdwhistell's influence on him (Sarles 1975: 19, note 1). Similarly, Zabor studied first at the University of Pennsylvania with Birdwhistell, volunteered at EPPI and worked with the NHI materials just after the project ended, studied at the University of Chicago with McQuown and Duncan, and then wrote about NHI for her doctoral dissertation at Indiana University (Zabor 1978: 386). Scheflen moved on to a project in New York (which Kendon joined). So, the people who went on to develop microanalytic techniques came through the forge of NHI, or, as in the case of Kendon, were much influenced by its work, once they became aware of it. The fact that Scheflen, Duncan, Condon were all first associated with NHI and then all accepted as the next generation of interaction scholars provides evidence of impact. The fact that a slightly later cohort, including Frederick Erickson and Ray McDermott, also stress the significance of NHI to their research, provides further evidence of impact. Whether the term used by a particular researcher is language and social interaction, ethnography of communication, conversation analysis, discourse analysis, ethnomethodology, multimodality, gesture studies, or embodied communication, there is a significant debt owed to NHI. Such a debt is rarely explicitly recognized any longer by most of those who owe it if they were not in some way part of the larger theory group, but that does not make it any less real.

Acknowledgements

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Histories of progress and media histories. Response to Wendy Leeds-Hurwitz and Adam Kendon

Seth Barry Watter

Leeds-Hurwitz and Kendon have done a lot to frame our general understanding of microanalysis; anyone interested in the subject owes them a debt. The current chapter (Leeds-Hurwitz & Kendon 2021 [this volume]) is in some ways a synthesis of overviews each has written since the 1980s. Their connection to many of the players involved allows them to write a history rich in its details. Moreover, their experience as practitioners of interaction research gives them an inside view of how the research was done. I learned a lot from reading them. I did, however, find myself wondering what a different kind of history would likely foreground. The history offered here is guided throughout by the question the authors pose explicitly at the end: "What can later researchers learn from the NHI project?" The authors are a part of a research tradition; the readers they address are potentially a part of it too. Work done in the present is indebted to the past, we stand on the shoulders of those who came before, and knowledge accumulates from a common pursuit even if the methods used are sometimes diverse. Such an idea of science is usually known as progress.

Professional historians tend to distrust the idea of progress, at least since Herbert Butterfield's *The Whig Interpretation of History*. They are more often taught to look for "a clash of wills" whose unpredictable outcome we retroactively label progress (Butterfield 1965: 28). My own particular interest as a media historian is in the clash of media, which do not have "wills" but which do have their tendencies. That is, a given medium – or as scientists say, an instrument – is more than just an arbitrary sample of the world. It *is* the world for as long as one engages it. And prolonged familiarity with one medium and not another will likely generate a theory specific to that medium. In the interest of extending Leeds-Hurwitz and Kendon's text, I will give an example from my own research: an

example of a trend within "the movement movement" (Davis 2001) that NHI sits uneasily alongside. For, if NHI is the model for current work on interaction, it would have had to defeat this other model in a clash – defeat it theoretically and media-technologically, which in this case are really the same thing.

The authors mention in connection with Kendon's biography that he began his career under Eliot Chapple, but grew "[d]issatisfied with Chapple's insistence on measuring only the 'actions' and 'silences' of conversationalists." One might have the impression that it was only a matter of adding more details to fill in a picture that Chapple left unfinished. We would still have to wonder about Chapple's "insistence" that his picture be left unfinished and, indeed, attenuated – really no picture at all by common standards. It would have been easy enough for him to film people and study their behavior that way. It would, at least, have been equivalent in cost to designing and manufacturing his Interaction Chronographs. But the entire structure of Chapple's thought and the details of his method were opposed to the use of film as an instrument of research. Microanalysis of film would leave social science "in the Athenian Agora," he prophesied unkindly; and kinesics he considered "a futile exercise" whose notations never reached the deep structure of interaction rhythms (1975: 625; 1982: 50).

What he proposed instead with interaction chronography preceded NHI by over fifteen years. Its intellectual background was an austere form of positivism that the physicist Percy Bridgman had called operationalism. To operationalize one's object was to reduce it to a form that would submit to some standard unit of measure. Therefore Chapple argued that the concept of interaction had no meaning at all unless it meant a form of measurement. Since one could measure time, he measured the length of utterance, which included the body motion that accompanied speech. To record this with greater accuracy he devised a machine. Different versions of his patented Interaction Chronograph had different forms of output, none of which were pictorial—and none of which allowed one to know the content of what was said. Their data could appear as sets of alternating letters, as parallel broken lines, as graphical curves, or as columns of numbers, but these were all just different renderings of various time quantities. The actual form of input remained the same for forty years. When person A or person B was observed as performing an "action," the Chronograph operator pressed a key A or B, and held down this key until the person A or B was finished. The result was a record of who was active, how often, for how long on average, and many other things besides (see Watter 2020).

One of the measures was the adjustment of A to B: how well A and B conformed to each other's patterns. Such an emphasis seems to bring Chapple close

to NHI, concerned as it was with the way that interactions "develop and are maintained." But the similarities end there, for Chapple was essentially a biological determinist. He believed that people's baselines were more or less invariant; that what we call life is a constant search for other people whose baselines are complementary to one's own in-born rhythm. Two people with high rates of initiating action were not likely to get along; they would interrupt each other constantly and it would be best to keep them separate. The Natural History of an Interview taught a generation that people in proximity are almost always an organic unit. They function conjointly at every level of articulation. For Chapple, on the other hand, two people are lucky if they are able to establish any synchrony at all. They are really separate rhythms that would play like broken records if left to themselves, and this discreteness is always with them. Hence his persons A and B had their separate keys assigned them. Film, of course, embraces all within a frame: it is the medium most appropriate to the communication "matrix" (Birdwhistell 1970: 95), a metaphor that Chapple never used in his work. Nor did he refer to interaction as a "stream", for it never appeared as a stream to his eyes. It was always punctured or intermittent like a line of Morse code. Both Chapple and NHI would agree on the point that Leeds-Hurwitz and Kendon claim is NHI's legacy: the lesson for social research "that bodily motion is patterned". But the implications of pattern are very different for each and this difference can be traced to a difference in media. Of course, some people crossed over from one method to another, such as Kendon himself. It would be interesting to explore more fully how clashes of media can affect the course of a career, a project, a school – with results that neither medium wanted or even dreamed of, to adapt a line from Butterfield's famous polemic.

As a side note, since we began with the question of scientific progress, we might ask which of these methods has found more adherents; and so which one has had – is having – more influence, for better or worse, on conceptions of human life. Birdwhistell once said that the work of kinesics could never be outsourced to a computer. That may be so. Counting and timing, however, are easily outsourced and many devices now exist to keep tabs on people. When we read of wearable sensors or "sociometric badges" worn in the workplace to chart people's contacts – to see who spoke to whom, how long, how often – we may well think that chronography has found more disciples than any other school of interaction study. It is just that these disciples are largely not human, at least not by common standards of what is deemed human.

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The NHI and the emergence of video-based multimodal studies of social interaction. Response to Wendy Leeds-Hurwitz and Adam Kendon

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5.6 Introduction

Initiated in 1955–1956 by an interdisciplinary group interested in the analysis of sound films, with the aim of developing a cybernetic vision of communication and pathology in psychiatry and a new model of social interaction more generally, the Natural History of an Interview (NHI) project constitutes a groundbreaking event in many respects. In particular, it offers an exemplary case for reflecting on the beginnings of video-based multimodal studies of social interaction and, more generally, on the nexus between technological innovations and analytical advances. While the NHI project has been clearly outlined and discussed by Leeds-Hurwitz and Kendon in this volume (see also Leeds-Hurwitz 1987), this response to their chapter aims at reflecting on what constitutes its novelty, by relating it historically and conceptually to contemporary endeavors in video analysis, with a special focus on the early work of Charles and Marjorie Goodwin, who are central contemporary references for multimodal approaches to video-based analyses of social interaction.

Exploring the new possibilities offered by sound film for studying human interaction, the NHI was the first project that made substantial use of film technologies for capturing the details of talk and embodied movements in their fine-grained temporality and precise coordination. This text reflects on the articulation between recording technologies, transcription practices and new models of social interaction (Kendon 1979; Erickson 2004; 2011), with a particular focus

on the use of video in conversation analysis (Goodwin 1993; 2018; Heath et al. 2010; Knoblauch et al. 2006; Mondada 2006; 2012; 2021). This articulation characterized the initial NHI discussions in 1955–1956 (§2) and was further developed as the project continued until 1968, especially in the work of Birdwhistell, Scheflen and Condon (§3). These scholars were also in contact with Goffman and the group which became a reference for micro-sociology, ethnography of communication and conversation analysis, within which future leading figures of present-day multimodal analysis, Charles and Marjorie Goodwin, were trained (§4). This response shows the enduring influence of the NHI for establishing a web of questions that are still central today, and for creating a network of scholars who opened up possible future pathways, and were crucial for the training of contemporary scholars in video studies and multimodal analysis.

5.7 Some innovations of the Natural History of an Interview project

The NHI was a revolutionary project for the study of social interaction in many ways. I insist here on the implications of the historical fact that it is the first project to take full advantage of the analytical potential of film with synchronized sound. As described in detail by Leeds-Hurwitz (1987) and Leeds-Hurwitz & Kendon 2021 [this volume], the NHI started as an interdisciplinary project involving psychiatry (Fromm-Reichman, Brosin), linguistics (Hockett, McQuown), kinesics (Birdwhistell), and anthropology/cybernetics (Bateson). The NHI members collectively engaged in the study of film materials provided by Bateson, most prominently what has been called the "Doris film" (film GB-SU-005 by Bateson and Myers) – in which Doris talks with Bateson in her living room, in presence of her young son Billy playing around, while Myers, the cameraman, films them. The NHI's exclusive focus on the film materials, rather than more generally on group communication or family therapy, privileged the *filmed* interaction, treating the film as preserving the naturalistic setting so documented (vs. the filmed practices that documented it, see Engelke 2021 [this volume] and Watter 2017: 47). This enabled a full appreciation of the analytical potentials of film for analysis (§2.1), the development of a sophisticated way of transcribing the action in the film (§2.2) and the elaboration of analyses sensitive to the details of social interaction (§2.3).

5.7.1 A naturalistic approach of filmed materials

Since the development of "chronophotography" and motion picture technology at the end of the 19th century (Tosi 2005), with Marey (1896) and Muybridge (1887), moving images have been used continuously in the scientific study of human behavior in the form of body movements (Mondada 2021), especially in anthropology (Hockings 1975; Ruby 2000). But for a long time, the film technologies that were available for social scientists in practice were limited to silent film. Even if commercial cinema introduced sound films in 1927, it was only after World War II that sound film became financially, materially, and practically accessible to the social sciences.

Bateson had experimented with film recordings when working with Mead in Bali already in the 1930s. Film, together with photography and fieldnotes, offered them a vivid way to document ordinary life. However, despite their richness, these materials were only loosely interconnected. Even if Mead's and Bateson's collaborative fieldwork was based on a division of labor in which she took notes about what people were saying while he was shooting silent films and photographs of what they were doing (Bateson & Mead 1942: 49–59, Jacknis 1988), this was not enough to provide for a fine-grained, precise analysis of the coordination of language and embodied action.

Later on, developing his interest in mental health with psychiatrists, Bateson became involved in the production of a series of 16mm films of therapy interviews, in a collaboration with Ruesch (a psychiatrist) and Kees (an experimental filmmaker) in Berkeley (Ruesch & Bateson 1951; Ruesch & Kees 1956) and later with Weakland and Haley at the Veterans Administration Hospital in Palo Alto (Bateson et al. 1956; Haley 1962). Kees played a crucial role in developing filming techniques for realistically capturing the ordered but fleeting patterns of communication (in "films that permit us to look at human beings as they actually are"; Ruesch & Kees 1956: 12). Film enabled them to make visible otherwise imperceptible details, small but crucial for revealing the mechanisms of mental life. This filmic approach was motivated – and at the same time made possible – by a conception of mental illness as generated by specific ways of communicating in groups like the family, as opposed to being located in individual pathologies. Mental illness was defined in terms of small recurrent behavioral patterns that could be captured precisely on film (Geoghegan 2017: 72-73). At the same time films were heavily edited for the purposes of communicating findings and to illustrate theories (Engelke 2021 [this volume]), rather than treated as raw materials serving as a basis for detailed analysis, as would be the case in the NHI.

The NHI films provided the basis for elaborating a *naturalistic* perspective on communication: "We start from a particular interview on a particular day between two identified persons in the presence of a child, a camera and a cameraman. Our primary data are the multitudinous details of vocal and bodily action recorded on this film. We call our treatment of such data a 'natural history' because a minimum of theory guided the collection of the data" (Bateson 1971: 6). Bateson's aim was the study of the natural history of human communication (Leeds-Hurwitz 2005). "Natural history" was a method, an approach, a perspective (McQuown 1971b; Scheflen 1971), which participants in the project opposed to the "experimental" approach. In the "natural history" approach, "the organism or group under study is maintained as far as possible under the customary conditions of living", while under the "experimental" method "the subjects are usually approached in the more-or-less specialized artificial conditions" (Brosin 1971: chap. 4, p. 50). Interestingly, a natural approach was also advocated, in the same period, in Sacks' lectures ("sociology can be a natural observational science"; 1989 [1966]: 211, see Lynch & Bogen 1994), and became a key concept in conversation analysis and ethnomethodology (Lynch 2002). The first video-based study by Charles Goodwin (1979) refers to "natural conversation" and this might not only be a reference to Sacks, but also to the NHI.

5.7.2 Transcribing sound film materials

The mere transcription of embodied behavior in the NHI project was not a novelty per se. There is a long history of attempts to annotate body movement. For example, there have long been various notation systems for dance and choreography, and these served as an inspiration to the participants in the NHI. Although dance notation and movement notation have different objectives, they share an interest in analytical precision and timing. Dance notation is intended for instructing future dancers, and aims to reproduce deliberate body forms set out by a choreographer, whereas movement notation captures fleeting movements as they happen/happened, including their uniqueness and contingencies. But both address the order of the moving assemblage, its gestaltic character and its unfolding in time—which film would make it possible to capture.

Influential dance notations emerged as early as the 16th century, such as Arbeau's *Orchésographie* (1588) and Feuillet's *Choréographie* (1700). Feuillet proposed a sophisticated system in which orientations of legs, arms, torso, head etc., with a specific focus on path of steps on the floor, are precisely described by combining elementary graphic symbols. Further systems of notation emerged later on, either based on the trajectories of dance steps on the floor, like Feuillet's, or

rather on time, in linear representations of several lines synchronized with the musical score. This is the case of Saint-Léon's *Sténochoréographie* (1852), in which each movement of each dancer is written above the corresponding note along a continuous temporal line. Saint-Léon is closer to the issues the NHI scholars faced when trying to associate the movements of the body and the progressivity of talk on a timeline. Moreover, Saint-Léon inspired late 19th century notations, such as Stepanov (1892) and Zorn (1987), who were searching for a systematic combination of signs to represent movements as body sentences. These notations might have inspired Birdwhistell too, even if he refers to other predecessors (Craighead 1942, a master thesis in dance, and Pollenz 1949, an anthropology study of ethnic dances), and dismisses the most famous notation at his time, Laban's *Schrifttanz* (also called *kinetography*; 1928, Hutchinson Guest 1970), as too complicated and as mainly used in industry rather than research (1970: 181). He developed his own notation: a temporally organized plurilinear score in which visual signs referring to a systematic inventory of positions and movements of body parts are used. Birdwhistell's notation is inspired by the combinatorial vision of units in structural linguistics and is perfectly fitted with Hockett's and McQuown's linguistic and paralinguistic notations of speech, which complete the NHI transcripts.

Transcripts constitute half of the NHI final report, which runs to 982 pages (McQuown 1971c). Its originality concerns several levels. First, it integrates "two systems of transcription, graphic symbolic, and alphabetic" (1971c: Foreword, p. 7), referring to phono-recording and kine-recording (1971c: Chapter 6, p. 2). More generally, it includes in a multi-layered system of paralinguistic (a term referring to voice qualities and non-linguistic vocalizations; Trager 1958; 1971), prosodic, phonetic and alphabetic notations for linguistics (developed by Hockett and McQuown), and multiple kinesic annotations for body movements (developed by Birdwhistell). These notations further distinguish various levels of granularity, metaphorically referred to by Bateson as the "ladder of Gestalten from the most microscopic particles of vocalization towards the most macroscopic units of speech" (1971: 24). Second, these layers of annotation are spatially arranged as multiple lines constituting a musical score, referring to a timeline that is not expressed in portions of seconds but in frame numbers. This time-based score enables the integration of very different annotations from various disciplines in a unique and cumulative multilayered object, the transcript. Third, these annotations were based on different recorded sources (chapter 6): the interview had been continuously recorded on audio tape, which was listened to "short section by short section" for detailed linguistic and sound annotations, whereas the sound film – which was interrupted every 3 ½ minutes because the 100ft

magazine had to be replaced, occasioning long gaps in the recording (Bateson 1971: 13ff.) – was watched again and again in a silent mode for the annotation of body behavior. Thus, different perceptive practices lay at the foundation of the transcripts, within different modes of listening and viewing the records (under the process that the participants in the NHI called "soaking", McQuown 1971a: 5, Leeds-Hurwitz & Kendon 2021 [this volume]) for more micro or macro details, supported by technologies such as the audio player, the moviola, and the slow-motion projector.

5.7.3 Filming and transcribing for the analysis of social interaction

The transcript is inspired by and in turn makes possible a rigorous conception of social interaction. In this respect, Bateson presents the NHI project as wanting "to see every detail of word, vocalization, and bodily movement as playing its part in determining the ongoing stream of words and bodily movements which is the interchange between the persons" (1971: 9). Likewise, Birdwhistell quotes Bateson's claim that "everything which occurs in a social interaction is meaningful in the sense of being part of the interchange as well as non-accidental" (1971: 1) – a position that is echoed by Sacks saying that "there is order at all points" (1992: 484, lecture 33, Spring 1966), which is considered a fundamental assumption of conversation analysis.

The availability of recording and transcription technologies enabled the NHI to develop a detailed, analytic and holistic view of social interaction as a complex web of relationships, organized in patterns in which all details might matter and which have a predictable order. Their order is conceived in the framework of structural linguistics, in terms of identification of minimal units combined at multiple levels of complexity. Although contemporary multimodal analyses rely on different theoretical principles, some of these issues are still central, such as the definition of the relevant units of social interaction, which continue to be crucially embedded in transcription choices (Ochs 1979).

5.8 Continuing the NHI: the work of Birdwhistell, Scheflen, and Condon

Initiated in 1955–1956, the NHI project continued until 1968, ending with an unfinished manuscript deposited at the library of the University of Chicago (McQuown 1971c; see Leeds-Hurwitz & Kendon 2021 [this volume] for a reconstruction of the (dis)continuities of these meetings). During these years, the participants in the project published their results individually – with all making use of

the film – and transcription-based approach developed in the project (Birdwhistell 1970; Brosin 1964; 1966; McQuown 1957) – and continued to work on ideas the NHI had made possible. Among these developments, I briefly consider further analytical usages of film and video technologies, and their consequences for future research on the multimodality of social interaction. These crystalized around the figures of Birdwhistell and Scheflen – who worked together at the Eastern Pennsylvania Psychiatric Institute (EPPI), the former from 1959 until he joined the University of Pennsylvania at the Annenberg School of Communication in 1969, and the latter from 1956 to 1967, when he moved to the Bronx State Hospital of New York. Condon collaborated with Scheflen and both are mentioned in later NHI chapters. Kendon – who later became a leading figure of gesture studies (1990; 2004) – came from the UK in the mid-1960s to work with Scheflen, and also collaborated with Condon. Scheflen is a key figure, considered as having made Birdwhistell's approach more explicit and systematic, and as having developed it for an overall comprehension of the structure of the encounter rather than only for a microscopic focus on single scenes (Kendon & Sigman 1996).

5.8.1 Developing film and video technologies

Sound film was crucial to making the NHI project possible, and the use of film and video continued to be refined during the 1960s. At the Eastern Pennsylvania Psychiatric Institute, Birdwhistell organized a film and video lab, with slow motion projectors for film and editing tables for video (Watter 2017: 52). In 1960 he hired a filmmaker, Van Vlack, who not only provided for professionally shot films, but also for their theoretical discussion (1965; 1966a; 1966b). He defined the scientific film as a "new type of motion picture […] produced under controlled, explicit conditions to produce a permanent record which may be repeatedly searched for the re-observation and re-analysis of the original ephemeral event" (1966a: 15–16). He also reflected on how technologies enable an analytical vision considering that "events have a regularity that can be isolated and described, so that they constitute predictable sub-patterns within the larger context" (1966b: 3). The identification of these patterns and searching for them depend on the use of slow-motion and stop-frame projectors (1966b: 5), which enables specific practices of perceiving these images, contrasting with practices of watching traditional movies: "observation of the film by trained observers. With repeated observation, scientists experienced in this technique can locate and abstract the patterns from their context. This is quite a different approach than our usual culture-bound passive acceptance of film" (1966b: 5). Practices of manipulating-for-looking also materialized in another device, the "B-roll frame number count

for every frame" (Van Vlack 1966b: 5). This technique involved duplicating the film to create a working copy (the B-roll), on which the references to frame numbers were printed in the upper central margin of the picture. This enabled the analyst to locate a scene within the film at a precise point (see Kendon 1979: 77). More generally, van Vlack shows that the use of moving images is not merely a matter of "watching" but more crucially a matter of equipping the audio-visual analytic perception of the observer.

At the Bronx State Hospital, Scheflen hired Schaeffer (who would write a dissertation on videotape techniques in anthropology, 1970, and who was also a musician) as field director of his Project *Human Communication*. The project included the continuous recordings of family life in six households over several weeks, with portable video and 16mm film cameras, which were remotely controlled. The data constitute a first impressive example of the use of several cameras for longitudinal filming sessions continuously made over several days, preceded by a careful ethnographic approach to securing acceptance of the filming on the part of the participants (Schaeffer 1975). The development of film as well as the emergence of video technologies made possible a further elaboration of the "naturalistic" way of filming, strongly articulated with fieldwork. The increasing portability of the camera equipment enabled the documentation of a diversity of social contexts.

However, even if video was becoming available, film was not superseded: both continued to be used together (Erickson 2011: 181, Schaeffer 1975; Scheflen et al. 1970). More flexible and easier to manipulate, video was used for initial filming sessions, and for rapid feedback to informants; however, film was of much better quality for a more detailed, high-resolution frame-by-frame analysis. Film projectors were of better quality than video projectors. Moreover, film could be projected in slow-motion; for example, with hand crank projectors, advanced by turning a handle, allowing frame-by-frame motion. Editing tables for video produced small and unfocused images, although they caused less wear and tear on the recordings (Kendon 1979: 77). For these reasons, both technologies were used at the same time – along with separate audio tape recording. Video progressively enabled researchers to make their own recordings, whereas the early uses of film relied on professional camera operators, who were also very often artists – like Kees with Bateson, Van Vlack with Birdwhistell and Schaeffer with Scheflen.

5.8.2 More transcriptions

Technologically supported manipulations of film/video produced an array of artifacts, diagrams, motion flow charts, scrolls containing transcripts organized

along horizontal timelines, like musical scores, and other forms of annotations on paper. Recorded by cameras and inspected with controllable projectors, events could be scrutinized for their real-time flow of details, units, boundaries of units, and recursive structures.

Consequently, a number of coding, transcribing, annotating, and charting conventions were proposed during this period. Linguistic, phonetic and paralinguistic transcription was elaborated by Hockett and others in a highly original book, experimenting with the material composition of the pages, integrating transcripts as a book within the book that could be leafed through independently of their analysis (Pittenger et al. 1960). Birdwhistell continued to work on his kinesic notations (1952; 1970). Condon proposed some careful notation of the temporality of embodied details (1970, see below). An alternative system was proposed by Hall (1963), founder of "proxemics" (1959; 1966), the study of spatial distribution of people in social activities, a factor that varies between ethnic groups. Hall's notation system includes not only distance, orientation and body arrangements of the participants, but also the wider sensorium (olfaction, touch) – a dimension that would be considered again much later on (Mondada 2019). Previously, Hall had written, with Trager, a handbook combining proxemic and paralinguistic analyses (1953). Hall, Trager, and Birdwhistell knew each other, after meeting at the Foreign Service Institute, a training facility for diplomats, where they were all teaching (Leeds-Hurwitz 1990; Kendon & Sigman 1996; Watter 2017: 38ff.).

5.8.3 The analysis of spatial and temporal details

Providing the necessary technological support for the collection of data, as well as for the inspection and exploitation of the collected data in transcripts, created the conditions for new forms of analysis that were able to document in detail phenomena that were previously only loosely described in ethnographic accounts or that even escaped notice. Birdwhistell repeatedly highlighted how many details can be seen in a body movement, how film/video enables the segmentation of movement into a multiplicity of units, and how their combination reveals ordered patterns (1970). Whereas Birdwhistell's aim of finding the "grammar" of the communicative system, built, by analogy with structural linguistics, on a hierarchy of units (kines, kinemorphs, kinesic sentences) seems to have failed (Birdwhistell 1970: 197–198), further work by Scheflen, Condon and Kendon explored these patterns, revealing more local but also more systematic orders. I briefly comment on two fundamental features made available by film/video: spatiality and temporality.

Space is the focus of Scheflen's work. Searching for the common denominator between various ways of interacting in therapy consultations, Scheflen proposed a hierarchy of units describing different levels of organization: the point, the position and the presentation (1964) – similar to linguistic units identified in structural linguistics (see Scheflen & Scheflen 1972: 46-47, fn.). The original contribution of Scheflen is to point out that these units, at all levels, refer to the territoriality of the body – and are visible, measurable, photographable and analyzable (Scheflen 1971). On a macro level, echoing Goffman's (1963) interest in forms of co-presence within space, and in distinct body assemblages for focused vs. unfocused interactions (see Scheflen & Scheflen 1972: 35-36 exemplifying these distinctions), Scheflen considered that the way bodies occupy space provides for a specific characterization of the event. On more micro levels, space intervenes in the orientation of fine embodied details, indexed by head movements, gestures, or even moves of the eyelid, working as "markers", such as indicating the end of a sentence (Scheflen 1964: 321, fig. 1; Scheflen & Scheflen 1972: 48ff.).

This primacy of spatiality enabled a view of human interactions focused on the entire body as it is mobilized – and constrained – in its ecology (from the architecture of the crowded Bronx households to the minimal space required for a body to gesticulate). This produced further research highlighting the "proxemic shifts" in an interaction (Erickson 1975), who obtained his PhD under Hall's supervision), the spatial dimension of "distant" vs "close" greetings (Kendon & Ferber 1973), and more generally the spatial "formations" (Kendon 1977) characterizing the relative positions of the participants engaged in an activity. Several decades later, these discussions would be influential for the study of interacting bodies considered in their entirety (Goodwin 2000; Mondada 2016; 2018), and in their ecology, enabling the treatment of the materiality surrounding them and used by them (artifacts, tools, objects, Heath 2012; Heath & Luff 2000; Goodwin & Gwyn 2003; Mondada 2019). They would also be influential for further work on "interactional space" (Mondada 2009) and mobility in interaction (Haddington et al. 2013).

Film also enabled the capturing of the dynamic temporality of these movements. Condon, who collaborated with Scheflen and participated in the late phase of the NHI, was particularly interested in the timing of speech and movements. He was famous for using a modified Bell and Howell time-motion analyzer to facilitate a manual scanning of the film frame-by-frame (1970). This made a detailed segmentation of speech and motion possible, by manually contrasting one segment with the previous one in order to detect isomorphisms and changes. Condon used speed cameras able to shoot 48 frames per second (vs. 24fs for

usual 16mm films), enabling him to double the precision of the sound/film analysis (see Condon 1976: 292, fig. 2 for an example of multilayered transcription using a 48fs-based segmentation). This provided a foundation for his description of synchrony: since different body parts can move independently, it is significant when they move together. By contrast, in pathological subjects, these relations are disarticulated (Condon & Ogston 1966). On this basis, Condon identified two forms of co-ordination: within the individual ("self-synchrony") and between different participants ("interactional synchrony", Condon 1976: 306, fig. 6), in which the participants adopt the same temporal structure for their embodied conduct.

This attention to the detailed timing of speech and body co-ordination anticipates other tools – such as computer-supported aligning software for transcription (e.g. ELAN) – which facilitate the creation of several lines of annotations and measures. They also anticipate transcription systems that are based on multiple superpositions of lines of annotations including the most diverse aspects of talk and the body (Mondada 2018). In addition, the focus on temporal details and their location within the emergent and dynamic flow of actions prefigures the interest not only in co-speech temporal regularities (Kendon 2004; Schegloff 1984) but also in more complex "multimodal Gestalts" (Mondada 2014), which consider holistically several types of resources, each of them characterized by a specific temporality, not mechanically synchronic but relevantly and reflexively adjusted one to another (Goodwin 2000).

5.9 The beginnings of multimodal conversation analysis: Charles and Marjorie Goodwin

At the end of the 1960s, Charles (Chuck) and Marjorie (Candy) Goodwin – whose work today serves as a model in video studies, multimodal analysis and conversation analysis, interconnected with linguistics, anthropology and communication – were beginning their PhDs at the University of Pennsylvania. Their emerging trajectory intersects with the legacy of the NHI, the work of Birdwhistell and colleagues, as well as the academic group around Goffman and Labov.

Chuck and Candy Goodwin repeatedly pointed to the importance of Goffman's and Labov's seminars at the Center for Urban Ethnography (newly founded in 1969, and which also funded Candy Goodwin's dissertation). Goffman arrived at the University of Pennsylvania in 1968, and Hymes was instrumental in bringing him there; Labov joined them from Columbia in 1971. Hymes and Goffman had both previously worked in Berkeley, where Goffman had collaborated with Garfinkel, Sacks and Schegloff (Schegloff 1992). Goffman had various connections

to Birdwhistell, who joined the University of Pennsylvania at the Annenberg School of Communication in 1969: he had been Birdwhistell's undergraduate student in Toronto (Leeds-Hurwitz & Sigman 2010), and both had participated in the Macy conferences on group processes, with Bateson and Mead from 1955 to 1958 (Winkin 1984). Although Goffman and Birdwhistell had different views on social interaction, they had an appreciation for each other's work. Goffman published Birdwhistell (1970) in his series at the University of Pennsylvania Press; Birdwhistell's famous analysis of the "cigarette scene" (1970: 227-240) was re-published in *Directions in Sociolinguistics*, a collective book edited by Gumperz and Hymes in 1972, in the same section as Garfinkel, Sacks, and Schegloff.

The fact that Goffman's group was caught up in the wake of the NHI can also be traced back to a letter Goffman sent to Hymes on October 26, 1967, proposing the organization of a workshop with McQuown, Bateson, Hall, Sommer, Ekman and "people from the Garfinkel school", mentioning Schegloff and Sudnow, as well as Garfinkel himself (correspondence Goffman/Hymes, Goffman archive). This project shows the converging interest on social interaction of scholars coming from the NHI project, Goffman's group, and Californian conversation analysts and ethnomethodologists.

It is in this context that Chuck and Candy Goodwin began their PhDs in 1969. Candy Goodwin was doing fieldwork with Afro-American children in working-class neighborhoods, under the guidance of Goffman, whose approach to social interaction inspired the project, and Labov, who was researching the grammar of Black English Vernacular. Chuck Goodwin wanted to work with scholars who had collaborated with Bateson: in 1969, he became the research assistant of Krippendorff, who was interested in cybernetics and soon became his PhD supervisor at the Annenberg School of Communication. He was also hired as a filmmaker at the Philadelphia Child Guidance Clinic (1971–1974) under the supervision of Haley, who had been Bateson's collaborator. In a way, Goodwin was joining the long line of filmmakers associated with protagonists of the NHI project. Furthermore, Chuck and Candy Goodwin attended Birdwhistell's classes in 1969, where they learned to use his kinesic annotations and analyses, and also saw numerous films shot by Bateson's, Birdwhistell's, Condon's and Scheflen's teams (such as the cigarette scene, Schaeffer's and Scheflen's films on the crowded households in the Bronx, and Bateson's and Kees' as well as Birdwhistell's and van Vlack's films shot in zoos). They were also in contact with Kendon, with whom they intensively examined film data (for example on greetings, Kendon & Ferber 1973). These connections show that even if the NHI did not directly inspire the work of Chuck and Candy Goodwin, they were still exposed to its intellectual spirit and representatives.

The work of Chuck and Candy Goodwin was at the crossroads of several emergent scientific programs. Goffman was influential for his approach of the interactional order, his conception not only of focused interaction, but also of the co-presence of bodies of not-yet-co-participants in space, intelligible thanks to their body-glosses (Goffman 1963; 1971). Although Goffman was critical of recordings and preferred participant observation, his vision of participation as an embodied phenomenon in its material and spatial ecology resonated with Scheflen's work on territoriality as well as Birdwhistell's interests in bodies in motion. Birdwhistell represented another way to approach bodies in interaction, not only through film but importantly through his sophisticated system of annotations and coding. For Chuck and Candy Goodwin, however, his emphasis on the exhaustive annotation of all possible details, within a notational system that integrated large *a priori* lists of pre-defined body-parts, strongly influenced by structural linguistics, contrasted with the approach to transcription they were learning from Gail Jefferson. Jefferson, who had just finished her PhD with Sacks at UCLA, had joined Labov at Penn in 1972, working for him in transcribing data and co-teaching with him a seminar in conversation analysis. She proposed a practice of transcription that was necessarily selective and related to the relevance of details along the emergent sequential organization of social interaction.

In this context, Chuck and Candy Goodwin – who had met in 1969 – developed a way of doing fieldwork collaboratively that was enhanced by the technologies at hand. Candy was strongly engaged in fieldwork and Chuck was developing professional film skills at the Child Guidance Clinic. Together they wanted to effect a shift from the therapeutic room-cum-TV studio toward ordinary social settings, indoors and outdoors – i.e. from arrangements of the participants *for* the camera view to an adjustment *of* the camera work to the filmed events. Confronted with the challenges of naturalistic settings they realized all the advantages represented by the mobile Portapak video camera newly produced by Sony (AV-3400/AVC-3400). Although very heavy and still relatively bulky (it actually required a wagon to be moved, and was almost impossible to carry over the shoulder), the Portapak enabled them to engage in the recording of everyday life, among family, friends, and neighbors, as well as encounters at the meat market. The Portapak made it possible to engage in new forms of naturalistic fieldwork-with-a-camera.

Presented in Goffman's and Labov's seminars, the analysis of these data turned out to be influenced by Jefferson, who guided Chuck's and Candy's Goodwin first steps into conversation analysis (they had already accessed Sacks' lectures thanks to Labov, but met Sacks and Schegloff only later, at the Summer Institute of Linguistics of 1973). In weekly meetings at their house with Jefferson and

Malcah Yaeger-Dror, a phonetician collaborating with Labov, they began to develop a conversation analytic approach to the videos they had collected.

Jefferson's contribution to the history of conversation analysis has been recognized in relation to her effective way of transcribing talk, her system of conventions still used in conversation analysis today (Jefferson 1983; 1985; 2004), and importantly, her analytical attention to the fine-grained temporality and sequentiality of talk – studied in relation to turn-taking (Sacks et al. 1974), overlapping talk as revealing the timing precision of turns-at-talk (Jefferson 1973; 1984; 1988), and the systematic order of repair (Jefferson 1972; 1974; 2017) among other topics. Jefferson's role mentoring Chuck and Candy's Goodwin early career reveals an underestimated aspect of her analytical eye: she embraced the study of video materials very early on, supporting Chuck's dissertation on gaze and embodiment in conversation (1981 [1977]). The intellectual role she played in relation to video has not been documented until now, and casts some new light on the importance of her contribution to the history of conversation analysis.

Jefferson transcribed the verbal aspects of some of Chuck Goodwin's data—for instance, the "cigarette utterance" fragment (so called perhaps in a kind of *clin d'oeil* to Birdwhistell's "cigarette scene"), which would later be published as Goodwin's first paper (1979). This intensive transcription work gave Jefferson a deep understanding of the data, which were discussed in their weekly sessions. More fundamentally, she was drawing their attention to details of gaze and body postures, and under her guidance they progressively discovered and learned to see these details. She also imagined practical ways of preparing the materials for analysis, such as making the suggestion of connecting the Portapak to a TV-monitor. This would enable them not only to watch the activities studied repeatedly (the Portapak camera had playback and slow-motion functions) but, more crucially, to stop the image in order to scrutinize its details.

On this basis, Jefferson developed a technique of tracing, consisting in drawing the silhouettes of the participants by putting a transparent plastic paper ("saran wrap") on the monitor. These drawings are very simple: they focus on the co-participants' body postures, and enable the tracking of their mutual involvement, gaze and manipulations of objects (for instance, cigarettes). Tracings are produced for an entire fragment of talk, tracking the moment-by-moment unfolding of postures: the verbal transcript and pauses are reproduced below each image, and their segmentation adjusts to the relevant shifts in postures and gaze (see Goodwin 1981 [1977]: 144–147 for an example). In the working transcripts, fragments of a few seconds are rendered by dozens of drawings. These tracings are in striking contrast to Birdwhistell's kinesic annotations: whereas the latter

are based on a textual representation, articulated in words and specifically pre-defined semiotic codes corresponding to body movements, Jefferson's notation privileges the holistic representation of the image drawn on the basis of the video footage, enabling one to catch at a glance the participants' reciprocal adjustments. In the NHI, the absence of images is noticeable. In Jefferson's tracings, the image is predominant – this connects her rather to the tradition of studies that made extensive use of photographic stills (Mead & Byers 1968; Ruesch & Kees 1956; Scheflen & Scheflen 1972). However, Jefferson's tracings are distinct in the sense that they enable understanding of the sequential organization of embodied talk, rather than just illustrating specific types of postures and typologies of spatial bodies arrangements.

This contrast between ways of re-presenting action refers to contrasting analytic approaches. Birdwhistell's coding approach was based on hierarchies and combinations of units inspired by structural linguistics, whereas Jefferson's approach was based on the local relevance of units shaped by the participants in the course of their situated emergence, assembled and adjusted by them in response to their recipient's actions. Likewise, in his dissertation, Chuck Goodwin (1981 [1977]: 25-27) reflects explicitly on the conception of units inspired by the structural vs. the sequential view: the former, associated with Duncan (1972) – also relying on Trager & Smith (1957 [1951]), for the suprasegmental phonemic transcription – is a "signal" model of turn-taking, in which some markers indicate that the speaker has come to the end of their utterance; the latter is an "anticipatory" model in which the speaker projects and the recipients anticipate what is to come, and mutually adjust to it, shaping the dynamic emergence of the utterance in an interactive way. Although recognizing Birdwhistell's and Scheflen's work (1981 [1977]: 27), and referring to Condon's (1981 [1977]: 28) notion of synchrony, Goodwin also shows that within a sequential perspective, the emergence of units, especially at transitions between one unit and the next, are made intelligible and anticipable thanks to differentiated projections achieved by distinct multimodal resources (1981 [1977]: 29).

Jefferson's mentoring of Chuck and Candy Goodwin, contains *in nuce* a multimodal sequential analysis of social interaction, which combines the "analytic mentality" (Schenkein 1978) generated by findings concerning the organization of turns-at-talk in interaction (Sacks et al. 1974), together with the analysis of video materials and embodied postures.

This intense analytic engagement with video data is not restricted to Jefferson's collaboration with Chuck and Candy Goodwin; Christian Heath reports very similar experiences: he also reports on the capacity of Jefferson to see things in the video data that nobody else had noticed, her holistic vision of sequential

organization including body postures, her skilled use of tracings on a diversity of data. Moreover, both Goodwin and Heath talk about Jefferson's generosity, not only offering ideas during data sessions but also providing extensive comments and annotations on their manuscripts (this resonates with the experience Goffman himself had with his manuscript on "response cries" 1978, reviewed by Jefferson in a detailed, eleven-page critical analysis, Bergmann & Drew 2015: 10-11). This contrasts with the fact that today Jefferson is above all recognized for her analyses of talk and for the excellence of her verbal transcripts, while her video analytic expertise is never acknowledged in the literature. Jefferson herself never published on video analysis. But her mentoring had a tremendous impact on the work of Chuck and Candy Goodwin as well as of Christian Heath, pioneers in the further development of multimodal analysis within ethnomethodology and conversation analysis.

In sum, at that time, Charles and Candy Goodwin were at the crossroads of various intellectual traditions. They were exposed to a variety of options – fieldwork à la Goffman, attention to speech patterns à la Labov, systematic coding of embodied communication à la Birdwhistell – which made Gail Jefferson's inspiration so valuable and innovative.

5.10 From NHI to contemporary multimodal analysis

This response has shown how the use of technologies for recording, listening/watching and transcribing sound films (and then videos) has shaped notation systems and analytical possibilities. It has shown the permanence of questions about how to adequately record human activities, how to transcribe/annotate/code a multiplicity of resources articulated in time, and how to analyze these resources in order to enrich our understanding of social interaction. It has demonstrated how it is possible to reconstruct a lineage from the NHI to the expanding work of Birdwhistell and his collaborators, and to the training of Goodwin, leading to contemporary multimodal analysis, by tracking the changing articulations between technologies and analytical issues.

Technologies evolve at a fast pace, but the conceptual constraints on adequate recordings for social interaction are relatively stable (cf. Kendon 1979; Heath et al. 2010; Mondada 2012). Despite other theoretical frameworks having replaced structural linguistics and cybernetics, the NHI project remains remarkably current in its holistic-and-analytical vision of social interaction and of the assembling of bodies in their communicative ecology. After a number of decades in which the transcription/annotation/coding of a smaller number of dimensions

was privileged, which enabled systematic analyses, there is again a scientific interest in the ordered complexity of the action of entire bodies in interaction. This is demonstrated by contemporary debates about social interaction and mobility, material and spatial environments, sensoriality, multiactivity, and coordination in silent activities. The study of these phenomena relies crucially on the reflections initiated by the NHI project on recording and transcribing for analysis.

Acknowledgments

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Retracing the NHI. Response to Wendy Leeds-Hurwitz and Adam Kendon

Henning Engelke

In this chapter Wendy Leeds-Hurwitz and Adam Kendon (2021 [this volume]) give a substantial and fascinating account of a project that, even though unpublished, shaped key assumptions about interpersonal communication and marked a cornerstone for interaction research. One of the central theoretical views emerging from the Natural History of an Interview (NHI) was that "face-to-face interaction is a continuous process and it is as much about the establishment, regulation, and maintenance of necessary behavioral interrelations as it is about the transmission of new information" (Leeds-Hurwitz & Kendon 2021 [this volume]: 148). At the same time, the project constituted a groundbreaking example of multi-, inter-, and transdisciplinary research. Combining a social history of the professional networks involved in the NHI project with considerations on its methodological and theoretical implications, this chapter contributes to an understanding of this seminal project that is of interest for interaction researchers, historians of science and media scholars. It draws on but goes beyond Leeds-Hurwitz's (1987; 1989) and Kendon's previous contributions to the field (1979; Kendon & Sigman 1996).

The authors succinctly describe the different methodological approaches and research interests of the core group members at the beginning of the article. It remains somewhat unclear, though, how these differences played out in the research process and how they manifested themselves in the final manuscript. It is also somewhat surprising that the relationship between analog and digital communication is not mentioned anywhere in the text. For Bateson, at least, the project seems to have been closely linked to the question of "what is analogous to what in the analogic messages" (Bateson, *Letter to Robert E. Pittenger, May 27, 1957*). A minor point that comes up in this context: It does not sound quite right to say Bateson's "interest in animal communication" was "beginning to overtake his concerns with human communication" (Leeds-Hurwitz & Kendon 2021 [this

volume]: 160). I think it was rather that he extended his theoretical concern with communication to *include* animals. In a brief autobiographical text (dating from the early 1960s, and written in the third person), he relates his earlier work on river otters ("something like Bertrand Russel's Theory of Logical Types must apply to the communication of animals") to his subsequent research on the double bind (he "continued these enquiries" at the V.A. Hospital), and then to his research on animal communication: "It was clear that even among people, the signals defining these classes of behavior are usually not verbal. Bateson therefore shifted his focus of inquiry to examine such communication at the animal level" (*Biographical Sketch*, n.d.).

Among the reasons why the NHI did not "have a more prominent place in the later development of interaction studies" (Leeds-Hurwitz & Kendon 2021 [this volume]: 151) the authors list that "the Natural History approach may have appeared to be too exotic or esoteric for it to be easily appreciated and also the importance of the kinds of questions that were being asked were also not yet widely appreciated" (Leeds-Hurwitz & Kendon 2021 [this volume]: 151–151). This may be true for interaction studies in communication, sociology and psychiatry. But there seems to have been some early influence on methods of visual anthropology (Jablonko 1968; Mead & Byers 1968; Byers 1972), as well as considerable overlap with tendencies in dance studies and choreometrics (Lomax et al. 1969; Davis 2001; Jablonko 2001). Given that two major contributors to the NHI, Gregory Bateson and Ray Birdwhistell, were, at least nominally, anthropologists and sustained intense contacts with other anthropologists, it might have been interesting to learn more about intersections of the NHI with the emerging field of visual anthropology.

But on the whole, these are minor issues that should not distract from the article's usefulness. Especially worth mentioning is the detailed description of the different academic backgrounds of the researchers: the linguistic, kinesic, and psychiatric perspectives that converged in the NHI. Another important aspect is the instructive account of the methods developed in the NHI. The brief paragraph on "Creating an Archive" suggests interesting possibilities for further research – not only with regard to questions of whether "the materials in these archives will prove useful in interaction research" (Leeds-Hurwitz & Kendon 2021 [this volume]: 178), but also for research on the media history of interaction studies and related fields such as visual anthropology.

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The NHI and visual anthropology. Response to Henning Engelke

Adam Kendon

It is indeed true that in the early 1960s we see the expansion of visual anthropology, and several of the pioneers of this field, such as Allison Jablonko, Paul Byers, Sol Worth, Jay Ruby, among others, knew of Birdwhistell's work and were sympathetic to what he was doing with kinesics and with his ideas about the nature of human communication. There were others too, such as Martha Davis and Irmgard Bartenieff, who were much interested in the role of the body in communication, who knew Birdwhistell and his work. Alan Lomax, in developing his work in choreometrics was also part of the network that included Birdwhistell and others who, in various ways, were related to those who had been involved in NHI. However, since our paper aimed to describe the origins and development of the NHI project specifically, we felt it was a bit beyond our scope to go into these other influences very much. If one were to write about Birdwhistell and his wider influence (which was considerable), then indeed visual anthropology and choreometrics (as developed by Alan Lomax) would certainly need to be discussed, but given the aims we had set for ourselves with this paper, it did not seem appropriate for us to write about these things.

It is certainly important in giving an account of Bateson's work to discuss who he was making films with, so a mention of Weldon Kees is indeed appropriate. Kees could not have had any direct role in the NHI project, however, since he disappeared (and was presumed dead) in July 1955. He certainly had some influence on Bateson, as is suggested in Engelke's chapter, and his account of how Kees worked with Bateson and Ruesch is very useful.

As for the discussion of the issues of coding and the distinction between analog and digital modes of encoding, to my knowledge this distinction does not appear to have received much explicit discussion in the NHI seminars, at least not as far as I have been able to gather from the extant texts. Birdwhistell's chapter for NHI, as republished in *Kinesics and context* (1970), does not have any discussion

of this issue. Bateson, on the other hand, certainly discussed this from an early stage, as may be seen from Chapter 6 of *Communication: The Social Matrix of Psychiatry*, which he co-authored with Jurgen Ruesch (Ruesch & Bateson 1951). The analog/digital issue does get one brief mention in Bateson's chapter in the NHI manuscript where he says, "We have seen that both the digital analysis of 'pip' or 'bit' phenomena and the analogic analysis of total pattern or system phenomena are appropriate on every level" (McQuown 1971: 5). Remember that the years of the NHI were early days in the development of discussions about the problems of coding in these terms. Discussions of this, especially in relation to language, became much more widespread later. As far as I can see, it was not an issue of focus in the NHI discussions, nor did it figure much in later discussions that followed on from the work of NHI.

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On some lessons of the NHI project and its forgotten holism of communication. Response to Wendy Leeds-Hurwitz, Adam Kendon and Henning Engelke

H. Walter Schmitz

In "A Natural History of the 'Doris Film'", H. Engelke 2021 [this volume] sheds light on a very important aspect of the NHI-Project which has remained in the dark up until now: the circumstances under which the film at the center of the project came into being as well as its technical and aesthetic design. Engelke arrives at the following important conclusion regarding the object of observation, transcription and analysis of the participants in the project:

> But ignoring Doris' misgivings also had to do with the researchers' wish to exclusively study *filmed* interaction. The film, in fact, became their prime object of study. Their focus, moreover, was on looking at this film as if it preserved traces of "naturally" occurring behavior. (Engelke 2021 [this volume]: 111–112)

However, if we were to accept Engelke's conclusion, it would mean that the film was not treated as a document to be examined critically for the perspectives it offers and the context in which it came into being, but rather as the event itself that was the object of the investigation.[1] On this view, Bateson's interview with Doris and Myers' film of the interview were conflated within the NHI project, so that film analysis could be undertaken on the assumption or in the hope that it would at the same time constitute analysis of the interview.

This position is not contradicted by what Wendy Leeds-Hurwitz and Adam Kendon write in their contribution on J. Van Vlack's recommendations for filming psychiatric interviews or Adam Kendon's advice for filming interactional

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[1] On the distinction between event, document and transcript, and the relationship between these, cf. Ingenhoff & Schmitz (2000).

events. This is because they refer exclusively to "later modifications and refinements due to the further work of Birdwhistell and Scheflen, also adding observations by Kendon who has discussed aspects of this methodology in several places as an outcome of his collaborations with Condon and Scheflen […]" (Leeds-Hurwitz & Kendon 2021 [this volume]: 166).

It is perhaps a commonplace to say that not only "terminological networks (whether everyday or 'scientific') create observations" and that, as Clemens Knobloch (2021 [this volume]: 45) reminds us in his contribution, the "placing of the camera […brings] an external perspective into play" [„Platzierung der Kamera […] eine externe Perspektive ins Spiel [bringt]"]. But pointing out this methodological weakness of the NHI project overlooks the fact that even today there is hardly a film-based or video-based study of interaction that offers the interested reader a precise account of the spatial, temporal, social and technical aspects of how the sound and film were recorded – including the consequences of these factors for transcription and analysis.

Engelke (2021 [this volume]: 123) mentions in passing what he calls "the NHI group's cybernetically informed perspective on communication systems and observable interaction behavior", but he does not really justify this description. It is true that the concept of communication entertained by the group is not the topic of his interesting paper, but it would still be worthwhile to address this issue. In their contribution, Leeds-Hurwitz and Kendon also have nothing to say about the quite different understanding of "communication" among, e.g., Bateson, Birdwhistell and McQuown, but only offer this rather general observation:

> The theoretical framework that arose from this collaboration supposes that communication in face-to-face interaction is a continuous process and it is as much about the establishment, regulation, and maintenance of necessary behavioral interrelations as it is about the transmission of new information. It supposes that for all participants any aspect of behavior could be communicatively relevant, and it is because of this that a new methodology developed. (Leeds-Hurwitz & Kendon 2021 [this volume]: 148)

Leeds-Hurwitz and Kendon do imply that they see the influence of Edward Sapir's ideas from the time between 1921 (*Language*) and 1934 (entries in the *Encyclopedia of the Social Sciences* on "Communication", "Language" and "Symbolism") in the conceptions of language and communication put forward by Birdwhistell, McQuown, Hockett, Trager and even Brosin. If we were to trace these connections more closely, we would no doubt be able to show that the NHI

project was deeply rooted in the Boas-Sapir tradition of American anthropology (cf. Schmitz 1975). Even the broad range of research questions that Frieda Fromm-Reichmann formulated in her proposal for the project had been anticipated in the Boas-Sapir tradition; for example, in Sapir's papers "Speech as a Personality Trait" (1927a), "The Unconscious Patterning of Behavior in Society" (1927b) and "The Contribution of Psychiatry to an Understanding of Behavior in Society" (1937).

By contrast, Bateson developed his own conception of communication largely independently of the Boas-Sapir tradition through his own ethnographic studies and then soon after under the influence of information theory and cybernetics. We may ask whether his ideas from communication theory did indeed affect the choice of methods or play a role in the analyses and interpretations within the NHI project. Leeds-Hurwitz and Kendon do not address this question directly, but they imply that Bateson showed little interest in the work of the empiricists within the research group.

Since there was clearly a shared conception of communication in face-to-face interaction as a *continuous process*, it would be of interest to us scholars today to examine how participants in the NHI project attempted to do this conception justice in their transcription work. A prerequisite for transcription is having a clear notion of units of behavior with a beginning and an end, which can then be represented with symbols in the transcription system. It may indeed be the case, as Leeds-Hurwitz and Kendon assure us,[2] that the published version of the NHI project had nothing to say about that, but the transcripts themselves and, above all, Birdwhistell's publications could tell us more. For Birdwhistell, communication is a continuous process of interaction that consists of *discontinuous* segments of behavior that are multi-layered and overlap. In the belief that the analytical techniques of structural linguistics were highly productive and in the hope of establishing the links between spoken language and bodily movements, Birdwhistell adopted the methods of a linguist. After all, he argued, we have found out "[...] that body motion communication behavior is both learned and structured" (Birdwhistell 1967: 59), and that the kinesic system structures bodily movements into forms that are comparable to the way in which the linguistic system structures the flow of speech into sounds, words, phrases, sentences and paragraphs. Others challenged or even outright rejected Birdwhistell's position. However, after this discussion concluded, it would seem that scholars simply decided to ignore the problems that even today are still associated with the transcription of so-called non-verbal behavior.

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[2] **"Transcription:** Again, there is a step which remains implicit in the description provided in the NHI compilation of 1971: transcription" (Leeds-Hurwitz & Kendon 2021 [this volume]: 172).

Since Leeds-Hurwitz and Kendon do not go into the process of analysis and interpretation in detail, I would at this point like to draw our attention to an analytical procedure which – as far as I know – was developed and used for the first time within the NHI project, specifically in the analysis of Doris' statement: "I suppose all mothers think their kids are smart but I have no worries about that child's intellectual ability." Birdwhistell described the procedure as follows (Birdwhistell 1973: 235 f.):

> In an attempt to get some kind of perspective upon the lexical aspect of this piece, twelve women of comparable age and social class background to that of Doris were given a typescript in standard English orthography and asked to comment upon it. All except one commented that this was standard "woman talk," i.e., a preliminary apology followed by a proud statement about the child, unusual only in the presence of the "but" rather than the expected "and."

Later Birdwhistell adds in parentheses:

> (It is worthy of note that four of a control group of six women, when showed this sentence among five other sentences and asked to recall them 5 minutes later, wrote this sentence as "I suppose [one case 'guess'] all women think their kids are smart [two cases, 'bright'] and I have no worries [one case 'I'm not worried'] about that child's [three cases, 'my child's'] intellectual ability.")

This procedure of confronting naïve observers with observational data (such as film clips, sound recordings, transcripts, etc) became, for example, part of the so-called *methodischer Dreischritt* (methodical three-step) of Kalbermatten and von Cranach (1981: 91, 93).[3] Under this approach, observational data are used to help explain manifest behavior, data from interviews with the participants in the interaction are brought to bear on conscious cognition, and naïve interpretations are used to elucidate social conventions. The value of the interpretations of naïve observers for research rests on the assumption that:

> What counts as an action is something that we only know through the understanding of its social meaning. We can find out what social meaning an

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[3]On the application of the "methodical three-step" to the examination of non-verbal communication, cf. Erb-Sommer & Schmitz (1989: 101–136).

action has in a community of communication if we take into account the interpretations of naïve observers from this group.[4]

Finally, we may ask what effects or influence the ideas and procedures of the NHI project might have had on subsequent research into communicative interaction. Is it really true that "many of the assumptions and features of their [the NHI researchers'] methods have had an *extensive* influence and have contributed *significantly* to the way interaction studies are pursued today" [emphasis H.W.S.], as Leeds-Hurwitz and Kendon (2021 [this volume]: 178) claim? There is a hint that they might doubt this claim themselves when at the end of their contribution they adopt the slightly more cautious formulation:

> NHI is an unavoidable "influence *shadow*" (or perhaps one might say it is like an "infusion", in the background), *but it is striking how scholars today provide few acknowledgments of its influence.* (Leeds-Hurwitz & Kendon 2021 [this volume]: 185, emphasis H.W.S.)

Of course, influences do not necessarily have to manifest themselves in explicit references and citations. But it is perhaps not unreasonable to request more detailed justification and proof before claiming such a wide-ranging influence for the NHI project:

> Whether the term used by a particular researcher is language and social interaction, ethnography of communication, conversation analysis, discourse analysis, ethnomethodology, multimodality, gesture studies, or embodied communication, there is a significant debt owed to NHI. Such a debt is rarely recognized any longer by most of those who owe it if they were not in some way part of the larger theory group, but that does not make it any less real. (Leeds-Hurwitz & Kendon 2021 [this volume]: 188)

On the contrary, I would argue that it is difficult to make out even a hidden influence of the NHI project in the relevant literature, even in empirical film-based or video-based studies of interaction from the past 40 years – with the exception of Adam Kendon, who can describe himself as "a member of the NHI theory

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[4]Original quotation: "Was eine Handlung ist, wissen wir nur durch das Verstehen ihrer sozialen Bedeutung. Wir können erfahren, welche soziale Bedeutung eine Handlung in einer Kommunikationsgemeinschaft besitzt, wenn wir die Interpretationen naiver Beobachter aus dieser Bezugsgruppe heranziehen."

group".[5] The influence of the generation following McQuown and Birdwhistell extended at most to the end of the 1970s; this is the generation that included such figures as Starkey Duncan, Albert Scheflen, William S. Condon, Harvey B. Sarles and Margaret R. Zabor (see Leeds-Hurwitz and Kendon's bibliography). However, I would argue that after this time there was not only no continuation or further development of Birdwhistell's kinesics or the paralinguistics of Trager and Smith, but that the "holism of communication" propagated by the NHI group – insofar as anyone even remembered this notion – was given up in favor of a strong focus on specific theoretical and empirical points. As a result, the respective objects of study changed significantly, although the explanatory claims were of course not necessarily adjusted accordingly.

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[5]This lack of real influence does not of course preclude invoking, either explicitly or implicitly, the pioneers of the NHI project in order to legitimize one's own video-based studies of interaction, e.g.: "Our multimodal approach to interaction is rooted in a tradition which has emerged around an interdisciplinary interest in the microanalytic study of interaction during the 1950s" (Deppermann et al. 2010: 1701; see also Mondada 2008).

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The heritage of the NHI. Response to H. Walter Schmitz

Adam Kendon and Wendy Leeds-Hurwitz

We agree that most recordings of interaction do not give much in the way of reflexive detail about the process of recording itself. In fact, Birdwhistell was quite interested in this, and often talked about it, though he wrote down his ideas far less often. WLH remembers particular comments made in his classes about the filming of families in ten zoos, and has found a relevant comment described by Catherine Bateson (Gregory Bateson and Margaret Mead's daughter), in her review of that film, where she says:

> With characteristic irony, Birdwhistell ends with two passages that deviate from the plot of the film, the series of zoos, and strengthen its meta-message about the nature of the film medium. The first shows van Vlack spontaneously *turning off* his camera when he realized he had happened to focus on a pick-up in Paris – and then, urged to resume shooting, focussing on a gendarme, a guardian of morality. The second shows a section, ten seconds long, which was almost discarded as poor quality and yet conveys volumes. Again, we get the message that the potential richness of film for anthropology lies in a growing understanding of filmmaking and film analysis; in the process as well as the product. (Bateson 1972: 192)

So at least he made comparable metacommunicative comments about the process on video, for others to discover, even if he did not typically write about this very often.

In his commentary Schmitz asks if it is really true that, as we wrote, "many of the assumptions and features of their [the NHI researchers'] methods have had an extensive influence and have contributed significantly to the way interaction studies are pursued today." In quoting this he added emphasis on the words "extensive" and "significantly". He does agree that there was some influence, but he

claims this to have been limited and that it did not last long. As he puts it, "On the contrary, I would argue that it is difficult to make out even a hidden influence of the NHI project in the relevant literature, even in empirical film-based or video-based studies of interaction from the past 40 years – with the exception of Adam Kendon, who can describe himself as 'a member of the NHI theory group'" (Schmitz 2021 [this volume]: 239). That is to say, Kendon was a Visiting Scholar at the Western Psychiatric Institute in Pittsburgh in 1966–1967 when Brosin and a small team of researchers connected to NHI work were present, which included William Condon, with whom Kendon collaborated. He also was in touch with Ray Birdwhistell in that period. Subsequent to this Kendon joined Albert Scheflen's "Project on Human Communication" at Bronx State, where he remained until 1976, when he left to take a research position at the Australian National University. His work as published from 1972 and subsequently, on his own admission, has been much influenced both by Scheflen and Birdwhistell (see Kendon 1972b,a; 2004; Forthcoming).

It is a pity that here Schmitz does not give any indication as to what "relevant research" he is referring to in which, according to him, "it is difficult to make out even a hidden influence of the NHI project". There has, of course, been a great deal of work on the study of interaction since NHI. The many investigators who have entered this field have done so with different disciplinary backgrounds and certainly much of this work has been initiated independently of the NHI work, or of work done by those who had been influenced by it. However, we think we are right in claiming that, nevertheless, important strands of work on social interaction since NHI do reflect its influence, even if in more recent years this influence is not directly acknowledged. Some of this influence is already well described in the chapter by Engelke 2021 [this volume]; we believe its influence is still evident in much of the work today on "embodied interaction", in the many studies of social interaction from a "multimodal" perspective, even though explicit references to the actual NHI work are not often encountered. The work of Birdwhistell, for example, was important for the growth of interest in the development of and an awareness of the importance of visible bodily action in human communication. Although his attempt to develop a *kinesics* modeled on the concepts and analytic methods of structural linguistics was not further developed in the way he had tried to do (and he himself came to acknowledge that this approach would not be as fruitful as he had originally envisaged), the idea that body motion is patterned and is consistently organized within the communication process is widely accepted today. This is clear from the fact that nowadays it is widely recognized that communication processes in interaction are "multimodal", and that much of what happens in interaction is "embodied". Birdwhistell's work was shaped in

very important ways as a result of his participation in the NHI project. Several of the scholars who are widely cited today as being important in the recent developments of interaction studies, such as Adam Kendon, Charles Goodwin, and Jürgen Streeck, were all influenced by the approach to the analysis of communication developed by the NHI project. This we have already noted in the case of Kendon; Charles Goodwin, as a graduate student at the University of Pennsylvania, where he primarily worked with Goffman, Birdwhistell's early student and later colleague – he cites both Birdwhistell's individual publications and the entire NHI as edited by McQuown (Goodwin 1981 [1977]; see also Erickson 2019); Jürgen Streeck was much influenced by Albert Scheflen who was, as already described, a close and longtime collaborator of Birdwhistell. The "multimodal" or "embodied" approach to the study of interaction, well exemplified by the volume edited by Streeck et al. (2011), although shaped in important ways by the approach developed in Conversation Analysis, insists on the importance of visible bodily action in the interaction and this insistence is a direct consequence of the work of scholars such as Streeck, Goodwin and Kendon, as may easily be verified by checking the bibliographies of the papers in this book. It is our claim that anyone who takes for granted that communication is patterned, learned, context specific, and multichannel (multimodal) is relying on ideas that were, early on, explicitly put forward and advanced through NHI, and through those who were immediately influenced by them.[1]

To reiterate: we have not argued that either Birdwhistell's kinesics or Trager's paralanguage started a major new research strand continuing today, but rather that the larger understanding of the value of analyzing the pattern, structure, and order underlying interaction as proposed by the NHI researchers as a group, what in this volume is termed "holisms of communication", is precisely what has had substantial influence on future generations of researchers, and their choices of what to study and how to study it. Birdwhistell, and the NHI project, get credit for "attending to the behavioral 'atoms' of everyday life, details that could be seen and heard in all manner of transactions", in Davis' phrase (2001: 43) – and more, for ensuring that others paid attention to these behavioral atoms as well. We did not take on the task of following all of those who adopted microanalysis after NHI, but at least we do mention most of the major strands of research developed later by others as a result of this early work. Erickson (2004) has done far more along these lines, with a specific focus on multimodal discourse analysis,

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[1]For example, Hymes' shift from the term "ethnography of speaking" to "ethnography of communication" owed much to Birdwhistell (see Hymes 1967, which could not yet cite NHI since it was still being written, but which does cite multiple sources by Birdwhistell, as well as Bateson and Hockett).

and he specifically talks about how it was NHI which demonstrated that "multimodal analysis of social interaction was the direction to take" (2004: 201). The debt owed Birdwhistell and the NHI project is akin to the debt anyone studying social interaction owes to Erving Goffman, who established the idea that everyday behavior was worthy of attention, even if his name is no longer attached to all publications in that strand of research (Leeds-Hurwitz 2018). Not everyone explicitly attends to the origins of the ideas they espouse but, when writing history, it is an appropriate part of the task to sort out who influenced whom, and where ideas originated and were developed.

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Chapter 6

Ray L. Birdwhistell, "Lecture at American Museum of Natural History, October 4, 1980"

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This 1980 lecture is the last extended statement by Ray L. Birdwhistell, one of the principal contributors to the Natural History of an Interview. The tone is highly informal and the content wide-ranging, including a number of personal and sometimes dubious anecdotes. The principal theme, however, is the difficulty both of making research films and of looking at them properly. Birdwhistell returns repeatedly to the question of the observer's discipline, or the need to develop new orders of awareness, and comparison is drawn to the stain in microscopy that changes the view entirely despite no change in magnification. Other topics discussed include filming psychiatrists, filming football, the history of ethnographic film, the adoption of new instrumentation, watching movies during the Great Depression, and looking in the mirror. The introduction by Seth Barry Watter puts this lecture in the context of Birdwhistell's career and explains the choices made in editing it for publication.

1 Introduction

Among social scientists, Ray L. Birdwhistell (1918–1994) is probably unique for appearing both at the Macy Conferences and in the funny papers in a single calendar year. Seven months before he lectured on "Kinesic Analysis of Filmed Behavior of Children" in October 1955, his alter ego Professor Fleasong held forth on such topics as "How to Jedge Character" in the cartoon *Li'l Abner* for three consecutive Sundays (Birdwhistell 1956: 141–144; Capp 2003: 83–85). Birdwhistell

had gained some notoriety beyond academia after the *Introduction to Kinesics* was published in 1952. Few people would have really understood what it was proposing, fewer still the technicalities of kinesic recording. But the world was more than ready for anything that looked like a reliable method of deciphering the body. Kinesics was easily vulgarized, as soon became apparent, and Birdwhistell would long complain of "the popular charlatans of body motion" who made his work seem like a way to pick up women.[1] He himself had designed kinesics in such a rigorous fashion that very few people ever learned its orthography. On the other hand, he felt that a science should be useful – should promote greater understanding – and to that end he used whatever means he could to disseminate the insights of his time-consuming labors. The means he liked best were his own voice and body. The term lecture-performance has now become trendy but it is most apt for Birdwhistell, "who," said a friend, "is a kind of communication medium himself" (Byers 1972: 192). His role in the development of audiovisual sequence analysis has been thoroughly explored in the preceding two chapters. What the following text displays is something of the personality that so captivated audiences, and through this captivation helped to found a new approach.

Entertainment alone would hardly justify inclusion, and in any case Birdwhistell can be experienced in greater fullness in several audiovisual and audio recordings. Rather, this present text of a lecture from 1980 speaks directly to the concerns of the present volume: how to preserve and analyze interaction holistically. It is, in fact, the last known public statement by a foremost representative of the holistic approach. Indeed, after his book *Kinesics and Context* appeared in 1970, Birdwhistell published very little at all; there are only three essays over the course of the following decade, an interview in *The Kinesis Report* with Ray McDermott published in the spring of 1980; then silence until his death in 1994. "The truth is I never liked to write," he told Martha Davis. "I needed and loved audiences."[2]

Several themes emerge in the course of the lecture, which Birdwhistell gave before a screening at the American Museum of Natural History – part of the Mar-

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[1] Ray L. Birdwhistell, speaking on "Dr. Birdwhistell's Body Language," Fresh Air with Terry Gross, WHYY, Philadelphia, 29 June 1979. Birdwhistell probably had in mind such books as Julius Fast's *Body Language* (1971), which was usually sold as a mass market paperback with a young woman crossing her bare legs on the cover. Compare Byers (1977: 135): "Ray Birdwhistell once told me that most of the inquiries he got from outside academia were from young men who wanted to learn better tricks for making it with women."

[2] Ray L. Birdwhistell to Martha Davis, undated but after 1988 as Birdwhistell has added "EX-" in his own hand above "The Annenberg School of Communications, University of Pennsylvania" on his letterhead. Birdwhistell retired from teaching in 1988. Personal collection of Martha Davis.

garet Mead Film Festival held there every year since 1977. One is the advantages as well as the dangers of technological "prostheses" such as the camera. Whatever one thinks of the justifications given elsewhere for the natural-history style of filming used in context analysis, one cannot say Birdwhistell naively accepted the camera as a form of direct access to events. Being aware of its dangers and "gaining control over your instrument" so as to avoid "systematically recording your own precept" is something that requires constant self-questioning. Another theme is the kind of personal history that prepares one for work in mircoanalysis. This subject can be dealt with only through anecdotes but they are nonetheless suggestive for cultural and media history. Some of these anecdotes are familiar from other sources. We know that Gregory Bateson and Margaret Mead's films were an important early influence, and it is unsurprising to learn that other ethnographic films made impressions on Birdwhistell. But some of these anecdotes are very surprising: that he worked in a small cinema during the Depression, that his father taught him microscopy, that one source for the natural-history style of filming may have been methods for shooting campus football. Some claims – "I had been in vaudeville", for instance – are frankly unbelievable and may have been said in a joking manner to the audience. In the absence of the original audio recording, one cannot be sure.

This brings us to the nature of the transcript itself. It was discovered in the papers of the writer Jane Howard as part of her research for *Margaret Mead: A Life* (1984). More specifically, it forms part of a running typescript commentary of about one thousand pages and is somewhat unusual, in that nearly everything else in the sheaf consists of notes on interviews with Mead's associates. This may be because Howard does not appear to have conducted an interview with Birdwhistell; perhaps the lecture was inserted as its functional equivalent. The document itself shows several peculiarities that an editor must deal with in preparing it for publication. For one, it begins by mixing summary notes with apparently verbatim passages, without clear distinction; but this phenomenon does not seem to occur again. I have dealt with the problem by leaving these beginning lines the way Howard wrote them and simply placing them in italics to mark them off as distinct. The second problem is that it impossible to know how complete the transcription is at any point. Howard often uses ellipses of varying length, from two to five dots – usually, it seems, to indicate a pause or a trailing-off in speech. On one occasion, though, an ellipsis begins a paragraph and this may indicate an omission in the content of the recording, especially as the transition seems rather abrupt. But it is no more abrupt, really, than many of the other transitions in the text as a whole. I have simply removed all ellipses

except those that seem actually to close a sentence, and I render them all as three dots each.

Then there is the difficulty that comes with all attempts to render extemporaneous speech as readable prose. Few people speak in grammatically correct and complete sentences, one sentence after another, and certainly not with the same logical and syntactic consistency that they display in formal writing. Even a practiced presenter like Birdwhistell is prone to trail off, interrupt himself, start over, and sometimes lose the thread entirely. Thus when conference proceedings or transcribed oral histories are prepared, they are usually first submitted to the speakers for correction. Since this is not possible here, an editor is faced with three choices: to let all mistakes stand; to correct them for clarity, but making all corrections known with footnotes and brackets; to correct them for clarity without footnotes and brackets. I have opted for the last procedure so as to avoid making the text an eyesore – in short, to privilege readability. Scholars who desire the *ne plus ultra* in primary sources can consult the original in Howard's papers (Birdwhistell n.d.). The changes, anyway, are not so very many. I have dropped a repeated word here or there, or added an article or preposition where they are clearly intended. I have also freely altered Howard's punctuation, which, being itself an interpretation from an oral source, never had anything sacrosanct about it. Sometimes I have added an entire phrase for clarity and in such cases the addition is placed within square brackets; all bracketed material is to be understood as mine. Often, in the original document, Howard will put words or phrases between forward slashes: this might indicate a lack of clarity in the original recording. I have omitted these slashes as well as any other interjections or queries of Howard's. Abbreviations, shorthand, and symbols have all been regularized.

As for footnotes, these are limited to four purposes: further editorial clarification as seems necessary; brief biographical and background information on people or things mentioned in the text; establishing the factuality of certain claims in relation to other sources; attempts at exegesis of especially cryptic passages and always with reference to other published writings.

2 Ray L. Birdwhistell, "Lecture at American Museum of Natural History, October 4, 1980"

10/4/80, during MM Film Festival, he addresses very crowded room. He looks very well, has apparently recently lost a lot of weight.

Talks of dangers of tape recorder – extent to which it takes over. Problem with computers is that they store things that go into "yes" and "no," bits & pieces, largely about things already stored in words.....must remember that man talks very few moments a day – for hundreds upon hundreds of moments a day when human beings are interrelated, they aren't talking /yet passing/ important, transmissible information

If you confuse the things you fed the computer with the human beings, then you're in trouble. Korzybski many years ago said, "Let's not confuse our maps with the territories."[3] This is multiply true when you turn to one of these *amazing* prostheses, like a tape recorder: a fantastic tool, *almost* as exciting as a plume, a pen, or a lead pencil, a stylus, or a typewriter, or any of the other ways that man extends the product of his observation, the organization of his discipline and his recording, to the point that he's able to use it for storage and for passing to other people who know the code of what gets stored in what.

I even know people who believe in money, and they forget that that's some order of shorthand and regulation, and that to study money is not the way you study economics.

The study of images of people becomes in itself part of the culture we are trying to understand, so that the culture of motion pictures, the culture of tape recording, or incidentally the culture of typewriting – because very few of us have sufficient control not to see the person we're writing to when we try to write an article, and of course it's decent that we do that...I'm not talking about documentaries, documentary people are another kind of people. They're fine, but they're noisy. Any time I look at one of their movies, all I can see is the editor, and the man who does the cutting, and the man who organized the shooting of the picture, and I get into a crowded room – a very crowded room. Any time you fool with the verities of a culture, you get in trouble.

For years when I worked with films of children – particularly sick children – when we were working with child psychiatrists, we always got a guard to stand by the projector because of the number of times we had our film torn up. They could not bear to see the films – because I took immoral pictures, by which I mean ones taken out of the agreement of the conventional style of taking pictures.[4] It's very hard to get a cameraman who'll stay with you if you do that. You've got to

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[3]Alfred Korzybski (1879–1950), author of *Science and Sanity* (1933) and founder of the approach known as General Semantics. His work was popularized in the United States by people such as S. I. Hayakawa and Stuart Chase. One of Birdwhistell's first essays, "Background to Kinesics" (1955), appeared in *ETC: A Review of General Semantics*, the journal of the International Society for General Semantics.

[4]Not reported in other sources; clearly exaggerated for effect.

have a very strong relationship because in a sense you become offensive to them, because you violate…unless you can make some order of strength, some order of relationship, so that they can take pictures without making noises like a cameraman, without projecting on the world "I am a camera." That's very difficult. It's more than one should ask, I think, very often – which is one of the reasons I have tried very hard never to use a professional cameraman.

Another thing is that I've never taken very many pictures, partly because of the hundreds and hundreds of hours that it takes to look at a minute of well-planned film.[5] Film is not necessarily an economic device; it can be a tremendously expensive device. I should like to talk today about taking films on purpose. Historically, if I can go back…(Can you hear me when I'm over here without a microphone? I really feel much easier, unprosthetized.) I went to undergraduate school at Miami in Ohio and because I had been in vaudeville, I knew some of the Schines.[6] I got the job being assistant manager in a small theatre, only one in town, that stayed open Friday, Saturday, Sunday, Monday. It meant that I saw two films over and over and over – didn't have any ushers, so I was also ushering. It gave me an order of patience because I could enjoy doing the movie with them, taking the various parts. The theatre was small: we leased out the back to lovers. Small town back in the Depression. No one had any money, no one had any place to go: students had to be back in the dormitory at 10 o'clock, so we leased the back, 25¢ per couple. I only got 20¢ an hour. I add this because – to explain the myth of how one becomes an anthropologist. I came to understand film because I spent so many hours with it. I had seen some of the early footage that Margaret Mead was making, by my junior or senior year. Because I was trained for it, I was able to abandon it fairly early. The people who got trained later stayed with it because they weren't trained long enough to see some of the difficulties of dealing with the concepts.

I began very early to get interested in what people were doing when they were taking pictures of one another: what was that about? I was to see my first back-from-the-field film in the early '40s, when Ben and Lois Paul brought their film from Guatemala – before a lot of the people began to be "trained" to go into the field to take film.[7] It had an innocence which does not appear later.

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[5]Perhaps Birdwhistell means he did not himself operate a camera often. Hundreds of hours of footage accumulated under his direction at Eastern Pennsylvania Psychiatric Institute over the course of the 1960s.

[6]Either intended as a joke or a complete fabrication. Birdwhistell did attend Miami University in Ohio. Junius Myer Schine (1890–1971) and Louis William Schine (1893–1956) owned a successful theatre chain during the period in question.

[7]Benjamin Paul (1911–2005) and Lois Paul (1920–1979) were both American anthropologists who specialized in Guatemala. They are not, however, known as ethnographic filmmakers.

One of the great things about Margaret's films is that they were not made for people who made films. They were made to try to explain to people who did not know about culture something about culture. They're not necessarily good research films. They were made with an idea in mind. Margaret was *doing* research when she took them, but many of the films themselves were edited with a very different purpose. They are, in a sense, exhortatory in nature. And it's very hard not to make an exhortatory film, as you will see in the first film here today, which is more a film about research than a research film. The second one that we will see later is a film *made* for the purpose of doing research.

My father taught microscopy.[8] He discovered that he didn't like people, he liked animals – then discovered the animals were attached to people. So he became a bacteriologist, discovered *those* were too big, became a virologist. Was crazy about the microscope – loved the time going into the microscope, when he was grinding telescope lenses (or making violins). He would go into the microscope and not be interrupted; he was so *totally* legitimatized by the microscope. And, incidentally, one of the things we always have to watch out for in any paraphernalia – whether it be a tape recorder or a camera or even a projector – is the extent to which we make it an extension of our own interruptibility.[9] The act of using it becomes confused with the act of utilizing it.

It is very hard in the modern world when a great many students want to walk around with a camera in their hand. See, in my day boys and girls went around with a large book, with their finger somewhere in it, which indicated that they had read it. We are now getting a tremendous number of people who do book

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[8]Robert Nevins Birdwhistell (1891–1968) had both MD and VMD degrees. He worked for the Division of Virus-Serum Control, U. S. Department of Agriculture in the 1930s.

[9]Birdwhistell may be trying to say something about the dangers of conventional viewing habits – habits that dictate the moment at which we choose to start and stop recording, or start and stop looking at a film on a projector. This interruption of the stream of behavior would, then, really be a function of our own interruptibility; we let ourselves be interrupted at moments that feel right to us, or when we are bored with what we are seeing. It is customary, for example, when watching a film at home, to wait for the end of a "scene" before pausing to use the bathroom – or simply attend to other things while the film continues to play because the content is deemed unimportant for comprehension. In any case, this problem of seeing in conventional patterns is one that vexed Birdwhistell for many years. Compare Birdwhistell (1970: 150): "First, we have discovered that viewing and listening habits ingrained by a half-century of audience behavior learning tends to control the shapes and sizes perceived by even the most highly motivated research or student spectator. For example, we will tend to register experience in chunks. That is, there seems to be a rhythm of pieces of given shapes and sizes which we, unaware, perceive as the 'something' in 'something has happened'…Performer, recorder, receiver, and spectator accede to a convention. This is difficult to penetrate if one is an investigator, or to vary if one is an artist, and almost impossible to talk about if one is an unconventional spectator."

research by carrying film about and by showing it. If I sound cynical, I'm not really. I'm talking about the difficulty of learning that there is a desperate problem of having to learn how to use film as research. It's as difficult as any other kind of research; all you have is a device that you take records with. The shape and the condition and the culture of the record-taking implants itself between you and your data and becomes a screen.

When I was a very small boy I adored my father, and when he was working with these microscopes – see, this was a period when parents did not have to play with their children, at least fathers didn't. You were supposed to grow up and be an adult; it was not a case where the adult had to come down. But he did look so grown-up using his microscope, and I *wanted* to do it, and I'd say "Daaaddy…" – I talked like that then – "can I look at the microscope, Daddy?" He'd say no. "Mamma! Daddy won't let me look in the microscope!" I could sneak up on it when he wasn't there, but I didn't know how to get the light on.

Finally, after I kept pestering and pestering him, he said, "All right." He called to my mother – whose name was Hattie but he called her Queen – he said, "Queen, the boy has come to the point where he's insisting, so he's going to do it. We'll have no interfering." So I was told that afternoon that, if I looked in it, I'd have to learn how to draw the things I saw. So I looked in…Every afternoon he got home at 4:30, and from 4:30 to 6:30 I sat there and cried because I couldn't get down – I had to be able to copy what was in that microscope. The day came when I drew it and he was pleased with it, and he said, "All right, that's lesson one." And he took a different stain out, put it on the same slide, and showed it to me again. I looked down in there and it was a totally different picture. I am so deeply, deeply, deeply grateful because that was the beginning of my understanding that, if I was ever going to use film, or any other prosthesis – that all you had to do was change the stain, change the small little _____[10] that was between me and that which I had to copy, and it was changed. And I had to know that the mediation was not merely the extension of the prosthesis, the magnification. And I tell you the truth, this is not an apocryphal story, and I almost had to tell it in a drawl, because that was when it occurred.

So that one realizes that any condition that you establish for the recording of behavior is always going to be shaped by the conditions and by the conventionality of observing and recording. That doesn't make it any more impossible than it is with the microscope, the telescope, but it is still not a direct, immediate picture of human experience any more than looking in the mirror is. And I suppose that's the second thing I'd like to talk about. One of the terrible things that

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[10]This is as it appears in the original document.

you discover is that you train yourself to look in the mirror – in what looks like an immediate, absolute, totally unfuckuppable thing like a mirror. And you put yourself in front of it and you see something that is shaped by the nature of your relationship with that image – see that you are not a tabula rasa, you are not unprogrammed, that you are patterned because you are human, because you are regular and regulated, because you are predictable in a culture. Then the shapes you see in the mirror become selections out of the conventional relationship with yourself.

The moment that you know that, the moment that you are willing to accept the fact that the mirror gives you not an objective image but a beautiful subjective image, then you are also beginning to be able to deal with the problem of looking even at yourself.

Very early, I was asked to come down to NIMH and watch psychiatrists watching psychiatrists.[11] And it became very clear very fast that none of them were trained to watch relationships; that what they were trained to do was to watch either the patient or the psychiatrist, and to watch them in turn; that, by and large, many of them never looked at the patient. They recorded the patient in their mind, they recorded an image of the patient, and then recorded what they saw within a range of theory: what the psychiatrist was measuring against his own set of values.

I became interested, in the early '40s, in a group that I observed in a factory: a group of people who were doing time-motion studies. And they were working on trying to make more money for management. And I was working a union[12] at the time, and we were hating them, and fighting them, and trying to force them out. And it became perfectly clear, watching them with a small Bell & Howell camera, that they were beginning to segment motion in a different way than I had ever seen. I had been an athlete.[13] So when I got to the University of Louisville, they had just introduced a new thing, which was a camera taking pictures of football players working on the field – and then showing the football players those pictures.[14] No one else could afford it in those days; this came right out of

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[11]The National Institute of Mental Health in Bethesda, Maryland, founded in 1949. The specific project Birdwhistell refers to is unknown.

[12]Possibly a reference to a 1948–49 study of union leadership in Kentucky, according to a 1952 CV in box B2, folder 1, Margaret Mead Papers and the South Pacific Ethnographic Archives, Manuscript Division, Library of Congress.

[13]Birdwhistell was "a former high school athlete in Cincinnati", according to an article by Gay Talese written in 1958 but published only recently as Talese (2010: 191).

[14]Birdwhistell taught at the University of Louisville as an Instructor in Sociology from 1946 to 1951, then as an Assistant Professor in the Department of Psychology and Social Anthropology from 1952 to 1956.

the grant.[15] And so what you had was, for the first time, the development of a slow-motion analyzer – very easy to work with, still one of the best things you can work with, because it doesn't destroy the film. The problem with many of the new film-readers and the more fancy devices, if you've ever worked with them, is that either the heat or the sheer manipulation eats your film. Heat begins to warp your film; you begin to have great difficulties.

On that old thing there was a little film counter up at the top.[16] It was inaccurate. Then I learned about the B-roll that Van Vlack and I developed.[17] (Margaret always taught me that if you wanted something, you'd better go out and give lectures to get money. So a B-roll came out of lectures.) A B-roll is a second roll that you develop: a strip of film with nothing but frame numbers on it. Put it together and have it printed at the same time you print your picture so that you always know where that particular picture is.

One of the problems that always comes with working with film is that you don't know what the shapes are in there – because the shape you make of a social act, of what you're taught of the culture, is the shape of the social act.[18] Anyone who's ever taken linguistics knows the sharp break that occurred in their

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[15]What or whose grant is unclear.

[16]Possibly the Bell & Howell 173BD "Time and Motion Study" 16mm Projector, which seems to have become available in the early 1950s. It had a Veeder frame counter extending from the arm of the supply reel. It was used in well-known studies by Birdwhistell's associates, such as William S. Condon and Adam Kendon.

[17]Jacques D. Van Vlack (1925–1975), longtime audiovisual technician for Birdwhistell, first at University of Buffalo and then Eastern Pennsylvania Psychiatric Institute. He appears as author or coauthor on many films shot at Eastern Pennsylvania Psychiatric Institute. His earlier films such as *Their Little World*, "set in a handicapped children's camp [and about] a boy with muscular dystrophy in his round of activities," have not been found (Turners 1958: 18). Birdwhistell claims, elsewhere, that Van Vlack also had some training in social science.

[18]This may be a reference to the notion of phonemic significance. The voice makes an apparently infinite variety of sounds but only certain parts of this acoustic spectrum emerge as meaningful within a language. Some sounds, though technically different at the level of acoustic production, will not be sufficiently distinct from one another to have meaning for other speakers of the language, and thus the sounds will be perceived as multiple instances of the same phoneme. Compare Birdwhistell (1952: 16): "In other words, we are concerned here not with the extent or degree or kind of difference in activity stimulated by one set of kines as against another. We are concerned with the variation in the kines within a kinemorph which make for some kind of difference in response." He then discusses an experiment in which people were shown expressions that varied in terms of eye, mouth, and nose position until the variation produced, for the informant, a meaningful difference: "Well, that changes things" (1952: 20). Here, in the 1980 lecture, Birdwhistell may be expanding this idea to talk about social psychology more broadly – as Ruth Benedict had done much earlier in *Patterns of Culture* (1934) with her notion of cultures as "segments" from a "great arc" of human possibilities.

head when they began to discover that words were things on paper, and that the subdivisions of an utterance came out of your theory.

And so the development of a theory with which you can deal with a stream of behavior becomes ultimately critical when you are taking a set of still shots: twenty-four frames a second, with the lapsed time in between. A conventional overlap allows you to make this continuous motion...Lovely, lovely thing. [There's a book called] *Experience and Prediction* by Hans Reichenbach, which deals with the theoretical problem of working with trajectories.[19]

In a very real sense, all that you can see in a film is a trajectory. You deal with the cutting, you deal with the connections. You deal with each of those, either out of awareness or in awareness. If you're going to do research with film, gaining control of your instrument – gaining control over yourself – becomes absolutely necessary. Otherwise, you are not merely but systematically recording your own precept, your own prejudgment. Because it is whatever the film is. It's easier when it is people far away because people can't make a liar out of you, but you're not necessarily doing a better job.

I began to get money. We had gone to the State Department and had done a little booklet called *Introduction to Kinesics*. And then it was forbidden. This was at the time of McCarthy, and they suppressed it because they had a line in there, "Mommy, I want to go to the bathroom." So the State Department suppressed it. And Henry Lee Smith, Jr. stole it from the government and sent it to us in Louisville, and we reissued it, and all our first money came from that little pamphlet.[20] So I've always been grateful to McCarthy, without whom we would never have done some of the early research.

We began to go through the pictures. And we began to discover, when we took the pictures back, that it was perfectly possible to take those to a major meeting and lecture about them and talk about things that were not in the picture – and people were very pleased. This was a very distressing thing: not because we were

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[19]Hans Reichenbach (1891–1953) was a German philosopher of science, sometimes associated with the Vienna school of logical positivism. *Experience and Prediction: An Analysis of the Foundations and the Structure of Knowledge* was published in 1938.

[20]This story of a suppression and theft is not reported in other sources. *Introduction to Kinesics* was originally printed in photo-offset by the Department of State, Foreign Service Institute in Birdwhistell (1952). Other copies bear the University of Louisville imprint. It later became quite rare and could only be had on microfilm. The line from the book is actually, "Mama. I gotta go to the bathroom" (Birdwhistell 1952: 26). Henry Lee Smith, Jr. (1913–1972) was a linguist and one of Birdwhistell's collaborators who, with George Trager, developed the field of paralanguage. He contributed much to the methodology of the Natural History of an Interview while working with Birdwhistell at the University of Buffalo and had previously advised on the development of kinesics when he and Birdwhistell were at the Foreign Service Institute in 1952.

being bad…but what happened was that once I gave a lecture with the wrong film, and nobody knew the difference. And I went back absolutely shocked. That film, in a sense, fit what I said but it was not what I was talking about. I began to ask myself the question. I began to determine that, since I had a small amount of budget and did not want to be a filmmaker, the thing that I wanted to do was film things that were already studied so I could begin to study them all over again. That is, if I knew from the analysis as an anthropologist and as an observer the world I was observing – if I knew the beginnings and ends, so I wouldn't cut it wrong – [I knew] that I would be able to record things that could be studied because I had them in a natural context.[21]

If one becomes innocent enough to think, for example, that if you record the speech of a language you don't know anything about, that there's no way to cut a piece of tape and know whether you have on it a thousand ideas, or one idea, or half an idea – the moment you begin to penetrate below the level of awareness of interaction, the same thing is happening. You are, in a sense, shooting a picture of an unknown universe.[22] And so, to determine the structure of that shape becomes a wonderfully exciting order of discovery because you are, in a very real sense, beginning to enter a world that no one has been in before in awareness. Not because of drugs, not because you're brilliant, not because you're bright, not because you've intuited, but because you're disciplined.

If you were to take something in the shape of a family, how would you study comparatively to help people see what goes on in a family? So that they would not be stuck with the unbelievable conviction that there was something called a nuclear family? In a society in which you have Adam and Eve as an origin? In which you have two grown-up adults, no kinfolk and no forebears and who then have offspring and see this as a natural unit? It is deep within the myths not only of the society but also in the myths of the social sciences to act as though there were something called the nuclear family which was really nuclear. But how would you look at comparative families – not in order to say that these

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[21]Compare Scheflen (1973: 313): "We try to locate a usual activity which people are used to enacting together. We seek participants who are native to the tradition of that transaction and experienced in taking part. And we make our observations under usual and favorable conditions. We prefer to study the transaction at sites where it usually occurs under customary conditions. We have to learn something about the situation to make such decisions. We read the literature about that kind of transaction and talk to colleagues who have worked in that area. Then we interview subjects who are experienced in that kind of transaction. We visit sites where it usually occurs and make preliminary observations."

[22]The original document contains the following clause here after a comma: "as if for example you were multiply magnitude a galaxy and taking pictures of it – it is not within human experience." I did not feel confident venturing a correction that would make this readable.

were different sufficiently, not because you're going to understand *how* they're different – but to know that they're sufficiently different to want to look at them?

That was the reason that Jacques Van Vlack, whose history is an important one, whom I picked because he was not a social scientist – I did not want him shooting pictures of old ideas...He was a great photographer of football games at the University of Buffalo. He was doing a tremendous job of filming because he was able *not* to keep his eye on the ball, and not to film just the ball, but to film the game. That order of sensitivity was what I wanted.

No way to teach a basketball team to play basketball – to take a film always of the ball. You don't follow the ball, you follow the play. The play is a different shape from the ball. The amount you have to know to take a picture of the play is very different than if you kneel down and take a picture of the ball. That means you have to study enough to understand the play before you understand the special variations within the play. And it is those special variations, as well as the things that seem to go beyond that, that...

First film I'm going to show you is a film about filming about filming: families around the world going to a zoo. It ain't a moving picture; it happens to be a record of a speech. Van Vlack and I went to the American Anthropological Association after we had taken the film and cut it, and decided that what we'd like to do is to film me and the audience looking at a film about families in zoos around the world. And only because we wanted to make it clear that it's an exhortatory film, the sound is outrageously bad. The sound that you hear is the sound that comes with very central direction. I should tell you also that we had our cameras confiscated in England, that we were held by the police in France – multiply it around the world. There are countries you can't buy your way out of. So we will see first this film, which is "Zoos Around the World," and at the end we will open for questions, and then we will get ready for the second film – okay?[23]

Acknowledgements

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[23]The film is actually called *Microcultural Incidents in Ten Zoos* (Birdwhistell & Van Vlack 1971) and records a 1966 lecture at the American Anthropological Association. The second film screened at this event in 1980 cannot be known from context.

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Holisms of communication

A central pillar of contemporary communication research is the analysis of filmed interactions between people. The techniques employed in such analysis first took on a recognizably modern form in the 1970s, but their roots go back to the earliest days of motion picture technology in the late nineteenth century. This book presents original essays accompanied by written responses which together create a dialogue exploring early efforts at audio-visual sequence analysis and their common goal to capture the "whole" of the communicative situation.

The first three chapters of this volume look at the film-based research of Gestalt psychologists in Berlin as well as psychologists in the orbit of Karl and Charlotte Bühler in Vienna in the first decades of the twentieth century. Most of these figures – along with many other Central European scholars of this era – were driven into exile in the United States after the rise of National Socialism in the 1930s. This scientific migration led to the cross-pollination of communication studies in America, an outcome visible in the leading project in interaction research of the mid-twentieth century, the Natural History of an Interview. The following two chapters examine this project in its historical context. The volume closes with a critical edition of a treasure from the archives: the transcript of a speech delivered by Ray Birdwhistell, a key participant in the Natural History of an Interview project and founder of kinesics.